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DIFFICULT FOLK?

A Political History of Social Anthropology



David Mills

Difficult Folk?

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Volume 19

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A Political History of Social Anthropology

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ABBREVIATIONS

AAA	American Anthropological Association
ACEC	Advisory Committee on Education in the Colonies
ASA	Association of Social Anthropologists of the UK and Commonwealth
BLPES	British Library of Political and Economic Science
BSA	British Sociological Association
CDaW	Colonial Development and Welfare Act
CRC	Colonial Research Council
CSSRC	Colonial Social Science Research Council
DSIR	Department for Scientific and Industrial Research
EAISR	East African Institute for Social Research, Makerere University, Kampala, Uganda. Renamed MISR (Makerere Institute for Social Research) in 1966
GEC	General Electric Company
HEFCE	Higher Education Funding Council of England
IAI	International African Institute
ICAES	International Congress of the Anthropological and Ethnological Sciences
ICI	Imperial Chemical Industries
IIALC	International Institute for African Languages and Cultures (later IAI)
IRR	Institute of Race Relations
JASO	Journal of the Anthropological Society of Oxford
JRAI	Journal of the Royal Anthropological Institute
LSE	London School of Economics, University of London
MRC	Medical Research Council
OUCA	Oxford University Committee on Anthropology
PEP	Political and Economic Planning
RAE	Research Assessment Exercise
RAI	Royal Anthropological Institute
RLI	Rhodes-Livingstone Institute, Northern Rhodesia

SOAS	School of Oriental and African Studies, University of London
SSRC	Social Science Research Council
UCL	University College London, University of London
UNESCO	United Nations Economic, Social and Cultural Organisation

INTRODUCTION: IDEAS, INDIVIDUALS, IDENTITIES AND INSTITUTIONS

Over the last three decades, scholars in the humanities and social sciences have energetically reflected on their intellectual role, their relationship to the world and their disciplines' potential contribution to it. This has taken the form of some hard and productive self-questioning. Despite the best efforts of sociologists like Pierre Bourdieu, academics have paid rather less attention to the role that universities play in legitimating and sustaining disciplinary knowledges. Many academics now depend upon the intricate and unique intellectual ecosystems that higher education institutions nurture and protect. Yet we still know relatively little about the institutional role universities and funders play in shaping how academic disciplines effloresce, evolve and mature.

This book is a political history of the emergence of social anthropology as an intellectual 'school' and disciplinary identity, with a particular focus on the United Kingdom between the 1930s and the 1960s. My substantive theme is the role that the institutions and resources of the British and imperial state played in fostering the autonomy of this new social science. *Difficult Folk?* tells the story of a tightly knit community of scholars using state funds and patronage to advance a new theoretical and methodological paradigm, steadily isolating themselves from a broader disciplinary community of colonial administrators, amateur scholars and museum curators. In 1930 social anthropologists emphasised their field's practical relevance to potential funders, particularly the British colonial authorities. By 1960 the discipline was sufficiently established for its

leaders to be able to distance themselves from those who sought to 'apply' anthropological knowledge, whether in the fields of race relations, industrial relations or social development. As they staked out a new disciplinary territory, the discipline's protagonists progressively isolated their field from these different publics. Social anthropology's history is a reminder of the protean and always provisional nature of disciplinary knowledge and methods.

This is a book for students of anthropology, their teachers, and all those interested in the political history of the social sciences. This is by no means the first such history of social anthropology. *Difficult Folk?* seeks to complement existing historiographic milestones – most especially work by Stocking (1984, 1991, 1996), Kuklick (1991) and Kuper (1996a [1973]), but also the important contributions of Feuchtwang (1973), Goody (1995), Pels and Salemink (1999a), Barth (2005) and Young (2004). Many senior anthropologists have also offered shorter accounts, whether in detailed obituaries (e.g. Firth (1956) on A.R. Radcliffe-Brown and Firth (1975) on Max Gluckman) or in perceptive autobiographical pieces – such as Fortes's memories of the LSE seminar (1978) or Leach's barbed observations about the 'unmentionable' role of class prejudice in the discipline (Leach 1984).

My own contribution is to draw on recently deposited personal and institutional archives that offer detailed new insights into this period. I use them to show how, during the final years of empire and the domestic higher education, this school acquired the bureaucratic foundations to defend its intellectual territory and sustain its future expansion.

For its practitioners the history of anthropology can be a kitchen-table affair, a recounting that everyone can join in. We are all involved in telling and retelling our disciplinary pasts, through anecdote, gossip and oral memory as much as through teaching and writing. We learn about disciplinary genealogies through our supervisors, we re-shape history through our bibliographies and citations and literature reviews, and we pass on a sense of disciplinary traditions in our teaching. By this definition, most scholars are involved in the production of 'insider histories' for students and colleagues.

How does this book differ from such 'insider histories'? During the thirty-year period I explore, the skein of individual ambitions and rivalries created a dense and tangled social tapestry. It would be easy to focus on charismatic personalities and their academic intrigues, for institutional histories can tend to be worthy, dull affairs. Fortunately, the archives are also full of the messy, complex details of everyday university life. These minutiae serve to bring formal bureaucracies alive. Staying close to the archive also limits any anthropological inclination towards theoretical abstraction. In so doing, I seek to

systematically chart the links between the private and public faces of this new intellectual identity, between the scholarly record and the less visible aspects of institutional politics.

Throughout this book I show how the success of this emergent disciplinary paradigm depended on the interplay of what I call the four 'i's – ideas, individuals, identities and institutions. For obvious reasons, intellectual history tends to dwell on the first of these factors – ideas. In social anthropology this now means the key methodological and theoretical advances of Bronislaw Malinowski or A.R. Radcliffe-Brown, their disputes and debates, their progenitors and disciples. There are numerous accounts of Malinowski's archetypical fieldwork practice and his influential 'functionalist' theories (e.g. Stocking 1983, Kuper 1996, Young 2004). The historians go on to show how Malinowski's ideas and scholarly influence were gradually superseded by Radcliffe-Brown's more formalist 'hyphenated functionalism' (Stocking 1996, 361), based on his re-readings of Durkheim and Mauss.

Whilst intellectual historians try to carefully historicise their accounts, those written for teaching purposes tend to be read with an eye to current concerns. Histories that focus primarily on ideas also risk reifying disciplinary identities and imaginaries. At worst, these histories become Whiggish origin myths, rhetorical pasts written to justify the discipline's subsequent development – what some call 'presentist' historiography (Stocking 1965). Perhaps none of this matters for the new undergraduate, who starts by trying to understand the dominant theoretical schools and their relationships. But it can make it harder to understand the political and social contexts in which theoretical ideas emerge and acquire legitimacy and influence.

My focus is on the inter-relationship of the remaining three 'i's – individuals, identities and institutions. I show how the reputations of individual thinkers depended on their ability to create, promote and manage a new definition of anthropology within the institutions (be they universities, funding councils, philanthropic organisations or professional associations) in which they worked. Success often came to those best able to manipulate the financial and administrative cogs of these institutions. The discipline's epistemological fortunes can also be closely linked to political developments at the end of the colonial era. Changing domestic and international attitudes to the British Empire, the post-war colonial settlement, and a huge expansion in domestic higher education funding in the 1950s and 1960s all had profound resource implications for the new social sciences.

Much of the action in this history revolves around a few highly ambitious and determined scholars, their relationships and their

students. Indeed, some have distilled the founding narrative of social anthropology down to a tempestuous intellectual psychodrama between two charismatic egotists: the mercurial and brilliant Polish polymath Bronislaw Malinowski, and the eccentric Birmingham-born Edwardian Alfred Reginald Brown. Nurturing fierce loyalties amongst their followers, they enrolled their students – Raymond Firth, Max Gluckman, Audrey Richards, Edward E. Evans-Pritchard, Meyer Fortes, Daryll Forde and Edmund Leach, to name a few – into this drama.

With the exception of Malinowski, who died in 1942, this group lay behind the founding of the Association of Social Anthropologists in 1946, the professional association dedicated to the new school. Between them, they sat on the key committees, held the influential posts, charmed the right people, and consolidated a secure place for social anthropology within Britain's elite universities. This makes it sound as if they worked harmoniously together. This was hardly the case – conflicts, disagreements and growing rivalries all shaped subsequent events. Their social, religious and class backgrounds shaped their attitudes and the professional opportunities open to them within the relatively rigid institutional and status hierarchies of British academic life.

The full *dramatis personae* for this institutional history extends greatly beyond this intimate group. Many of the decisions that went social anthropology's way were made by outsiders. Examples would include the political backing offered to Malinowski by LSE Director William Beveridge, or the recommendation by Lord Hailey, an influential imperial reformer of the 1930s, that anthropology should be at the centre of a programme of colonial social research. Allies like this mattered to anthropology. These figures were part of the extensive social network that linked the political 'establishment' and the British upper classes, a network that Malinowski and Radcliffe-Brown both aspired to join. Lord Hailey was a close friend of Audrey Richards's family, whilst Beveridge relied greatly on his personal secretary Jessica Mair, a friend of Malinowski and the stepmother of one of his students, Lucy Mair.

Other figures can only play walk-on roles. Malinowski's disagreements with his senior colleague at the LSE, Charles Seligman, and Radcliffe-Brown's tempestuous outbursts against the physical anthropologists and ethnologists at the RAI are discussed in some detail. Much more could be said about the influential allies of social anthropology, such as the Oxford classicists John Myres and Ranulph Marett, or about its opponents – such as the diffusionist scholar Grafton Elliot Smith at UCL, and ethnologists like Henry Balfour and Beatrice Blackwood at Oxford. Fortunately, these figures are

increasingly the subject of historical attention in their own right (e.g. Riviere 2007).

The action takes place upon a metropolitan stage. The intellectual influence of individual anthropologists depended on their ability to shape policy decisions and access funding. Everyone in this new school of social anthropology had done extensive fieldwork and spent a great deal of time outside the UK. Yet the main action was played out in the meeting rooms of the Colonial Office, the senior common room at the LSE and the pubs of Oxford. Lobbying for funding or challenging policy decisions depended on being on the right committee at the right time. I also chart the changing relationships between anthropology's own institutions. The tensions that developed in the 1930s within the Royal Anthropological Institute (RAI) led ultimately to the foundation of the Association of Social Anthropologists (ASA) after the war. The growing rivalries between anthropology departments at Oxford and LSE, and later between Oxford and Manchester, were equally a symptom of intellectual divergence.

Whose stories can't I include? The most interesting tales are often the ones that might have been (Handler 2001). There are many such 'what-if' histories, glimpses of fascinating intellectual journeys not taken. What if Franz Steiner, Czech refugee and author of an influential work on taboo (Steiner 1999), had not died at the tender age of 44? How might he have influenced the intensely humanistic turn of Oxford anthropology in the 1950s? What if Gregory Bateson, philosophical anthropologist and partner of Margaret Mead, had been offered – and then accepted – the Edinburgh professorship in the 1940s? My choice of focus leads me to neglect other aspects of this past. Because of the increasingly strong demarcation line that academic social anthropologists drew between themselves and applied anthropologists, the important role of administrative and government anthropologists are also only mentioned in passing. Each department has its own folklore and foundation myths, and not all can be granted equal space. The departmental vignettes I do offer – such as on the work carried out in Manchester and Edinburgh – describe less well-known aspects of this past.

Book outline

I begin with the challenge of delimiting academic disciplines through writing their histories. Why not leave this task to the professional historians who are best equipped to call discipline-based scholars to account? Many social scientists feel that, by virtue of their knowledge and training, they are entitled to tell the story of 'their' identity. I

devote the second chapter of this book to the vexed discussion between anthropology and its historians. Whilst the subsequent chapters can be read without recourse to this debate, it explains my own commitment to a carefully historicised social science.

After this cautionary prologue, the book adopts a broadly chronological structure. The early chapters describe the discipline's struggle to gain an institutional presence within British universities in the 1930s. Its successful search for research opportunities, funding and patronage all culminated in the creation of a new professional association in 1946. Each chapter introduces new characters and social networks, and focuses on a different type of institution.

My account begins with a description of the vibrant intellectual life at the London School of Economics in the 1920s and 1930s. The intellectual energy surrounding this new institution, as well as the charisma exuded by Malinowski himself, was in marked contrast to the genteel Victorian intellectual orthodoxy of Oxbridge. I demonstrate the significance of these institutions for our protagonists – Bronislaw Malinowski and A.R. Radcliffe-Brown – as they each sought to promote their own vision of the 'new' anthropology.

By the 1930s social anthropology had begun to don an institutional mantle, as scholarly debates migrated into the institutional niches provided by the Universities of London and Oxford. Cambridge remained relatively marginal, dominated as it was by biological anthropologists and administrative ethnographers.

The migration itself was in part the result of growing tensions between different scholarly factions within the discipline. The Royal Anthropological Institute, derived from the discipline's first professional association (Stocking 1971), represented a colourful pot-pourri of academic interests, with colonial administrators, physical anthropologists and ethnologists all members. It also catered for an enthusiastic bunch of amateur collectors and upper-class explorers. The new theoreticians of social form and function increasingly found they had little in common with this more inclusive vision of disciplinary belonging. Chapter 4 describes how A.R. Radcliffe-Brown and his students sought to take control of key committees of the Institute in 1939. When this failed, the new Oxford Chair, Edward Evans-Pritchard, led the founding of a rival association, the Association of Social Anthropologists (the ASA) in 1946.

At the root of these intellectual tensions was competition for state patronage and funding. Since the beginning of the twentieth century, social anthropologists had repeatedly, and unsuccessfully, sought government funding for their research students. Whilst many anthropologists and administrative ethnographers were employed by colonial governments, the discipline's campaign for a guaranteed

source of research funding paid off during the Second World War. This new theoretical school found that it fitted an emerging political and epistemological conjuncture created by the last days of empire. The felt need to justify Britain's imperial possessions led the British Colonial Office to design an elaborate colonial development and welfare programme – with an extensive programme of social research at its centre. I describe the involvement of anthropologists in designing and influencing this programme, and the rewards that flowed to academic institutions as a result.

The sixth chapter documents the optimistic post-war period of domestic university expansion in the UK. In many ways, it was a golden age for the discipline, yet new posts and research funding created both possibilities and dilemmas. I focus particularly on the creation of new departments of anthropology at Manchester and SOAS in 1949. This development exacerbated growing departmental and individual rivalries, but also ushered in new opportunities for interdisciplinary collaboration and fields of enquiry. Yet by the end of the 1960s many of these trends had been overshadowed by an increasingly stormy relationship with sociology.

The following two chapters focus on particular aspects of post-war anthropological practice, that were seen, at that point, to be extraneous to the discipline's core concerns. Chapter 7 returns to the application of anthropological knowledge, and describes the tensions created by industrialists who sought anthropological answers to growing industrial relations tensions in the 1950s. Chapter 8 explores how a few social anthropologists sought to write about the new post-colonial phenomena of 'race' and racism. I describe Kenneth Little's pioneering work at Edinburgh, and explore how this field of research informed the new field of ethnic and racial studies.

The ninth chapter describes social anthropology's increasingly defensive outlook at the end of the 1960s, and the prolonged epistemological hangover caused by the end of the British Empire and accusations of the discipline's colonial complicity. Rhetorical denunciations apart, the demise of Colonial Office support created a real research funding vacuum. As a result, some urged the discipline to expand, proselytise and spread its wings within British universities. They wanted to see anthropology taught in the new universities and to become part of school A level curricula; more conservative voices urged caution and consolidation. These different visions were played out in debates over textbooks and teaching and even over the membership criteria for the Association of Social Anthropology. The decisions made during this time continue to influence the British discipline today.

Each chapter adds a new perspective on the emergence of social anthropology as an academic practice within British universities between the 1930s and 1960s. Taking up Bourdieu's challenge to make sociological sense of our own academic worlds (1988, 2000), I make use of a diversity of sources – oral, personal and institutional – to paint a picture of the changing social worlds of *Homo anthropologicus*.

In the Afterword I reflect on the contemporary status of disciplines in British universities and ask whether interdisciplinary work is reshaping the social sciences? The expansion of higher education and the funding fashion for interdisciplinary and post-disciplinary research pose questions about the continuing relevance of a discipline-based professional culture. Acknowledging the arguments made by both advocates and critics of this new orthodoxy, I suggest that there remains a place for a disciplinary identity that is able to come to terms with transience. The challenge is to account for, and set limits to, our disciplinary attachments. I offer this book as one such self-accounting.

Conclusion

More than two decades ago, Clifford Geertz argued that disciplinary boundaries were breaking down and intellectual genres were blurring (Geertz 1983). Since then anthropology's historians have begun exploring the changing meaning of disciplinary practice (e.g. Marcus 1999), the decline of national traditions (Stocking 2001a, Barth et al 2005), as well as revisiting less well known anthropological ancestors (Handler 2001). Is this the right moment to replay a discipline-centred narrative, even if one amplifies the political and organisational aspects of this past?

One way of answering this question is to use this history to think about the claims made for social science's interdisciplinary futures. Not everyone is convinced that we are entering a post-disciplinary episteme, despite the utopian claims made for 'mode 2 knowledge' production (Nowotny et al. 2001). Evidence for the continued influence and salience of a disciplinary 'order of things' can be found in institutional histories such as this. The political ecology of universities, along with their complex funding and organisational structures, serves to mitigate against rapid change. The social capital invested by individual scholars in discipline-based appointments, publications and rewards make these structures difficult to dismantle (Henkel 2000). New fields and sub-disciplines do emerge and seek recognition, but this is often alongside, rather than replacing, existing disciplines.

This is not to deny that disciplines do constantly change and evolve. They have a complex symbiotic relationship with universities, funders and other institutions that create and protect this space for enquiry. Perhaps they are best envisaged as tented transit camps, as migrating scholars stop to construct a makeshift epistemological home. These disciplinary camps train novice scholars, legitimate intellectual traditions and explore possible futures. The danger remains that their inhabitants overestimate the significance of the settlement. Whilst material and institutional traces of the camp may remain, its size and intellectual prominence is never guaranteed.

One way of guarding against such 'overestimation' is through thoughtful, critical and empirically informed histories. Beyond anthropology, there is a growing number of studies of social science disciplines (e.g. Lepenies 1988, Soffer 1994, Platt 1996, Halsey 2004, Halsey and Runciman 2005) in what amounts to a new field of 'disciplinary studies'. Within anthropology itself, this historiographic work is now a distinct sub-field of the American discipline. This is yet to be the case in British anthropology departments. In the meantime, I hope this book dispels some of the mystique cast over social anthropology's origins. If it helps the reader understand how 'difficult folk' created, reproduced and sustained a way of knowing about the world, then it serves its purpose.

WHY DISCIPLINARY HISTORIES MATTER

Scholars in the humanities and social sciences tell disciplinary tales with deceptive ease. We tell our histories in private and in public, in gossipy intrigue and in the published record. Yet the narration of these pasts always legitimates some forms and traditions of disciplinary knowledge and practice above others. Intellectual history is never simply self-evident, a neat and seamless evolution of ideas and methods.

If one is to write a disciplinary history, which stories are important to tell, and who is best placed to tell them? Should we focus on the histories of ideas, or histories of the institutions and identities that nurtured individual thinkers? I want to argue that the key to understanding academic disciplines is the relationship between four 'i's – individuals, ideas, identities and institutions, a nexus best understood both historically and sociologically. This principle raises further questions. What should be included, and what excluded, from such histories? How should they frame their subject? Finally, should they be written by 'insiders' or 'outsiders', by professional historians or practitioner anthropologists?

In this chapter I explore the roots of my fascination with these questions. I ask about the implications of defining and delimiting a discipline and its history, about the emergence of a disciplinary way of knowing, and about the emotional, personal and social investments scholars make in these imagined academic communities. I go on to explore the challenge of writing disciplinary histories. Disciplinary historiography is a risky and complex task, especially for 'insiders', and can leave one vulnerable to criticism from historians and one's

colleagues alike. Historians of anthropology and anthropologists themselves have adopted different approaches, leading to territorial skirmishes between the two disciplines over the right to define this past. The debate raises important questions about the role history plays in legitimising disciplinary knowledge.

Why write disciplinary histories?

On the whole, academics tend to be rather unaware of the actual conditions of their own genesis. The symbiotic relationship between universities and scholarship is relatively recent. Until the late nineteenth century, few British scholars occupied academic positions, both because of the religious missions of Oxbridge, and because there were so few university posts to hold. Victorian scholars like Charles Darwin and Francis Galton were never affiliated to universities. For these reasons, the rapid expansion of universities during the first half of the twentieth century makes their developing social and bureaucratic organisation important to understand.

There are good pedagogic reasons for writing sociologically informed histories of the social sciences. Our students deserve nothing less. Intellectual work in any field is narrowed and diminished by a studied ignorance of the theoretical school's original rationale, its values, its principles and its contradictions. But writing a political history of such a school, especially of one as small and seemingly well defined as social anthropology, is not a journey undertaken lightly. In an age when anthropologists are wary of the consequences of their depictions and representations, it takes chutzpah to speak for such self-reflective, articulate and iconoclastic natives. What motivates me? Partly it is a wilful intellectual naivety. As a first-year undergraduate, I remember my fresh-faced bewilderment at encountering the same thinker – Emile Durkheim – being interpreted in radically different ways by teachers on my sociology and anthropology courses. We read different passages of his work, learnt different terms, and thought of him in different ways. I became fascinated by the way two disciplines used the same social theorist to legitimate their own intellectual trajectories.

Three years later, after finishing my degree, the puzzlement returned. I was browsing in a bookshop. I encountered the landmark American volume *Cultural Studies* (Grossberg et al.1992) in a bookshop. I greedily started reading. But I could make neither head nor tail of it. Had I not just done a degree in the study of culture? The topics – AIDS, sexuality, representation – all seemed vitally important. In Stuart Hall's words, these were topics which had 'something at stake'. So why couldn't I understand it?

Perhaps the discipline had a hidden history? After all, histories of ideas are also histories of exclusions, of denials and of disavowal. Amnesia and total recall sometimes coexist in the same account. Long dead scholars are read and referred to as contemporaries whose ideas can be made to illuminate current issues (Handler 2001). Others, who perhaps do not fit so neatly into the currently fashionable genealogy, are quietly forgotten. As di Leonardo comments with regard to American anthropology, the discipline 'embraces its own culture of forgetting and of convenient remembrance' (1998, 15).

How was I to deal with these problems? One way was to gradually accumulate formal, informal and embodied guild knowledge and history, not to mention the all-important gossip, anecdote and oral mythology. The other was to analyse experiences of disciplinary socialisation, reproduction and identity formation in a more scholarly way. I chose the latter path, and my historical research has sought, as any 'good' anthropologist might, to both appreciate and question the discipline's own self-assumptions and self-image.

One question repeatedly troubled me as I undertook this work. Could I tell the history of the new theoretical school in a way that did not take its emergence in some way for granted a priori? Pels and Salemink (1999b, 1) insist that one should not back-project the 'self-image of twentieth century academic anthropology onto all ethnographic activities that played a role in the formation of the discipline'. Did not the 'social anthropology' of my title pre-empt and pre-determine my field of vision? The reactions of anthropologists to this project have been revealing. Several have criticised my implicit definition of the discipline as an institutional presence within British and Commonwealth universities. By doing so, they infer, I end up reinforcing a narrow understanding of the discipline, its elite and their relationships. What of the hidden and neglected influence of Gregory Bateson, they say? What of anthropological practice beyond the university? Or beyond the metropole?

A provocative example of this revisionist history would be Grimshaw and Hart's insistence that 'anthropology's drive for professional status and acceptance by the academy sacrificed much that was new and radical in its twentieth century origins' and that 'accommodation to bureaucracy compromised the discipline's commitment to a conception of science which was open to the democratic impulse of a world in movement' (Grimshaw and Hart 1993, 10). For them, it is a matter of great regret that the theoretical openness of the Cambridge psychologist-cum-anthropologist W.H. Rivers was subsequently overshadowed by the scientific and systematising rhetoric of the Polish LSE-based ethnographer Bronislaw Malinowski.

Implicit in these critiques, I would argue, is a claim to an 'anti-disciplinary' identity, a claim that there was no dominant trend within social anthropology, and that in its initial outlook it was different from other disciplines by virtue of its history, size and epistemology. This claim to exceptionalism exemplifies the very academic identity politics that interests me. The critique also presumes that we know everything we need to know about the 'official history'. I disagree, as I demonstrate in the chapters that follow.

Others have suggested that such a history can and should be primarily a history of ideas – and that institutions are simply a necessary appendage to the life of the mind. Yet the history of the foundation of social anthropology is the history of the expansion of the social sciences within British universities, amidst increasing state and philanthropic funding and patronage. The energy invested in creating and nurturing bodies such as university departments and scholarly associations gave them a life and force of their own that deserves to be explored. Anthropology offers the insight that 'institutions' and 'ideas' are not as opposed as one might like to imagine. It is too easy to think about bureaucracies as rational and impersonal, the very things that scholarly ideas are not. But, as Mary Douglas reminded us, we act through institutions, constructing them in certain ways that allow them to 'think' and act too, conferring identities and classifications (Douglas 1987). We have come to learn how power is located in the informal ways in which institutions, and the people within them, operate. The energy people invest in university departments and scholarly associations gives them a life force of their own. So I make no apologies for foregrounding the politics surrounding academic professionalisation and intellectual work.

Processes of identification are never complete. There will inevitably be those who do not recognise their intellectual world in my portrait. For some, a disciplinary affiliation is less important than their commitment to a particular region or area, or to a particular institution. Others develop more hybrid identities, working within departments of religion or sociology or in museums. My intention is to paint a good enough account, one that explains why a discipline draws heavily upon its past as an intellectual and social resource.

Disciplines as imagined communities

Intellectual arguments are often prefaced with a moment of identification: 'As an anthropologist, I can ...', or 'A sociological approach to ...'. These are analytical short-hands, identity claims to a shared body of professional expertise and methodologies, a way of

establishing consensus and defining the bounds of the debate. The social sciences, as a set of intellectual fields, have a sophisticated level of institutional, as well as methodological, self-awareness. Yet they socialise everything they study more effectively than they do themselves. We now appreciate the psychic and somatic intensity with which social identities are held. Yet the personal investment, both emotional and intellectual, in one's chosen disciplinary 'vocation' can make it hard to stand back and be truly dispassionate about scholarly 'belonging' and affiliation. Belonging and relatedness are themes close to the heart of anthropologists. Never, it seems, quite close enough.

Writing a political history of an academic 'discipline' is a tricky business – not least in defining its limits. For starters, what does one mean by 'discipline'? Is it a genealogy of ideas and research practices, a like-minded community of thinkers and practitioners, a scholarly 'vocation', an embodied social identity, a formalised institutional presence within a teaching curriculum, a way of imagining and engaging with the world, or all of these things? Disciplines are forms of identification and affiliation, social as much as intellectual, psychic as much as political, ethical as much as methodological. Like much lived experience, they are deeply felt. Few histories of twentieth-century British intellectual life get to grips with the intense hold that disciplinary affiliations have on their inhabitants, and the way disciplinary and departmental divides create and constrain scholarly work. The smallest communities are often the most loyal. Social anthropology is no exception. One of the aims of this book is to underscore the influential role of these generative emotional attachments and discontents.

Disciplinary identities depend on their very ordinariness. Think of decisions over where to shelve books in a library, the hoary ritual of the weekly departmental seminar or the preparation of reading lists. The everyday affairs of institutional life are at once tediously mundane and highly significant. Affiliations are unconsciously drawn upon to order everyday conversations, to make sense of intellectual problems, and to provide ethical boundaries from which to judge others. They are also largely taken for granted, viewed as an inevitable and subconscious aspect of one's epistemological tool-kit. This makes them more difficult, and all the more important, to depict and understand.

The process of 'disciplining' both hones and delimits creativity. It lends social capital to those with the best ability to sense and pick up the tacit and embodied knowledges that all social identities confer. A sense of disciplinary history is often conveyed in casual conversation or through anecdotal memories, heightening the air of mystique surrounding them. Journals and books act as official disciplinary archives, effacing other struggles and other histories. Such implicit

understandings are particularly puzzling for students or those new to disciplinary guilds. Neophytes find themselves asked to genuflect before key individuals and ‘their’ ideas, sometimes without understanding the historical and contextual reasons for their importance.

I have argued that one can explain an academic discipline in any number of ways: as an intellectual endeavour, as a departmental and institutional profession or as a set of engagements – through teaching, application and practice – with a broader public culture. All too often it is the intellectual endeavour that is privileged. This is hardly surprising, for theorising is ultimately what most humanities and social sciences academics ‘do’. Yet the power of academic disciplines, particularly in the humanities and the social sciences, lies in their Promethean nature. Different definitions can be called upon to different ends. The creative admixture of such understandings is key to understanding the shape and cultures of the humanities and social sciences today. A vision of disciplines as ‘tribes and territories’ (Becher 1989) is too static and territorial – divisions are imagined and embodied as much as they are enacted or enforced. It also effaces the increasingly powerful role universities have played in supporting and mediating disciplinary identities. Without institutional legitimation, scholar enthusiasts remain ‘*sans papiers*’, outside the powerful status hierarchies historically constructed around the ‘idea’ of the university.

Bourdieu offers pithy insights into the ‘gold-fish bowl’ vision that can result from a ‘scholastic’ disposition. He sees scholarly detachment as both ‘liberatory break’ and a ‘potentially crippling sensation’. Bourdieu compares it to being a fish in the water ‘in the situations of which their disposition is a product’; like fish we find it hard to articulate how we managed to swim rather than sink. It is from what he calls the ‘supremely banal’ social history of educational institutions ‘that we can expect some real revelations about the objective and subjective structures that always, in spite of ourselves, orient our thought’ (Bourdieu 2000, 14). As Fuller (1993, 126) similarly notes:

the discipline is one of the few units of analysis that requires the co-operation of rival historiographical approaches in science studies: the internal approach, devoted to charting the growth of knowledge in terms of the extension of rational methods to an ever-larger domain of objects and the external approach, devoted to charting the adaptability of knowledge to science’s ever changing social arrangement.

This chimes with a general criticism made of disciplinary histories – that they are often somewhat insular, understanding their past in endogenous terms, and describing the development of ideas in a way that is relatively inaccessible to outsiders. It is a particular problem for

anthropology. Writing as an intellectual historian, Collini (1999, 280) suggests that ‘anthropologists have perhaps been exceptionally prone to feel that their enterprise has developed in relative isolation from the general intellectual culture around it’, leading to disciplinary histories being told in ‘markedly internalist and self-contained terms’.

By this argument one needs to understand an academic discipline like anthropology from the ‘outside’ as well as the ‘inside’. One needs to be attentive to those dynamics it distances itself from, such as colonial ‘problems’, ‘race’ or rival disciplinary epistemologies, and its relationship to state funding and institutional patronage. Intellectual debates make sense in relation to the social and political contexts in which anthropological knowledge is produced and deployed. To understand the social aspects of any science, it is vital to begin to map the complex relationship of science to society. The problem then is how to weave these diverse and often contradictory perspectives together.

Yet even this distinction between internal factors and external contexts is too simplistic. If one is to truly explore what Knorr-Cetina calls the ‘epistemic culture’ of science, ‘those amalgams of arrangements and mechanisms – bonded through affinity, necessity and historical co-incidence – which in a given field make up how we know what we know’, then we have to take seriously the composite and multiple nature of academic work and identity (Knorr-Cetina 1999, 8). It is these bonds, affinities and networks that I seek to trace, in and out of departments, universities, conferences, grant applications, publications, classrooms, scholarly associations and the broader public sphere.

The emergence of disciplines in the social sciences

All histories face the challenge of defining the boundaries of investigation. Concepts of belonging and affiliation are particularly sensitive within intellectual histories, for they are also constantly mobilised by key protagonists and their interpreters. But categories also constrain analysis. Perhaps one needs to take one further step back, and unpack the very concept of ‘discipline’ itself. A word of medieval origins, it is both verb and noun, invoking both the content of learning and the process of mental (and social) disciplining to ensure obedience, often through force. The use of the term to connote a set of defined fields of learning is a recent one, paralleling the sudden explosion in scholarly fields of knowledge within the human sciences during the mid-nineteenth century. The *Oxford English Dictionary* cites one of the first examples of this contemporary relational understanding of ‘discipline’ in an 1878 scholarly paper discussing

the relationship between botany and zoology. Similarly, one could only talk about the 'social science disciplines' at a point at which they existed in relationship to each other within universities in the early years of the twentieth century. In anthropology, the term 'discipline' is first used in this sense in 1923 to discuss the relationship between anthropology as a 'discipline of type', geography as a 'discipline of place' and history as a 'discipline of time' (Myres 1923, 168).

For all its monolithic assertions, Foucault's *Discipline and Punish* (1977) was never just about the birth of the prison, but about the very methods of categorisation and normalisation that marked the growth of a 'modern' disciplinary society, a theme explored in his earlier *Archaeology of Knowledge* (Foucault 1972) with its attention to epistemes and the genealogies of disciplinary knowledge. Contemporary *Homo academicus* now largely takes for granted a disciplinary 'division' of knowledge, even if discomfited by talk of 'territories' and 'boundaries'. Many see this as the inevitable price of specialisation and professionalisation, but few would deny the importance or relevance of these affiliations (di Leo 2003) for their own sense of identity.

Many have explored the history, meaning and power of a disciplinary division of academic knowledge production, sometimes creating taxonomies of disciplines for comparative purposes. Kuhn (1962) separated what he saw as closely knit 'mature' scientific disciplines from the more permeable communities of scholars of disciplines still at a 'pre-paradigmatic' stage. Pantin (1968) and Whiteley (1984) sought in different ways to categorise types of scientific endeavour. Becher's survey of the cultures of 'disciplinary territories' and 'academic tribes' leads him to insist that 'the attitudes, activities and cognitive styles of groups of academics representing a particular discipline are bound up with the characteristics and structures of the knowledge domains with which such groups are professionally concerned' (Becher 1989, 42). Yet this territorial logic and focus on 'domains' and 'boundaries' lead one to create disciplinary artefacts where perhaps none exist. Knorr-Cetina suggests that instead of the language of disciplines we should be talking of 'epistemic cultures' to capture more accurately the 'strategies and policies of knowing that are not codified in textbooks' (1999), and the diversity of places and practices through which knowledge is produced. Fuller points out that the 'rhetorical character of disciplinary boundaries in the social sciences provides an especially good context for examining the embodiment of knowledge as a source of worldly power' (Fuller 1993, 125), and that 'disciplinary histories of the social sciences more easily show the rhetorical seams of appearing to represent the world without substantially intervening in it' (ibid.).

'It's the way you tell them' – a history of histories of anthropology

In Zadie Smith's *White Teeth* (2000), there is a discussion between two British Bangladeshi waiters about girlfriends. One says to the other, 'I've been out with a lot of white birds ... but never an English girl. Never works. Never.' When asked why, his reply is simple. 'Too much history, too much bloody history.'

This notion of 'too much history' sheds light on the troubled relationship of social anthropology to its many pasts, be they the personal histories of professional rivalry, micro-histories of departmental tradition or the broader histories of British colonialism amidst the *longue durée* of empire and conquest. These histories surround us, either as admired ancestral spirits or as restless spectres. A strong sense of kinship with our own disciplinary ancestors makes us emotionally attached to their legacy. Because there is too much history, it is easier to make the past suit the present, either by simplifying it, ignoring it or trying to escape it.

Given this caveat, a sensible place to start one's own disciplinary history is to review those written by others, and in particular the tensions that exist between popular, practitioner and 'professional' histories. Let us first look at 'popular' renditions of disciplinary history. Who are they written by, and for whom? Introductions to social anthropology, an important moment of 'first contact' for novice students, are commonplace. They often contain an individual's own account of the discipline's historical development. In 1910, Tylor wrote an entry for anthropology in the eleventh edition of the *Encyclopaedia Britannica*, in the style of a historical narrative (Tylor 1910), whilst Marett and other early presidents of the RAI repeatedly spoke and wrote about the development of their discipline, as a way both of legitimising their profession and of reshaping its past in a way that provided a charter for current concerns.

Some recent introductory texts go further still, seeking to legitimate anthropology by associating it with the scholarly interests of the ancient Greeks (Barnard 2000, Eriksen and Nielsen 2001). Other introductions, such as that by Pocock (1961), situate themselves within broader debates in the philosophy of science. An influential introductory text has been Adam Kuper's frank and telling 1973 history of the modern British school of social anthropology. Very much the discipline's first unauthorised biography, it antagonised many with its frank depiction of personal rivalries and caustic predictions for the discipline's future. Initially excommunicated, his reputation as social anthropology's in-house historian is now secure, and subsequent

editions of the book (Kuper 1983 and 1996a) presented a far more optimistic picture of the contemporary discipline.

There is a further genre that represents the history of the British discipline from the perspective of individual anthropologists. Examples here would include biographies of Mary Douglas (Fardon 1999) and of Colin Turnbull (Grinkler 2001). There are also scholarly re-evaluations, including those of Marcel Mauss (James and Allen 1999) and Franz Steiner (Adler and Fardon 1999a, b).

Two challenges come with focusing too closely on personalities. The first is that intellectual genealogies quickly become disciplinary charters. There is also the risk of assuming that, as Kuper puts it, 'our forebears are either our contemporaries or they are of purely antiquarian interest' (1991, 128). Few would dispute the vital role of historiographic recovery, bringing to light hidden figures or unrecognised influences that challenge conventional disciplinary wisdom. Yet there is a problem even with this approach. As Handler notes (2001), any discussion of 'excluded ancestors' assumes that the 'boundaries of that discipline, and the roster of accepted, acceptable, and/or canonised practitioners/ancestors, can be agreed upon'. This is doubtful – as with theoretical predilections, one person's sense of historical influences might not be shared by another. Debates about the 'canon' in the singular also ignore the way that individual scholars, departments and disciplinary collectives construct their own sense of what counts as significant knowledge.

How about one of the other key places in which history is reproduced – the lecture theatre and seminar room? A review of British undergraduate anthropology courses points to the diversity of approaches taken to the study of the discipline (Mascarenhas-Keyes and Wright 1995). However few departments offer courses on the history of anthropology, and at best teachers try to contextualise theorists within their historical milieux. In contrast, Darnell (1977) suggests that in the American context every major American graduate programme has a course in the history of anthropology. She suggests, though with little evidence, that 'the required course is frequently taught by the eldest member of the department, who is presumably qualified to teach the history because he [*sic*] has lived through more of it than anyone else'. At best, such a course 'provides the fledgling anthropologist with a collection of anecdotes, later to prove useful in socialising his own students' (Darnell 1977, 399). Far from the prediction of one historian that anthropology would 'become history, or nothing', we still rarely provide our students with the skills and resources to assess and use historical evidence or to think historically.

The desire to historicise anthropology turns one intellectual wheel full circle. Both Malinowski and Radcliffe-Brown, key figures in this

new intellectual school, were determined to escape the evolutionary assumptions of their forebears. The latter was dismissive of overly historical explanations of social processes, and he was reluctant to write about anthropology's past. In Britain, Evans-Pritchard's post-war historical turn caused a stir, but also marked the start of a retreat from earlier anthropological disquiet over the use of history. His Marett lecture 'Social Anthropology: Past and Present' proposed that 'there is no fundamental difference here in aim or method between the two disciplines' (Evans-Pritchard 1950, 123), and accused anthropologists of taking 'one or other of the natural sciences as their model' and turning 'their back on history'. He acknowledged that his observations would be 'hotly disputed' by most of his anthropological colleagues, but was convinced that 'with the bath water of presumptive history the functionalists have also thrown out the baby of valid history (ibid., 121). Yet he too rewrote the history of anthropology to support his own personal intellectual journey, tracing its antecedents not just to the 'early classics' of Maine, McLennan and Morgan, but also to the writings of eighteenth-century Enlightenment philosophers. His expectation was of a discipline in future 'turning towards humanistic disciplines, especially towards history, and particularly towards social history or the history of institutions' (Evans-Pritchard 1961, 28).

Who should write the history of anthropology?

Back in 1964 the anthropologist Irving Hallowell's solution to the writing of the history of American anthropology was to 'focus upon anthropological questions, rather than upon labelled disciplines' (1965, 24). This neat side-stepping leaves unanswered the question of whether these histories are best left to the historians, capable of the 'distanced empathy' that characterises ethnographies? Are 'practitioner histories' akin to 'native' ethnographies? Or is any outsider/insider divide too simple?

As social anthropology has professionalised and institutionalised, so too has the new sub-field of the history of anthropology. Predictably, this happened first in the USA, beginning in 1962 with two symposia on the history of Anthropology under the auspices of the Social Science Research Council and the American Anthropological Association (AAA). As Stocking noted at the time, this was no doubt partly due to the 'passing of a long-lived giant (Boas)', and the inevitable way that 'aging survivors turn to their origins' (Stocking 1966, 281). For him the most fundamental factor was 'the state of anthropology itself', and here he suggested that with the disappearance of what he called the 'last of the Gitchi-Gumis',

anthropologists were turning to the 'reconsideration of problems which were central to the anthropology of earlier periods' (ibid., 283). At the same symposium, the American anthropologist Irving Hallowell argued that this turn to history should not be viewed as 'antiquarianism', or even as 'marginal to current interests', and rather that the 'history of anthropology was an anthropological problem' (1965, 37).

At this early stage of his career Stocking's own affiliation was to history, seeing himself as an 'outsider' whose 'status in the tribe is at best honorary' (Stocking 1966, 282). Yet he was sociologically minded enough to send out 135 questionnaires to survey current research on the history of anthropology. His concluded that professional historians were uninterested in the field, and that anthropologists were writing 'general histories' without 'any firm monographic and archival groundwork' (ibid., 285). Whatever the utility of these 'general' texts, he curtly warned that 'they will not provide us with a history of anthropology'. He questioned the disciplinary bias towards oral history and the dismissive attitude shown by some towards documentary evidence. In short, he saw 'strong arguments both of historical precedent and programmatic prescription against the assumption that the history of anthropology will or should develop solely by an incremental process within the discipline itself' (ibid., 286). He foresaw the increasing professionalisation of the discipline of the history of science, proposing the ideal as a 'professional training in both history and anthropology'. If the dilemma is summarised as 'who shall write the history, anthropologists or historians?' (Darnell 1977, 399), Stocking took the position that 'this history should be written by historians, and perhaps in the first instance for historians' (Kuper 1991, 127).

It is easy to dismiss what Herbert Butterfield famously called 'Whig history'. For Butterfield 'the study of the past with one eye, so to speak, upon the present, is the source of all sins and sophistries in history' (1973, 30). Stocking articulated this as a contrast between 'presentism' and 'historicism' – 'a commitment to the understanding of the past for its own sake' (Stocking 1968, 4). Whilst he recognises that this is a crude dichotomy, Stocking goes on to make a qualified case for historicism. For him 'presentism assumes in advance the progressive change of historical change, and is less interested in the complex processes by which change occurs than in agencies which direct it' (ibid., 4). He sees professional social scientists as being motivated by 'utilitarian' concerns, demanding 'of the past something more; that it be related to and even useful for furthering his professional activities in the ongoing present' (ibid., 6). He accuses them of 'anachronism, distortion, misinterpretation, misleading analogy, neglect of context,

oversimplification of process' (ibid., 8). On the other hand, to presume that one can totally distance oneself from current concerns is unrealistic. As Kuklick puts it, the key was 'to distinguish between those questions asked by disciplinary ancestors that were quite different from their own and those that remained current' (1999, 227).

In lending his support to these views, Clifford Geertz powerfully evokes his distaste for what he calls 'practitioner history', and the way in which:

instead of doing what one would think a 'real' historian ought to do – examine previous scholarship of various kinds, and draw one's notions of what anthropology 'is' from such an inquiry – it works the other way around. It takes a view of what anthropology 'is' and works back from that to find rudimentary, prefigurative examples of it *avant la lettre*. (Geertz 1999, 306)

Yet not all accounts by practitioners are guilty of intellectual presentism. It also depends how one uses such histories. Read with a critical eye, many reveal the complex links between scholarly innovation, academic identity claims and the broader politics of funding, prestige, utility and application.

This disagreement over the purpose, focus and authorship of disciplinary histories continues. Intellectual positions relate largely to scholars' disciplinary affiliations. Kuklick, a historian of science, argues that 'indoctrination in presentist constructions of the ideas of disciplinary predecessors has been an important feature of the occupational socialisation of human scientists' (Kuklick 1999, 227). She surveys introductory textbooks to show how they are 'suffused with current received wisdom about professional ancestors, who are represented as sources of still-useful inspiration' (ibid., 227). She contrasts this with physics textbooks, where 'personal idiosyncrasies, institutional peculiarities, general social trends ... have been forgotten as they have become routinised and elaborated' (ibid., 228). Kuklick suggests we seem determined to make our forbears into contemporaries – 'Weber, Marx and above all Durkheim are regarded as still-active participants in sociological debates' (ibid., 232). In particular she cites the 'Durkheim industry' as an example of 'presentism of an exceptionally high order'.

Kuklick's main concern is that students are expected to read these classic works directly, 'each of which she or he is evidently assumed capable of understanding without benefit of historical guides to interpretation' (1999, 230). Like Stocking, she points out 'how few serious historical studies' have been published in the discipline's journals during the 1990s, and anthropologists' continuing reliance on what she calls 'near history', oral tradition and 'mythistory'

(Stocking 1995). However, she seeks to maintain a balance. 'We may deplore the sort of history that anthropologists are inclined to write, but we may sympathise with their feelings that they must effect control of their own history' (Kuklick 1999, 236). She speaks with first hand experience, such as the angry reaction by Jack Goody (1995) to her social history of British anthropology *'The Savage Within'* (Kuklick 1991).

For Kuper, anthropologist first and historian second, this opposition between the disciplines is largely artificial. The more important divisions are those of theoretical allegiance – to Marxism, culturalism or structuralism within both disciplines. For Kuper, the difference is that 'the practitioner demands lessons from history', and that in writing the history of anthropology one cannot avoid providing a form of 'applied anthropology' (Kuper 1991, 138). He suggests that 'the purpose of history may then be to make the practitioner conscious of these constraints, of the forces which shape practice. It would then facilitate dissent, criticism and innovation' (ibid., 139). This is a much more nuanced view of practitioner history than the one Geertz dismisses, and one that many anthropologists would be sympathetic to. It also answers the accusation of presentism, acknowledging that one always writes and reads the present in dialogue with the past.

Do recent histories of anthropology line up along this practitioner/professional divide that separates Kuper and Kuklick? The best-known historical accounts of the British discipline of social anthropology either focus on the 'epoch-forming' early years of this century (Kuper 1973; Kuklick 1991; Stocking 1996), the colonial era (Asad 1973), or on its nineteenth-century historical antecedents (Stocking 1968; Urry 1993). Of these, Kuper, Asad and Urry would probably describe themselves as anthropological historians, the others as historians. The work by George Stocking is undoubtedly the most influential, but as Jose Harris (1999) notes, it has tended to be more influential amongst anthropologists than amongst historians more generally. Stocking's closely written, recursive prose is exhaustive in its coverage, untangling the nuances of intellectual debate between the different key figures of the early twentieth century and their rival social and ideological assumptions. In order to take theoretical and social contexts equally seriously, his work tends to be structured into a series of essays rather than a single narrative.

Stocking has his critics. In one review of Stocking's work, the historian Stefan Collini praises a 'dense and thickly textured account of the interplay of ideas, personalities and institutions', but suggests that the history of anthropology might have been better contextualised as 'conforming to a common pattern of intellectual change within a given institutional framework' (Collini 1999, 281). Harris points to

the contradiction between the nuanced, wide-ranging case studies and the 'unilinear sequence of development outlined in his [Stocking's] conclusions' (1999, 327). Kuper has also been critical of what he calls the 'historians' revenge' (Kuper 1985) in his review of Stocking's *Functionalism Historicised: Essays on British Social Anthropology* (Stocking 1984), where he feels the 'historians of anthropology..stand revealed as its legislators' (Kuper 1985, 524). In it, he suggests that Stocking's 'obsession with origins' leads the latter to caricature 'British' functionalism as anti-historical and to underplay the intellectual and political contexts of British anthropology between the wars. Coming from an anthropologist specialising in history, these criticisms cannot simply be dismissed as the result of disciplinary affront. But the aggrieved tone of these exchanges shows that disciplinary loyalties and academic identities do intrude on the genuine possibilities for interdisciplinary collaboration. The borders between 'historyland' and 'anthropologyland' are carefully patrolled. In this context, dichotomies such as 'historicism' vs. 'presentism' and 'practitioner' vs. 'professional' become epithets and identity claims as much as analytical categories. The history of anthropology is also the history of its rival interpretations.

'Actually not anthropologists at all'

What of those who deny all disciplinary labels, either for themselves, or for others? In 1951 an intriguing transatlantic spat was played out in the pages of the *American Anthropologist*. George Murdock, a prominent American cultural anthropologist, wrote a critique of what he saw as the blind spots of 'recent trends in British anthropology' that others found 'impossible to defend' (Murdock 1951, 467). His paper was simply titled 'British Social Anthropology'. In it, he questioned their 'complete disinterest' in history and general ethnography, their exclusive focus on kinship, and their 'indifference' to international debates. He went so far as to suggest that 'they are actually not anthropologists' at all, but rather a 'specialised school of sociologists' (ibid., 468). Raymond Firth's response was entitled 'Contemporary British Social Anthropology', and he courteously acknowledged that 'much of what Murdock has said is just and calls more for reflection than for reply' (Firth 1951, 477).

Together, these two papers provide what Stocking calls a 'historiographic microcosm' wherein 'a presumably unitary historical phenomenon is examined from two distinct standpoints' (Stocking 1996, 432). Stocking goes on to examine the relative merits of the case that Firth and Murdock make. I am more interested in this

'presumably unitary historical phenomenon' that Murdock and Firth seem to take for granted. Where had this entity, and its label 'British Social Anthropology', come from? This was not only a reference to an existing genealogy of thinking, but an act of political identification. Or, rather, dis-identification, for Murdock had sought to make the case that this British tradition wasn't anthropology at all. His attempt backfired; the label stuck. Murdock's caricature provided a self-description with which British scholars could identify. The term 'British social anthropology' had not been used before this time in scholarly journals such as *Man* or the *JRAI*, but was a powerful shorthand – it soon began to appear regularly in the journals. Part of the appeal of the term was that it offered an imagined scholarly community that linked back to Malinowski's iconic seminars at the LSE, and quietly invoked a national tradition that had been nurtured by its colonial past and that could be juxtaposed to both American and European debates. It offered a framework with which the discipline's practitioners could find affiliation. The label continues to stick – many anthropologists still talk about 'British social anthropology' without further thought, and historians like Stocking also use the term (Stocking 1996; Spencer 2000).

One hundred years ago, in his presidential address to the Royal Anthropological Institute, the Cambridge anthropologist Alfred Haddon expressed the view that 'a peculiarity of the study of Anthropology is its lack of demarcations; sooner or later the student of Anthropology finds himself wandering into fields that are occupied by other sciences' (Haddon 1903, 12). This determination to be free of artificial boundaries, coupled with a highly ambitious intellectual remit, has become a characteristic disciplinary refrain. Malinowski acknowledged his fascination with the 'universal scheme which underlies all concrete cultures' (1931, 15); Tim Ingold has repeatedly insisted that anthropology is the 'study of humanity' (1985, 15). Even Stocking is not immune, announcing that anthropology's embracive approach is the 'imperfect fusion of quite different traditions of inquiry: biological, historical, linguistic, sociological' (Stocking 2001b, 286), such that it has been in a profound sense interdisciplinary. In a rare moment of conjecture, Stocking argued that 'the boundaries of anthropology have always been problematic – more so, one suspects, than those of other social science disciplines or discourses' (Stocking 1995, 933), and that 'despite the apparent inclusivity of its subject matter, the actual content of anthropology has varied greatly in different times and places' (*ibid.*, 936). This depiction of anthropology as an 'unbounded discipline' is an identity claim, evidence that he is perhaps less detached from anthropology than he used to be.

Conclusion

Any analysis of the historical sociology of disciplines from a single viewpoint is limited, be it anthropology or history, the past or the present. It should be no surprise that in anthropological hands, disciplinary history takes on an anthropological shape. This is not simply because anthropology and the histories of anthropology rely as much on oral narrative as on more 'objective' written accounts. It is also part of our disciplinary socialisation to attend to emergent social forms – we find it hard to think but from the present. Fardon notes how 'recursively and insistently the intellectual strategies of modern social anthropology urge the present upon us', such that we place 'unsustainable weight upon the idea of the present' (2005, 2–3). This is perhaps a disciplinary ethic as much as an intellectual strategy. In our ambitions for the discipline, perhaps we have never left Tylor's ambitions for a reformist science behind.

Rather than writing, as Foucault put it, 'histories of the present', perhaps the best we can do is to write histories in dialogue with both the past and the present. La Capra suggests that this dialogue requires a subtle interplay between proximity and distance in the historian's relation to the 'object' of study (La Capra 1983, 25). He suggests that 'the very point of a dialogical approach is to stimulate the reader to respond critically to the interpretation it offers through his or her own reading or re-reading of the primary texts' (ibid., 48). This takes us beyond an unhelpful dichotomy of presentism vs. historicism to a more nuanced understanding of the uses of history: 'an interest in what does not fit a model and an openness to what one does not expect to hear from the past may even help to transform the very questions one poses to the past' (ibid., 64). There may well be 'too much bloody history', but that doesn't allow us to avoid grappling with its claims upon us. Kuper (1991, 129) points out that the purpose of history ought to be 'make the practitioner conscious of these constraints, of the forces which shape practice'. To this end, the history of anthropology offers the possibility of a 'really challenging reflexivity' (ibid.).

I have argued for accounts of the history of social anthropology that are less genealogical, less polemical and less narrowly 'presentist'. My own approach is to draw closely on an eclectic variety of primary sources and oral histories to create a set of grounded histories of anthropology that are rich, empirical and contextualised accounts of disciplinary practice and engagement. Throughout what follows, I take academic identity politics and institutional dynamics as seriously as individual theorists and their ideas. The four are inseparable.

A TALE OF TWO DEPARTMENTS? OXFORD AND THE LSE

Introduction

All disciplines have founding legends and hero figures. Social anthropology is no exception. Foremost amongst these was the brilliant young Polish émigré Bronislaw Malinowski, appointed to a lectureship at the LSE in 1923, on the basis of the ‘ethnographic magic’ he spun in the Trobriand Islands (Stocking 1992). He continues to rule pre-eminent over histories of the discipline, perhaps because of the romantic mystique surrounding the emergence of a new style of social research – the ethnographic method – from a South Pacific island.

An exhaustive and comprehensive biography of Malinowski’s early life has now been published (Young 2004), and the second volume is on the way. Can any more usefully be said about the ‘archetypal moment’ of Malinowski’s fieldwork, or his powerful influence over subsequent generations of anthropological researchers, commentators and critics? The histories have pointed to his penchant for self-publicity and the self-aggrandising way in which he designated himself as the founder of ‘functional school of anthropology’. Still the lustre remains. So why start yet another history of social anthropology with Malinowski?

I do so in order to offer a different perspective on his success. My focus is less on Malinowski’s tent in Mailu than on his office and seminar rooms at the London School of Economics (LSE). It was from here that he won the funding and support that underpinned his vision

for the discipline. Rather than attributing everything to individual intellectual bravado, credit is also due to the institution that allowed him to pursue his own iconoclastic manifesto.

In this chapter we also meet another key member of our cast – A. Reginald Radcliffe-Brown. He escaped from a very different social background – that of a poor Birmingham family – through a grammar school education and a scholarship to Cambridge. His role as the discipline’s arch-theoretician and Malinowski’s rival and alter-ego adds to the overall dramatic effect.

Rather than sizing up their egos and intellectual legacies, in this chapter I compare their places of work – the intellectual worlds enclosed within LSE’s ‘rabbit warren’ of buildings and Oxford’s cloistered common rooms. The comparison may sound humdrum, but is surprisingly revealing. At a time when Oxford, Cambridge and University College London were the only universities with active programmes of research in anthropology, LSE could afford to define itself differently. Malinowski’s experience of the LSE was of a somewhat chaotic, left-leaning and freethinking intellectual milieu (Dahrendorf 1995). This was in marked contrast to the hushed conservatism of Oxford that frustrated Radcliffe-Brown on his appointment to his professorship and All Souls fellowship in 1936. I set the scene for the book by describing how the institutional and academic cultures of the two universities shaped the fortunes of the two protagonists.

Anthropology in the 1920s

Any new initiate of ‘British’ social anthropology is soon introduced to the classic ethnographic works of E.E. Evans-Pritchard. His tutor, Ranulph Marett, a classicist and Reader in Social Anthropology at Oxford since 1905, regarded him as one of the most promising undergraduates to come through what he called his ‘Exeter nursery’. With a semi-aristocratic background in a clerical family, and a public-school education at Winchester, he seemed destined to inherit Marett’s mantle.

Yet in 1924 Evans-Pritchard caused a stir by leaving Oxford to commence graduate studies at the LSE. He began there the same year as Raymond Firth, a young New Zealand research student who became Malinowski’s most prominent, and loyal, student. Almost 75 years later, Firth still recalled Evans-Pritchard’s telling comment: ‘Raymond, you have no idea what it meant leaving Oxford to come to the LSE.’ Firth remembered that ‘Marett was horrified. The idea that

an Oxford man should leave Oxford in order to come to LSE was almost inconceivable.'

So what was the appeal of the LSE for Evans-Pritchard? Perhaps its main attraction was precisely that it wasn't Oxford. In comparison with Oxford's conservative intellectual environment, the Fabian-inspired School, housed in a muddle of streets off the new Kingsway thoroughfare, was progressive, well connected, and dedicated to the social sciences. For Firth, 'EP came to the LSE rather than to Malinowski'.¹ Yet he can hardly have failed to notice the dynamic and cosmopolitan atmosphere around Malinowski's research seminar.

There were also reasons for Evans-Pritchard to leave Oxford. There was little dynamism at either Oxford or Cambridge in the decades after the First World War. Stocking describes social anthropology as having only a 'precariously marginal existence' in 'a conservative and socially arrogant institutional culture' (Stocking 1996). There was a self-described 'triumvirate with equal powers' who between them were responsible for all teaching in anthropology at Oxford for thirty years. Ranulph Marett taught social anthropology, Henry Balfour prehistory and material culture, and Arthur Thompson physical anthropology. They sometimes referred to themselves as the 'Trinity', and saw their three subjects as 'on a par'. Research was not a priority, given their multiple responsibilities of curating, demonstrating, and teaching colonial probationers. Marett had extra administrative responsibilities, becoming Rector of Exeter College in 1928. He also had a golf handicap to maintain, heading to the Cowley golf course at lunchtimes (Marett 1941).

After an attempt at the turn of the century to include undergraduate anthropology courses within the 'natural science' honours school (the Oxford undergraduate degree) had been rebuffed, anthropology remained a diploma course. A subsequent attempt in 1910 by the Committee for Anthropology to appoint a permanent 'Professorship of Anthropology' revealed their vision for a cohesive disciplinary identity. They defined the putative professor's duties as 'to link together and harmonise the various branches of its study'. Without this synthesising function, it was feared that 'undue specialisation should assert itself, to the prejudice of Anthropology as a whole'. They need not have worried. Even though no professorship was created, it took a further twenty-five years before social anthropology finally dominated the other sub-fields.

Malinowski at the LSE

The London School of Economics could not have been more different. The idea of a new school of economics was first floated as an idea in August 1894 at a now famous breakfast discussion – about the best use of a Fabian supporter’s legacy – between Sidney and Beatrice Webb and George Bernard Shaw. It became a reality, with more than 200 students studying political economy, only fourteen months later. Founded at high speed, its fortunes prospered with state funding, and it soon established its own characteristic intellectual culture and pedagogic style.

Bronislaw Malinowski was a doctoral student at the LSE from 1910 to 1914, and rapidly became the protégé of Charles Seligman – the first Professor of Ethnology at the University of London, based at the LSE. Malinowski’s subsequent appointment in 1921 as first lecturer and then reader was unsurprising. It was personally engineered by the LSE’s mercurial, far-sighted and often despised director, William Beveridge. Firth felt that ‘not many institutions would have accepted Malinowski as a Reader, and it was this freedom in the social field of the LSE which led to his appointment’.² Harold Wilson famously described Beveridge as ‘probably the greatest administrative genius of this country, but almost certainly the worst administrator’ (quoted in Harris 1997, 11). Beveridge’s intimate personal relationship with the School administrator Jessica Mair (Lucy Mair’s stepmother) was also key, even if it did challenge even Bloomsbury’s social mores.

In his 19 years at LSE, Beveridge transformed a small institute that Sidney and Beatrice Webb had apparently told him ‘would run itself’ into the largest centre for the study of social sciences in Britain. But his dislike of formal administrative cultures meant that he conducted business in a highly personal fashion, leaving it with a governance structure that a subsequent director described as ‘bewildering’ (Dahrendorf 1995, 179). There were no formal departmental administrative structures at the LSE until the 1960s, and strong disciplinary loyalties coexisted within a congenial senior common room where academics ‘met and felt part of a single-faculty school’ (ibid., 209).

Bronislaw Malinowski’s mythologised role as the ‘Joseph Conrad’ of social anthropology is well known and has been carefully dissected, most notably by George Stocking (1992, 1996). By the time of his appointment as reader, he had published his influential *Argonauts of the Western Pacific* (Malinowski 1922), and was developing an increasingly cosmopolitan network of followers. But there was more to his teaching than the much-admired research seminar. Trained at the ancient universities of Cracow and Leipzig, the Humboldtian idea of a research-led model of academic practice would have been familiar to him. One of

the reasons for Malinowski's close alliance with William Beveridge was their shared belief in the importance of a more scientific approach to the social sciences.

On his appointment, Malinowski was at first keen to please. He working closely with his mentor, Professor Charles Seligman, a one-time medical doctor who had become interested in anthropology during an expedition to the Torres Straits, and been appointed to the chair in 1913. Their relationship was sometimes stormy, but Seligman was like an 'elder brother' to Malinowski, and they were united by a 'collegial bond of suffering' (Young 2004, 161) from a variety of physical ailments.

On his appointment Malinowski offered to lead a course in the methods and aims of anthropological fieldwork, only to be reassured that he was already doing more than enough teaching. He spent his first years building relationships both with influential missionaries like Joseph Oldham and Edwin Smith (see Smith 1934), but also with philanthropic institutions like the Rockefeller Foundation. This resulted in a major grant for the International African Institute for Languages and Cultures (later the International African Institute) that Edwin Smith had helped set up (see Richards 1944). The award complemented other grants that the LSE had received from Rockefeller. Malinowski was not one to hide his achievements, and, in a letter to the School Secretary in 1929, pointed out that this 'new approach' of functional anthropology 'lends itself more readily to the practical application of anthropology in colonial affairs'. Malinowski's success at winning over the LSE to his vision for anthropology depended on garnering research grants. He had a talent for convincing others of anthropology's direct utility, and getting them to fund academic research on that basis.

Given their very different temperaments, Malinowski found working under Seligman increasingly difficult. Right from the start he declared himself interested in 'advanced teaching', leaving Seligman to teach general undergraduate courses in ethnology and technology. In 1930, Malinowski wrote to Jessica Mair stating his belief 'that post-graduate work is of the greatest importance in a new science still in a process of formation, as Anthropology is, especially its social or cultural side'.³ Indeed, he was disparaging about delivering 'elementary training' to amateurs (in 1932 he had only one taught masters student), feeling that it was a distraction from the research work that is 'essential'. As a result, his lectures (read seminars) had 'reached an advanced level', to the extent that he refused to be tied to formal course titles. Raymond Firth recalled how Malinowski in 1933 'was using the seminar as a vehicle for Coral Gardens ... Each term the school issued a list of

lectures, and a long reading list, but in fact it usually turned out to be Malinowski's seminar.'

One of the myths that has been perpetuated within anthropology is that its early students received very little training, and that this, paradoxically, contributed to their success. Kuper's suggestion that there was 'no formal teaching' (Kuper 1996a, 66) at the LSE is contradicted by Malinowski's own view of what he was doing. Malinowski laid great store by the formal scientific training and socialisation he offered to his students, to the extent of even preparing a written programme of research training. As well as seminars on field methods and carefully structured (and transcribed) graduate seminars, he insisted that all students submit written plans for their field research, and these were carefully vetted and discussed. His letters to the LSE Secretary go into teaching and training arrangements in great detail: 'Every time a student of mine has been going into the field, I have arranged a series of seminars on methods of field work, in which the protagonist and several of the older research students have participated. I gave such seminars to Raymond Firth, Edward Evans-Pritchard, and Hortense Powdermaker.' In 1932, with a growing set of students, he also asked all his students to prepare a thorough fieldwork proposal, asking them to detail the particular issues they wished to explore. Insisting that 'all empirical observations must have the backing of a theory', he offered to provide for them a special course on the 'functional analysis of culture in relation to the technique of fieldwork'.⁴

Malinowski was highly strategic in his use of the Rockefeller grant. Of the thirteen IAI fellowships awarded in 1931–2, all were his students. He also employed a number of returning students as research assistants. In one report he acknowledged how his first assistant, Raymond Firth, had since produced a valuable book, and another, Isaac Schapera, a good thesis, 'so that the other aim of a research assistantship, the training of assistants, is yielding also, I think, good results'. If his approach to fieldwork was novel, so too was this explicit focus on training social researchers. In his reports to the foundation, Malinowski repeatedly emphasised the importance of the 'special training which they received' for their subsequent development, describing their role in leading seminar discussions as the 'supreme form of academic teaching which consists in mental cross-fertilisation'.

Malinowski's students have written about 'his real love of teaching' (Firth 1957) and his captivating 'Socratic' teaching style. He was, by all accounts, dedicated to his students, and in his obituary Audrey Richards described how:

the sheer intensity of his work was probably the strongest impression he made on students ... He gave his time generously to students and demanded theirs in return. Students worked at any problem in which he was at that moment intensely absorbed ... They might be irritated by his intolerance, or inspired by his enthusiasm. They were never bored. (Richards 1943, 3)

Seminar discussions were often led by his senior students like Meyer Fortes and Siegfried Nadel. Malinowski called them the 'mandarins', because they had done degrees in other fields before coming to the LSE. Meyer Fortes had come from Cape Town in 1926 to do a PhD in Psychology and to work on the causes of juvenile delinquency in the East End of London. Siegfried Nadel from Vienna where he had done a doctorate in Psychology and Philosophy.

In 1933 Malinowski wrote to his wife at the start of the new academic year: 'I got a new batch of mandarins – some of them quite nice. The class promises to be almost as good and as numerous as last year' (Wayne 1995, 180). Recording everyone's addresses, Malinowski would start by carefully dividing his students into two groups. Discussions were regularly transcribed, and the transcriptions show that his students played a vital role in expounding and developing Malinowski's theoretical positions to others. These transcripts, curt summaries of the debates, served as a resource for students and for Malinowski's own theoretical work.

Charisma and Conflict

Not everyone shared Malinowski's perspective or liked the psychodynamics of the seminar. Meyer Fortes, never fully converted to Malinowski's brand of seamless and over-programmatic functionalism, described how his 'catalytic virtuosity kept the seminar at a high pitch' (Firth 1978, 5). Malinowski was of course a skilful rhetorician, so one has to take his own correspondence about his teaching with a pinch of salt. If he praised them in writing, Fortes felt that he sometimes treated his students in his seminar 'abominably' (*ibid.*, 19). As Stocking put it, 'if he did not demand discipleship, he did demand allegiance' (1996, 403). On a number of occasions Malinowski scolds his students for being difficult, for missing seminars or for disagreeing with him. One letter to Paul Kirchoff, a student and leftist activist, stands out:

there is extremely little chance of any fruitful collaboration between you and me ... on the one hand a pupil ought to choose those teachers with whose method he feels himself to be in sympathy, and on the other hand, I shall not be able to recommend for field-work somebody whose point of view I do not understand.⁵

A subsequent letter to Kirchoff ended 'It is really a question of whether I am going to believe you that you have done all to make yourself familiar with my point of view or not.'

Edward Evans-Pritchard had also been at the receiving end of Malinowski's scorn. In 1928, Evans-Pritchard, then a young researcher in Sudan, wrote a beseeching and disingenuous letter to his former teacher. 'EP', as he styled himself, had fallen out 'very deeply' with Malinowski 'sometime around 1925'.⁶ Despite this, he still needed the latter's support. He put pressure on Malinowski to publish his paper comparing Trobriand and Azande magic. 'If it is ever to be published it must be published now ...,' he writes. 'After all the nice things you say I shall feel very disappointed if when I return home I find the publication is as far off as ever.' In return he seeks to ingratiate himself with Malinowski, sending an extensive set of notes from his fieldwork on taboos. In the final paragraph he also piles on the flattery, reaffirming his loyalty to Malinowski, as against any commitment he might have had to A.R. Radcliffe-Brown (who at this point had not added the Radcliffe to his name):

as you say when I wrote it I stood very much where Brown stands. Since my further experience I have moved very considerably from this position. I don't want to flatter you but quite sincerely I think that I was standing where Brown stood because my field-work was on Brown's level. I now think that I have had better field-work experience than Brown and move more from his position.⁷

He goes on to capture this intellectual evolution in diagrammatic form. He draws an arrow from his own name vertically up to Brown, and then further on to Malinowski. On reading the letter, Malinowski adds a self-mocking scribble of his own, perhaps signalling his own dislike for Evans-Pritchard. Next to his name, he adds 'God's view – only a very short distance'. The diagram is a cryptic affair, but captures the tight nexus of rivalry, patronage and competitiveness amongst a group whom Firth later referred to as 'a band of brothers'. Evans-Pritchard's antipathy for Malinowski became well known, and he used to regularly recall that, when he had asked Malinowski for advice on fieldwork, he had been told simply to 'not be a bloody fool'.

Malinowski's relationship with Seligman also turned sour as the former's reputation grew, especially after his appointment as professor in 1927. This conflict came to a head when in 1931 Charles Seligman appointed an archaeologist from University College London over Malinowski to chair the 'University Board of Studies'. It was a snub that Malinowski found particularly hurtful given his dislike of his rival Grafton Elliot-Smith's 'ill-will and boorishness'.⁸ Elliot-Smith was

professor at UCL, and championed the rival 'diffusionist' theoretical school that Malinowski saw as a threat to his own influence.

But there were other reasons for this antipathy. As well as Seligman's dislike of Malinowski's 'semi-popular propaganda work' and 'frequent railing against your colleagues', a recurring tension was the loyalties of their respective research students. One of the increasingly vituperative letters written by Malinowski to Seligman in 1931 revealed the reasons behind Evans-Pritchard's 1925 'break' with Malinowski. The latter declared himself 'deeply convinced' that Evans-Pritchard, like Audrey Richards and Raymond Firth, ought to do a theoretical thesis before fieldwork, but Seligman had given him 'advice diametrically opposite to this'. When Evans-Pritchard 'felt inclined to accept your advice' and 'decided to specialise on the Sudan and to write a thesis on his field work, and not on a theoretical subject, I also asked him to register with you'. Malinowski went on to explain that he had then resigned from supervising him, and not subsequently advised him 'on a single point of his plans or his work'.⁹ The beseeching letter Evans-Pritchard had sent from Sudan is likely to have further antagonised Malinowski. Their rivalry never ceased. Later Audrey Richards would send Malinowski regular letters detailing her 'eaves-dropping among the enemy', and describing how Evans-Pritchard and Paul Driberg were openly blaming Malinowski for forcing them out of the LSE and the International African Institute.¹⁰

Part of the issue was the supposedly cooperative but actually competitive relationship between the different colleges of the university. This was aggravated by the presence of the 'children of the sun' (as Elliot-Smith and his 'school' of diffusionist thinking at UCL were pejoratively described). At one point, more students were attending UCL seminars than those at LSE (Stocking 1984, 10), and Malinowski felt deeply threatened by this. Seligman felt that a broad introduction to anthropology ought to be offered to LSE students in order to attract students who might otherwise go to University College, whilst Malinowski's view was that the colleges should go their own ways and not seek to collaborate over the courses offered. He saw this as a way of avoiding duplication, and relieving the LSE – and himself – of undergraduate teaching obligations.

Relations between Seligman and Malinowski worsened further when the former pushed for a ruling that all Ph.D. students should do a year of general training in 'the foundation of the science (Physical Anthropology, Prehistory and Simple Technology) before being allowed to take a higher degree in one of its branches'. Seligman felt 'it is a great mistake to allow a man to take a Ph.D. in Anthropology who is in fact only studying one branch of Anthropology'. Of course, Malinowski disagreed – many of those who attended his seminar had

very little prior training, and indeed he had inserted a clause in the regulations that 'prior knowledge of native situations' could be counted (which is how Jomo Kenyatta became a student). Malinowski immediately ensured that he had the LSE administration on his side. The LSE registrar obligingly confirmed that requiring postgraduates to 'attend Undergraduate courses in anthropology and pass a qualifying exam' would be a 'retrograde step', and that the ideal state 'is freedom from restriction by unnecessary University regulations'.¹¹ Malinowski dealt with the issue by simply not turning up to the relevant committee meetings; his presence as convener was vital in order to make a ruling on the matter. In 1933 Seligman retired in ill health, and, despite his further efforts to influence the teaching covered at the school, Malinowski now had free rein over the development of the subject.

Malinowski's close working relationship with the School secretary, Jessica Mair, proved invaluable. Malinowski regularly wrote to her about his bodily afflictions, a seemingly integral part of his creative persona. One time he wrote asking that he be let off the first week of lectures because of the 'grippe that inevitably greets my arrival in London'. Another time he retreated, with the director's agreement, to Champneys health farm for three weeks, coming in only for his seminars. In 1931, using Radcliffe-Brown's apparent threat to transfer anthropology from LSE to the School of Oriental Studies, he suggested to Jessica Mair that 'if we can present the sociological world with a *fait accompli* of a supremely strong anthropology department at LSE we are out of danger'. She advised him to approach the director with proposals for a new readership and lectureship. For Seligman, Evans-Pritchard was an obvious candidate, but Malinowski refused point-blank, confiding in the director that he would not like Evans-Pritchard to be 'permanently attached to the teaching staff of the School. It would not be fair to him or to the school. I mean his talents are definitely not in the teaching but in the research line.' Instead, Malinowski managed to convince everyone that Raymond Firth should be tempted back from his Rockefeller Professorship in Sydney. As Malinowski wrote to Jessica Mair, after his strategising had borne fruit: 'EP who would have been foisted on us, as you know, had we not anticipated the move'.¹²

The archives reveal Malinowski's skill at getting his vision of anthropology institutionalised within the LSE, proselytising a style of postgraduate research-led teaching within the LSE and the university at large. He was, however, aware of his effects on people. In a moment of honesty, he admitted to Seligman that having:

a junior colleague in one's dept with a 'mission' would I am certain be extremely annoying to me, especially if I were not in sympathy with this

mission. I sometimes imagine that you must think of me as I do of Elliot-Smith, that is, as a perambulating compound of megalomania, monomania and self-seeking – and a bee or two in my bonnet.¹³

Michael Polanyi's (1958) emphasis on the unconscious and tacit ways in which craft skills are passed from master to pupil has been an influential explanation of the process of disciplinary training and reproduction within the academy. But some historians of science now argue that intellectual and methodological precepts need to be articulated before they can be embodied in practice, and that being explicit is equally important to imitation and socialisation (Olesko 1993). This seems to be a better explanation of Malinowski's style. Academic jockeying and politicking necessitated incessant self-promotion, even if Malinowski was able to mock his own shortcomings.

Anthropology in 1930s Oxford

Back in 1930s Oxford, there is a similar struggle for influence and control. After many years of equilibrium, Ranulph Marett began to plan for his own succession and to reinvigorate the teaching of Oxford anthropology. One reason was chronological, or, as Marett phrased it in a letter in 1933, 'we must be putting our house in order, since the present teachers are all getting rather old'. But there was also a growing jealousy of Malinowski's success at attracting grants and the best students from Oxford and Cambridge. So Marett with his classics colleague John Myres began planning an approach to the university for a new professorship, collecting statistics about the salaries physical and social anthropology faculty at Cambridge and LSE. By this stage, Marett no longer felt he had to argue for treating all aspects of the field equally. As he put it in a letter to Hodson at Cambridge, 'If we made a full professorship out of it, I should myself of course like to see the Social Anthropology made the main subject.'¹⁴ Marett was philosophical about the chances 'to do something more for the subject', recognising that as the university was 'completely broke, I doubt if anything will come out of it'.

But Henry Balfour soon got wind of the plans for the new Professorship. Writing only a month after Marett and Myres had met, Balfour voiced his concern about the 'undesirable' suggestion that 'anthropology should be dominated by one of its sections ... against the original scheme' and that the 'independent boosting of one branch has reacted unfavourably upon the others'.¹⁵ However, he did add his name to a memorandum that proposed the appointment of a Professor in Social Anthropology, advocating the provision for an undergraduate

degree in the subject, and the need for adequate teaching premises. The proposal was accompanied by a list of 'distinguished ex-students', and compared Oxford's spending on the subject (£1,150 a year) with that of London (£2,750). What was the university's reaction? After establishing a further subcommittee to consider the matter, the Hebdomadal Council finally declared itself unable to help, but stated that the request would be included amongst the 'urgent needs' to be submitted to the national University Grants Committee (UGC).¹⁶

Aware of LSE's success at fund-raising, Hebdomadal Council also decided to create yet another committee to approach the 'Rockefeller trustees or other benefactors for funds for the development of social studies in Oxford' and asked the anthropologists for an account of the 'work which is now done and suggestions for its development'. At this point the social sciences were greatly outnumbered by the Arts faculties, who between them taught 80 per cent of Oxford's students. This was the green light for Marett and Myres to put together a series of elaborate building and staffing plans for the development of anthropology. They proposed a whole new Faculty of Human Sciences, with its own Institute bringing together included the sub-faculties of geography, anthropology and economics. It was yet another political fudge, full of references to the importance of studying the 'dynamic process of Man's physical and mental evolution' in order to appease Balfour. Even the specific request for a Professorship in Social Anthropology was hidden in the section on 'Accommodation and Equipment'. In order to keep the rest of the committee on board, Myres and Marett had been forced to push the case for a broad definition of the subject. Their proposal was finally completed in the autumn of 1934. But by then Rockefeller had decided to terminate its funding of anthropology programmes, faced with a huge drop in income caused by the world recession (Stocking 1996).

Oxford's elite All Souls College stepped in to fill the gap. After it promised to fund and host a new University Chair in Social Anthropology in 1935, the University's Hebdomadal Council finally approved its creation. Still wary of over-specialisation, the Council had suggested that the 'title of Chairs should be as wide as possible' and proposed that the Chair should simply be titled 'Professor in Anthropology'.¹⁷ Again, the Committee for Anthropology went to some length to explain the subtle but important distinctions between the different branches of anthropology, making the case that 'social anthropology is no more limiting a phrase than Colonial history, International Law, International Relations and so on'.¹⁸ The politics behind the All Souls decision to fund anthropology are explored by Davis (2007).

Marett could afford to be magnanimous in victory. Writing to Balfour on his impending retirement, he praises him for being the only one of the 'three men in a boat' surviving to 'manage the tiller'.¹⁹ Balfour's one consolation was that he was made Professor in Ethnology in 1935, despite the protestations of the Anthropology Committee that the title was a misnomer.

Anthropology as an Oxford Honours School?

Radcliffe-Brown was appointed to the Oxford professorship in July 1936. There were more than a dozen candidates, including, by his own account, E.E. Evans-Pritchard. One of the members of the appointment panel, the historian Reginald Coupland, also invited Malinowski to apply for the post. Malinowski ruled himself out of the race, explaining that he had 'a very real debt of gratitude towards the School of Economics and the University of London' and 'that I would not like to sever my connection with this institution which has assisted me so generously and effectively in developing social anthropology in this country'. Instead he proposed Radcliffe-Brown as 'by far the most suitable from every point of view', proclaiming his 'genius' at organising departments as much as his theoretical contributions. Coupland responded by expressing disappointment that they 'must be content with second best', but expressing the hope that Radcliffe-Brown would indeed actually apply for the post, as 'the electors might not think R-B's claims so outstanding as to set all our candidates aside and issue an invitation which might be refused'.²⁰ Marcel Mauss was one of Radcliffe-Brown's referees. Whilst Mauss complained about Malinowski's 'despotism' in his letters to Radcliffe Brown, he too was less than fulsome in his praise of Radcliffe-Brown's theoretical achievements.²¹

Much has been made of the public rivalry and seeming enmity between the two champions of the new school of anthropology. Radcliffe-Brown's letters show a different dimension to this relationship, as he repeatedly sought to flatter Malinowski, asking him for advice and support, and even sharing confidences about his chest problems. Back in 1929, whilst in Sydney, Radcliffe-Brown had confessed to Malinowski that 'the trouble about Anthropology is its name'. He went on to bemoan the inclusion of physical anthropology and prehistoric archaeology in a 'conventional curriculum', and wished 'never to have to teach either physical anthropology or archaeology again'. In a moment of candour, he proposed that 'you and I and anybody else who will help us ought to build up the new sociology or anthropology that is needed', preferably in Oxford,

because 'Cambridge does not suit my health'. He even felt Oxford was a risk for his chest, but a risk that would be worth it 'if I was free to treat the subject in my own way and not be required to lecture on the "Races of Man" etc.'. The letter ended with a further bout of flattery – 'I should greatly like to be working near you and co-operating more closely.' So it came to pass. Whatever Malinowski thought of his protagonist, his intervention with regard to the Oxford post was influential, and Radcliffe-Brown was, on paper at least, grateful.²²

Radcliffe-Brown finally arrived in Oxford a year later in October 1937, but had already begun to develop his vision for a more specialised research-led field. Contacting the influential classicist John Myres after his appointment, he wrote to express his appreciation at being 'called to become your junior colleague in the oldest school of anthropology in the British Empire', and seeking his advice over the 'further development of social anthropology at Oxford'.²³ Whilst Malinowski had left for the USA by this point, his success at getting his style of social anthropology institutionalised within the LSE over the 1920s and 1930s would have been closely watched by Radcliffe-Brown. Yet far-reaching pedagogic reforms were much less simple within the Oxford system.

Like others before him, Radcliffe-Brown was attracted to the idea of a final honours school in anthropology, with candidates being 'permitted to specialise to a limited extent' within one of the four fields. Before his arrival, he wrote to his colleagues about his ideas. But any proposal he drew up had to be vetted by the Committee for Anthropology, which still included Ranulph Marett, the ethnologist Henry Balfour, Leonard Buxton and the biological anthropologist Professor le Gros Clark. At first, they were in agreement, and in Radcliffe-Brown's absence, they prepared a fuller specification for the degree, which included a comparison with the London and Cambridge degrees, and the numbers of students they attracted. But they also put their own spin on the proposal, concluding that the proposed recommendation should be for an Honours School based 'on a combined study of Race, Culture and Evolution in their combined bearing on the evolution of society'. The committee took the view that the combination 'has hitherto worked so well that there is no reason to depart from it'. They concluded that the honours school should retain the principle of the present diploma and 'be on a broad educational basis'. They decided to await the reply of the new professor before submitting the new statute.²⁴

This was hardly what Radcliffe-Brown had in mind. But as he only arrived in Oxford in October 1937, he had not had the time to convince the committee members of his vision. Whilst they were all for a final honours school, they each had rather different views about its contents.

The institutional constraints on introducing an undergraduate honours degree became clearer at a meeting he had held with the university registrar in March 1939 to seek support for his vision. The registrar, Douglas Veale, in a subsequent briefing note to the vice chancellor, reported that Radcliffe-Brown had announced that 'unless he could develop a school of anthropology, preferably an undergraduate school ... a real cultural school, he would much rather go away'. He named his price – £600 a year. Veale retorted that the only way to find this sum was to save it elsewhere, asking whether 'there was any need for a reader in Physical Anthropology at £550 a year'. Radcliffe-Brown agreed wholeheartedly, saying that 'people had been measuring skulls for 60 years without producing a single result of real scientific importance', and that money for the development of social anthropology could best be obtained by retitling the Readership in Physical Anthropology. He also pointed out that Nuffield College was keen to draw on anthropological expertise in developing its colonial studies provision.²⁵

But Veale was not convinced, and asked about the competition for undergraduate students with Cambridge (which had sixty students at this point) and London, as the 'total number of undergraduate students is fairly strictly limited'. Veale then wrote to the Vice Chancellor noting that it was 'unlikely that Anthropology will ever become a school like Greats, Modern History or English Literature, which will be taken merely as an intellectual discipline by people who intend to follow careers for which special knowledge is not required'. He concluded that 'the time has arrived when we ought to concentrate on what we are doing well', leaving Cambridge to 'develop social anthropology without competition from us'.²⁶

Radcliffe-Brown was not totally alone in this fight. Evans-Pritchard had accompanied him on his visit to the registrar, having been appointed to a Research Lectureship in African Sociology in 1935. In 1939 he was joined by Fortes, who also held a research lectureship for two years, and by Max Gluckman, before he left to take up a post at the Rhodes Livingstone Institute in September of that year. This small coterie met regularly with Radcliffe-Brown, both in the latter's rooms at All Souls and in various north Oxford public houses. Radcliffe-Brown dominated, and continued to cast an intellectual spell over their debates. As Evans-Pritchard later recalls, it was these discussions over 'system' and 'structure', later reiterated in the 1940 RAI presidential address 'On Social Structure', that formed the analytical underpinnings of *African Political Systems* (Fortes and Evans-Pritchard 1940) and *African Systems of Kinship and Marriage* (Radcliffe-Brown and Forde 1950). But the relationship between 'master' and 'student' was not straightforward, given Evans-Pritchard's own charismatic personality,

and the fact that, by some accounts, he had come a close second to Radcliffe-Brown in the 1936 election.

Next Radcliffe-Brown began to push for a one-year diploma that specialised in social anthropology, seeing this as a quicker route of reform. Part of his case was the necessity to make 'better provision for an important class of colonial officials who wish to study social anthropology, but who have not the time at their disposal for the full diploma course'. No revision of the diploma regulations had been made since 1905, despite the developments in the field, which Radcliffe-Brown saw as playing in his favour. His proposal was to have a general one-term introductory course, allowing admission into a two-term specialisation for 'advanced work' in either social anthropology, physical anthropology or technology.²⁷

The proposal, like Radcliffe-Brown's other initiatives, was controversial. The General Board of Faculties – a university-wide committee – was not inclined to register three separate diplomas, given the costs of examining and the precedents they would set for other departments to establish their own diplomas. But it was his anthropology colleagues who led the opposition.²⁸ Offering what amounted to a specialist diploma in social anthropology would mean that 'physical anthropology' and 'primitive technology' would no longer be compulsory, breaking up the long-respected 'trinity' of anthropological teaching at Oxford. Henry Balfour was outraged, and was driven to write one desperate postcard to Professor John Myres, headlined 'This is an SOS', protesting at what he described as the 'extremely one-sided and narrow minded proposal now afoot'. He felt that a one-term general introduction was a 'ludicrous allowance', and would turn out a lot of incompetent 'specialists', with too little general preparation, and not enough within their own field to be of great use.²⁹

The question that divided them was simple. Would an application for three specialised diplomas weaken the case for a final honours school that could bring together the different fields? Most saw Radcliffe-Brown's preference for the specialised diploma as working against the longer-term aim of an honours school. During 1938 and 1939, letters and counter-proposals flew backwards and forwards, a phenomenon Penniman described as a 'pamphlet war' as Radcliffe-Brown sought to 'torpedo' the existing diploma. Radcliffe-Brown did not mince his words – he was of the opinion that 'Diploma students are not worth teaching, they do not have the time to do even the minimum of reading.'³⁰

Things came to a head when in late 1939 Radcliffe-Brown circulated a damning memo declaring that the standards of the diploma students were embarrassingly low, and that 'both physical and social anthropology suffer very much from persons who know a

little (or think they do) and do not know how little they know'. He felt that it should be made impossible for anyone to 'obtain a diploma in anthropology by spending three terms in picking up a few miscellaneous and disconnected scraps of knowledge about a number of subjects which it is quite impossible to study systematically in that time'.³¹

Penniman took up the debate on behalf of the Pitt-Rivers Museum, tabling a counter-resolution to maintain the existing diploma and to push for a final honours school. Citing Evans-Pritchard's support for the honours school, Penniman pointed out that the diploma had always been intended as an introductory training, and that, whilst it was 'impossible to eliminate entirely the type of student who will not do credit to his training, much can be done in this direction without wrecking the existing diploma'. Penniman felt that if they continued 'tinkering' and 'monkeying' they would be laughed at, and he was loath to accept the 'uncertain schemes of a man who always appears to change his mind halfway through any plan he sponsors'.³²

Eventually it was left to John Myres to adopt the elder statesman role, smoothing over the tensions with a set of highly complicated new regulations for the diploma that all sides could agree on, meeting several of Radcliffe-Brown's demands along the way. It remained a single diploma, but allowed for increased specialisation in one of the three sub-fields. The final syllabus was not agreed until 1940, by which point the onset of war had made students scarce on the ground.

Radcliffe-Brown was unbowed. In the same provocative vein, he decided to rename the Department of Social Anthropology as an Institute. After getting agreement from the Faculty Board to his proposal in June 1939, he then wrote to the Curators of the University Chest explaining that 'If social anthropology is ever to have any real importance in Oxford it will be because this becomes a centre for research.' He went on to note that 'there may be some chance of appealing for outside financial help' but that it 'would be somewhat easier to appeal for funds for an Institute than for a department'. Its implications would be that 'we intend (or at least hope) to be concerned not only with teaching but also with research'.³³ The following April, long after Radcliffe-Brown had physically changed the sign on the front door, the General Board of the Faculties agreed to the retitling.

The new title was no flight of fancy. Radcliffe-Brown was aware of the Colonial Office's plans for a major Colonial Development and Welfare Act, and wanted to ensure that Oxford was able to benefit from the research funding that Lord Hailey – author of *An African Survey* (1938) and architect of the act – had insisted be part of the programme. In the same year he applied for resources to support a

programme of research training in social anthropology. It was accompanied with another threat, testimony to the frustrations he faced over the final honours school and the specialist diploma. He hinted that if 'the University decides to take no effective part in the development of a subject the importance of which in a colonial empire is being increasingly recognised' it would 'leave serious work in anthropology to Cambridge and London'.³⁴ It was a vision that he was never to put into place. During the war he took up a Visiting Professorship in Sao Paulo from 1942 to 1944, and afterwards, reaching statutory retirement age, retired to the Welsh hills from where he continued to write until his death in 1955.

Conclusion

Like other historians of modern social anthropology, Kuper (1996) sees Malinowski as the 'founder of the profession of social anthropology in Britain', describing him as its only 'master ethnographer' during his fifteen years at the LSE. Yet Kuper's description of Malinowski's 'mythical charter' for his new discipline and his 'messianic self-image' underplays the role that LSE as an institution played in supporting his meteoric career. The intellectual ferment he created through his famous research seminar depended on the autonomy granted to him by LSE director William Beveridge, as well as on the left-leaning reputation that attracted so many to the LSE during the 1920s and 1930s. The personalised and informal nature of decision-making in the LSE, and the high level of autonomy offered to individuals, made it much more likely that Malinowski's iconoclastic approach to teaching and training could succeed.

This period in anthropology's history mirrors an important shift within the academic culture of British universities at this point. With growing state support for research, anthropologists did not need to subscribe to the Oxbridge vision of 'character-building' undergraduate education, but rather could focus on developing a professional research culture. At an organisational level, this shift required new centralised financial and organisational nous – the universities needed to be able to apply for and manage large philanthropic grants for research. At a personal level, this shift was often achieved by dominant, dare one say egocentric, personalities who were prepared to act as missionaries for their particular view of the world, trampling over the strongly held views of others. This style of leadership was less appropriate in the conservative and consensual decision-making culture of the Oxford faculty boards.

A comparison of the LSE and Oxford at this point reveals the powerful role that universities play in shaping disciplinary fortunes, determining what can be taught and how. The Oxford committees and the faculty boards served both to protect scholarly traditions and to constrain change and innovation. Marett may have been the consummate operator and skilled in Oxbridge institutional politics, but he could never have wrought the far-reaching changes that Malinowski achieved at the LSE. On the other hand, Oxford anthropology might well have been less successful in developing its vibrant post-war intellectual atmosphere if its application to create an Anthropology Final Honours School had been approved – given the amount of undergraduate teaching this would have necessitated.

Perhaps only an egocentric figure like Radcliffe-Brown was able to challenge Oxford's unspoken social codes. Because of his hauteur, he was seen as an *arriviste*, an outsider who didn't understand Oxford. He was treated accordingly, and his antagonistic attitudes provoked bitter resentment amongst his colleagues. On the other hand, the post-war institute inherited by Evans-Pritchard owed much to Radcliffe-Brown's success at reforming the department. Whilst Evans-Pritchard wrote regularly about Oxford anthropology (1951, 1953, 1959) he did little to acknowledge his forebear, distancing himself from Radcliffe-Brown's divisive reputation. Nonetheless, the centre of the British social anthropological universe shifted after the war back from the LSE to Oxford, at a time when the Cambridge faculty was still dominated by physical anthropologists. This changed over the following decade. Meyer Fortes was appointed to the William Wyse Chair in 1950, Edmund Leach to a readership in the same year, Jack Goody to a lectureship in 1954 and Audrey Richards to a readership in 1956.

I have tried to portray something of the protean nature of anthropology and its disciplinary identity during the pre-war period. Its intellectual shape and its status within British universities were far from clear-cut. Whilst the Oxbridge 'establishment' was able to resist the LSE approach for a while, it could not compete with the growing move towards a professorial, research-led academia, and the need to attract funds and international prestige.

Notes

1. Interview with Raymond Firth by Maurice Bloch, LSE history, LSE archive, BLPES.
2. *Ibid.*
3. Bronislaw Malinowski (BM) to Jessica Mair (JM), 10.2.30. Malinowski Archive, BLPES.
4. BM to JM 10.2.1930, BLPES.
5. BM to Kirchoff, 18.3.31 File 3, Malinowski student correspondence, Malinowski papers, BLPES.
6. Interview with Raymond Firth, 1989, LSE archive, BLPES.
7. Evans-Pritchard to BM 25.1.1.28, Malinowski papers, BLPES.
8. BM to Charles Seligman 05.9.31. Malinowski papers, BLPES.
9. *Ibid.*
10. Audrey Richards papers, AR 2/1, BLPES.
11. Evans to Jessica Mair 13.3.31, LSE archive, BLPES.
12. BM to JM 30/1/34, Malinowski papers, and Malinowski to Beveridge, 1929, BLPES.
13. BM to CS, 5.9.31, BLPES.
14. Ranulph Marett, letter to Hodson, 11.1.33, Marett Archives, Exeter College
15. Henry Balfour to Myres 2.2.33, Balfour papers ff84, Oxford University.
16. Paper 210, Oxford University Committee on Anthropology (OUCA)
17. Oxford Hebdomadal Council papers, CC1/1/2 Dec. 34.
18. DC 1/1/2, 183rd meeting, OUCA.
19. Marett to Balfour, Balfour papers, Oxford University.
20. BM to Coupland 2.7.36, Coupland to BM 31.7.36, Malinowski Archive, BLPES.
21. Marcel Mauss to Radcliffe-Brown (R-B) 22.1.35, and job reference dated 22.9.36, both re-published in *Journal of the Anthropological Society of Oxford* 25(2) 1986, translated by William Pickering, with notes by Peter Riviere.
22. R-B to BM, 3.12.29, Malinowski correspondence, Malinowski Archive, BLPES.
23. Myres manuscripts, Oxford University, MS 81 fol. 25.
24. Oxford Registry Archives, DC1/1/2 Paper 235, March 1937.
25. Memorandum by Douglas Veale 18.3.39 Oxford University Registry files UR 6/ANT/4.
26. 'Note for VC on 10th Jan' by Douglas Veale UR6 /ANT/4.
27. Myres manuscripts, MS 81, fo. 33, 138.
28. The Anthropology Committee officially became a sub-faculty of a new Faculty of Anthropology and Geography (A and G) in 1938, in order to allow research students to be supervised. Radcliffe-Brown had originally hoped to create a dedicated Board of Studies for Anthropology – here too he had to be content with second best.
29. Myres, MS 81, fo. 35, 131.
30. Penniman to Myres, 27.10.39, part of a series of letters in Myres, MS 81, fos. 33–44
31. R-B Memo to Fac. of A and G Board, entitled 'The diploma in Anthropology', dated Oct 1939 in Myres MS81
32. Penniman to Myres 27.10.39, Myres Ms 81
33. R-B to Congregation (UC/FF/228/2 1934–1947). The Faculty Board of Anthropology and Geography agreed to the change of name on 8/6/39. On 26.6.39, R-B wrote to the Curators of the University Chest, who gave their agreement, subject to confirmation by the General Board of the Faculties on 19/4/40.
34. Faculty of A and G Archives, FA 4/2/1/1 p80, Oxford University.

THE POLITICS OF DISCIPLINARY PROFESSIONALISATION

Introduction

Social anthropology's rise to post-war intellectual favour was a triumph of methodological and analytical innovation. Yet it was a triumph that depended on finding organisations prepared and willing to fund this new approach to anthropological research. Up till the 1930s, the search had been relatively fruitless, even though anthropologists had been employed by the Colonial Office and by individual colonial governments. Malinowski's breakthrough came from his skill at cloaking his vision in the rhetoric of 'practical anthropology'. His success in getting the US-based Rockefeller Foundation to support the work at the LSE transformed the discipline's fortunes.

This chapter explores these early attempts at applying anthropology and the way in which struggles over the place of this work within the discipline were played out in the professional association. Radcliffe-Brown's stormy tenure as President of the Royal Anthropological Institute (RAI) in the late 1930s provoked a major rift within the Institute. The affair led E.E. Evans-Pritchard, who was Radcliffe-Brown's replacement in the Oxford Professorship, to found a new professional body after the war. The Association of Social Anthropologists (ASA) was launched in July 1946.

The ASA was memorably described by Raymond Firth as a 'band of brothers' (Firth 1986, 5). An aura of intimacy and clubbiness

infused the shared scholarly project and theoretical outlook of its members. Membership was carefully controlled. The restrictive criteria for membership determined the new field's professional image, its public profile and its subsequent development. Nurturing new academic identities requires determined cultural work, and limiting access to this identity can be part of this process. It is in the mundane minutiae of committees that an epistemological identity gets created and maintained. The ASA's endless debates over who should become a member helped define 'British' social anthropology.

Anthropology and the Colonial Office

The first years of the twentieth century saw repeated attempts by anthropologists, and their representative association, the RAI, to convince the imperial government that anthropology deserved funding. This is a story told at greater length by both Kuklick (1991) and Stocking (1996). The first attempt was in 1896, when the British Association for the Advancement of Science passed a resolution urging the funding of a 'Bureau of Ethnology for Greater Britain' on the grounds that 'collecting information with regard to native races within and on the border of the Empire would be of immense use to science and to Government itself' (quoted in Myres 1929, 38). Despite the offer of a room in the British Museum, no government funding was forthcoming. A further deputation to Prime Minister Asquith in 1912 was equally unsuccessful.

A committee was set up at the British Association meeting in 1914 to 'devise practical measures for the organisation of anthropological teaching at the universities'. Henry Balfour, then curator of the Pitt-Rivers Museum, was enthusiastic about the idea, viewing it as of the utmost importance 'that the Empire should encourage and subsidise Schools of Anthropology which aim at promoting Imperial interests and equip future administrators'.¹ A prominent early endorsement of the importance of teaching anthropology to administrators was also made by Sir Richard Temple, a senior figure in the Indian service, who felt that Colonial Officers should 'imbibe the anthropological habit' before being entrusted with the responsibilities of 'administrative, commercial and social control' (Temple 1913). The appeals of 'practical men' such as Temple and other colonial administrators were felt to be more effective than lobbying by anthropologists themselves. Courses for colonial cadets were already in existence in Oxford and Cambridge, and in 1912, twenty-one of the thirty-four students on the Oxford diploma were in training as colonial officers. The experience often made deep impressions on these new recruits, a number of whom went on to develop careers as anthropologists (Pels 1997).

If some early colonial administrators found themselves drawn towards anthropological enquiry, the majority were disdainful. In particular, the tale of Northcote Thomas served as a 'cautionary tale in official circles for decades' (Kuklick 1991, 201) after his removal from service in both Nigeria and Sierra Leone. Northcote Thomas was initially appointed in 1909 as the first government anthropologist in Nigeria. He was tasked with making sense of survey data collected by political officers in southern Nigeria, but on the Colonial Office's condition that 'purely scientific research ... must not interfere with his main work' (*ibid.*, 199). However on both tours he pursued his own linguistic research, seen both as impractical and as irrelevant to the practical tasks set out in his conditions of employment. Somewhat of an eccentric, he had famously proposed to file his false teeth to win favour with the communities in which he worked. He was subsequently viewed within the Colonial Office as a salutary example of the risks of employing scholars to do practical work, even though a Royal Anthropological Institute committee appointed to examine his work declared itself most 'impressed with the thoroughness of his enquiries', and concluded that the materials would give 'utmost service to officers serving in that part of Africa'.²

If there was ever a honeymoon relationship between anthropologists and British Colonial authorities, it was short-lived – perhaps lasting the few years after the First World War. During this period the governments of the Gold Coast and Nigeria seconded district officers such as Charles Meek and Robert Rattray to do research on anthropological topics. Both had studied with Marett in Oxford. Rattray even founded a new Anthropological Department of Ashanti. Later, government anthropologists would be intermittently appointed in various parts of Africa, but their role was never institutionalised. As Stocking notes, 'they were for the most part consumers rather than producers of anthropological theory' (1996, 390), and their peripheral status, in both geographical and disciplinary terms, was held against them.

When funding for the RAI's proposed 'central institute' was still not forthcoming by the 1920s, it became involved in a 'Joint Committee of Anthropological Teaching and Research', comprised of representatives of universities, to discuss 'matters of common concern' and to be the 'accredited mouthpiece of all British Anthropologists' (Myres 1929, 49). Malinowski was enthusiastic about such a committee, feeling that it would 'obviate certain misunderstandings' between universities and colonial authorities, and enable British universities 'officially to cooperate with dominions ... in carrying out research which is regarded in the major portion of the Empire as of definitive value to the Empire as a whole'.³

None of this translated into direct funding opportunities until the mid-1920s. At that point, the newly launched Laura Spelman Rockefeller Memorial Fund began to support projects in human biology, providing a grant for the LSE in 1923. A series of much larger grants followed, partly because of an unlikely alliance between Joseph Oldham and Bronislaw Malinowski. Oldham was a leading figure in the Protestant missionary movement, one of the founders of the International Institute for African Languages and Cultures (IILC), and active in colonial reform. Initially unconvinced by the virtues of academic research, he and Malinowski found common ground as they courted the Rockefeller Foundation magnates.

Malinowski's talent for reading the funding runes is visible in his memorandum to the Rockefeller Foundation in 1926, headed 'Practical Applications of Anthropology'. He begins by admitting that the 'affectedly academic attitude of entirely non-practical interests is often a cloak for incompetence'. But most of his frustration is aimed at 'men of affairs'. He chides the British colonial administration for 'the unscientific manner of treatment in racial problems in India, Egypt and Africa', seeing it as likely to cause trouble in the same way that the 'unscientific way of treating Irish nationalism led to deplorable results'. He bemoans the Colonial Office view of anthropology as a 'purely antiquarian science', and quotes one official as saying 'we do not want future officers in our colonies to study anthropology', fearing that 'they would measure skulls instead of administering law; they might dig up old graves instead of looking after sanitation, or study savage superstition instead of keeping down crime and rioting'. This may, he admit, have characterized 'old fashioned anthropology' but grossly misrepresented 'the new spirit in anthropology'. Referring to the Netherlands as 'the most successful Colonial power [*sic*]', he pointed out that it 'recognises, uses and supports anthropology'.⁴

Amidst the bluff rhetoric, Malinowski's focus on the 'practical applications of anthropology' marks a subtle change in his disciplinary self-conceptualisation. Anthropology for him is no longer a 'habit of mind' to which administrators could aspire, but a rigorous and professional 'science' to be learnt and applied to the study of land tenure issues and even 'black bolshevism' (Malinowski 1929, 22). In his paper entitled 'Practical Anthropology' Malinowski suggests that, without a research base, the 'practical man' often merely 'gropes in the dark' (*ibid.*). His words were carefully chosen, all too aware that the Rockefeller philanthropy now favoured academic research that promised practical outcomes. His charm offensive succeeded, leading to a \$250,000 five-year Rockefeller-funded programme of research under the aegis of the IILC, a new missionary-influenced bureau that promoted language research. This was awarded in the face of stiff

lobbying from A.R. Radcliffe-Brown, involved in a rival bid with the School of Oriental and African Studies.

Despite this new rhetoric, many administrators still felt that anthropologists were simply not doing enough to make themselves useful. P.E. Mitchell, Chief Secretary of Tanganyika, was a prominent critic of Bronislaw Malinowski and vented his frustration at the irrelevance of academic anthropology to administrative concerns. He was of the view that the anthropologist tended 'to look out at the busy world from his laboratory window, and when he offers help, it is in terms of laboratory methods. He must learn to come down into the street and join in the life which he desires to influence.' Mitchell went on to call for some 'general practitioners of the trade' who could apply the scientific results of scientific investigation (Mitchell 1930, 220).

True to his word, Mitchell supported an 'experiment in applied anthropology' in the Iringa province of Tanganyika, involving a collaboration between an anthropologist (Gordon Brown) and an administrator (Bruce Hutt). Hutt was the District Officer of Iringa district, whilst Brown had been a member of Malinowski's LSE seminar, coming to anthropology with a doctorate in psychology. Their 1935 book *Anthropology in Action* described an attempt to closely link 'specialist research to the day-to-day business of administration'. There was one condition; that it was up to the 'administrator to ask questions, and for the anthropologist to answer them' (Brown and Hutt 1935, xvii). For Mitchell, the key question was 'Are the people well governed and content?' (ibid., xv). This circumscription of the scholarly project to the immediate demands of administrative rule exemplifies an instrumentalism often assumed by critics of anthropology's colonial complicity. Yet the collaborative relationship that Brown and Hutt developed and documented – despite its complications – was virtually unique. Both were enthusiastic about the project, and together advocated 'not only a series of partnerships between administrators and anthropologists, but the institution of some central clearing house for disposing of the knowledge so obtained' (ibid., 1935, 237). Paradoxically, for a project so tightly defined by utility and the sixty-nine questions asked of the anthropologist by the administrator, the resulting book resembled a 'classic' functionalist and holistic ethnography.

By the late 1930s Raymond Firth and Audrey Richards, both steeped in the LSE and Fabian tradition of research for social reform, had become forceful advocates for the use of anthropological skills by colonial governments implementing development initiatives. Their subsequent influence within the Colonial Social Science Research Council (CSSRC) is the culmination of numerous attempts to 'sell' anthropology to the colonial authorities. In 1938, Raymond Firth,

anthropology's consummate civil servant, prepared a seventeen-page 'Memorandum on the utilisation of anthropological services by colonial governments' for the Colonial Office Advisory Committee on Education in the Colonies (ACEC). Serving on a committee concerned to establish programmes of 'social and economic development' executed 'with the aid of community education', Firth circulated a questionnaire to a variety of administrative officers and anthropological colleagues asking about the discipline's 'utility'. Some of the responses were predictably off-centre, such as one from a district officer in Nigeria who had suggested that the anthropologists could work as 'shock troops', able to 'accompany any patrol, whether of Police or Soldiers' whenever necessary to 'clear up misapprehensions'. Most were less melodramatic, and nearly all supported Firth's views as to the utility of anthropologists, who by dint of their 'systematic training' were in a position to 'cover the whole field of systematic information required for an adequate programme of rural development'.⁵

In his final report Firth cited the uniqueness of the Brown and Hutt collaboration to make the point that, in many other territories 'with problems just as pressing', there were no government anthropologists, and that a far more 'systematic utilisation' of the discipline's services was possible. Referring to the work carried out for the government of Anglo-Egyptian Sudan by E.E. Evans-Pritchard, and that by Dr Margaret Read for the government of Nyasaland, Firth argued unashamedly for 'the appointment in each territory of a specific Government anthropologist ... trained in modern methods of field research'.⁶

Firth was open-minded about whether such appointments should be 'administrative officers of proven capacity' or someone from outside the service, who might have a 'more dispassionate approach to the problems'. His only concern was that people had prior training. Firth was emphatic that the role was not to teach 'the District Officer to do his job' but rather one of 'technical advisor ... guided by the declared policy of the Government, though the results of his investigations might sometimes influence that policy'. His memo ended with a remonstrance that large-scale plans for social and economic development without the help of anthropology would seem a 'neglect of useful aid'. It was a vision that Firth went on to pursue very effectively within the Colonial Office. In the meantime, the very promise of government funds for applied research was causing a major rift within the anthropological community.

Anthropology's professional associations

The Anthropological Institute of Great Britain and Ireland was founded in 1871 from the conjoining of two rival scholarly associations with different perspectives on human evolution and the importance of social reform (Stocking 1971). Its ambitions mirrored that of comparable Victorian scientific societies. Holding monthly London meetings, it became known for its intellectual tolerance and holistic remit. After being granted a royal charter in 1907, the Royal Anthropological Institute's (RAI) membership grew, attracting explorers, private scholars, missionaries and administrators. Whilst active in lobbying governments both home and abroad, the RAI did not see its primary role as supporting the growth of academic anthropology – not least because so few academic posts existed in the early years of the twentieth century.

Across the Atlantic, the American Anthropological Association, founded in 1902, had already demonstrated the role a professional association could play in promoting the interests of a university-based discipline. Its birth was not straightforward. The months leading up to its incorporation were marked by tension between those members of the existing Anthropology Society of Washington, who wanted to restrict its membership to forty elite 'professionals', and those led by one W.J. McGee, who wished to make it more open, valuing the contributions that 'amateurs' could give (Stocking 1960). Franz Boas, aware of the duplication of effort by local scientific societies, proposed a rationalisation of such societies within the American Association for the Advancement of Science. Boas was outmaneuvered in his own plans, partly because of growing antagonism towards the Washington clique. The majority of those invited to join the new association were against an 'exclusivist' membership policy (*ibid.*, 8). Yet Boas's fears about amateurishness and scientific populism were unfounded, the new association expanded rapidly, and soon became dominated by practicing academics. Whilst the 'four-field' terminology to describe the American academic discipline was not articulated till much later, the association welcomed a wide variety of scholarly interests. By 1917 it had 300 members, and continued to grow rapidly.

Back in the UK, social anthropology in the 1930s faced the challenge of gaining a foothold within British universities. Despite Malinowski's influence, LSE still had only three permanent staff. Variants of 'social' anthropology were being taught in only three universities, and were also competing for influence with the 'diffusionists', led by Elliot Smith at UCL.

The RAI, until 1946 the British discipline's only professional body, held one of the keys to the promotion of social anthropology – its

scholarly journals and intellectual leadership. Its mandate was to represent all branches of the discipline, a large and diverse constituency of administrators, academics and independent scholars. Those seeking to promote the academic fortunes of social anthropologists were a minority, albeit an influential one. They had to compete with those who held that anthropology should by definition include the biological and physical study of humans, as well as the study of all aspects of material culture. Some of the biological anthropology published under the RAI's imprimatur (Rich 1984) exemplified the least palatable aspects of Victorian racial science. Yet the unwieldy nature of the RAI's decision-making process, unpredictable council members and its inclusive membership policy made factional initiatives unpopular. This tension came to a head during Radcliffe-Brown's presidency of the RAI in 1938 and 1939.

Infighting at the RAI

The RAI had been at the fulcrum of sporadic attempts to involve anthropology in policy debates during the 1930s. In 1937 this led to the creation of the 'Applied Anthropology' committee. At a time of growing political foreboding, there was concern over the message that would be conveyed if the British government decided to implement a 'return of territories' to their former colonial power, Germany. Gertrude Thompson, RAI Vice-President and prominent archaeologist, led the campaign for the RAI to take a public position on the politics of appeasement. Writing to Raymond Firth, she acknowledged that 'the objection of "politics" might be raised'. She went on to note that 'the contribution anthropologists can make to the question is anthropological: the relations of the British Empire to Germany need not be discussed, but the relation of white rule to native life should be'.⁷ Firth was reluctant to get involved, and Lucy Mair and Audrey Richards initially poured cold water on the idea. But others in the RAI decided to take action, and a declaration from the grandly named RAI subcommittee on the 'Anthropological Implications of the transfer of territories from one power to another' made a report to RAI Council in 1937, recommending 'systematic and public investigations of the implications of such transfer generally' that was 'strictly from the anthropological point of view'. It also concluded that the RAI was competent under its Articles of Association to 'promote representations in this sense to His Majesty's Government' on the matter. Nearly every senior social anthropologist decided to add their signatures to the report, including Evans-Pritchard, Firth, Mair, Myres, Richards, Seligman and Edwin Smith.⁸

This new committee, with Lucy Mair as chair, suddenly became very politically active. It requested Council to make a deputation to the Royal Commission on the union of the Rhodesias, nominating Radcliffe-Brown and Edwin Smith to represent the RAI, and produced a further memorandum on the 'Condition of the Australian Aborigines' for submission to a federal government conference, again under the representation of Radcliffe-Brown. It also mandated a deputation of members in February 1938 to meet the Secretary of State for the Dominions on the question of the transfer of the territories of Swaziland, Bechuanaland and Basutoland to South Africa, insisting that 'native opinion' needed to be consulted, and that anthropologists were best able to advise on the 'most effective methods of explaining the position and consulting native opinion' and the need to devise 'terms of transfer which would safeguard essential interests of the natives involved'.⁹ Such statements were always surrounded by qualifiers, such as 'Policy is the business of Government', as the committee sought to deflect any accusation of playing politics. Yet this wasn't enough for the RAI President Lord Raglan, and he subtly rebuked the committee the following month. Writing to Firth in 1938, he acknowledged that 'the question of the transfer of the South African protectorates is one upon which individual members of the Institute may well hold strong opinions, but it is a purely political one, and the Institute, as a scientific body, should do its utmost to avoid any suspicion of interfering in politics'.¹⁰

Driven largely by Lucy Mair's enthusiasms, the committee also proposed a programme of research, including a study of changes in bride price, whereby members contribute pieces exploring European influences on the African custom of 'bride-price', seen as an 'aspect of native culture which presents particularly acute problems of adaptation to modern conditions'. As part of its promotional efforts, it enlisted the support of the colonial office representative on the committee to carry out a postal survey of Colonial Governments to assess their perceived need for trained anthropologists. A variety of responses were received, such as the highly revealing response of the Kenyan Governor, who welcomed the 'opportunity of working out detailed proposals for co-operation over a period of some years on the problem of native land tenure in Kenya'. Such initiatives are indicative of the growing political confidence possessed by anthropologists during this period.

With power comes dissent, and the story of the Applied Anthropology Committee exemplifies the internal rivalries and intrigue within the Royal Anthropological Institute. The proliferation of committees was not simply a testament to the Kafkaesque aspects of the RAI's bureaucracy, but also a jockeying for position and authority

by rival academic factions, particularly during the period from 1939 to 1940 when Radcliffe-Brown was President of the RAI. His allies, especially Meyer Fortes, but also Lucy Mair, found they increasingly antagonised those who, by dint of their academic training or professional lives, valued a more inclusive vision of anthropology. This rival group included Herman Braunholtz, Keeper of Ethnography at the British Museum, the RAI Secretary William Fagg, who was Deputy Keeper of the Department of Ethnography at the British Museum, and the managing editor of *Man*, Ethel J. Lindgren, who had been trained at Cambridge. Other supporters included Charles Seligman, ex-Professor of Ethnology, and the William Wyse Chair at Cambridge, J.H. Hutton, previously employed by the Indian Civil Service. Sharing a view of anthropology that reached beyond academic concerns, they resented Radcliffe-Brown's attempts as RAI President to 'run down' or remould the Institute as 'mouthpiece for his particular brand of social anthropology'. Writing in 1940 to Herman Braunholtz, William Fagg's fears over 'R-B's defeatism' and the future 'ruin' of the Institute were prescient:

Radcliffe-Brown seems to be hag-ridden with the idea that there is a New Order impending in this country in which there will be no room for the Institute; he seems bent on shutting down the Institute for the duration, maintaining perhaps its skeleton at Oxford (or even in Raglan's vaults) ... Either anthropology will be left without an effective central organisation, or a rival body (of which I have often heard dark threats) will be set up at Cambridge. I believe, in opposition to R-B and his school, that the Institute can and should use the war to consolidate its position with the Fellows it can retain, so that when better times come it will be 'in training' and ready for a thorough-going crusade.¹¹

For this reason, William Fagg vowed to stay on as RAI secretary, despite Radcliffe-Brown's attempts to replace him with Raymond Firth; the latter refused point-blank to take the job, given his wartime responsibilities in Naval Intelligence. The earlier activities of the Applied Anthropology committee had been controversial, and Fagg particularly resented what he called Radcliffe-Brown's 'injudicious' decision to revive its 'ghost', which had gone into abeyance whilst Mair had been doing fieldwork in Africa. The conflict came into the open over appointments to yet another new committee – the 'International Affairs' (later known as the Colonial Affairs) subcommittee of the Applied Anthropology Committee.

The crisis had begun in February 1940, when Meyer Fortes wrote to Fagg from Oxford, proposing a 'sub-committee' to draft a memo to the British Colonial Secretary about research funding. Meyer Fortes, acting as secretary of the RAI's Applied Anthropology committee,

sought the support of the RAI council for a memorandum to the Colonial Office on the need for anthropological contributions to research in the colonies. The British Government was planning a major research effort in its colonial possessions as part of the Colonial Development and Welfare Act (CDaW). Fortes mentioned that he had been asked the previous year by the Colonial Office about 'anthropological research needed in the colonies ... if and when money should be allocated for this purpose'. Recent statements by Lord Hailey had made clear just how much money was available for colonial research, and Fortes felt that an authoritative RAI statement on the need for anthropological research would carry weight in the Colonial Office. In their submission Fortes and others wished to stress the potential contribution of social anthropology, and tempers rose over whether the memo should mention the study of material culture and technology and whether funds should be directed to research centres based in the colonies.

William Fagg was far from enthusiastic about the creation of yet another subcommittee, and was also suspicious of Fortes's position in the Radcliffe-Brown camp, feeling that he was not acting with the RAI's interests at heart. Quickly searching for someone seen as more supportive of the Institute, Fagg nominated Dr Margaret Read – soon to be made Professor of Education at the Institute of Education in London – as chair of this new memo-drafting committee. Writing to Fagg, Ethel Lindgren warned that 'we must, of course, be prepared to have our plan defeated by the President, who is accessible to influences from Fortes at Oxford – with the result that Mair or Fortes will be placed in the seat warmed for Dr Read. Needless to say, I shall support Dr Read.' Fagg agreed, strategising that Read's 'nomination should be sprung on the President and his faction only at the meeting'. There was an extra complication. Whilst Lucy Mair was a Radcliffe-Brown sympathiser, she was thought to be 'very wet blanketish' about Audrey Richard's ambitious ideas for a set of regional research institutes. Radcliffe-Brown however was in support, and had 'insisted they should be dealt with by the Applied Anthropology committee'.¹²

Once the committee was finalised, the memo-writing commenced. Unsurprisingly, the first draft called again for a central bureau, such as the RAI, to coordinate research. However, Radcliffe-Brown was far from happy with it, and prepared an alternative draft, cleverly titled as a 'Memorandum on the Hailey Report'. He insisted that 'research into the anthropological side of colonial problems will yield best results ... if carried out from research institutions in this country'. Radcliffe-Brown argued 'that in the first place the task for research as well as that of training should be entrusted to British Universities' with 'departments of Social Anthropology headed by a Professor of the

subject'. This amounted to a call for an expansion of appointments at Oxford, Cambridge and London; the creation of an Institute for Colonial Anthropology was added as an afterthought. Predictably the RAI officers were furious, as it made no attempt to build upon what Lindgren felt had already been achieved, 'anthropological training for all colonial probationers, anthropological research by government officials, the appointment of government anthropologists'. For her, it simply emphasised a 'purely academic viewpoint'.¹³

At this point things became more Machiavellian. Fagg nurtured John Hutton's opposition to this 'new' anthropology and John Myres's growing unease with Radcliffe-Brown's style at Oxford to mount a counter-offensive. Capitalising on what Fagg later described as a 'considerable error of judgement on R-B's part' in not consulting Seligman whilst drafting his document, Fagg ensured that the final meeting to approve the draft was packed with people aggrieved at the manner in which Radcliffe-Brown had handled the matter. Later, writing to Lindgren, Fagg commented with satisfaction that 'the enemy came prepared to negotiate', and that, going through the whole thing 'sentence by sentence', we 'exacted fairly severe terms'. But the dispute seemed primarily to have focused on how to mention the study of technology and material culture, and whether to take a position on the importance of 'team-work', both aspects that Fortes and Radcliffe-Brown disapproved of. The final draft, endorsed by the RAI Council, ended with the muddled compromise that, whilst 'the setting up of research institutes in British universities and in the colonies themselves is recommended as the primary necessity, plans should be made for establishing an Institute of Colonial Anthropology'.¹⁴

As Fagg later acknowledged, comparing the event to his fears over the current war, 'the victory of which I spoke was of course purely relative ... we have neutralised the worst features of the original draft, and ensured that our own sociology Hitler will not achieve "world domination"'. Fagg feared that the neutering of the memorandum would 'hammer anthropology in general by showing us up as a muddle-headed lot who have the greatest difficulty in restricting our chauvinistic elements', adding that the Colonial Office 'may think we must put our house in order before we expect much consideration'. Dark rumours began to spread within the RAI about a potential new rival association for social anthropology, portents that came true the year after the war.

The 22nd of July 1946 and the foundation of the ASA

The infighting and rivalry no doubt concentrated the minds of Meyer Fortes and his colleagues at Oxford, most particularly that of Evans-Pritchard. It was Evans-Pritchard who masterminded the creation, launched as the Association of Social Anthropologists (ASA) on the afternoon of Tuesday, 22 July 1946. Historians are in agreement about its significance. Kuklick (1991) suggests that this event marked 'the era of professional domination of British Anthropology', whilst Kuper describes its first committee as 'reflecting the power-structure in the profession ... this was the power map and it remained the same for twenty years' (Kuper 1973).¹⁵

Intellectually, the association's roots lie in regular pub discussions (and the occasional meeting at Radcliffe-Brown's rooms in All Souls) in Oxford before the war. Stocking describes how, at the initiative of Evans-Pritchard, 'the little group that had met with Radcliffe-Brown on Fridays before the war was joined at Oxford by another half-dozen younger anthropologists' (Stocking 1996, 423). The earliest meetings between Radcliffe-Brown and Evans-Pritchard dated back to the early 1930s, when the former had been visiting Europe on the way to his new post in Chicago. Audrey Richards wrote to tell her supervisor Malinowski about these meetings, saying 'EP is making up to him'. Evans-Pritchard subsequently brought Fortes into these discussions on the nature of the lineage, and the following year invited Gluckman to join them.

During 1938–39 the pub debates focused on Evans-Pritchard's and Gluckman's field data, as well as on kinship and the whole the notion of the social 'system'. This led to the publication of *African Political Systems* (Fortes and Evans-Pritchard 1940). Evans-Pritchard's correspondence with Fortes reveals the former's key role in these events, along with his military style campaign to consolidate this new school of social anthropology. He and Fortes planned how they would succeed to the Oxford and Cambridge chairs respectively. 'In the end, we will win our fight and it looks as if the fortunes of war have changed in our favour' (quoted in Goody 1995, 179).

Evans-Pritchard clearly felt that his own role in the founding of the ASA was not as acknowledged as it might have been, for in 1971 he wrote to the ASA Secretary 'to record matters for the record'. In his letter to Banton he described the sequence of events: 'If I remember rightly, I wrote to Radcliffe-Brown from Cambridge when I was a Reader there in 1945–46, suggesting that social anthropology might be considered an autonomous discipline and that we should combine in an association to further its interests.'¹⁶ As the letter implies, the association was always intended as a pressure group as much as an

intellectual talking shop. From the first meeting, the new association sought to cultivate its own institutional culture and disciplinary identity, and this was best achieved by keeping close control over who would be allowed to join. Whilst the symbolism of this new association was much welcomed by its members, the bureaucratic momentum it initiated was less anticipated.

Evans-Pritchard's initiative began at the end of the Second World War, a period that had seen anthropologists dispersed, and many involved in active service. Evans-Pritchard himself had served in Sudan. With the impending retirement of Radcliffe-Brown from the Oxford chair in 1947, Evans-Pritchard seized the opportunity to bolster the nascent discipline and his own place within it. Having discussed his idea of a professional association with Meyer Fortes and Radcliffe-Brown, Evans-Pritchard then went on to invite Siegfried Nadel, Max Gluckman, Audrey Richards, Brenda Seligman, Raymond Firth, Daryll Forde and Edmund Leach to attend the first meeting in 1946. The archaeologists Anthony Arkell and Louis Leakey were also in attendance, but were not invited to become members. They were a cosmopolitan – if male-dominated – group. Two were South Africans (Gluckman and Fortes), one a New Zealander (Firth), one a Welshman (Forde), and two had relatively aristocratic English origins (Richards and Evans-Pritchard).

Ten days beforehand, Evans-Pritchard circulated a short one-page memo on his proposed agenda. 'It is suggested', he began, 'that Social Anthropology is now sufficiently distinct a study to have its own Association and Journal and that a co-operative undertaking of the kind is desirable in the interests of the science.' Diplomatically, he went on to insist that the association should not be 'in rivalry with existing institutions', suggesting that it might even be an autonomous section of the RAI. He carefully articulated what he saw as being the objects of the association, which included ' a) to propagate the interests of Social Anthropology, particularly by strengthening the existing university teaching departments and encouraging the formation of others b) to co-ordinate research c) to constitute a body, representing the interests of the science as a whole, to which governments and other corporations desiring advice on questions of research can apply'.¹⁷ These aims were adopted word for word at the first meeting of the association, a measure of both his dominant position and force of character.

This blueprint for the association was explicit about the ASA's political role. This can be contrasted with the formation of the British Sociological Association (BSA) a few years later, distinguishing itself from the older Sociological Society, founded in 1904. The BSA was launched through a letter to *The Times* newspaper declaring its intent

to foster discussion and ‘coordination’ of research, with the aim of promoting a ‘systematic science of society’ (quoted in Platt 2003). The BSA was not linked with a specific theoretical ‘school’, indeed an initial recruitment letter suggested that it was ‘not desirable to define the terms “Sociology” and “Sociologist” in a very strict way’ (ibid.). Nor was it so concerned with gatekeeping its membership. Of the twenty-four original sponsors of the BSA, two would have seen themselves primarily as anthropologists (Raymond Firth and Meyer Fortes).

Two related aspects of Evans-Pritchard’s original vision were the source of much subsequent conflict within the ASA. He had proposed that one of the nascent group’s roles would be to produce a ‘register of anthropologists in the British Empire’. He felt that ‘social anthropology might in the first place be limited to teachers and research workers in Social Anthropology in Great Britain, the Dominions and the Colonies, but could later be expanded to include teachers and research workers in America and elsewhere’. Why not just Great Britain? Radcliffe-Brown and Malinowski, the theoretical and methodological progenitors of social anthropology, had taught in numerous places, and it was their students that the association was intended to bring together. This vision of a Commonwealth association was not initially reflected in the association’s title, for which Evans-Pritchard had provisionally suggested ‘The Association of Social Anthropologists’, but did closely guide the committee’s decisions on whom to invite to join.

The second was Evans-Pritchard’s proposal that the association would publish a journal ‘devoted solely to Social Anthropology ... such a journal would publish only contributions to theory and methodology, and not ethnographic fieldwork reports’. Aware of the importance of having a sufficient membership to support the publication of a journal, he suggested that the project should be discussed ‘with a view to starting publication in two years’ time’. Once this happened, he envisioned that ‘the association could be thrown open to all who care to join it’. This may well have been a throwaway gesture of inclusivity, as it flatly contradicted his other views of the association’s purpose. The proposal to open up membership was not revisited.

Along with this memorandum, Evans-Pritchard strategically circulated two very different responses that he had received to his proposals, from Siegfried Nadel, who had studied under Seligman, and Max Gluckman. Nadel urged that the association should address the issues of applied anthropology, and provide some ‘scope for discussing colonial problems so far as they come within the purview of social anthropology’. Gluckman, on the other hand, was adamant that ‘in the present situation there is a grave danger that the demands of colonial governments for research worker may lead to an excessive concentration on practical problems to the detriment of basic research

and the lowering of professional standards'. Gluckman was, at this point, at one with Evans-Pritchard on the importance of prioritising the theoretical development of the discipline, and Evans-Pritchard's circulation of these memos served to highlight the issues at stake.

Everyone that Evans-Pritchard had invited was present at this first meeting in Oxford in July 1946. The minutes recorded the resolution 'that a professional association of teachers and research workers in Social Anthropology be here and now formed as an independent body'. At this first meeting, chaired by Radcliffe-Brown, Evans-Pritchard was appointed 'Chairman and Secretary-General', whilst Firth, Forde and Fortes made up the committee. The minutes note that 'until next conference the committee have power to invite anyone to become a member. One black ball to exclude.' The last business of the meeting was to draft a letter to inform the RAI of the new association, 'hoping for collaboration with the Institute'. There was no formal response. Many involved in the new association were already active officers and council members within the RAI, and it was not felt that the two were mutually exclusive. The ASA did, however, take on the task of lobbying the Colonial Office over the allocation of research funding of anthropologists

The first committee meeting was held in Raymond Firth's office in the LSE a few months later, and two lists of potential members were drawn up, nine from Great Britain and double that from the 'Dominions', most of whom subsequently agreed to join. As Firth (1986) notes, the emphasis was on choosing 'people who had been trained by either Malinowski or Radcliffe-Brown'. Gradually, tensions surfaced. Firth comments, a little disingenuously, that they never actually used a set of black balls. He describes how the qualities of each of the proposed candidates were discussed in the committee meetings, and remembers how decisions were often 'idiosyncratic', with 'Fortes in particular vehemently protesting against some of the names raised' (Firth 1986). As a result, after this initial expansion, only four further members were elected over the next three years, one of whom was Elizabeth Colson, then Assistant Director of the RLI, and the first American member. By 1950 a sense of staleness was developing, and it was agreed to change the process to one of majority voting. Gluckman wrote to Clyde Mitchell, telling him that 'you'll be pleased to hear that the ASA is going into younger people more rapidly – as soon as they show they are likely to stick it as professionals. We all feel we need younger blood.'¹⁸ A significant number of new members were appointed in the early 1950s, including another American, David Schneider, who lectured at LSE from 1949 to 1951.

For the next few years Evans-Pritchard was the energy behind the association, acting as Chairman and Honorary Secretary, and taking

on all the administrative duties of calling meetings, organising agendas, and sending short communications to *Man*. The committee was reconstituted in 1952, separating the offices for Chairman and Secretary, and appointing three other members. Gluckman's appointment as Secretary began in that year, and he took over the extant administrative files from Evans-Pritchard. Much later again, in April 1960, with the subscriptions of more than 120 members to attend to, Gluckman proposed that the offices of Secretary and Treasurer should be further separated.

Raymond Firth has referred to the pioneers of the Association of Social Anthropology of the Commonwealth (ASA) as a congenial 'band of brothers'.¹⁹ Even if not all brothers – Brenda Seligman and Audrey Richards were founder members and highly influential anthropologists – this phrase captures of the association's fraternal ethos. Firth notes how its gatherings, initially in Oxford, London or Cambridge, were 'eagerly anticipated'. One of the appeals of the ASA in its early years was that its meetings were, appropriately enough, intensely social occasions.

As Jack Goody recalled, 'the closeness of the fraternity was one way in which the highly amorphous subject of anthropology was given some manageable bounds, and some continuing focus was provided for current investigations' (1995, 83). Gluckman describes one such meeting in the mid-1950s as a 'riotous time ... we drank and joked and talked'. Each character gets a mention: 'Radcliffe-Brown was on good form, Evans-Pritchard at his best.' He described how a 'shocking psychotic' paper by Reo Fortune had 'told us all about Papuans waging war because the un-avenged ghosts of their kinsmen set wild boars to break the garden fences, so we ended by calling social anthropology the study of piacular pigs jumping through holes in the social structure'. Yet his letter to his student Mitchell also reveals the cliquy and masculine intellectual rivalry that equally pervaded the meeting, with rude comments directed at the female members of the LSE faculty in particular. 'Kaberry was horrified when E-P and I said a lineage was a political group more than a kinship group. She, Audrey, etc will never understand.'²⁰

Until 1958, the association met twice yearly. Declining attendance at meetings in the late 1950s led to calls for just one annual meeting to be held each year. As well as group discussions on theoretical themes (the first in January 1947 was on social structure, opened by Radcliffe-Brown and Firth) the meetings also addressed more prosaic institutional, pedagogic and disciplinary concerns. During early meetings Firth gave a paper on research funding, Audrey Richards on the relationship of anthropology to related disciplines, and Fortes on the teaching of anthropology. Yet one issue repeatedly dominated the business meetings – membership.

One black ball to exclude? Disciplinary and national exclusivity

What should one make of the energy the association devoted to debating its membership? The association stands accused, by Edmund Leach amongst others, of being more a professional trades union than a learned society, and the colonial administrators who made up many of the students on anthropology courses were excluded from joining the ASA.²¹ Even during the 1950s, there were regular committee discussions about membership criteria. One could see these as merely internal matters, a concern only for the keeper of the records. Yet the minutes reveal a great deal about the self-perception of the association's members.

Philosophical viewpoints or scientific schools are often associated with particular nations. For the ASA the implied link between disciplinary and national identity ('British social anthropology') had powerful historical symbolism. The British government was at that point wrestling reluctantly with the implications of decolonisation, reinventing the Empire as a Commonwealth, a new, more equal partnership of states. Yet anthropology had been centrally funded by the Colonial Social Science Research Council. The ASA had brought together a community of scholars in institutions across the world, but held all its meetings in Oxford, LSE or Cambridge. This presented problems. Could the ASA reinvent itself as a Commonwealth association, rather than a colonial one? The reality was more complex still – the ASA was a metropolitan scholarly community, brought together through the colonial intellectual exchanges of its key teachers. The 'British' aspect of 'British social anthropology', a title first ascribed by the American anthropologist George Murdock was a seemingly neutral unifying concept, a label for a school of anthropology fostered first at the LSE. Its colonial resonances were left unexplored.

This vision of a Commonwealth organisation immediately created conflicts over who counted as 'one of us', and was therefore eligible for membership. The initial qualifications for joining seemed simple. 'Membership of the association is limited to persons holding, or having held, a teaching or research appointment in Social Anthropology.' Yet such simplicity was deceptive. One early committee minute noted wearily that there had been 'the usual discussion of rules of election'. The tensions and recriminations surrounding the open discussion of candidates' strengths and weaknesses led to Audrey Richard's proposal in 1948 that 'election should be by secret ballot, with a two-thirds majority necessary'. In a 1953 committee meeting a 'research post' was interpreted as possession of a Ph.D., being an indicator that the person was 'on the way to becoming a professional anthropologist'.

The significance of these election procedures is revealed in the 1956 debate over whether to expand the committee to include younger members of the association. The proposal was rejected, because it 'might be embarrassing to have young anthropologists sitting in assessment of people almost their coevals'.

The demographic history that resulted is important. From the eleven founder members, recruitment occurred primarily along personal networks of colleagues and promising students. Over the first twenty years, the ASA's membership doubled every five years, but its modest size was no measure of social anthropology's prominent intellectual profile at the time. In 1963 all fifty-five academic anthropologists with posts in the UK were members of the ASA, and most sought to attend the twice-yearly meetings. By 1961 ASA membership totalled 140, going up to 220 by 1966. In contrast, seventy attended the British Sociological Association's (BSA) founding meeting in 1950, and a letter to *The Times* led to 250 further members by the following year (Platt 2003). In 1966, the BSA had 800 members.

Students equally caused the association repeated headaches. After Fortes gave a paper to the ASA meeting in 1949 on the teaching of anthropology, the next business meeting proposed that graduate students should be encouraged to arrange parallel meetings, as they were not invited to attend ASA meetings themselves. The committee agreed to look into 'ways and means' of supporting such an initiative, but it was a controversial idea, and was quietly dropped.²²

What is the difference between a learned association and a professional association? The former, according to Barnes (1981), is concerned solely with the pursuit of truth, whilst the latter focuses instead on the promotion and defence of its members. The distinction is a neat one. Yet it rarely holds for long. There can be few learned societies that are so altruistic or devoted to the disciplinary vocation that they totally neglect their members' more mundane interests. Stocking's pithy appraisal of the Association of Social Anthropologists of Great Britain and the Commonwealth as 'part trade-union, part debating society' aptly captures this tension (1996, 429). The founding members would not have described themselves as 'professionals': their academic standing implicitly legitimated their authority. Yet they used this dual identity to create a disciplinary 'closed shop' and an association that was highly effective at promoting its members' interests and scholarly identities.

Notes

1. RAI A58/2/2/1.
2. RAI A22/3/5.
3. RHO 58/2/15/26b from Malinowski, 27.5.31.
4. Oldham MSS Afr. 1829, RHO, pages 1–2.
5. Firth Archives 2/1/1–5, BLPES.
6. *Ibid.*
7. RAI 43/1.
8. RAI 43/5/1.
9. RAI A43/7/5.
10. RAI A43/9/5.
11. RAI 6/28.
12. RAI A43/13/4, 95/7/10, 95/7/12.
13. RAI 43/13/13, 43/13/10.
14. RAI 13/25 p21.
15. Kuklick (1991, 10) suggests that this event marked ‘the era of professional domination of British Anthropology’, Kuper (1973, 119) describes its first committee as ‘reflecting the power-structure in the profession ... this was the power map and it remained the same for twenty years’, and Stocking (1996, 429) notes how the ‘association became a very effective professional group, part trade-union, part debating society’.
16. ASA archives, LSE A1.7.
17. ASA Archives 1.1. Unless noted otherwise, subsequent references are to ASA committee minutes.
18. Mitchell MSS, 5/123 Rhodes House, letter from Gluckman 2.9.1950.
19. Interview with Sir Raymond Firth, December 1999, Highgate.
20. Gluckman to Mitchell, 2.9.1950, Mitchell archives 5/1 ff123, Rhodes House.
21. With hindsight, Leach was explicit about this, claiming that ‘the original role of the ASA was to prevent the Universities from employing unqualified refugees from the disappearing Colonial service to teach “applied anthropology”’ (Leach 1984, 18).
22. It was not till the 1990s that students were offered associate membership of the ASA.

ANTHROPOLOGY AT THE END OF EMPIRE

Introduction

In this chapter I describe the evolving relationship between the discipline of social anthropology and the main funder of social research in Britain in the years after the Second World War – the Colonial Office. The Colonial Social Science Research Council (CSSRC) funded a whole generation of anthropological researchers, and a major programme of training and research institutes in Africa: all the key building blocks of an academic discipline.

The story begins in the dark days of war, and with the networks of reformers who sought to influence British Colonial Office policy. At home, debate about the eventual post-war reconstruction of Britain led to plans for the building of a new ‘welfare state’. At the same time, the British Colonial Office began to develop an ambitious blueprint for a new ‘developmental’ empire to complement domestic reconstruction plans. Known as the Colonial Development and Welfare (CDaW) Act, this major piece of legislation was passed in 1940, and sought to transform and revitalise the British colonial state (Butler 1991, 1999). Wartime collectivism and a vogue for strategic planning led to a highly ambitious £120 million spending programme. A programme of social science research would inform planning decisions.

Two of Malinowski’s students ensured that anthropology was well placed to influence CSSRC deliberations. Audrey Richards was a family friend of the imperial reformer Lord Hailey, and had worked with him

on the influential *African Survey* (Hailey 1938). Her aristocratic background provided her with the social connections and confidence to lobby effectively for the discipline. As a result, Raymond Firth, a serious-minded New Zealander, who worked for British Naval Intelligence during the war, became the Council's first secretary.

Once established, the Council provided extensive funding and support for the research into colonial social 'problems' that would inform 'development' planning. An elaborate programme of grants, studentships and regional research institutes was designed. The key challenge facing the CSSRC in its first years was finding appropriately trained people willing to serve in the colonies. It was a gap that anthropologists were all too happy to fill. The Council tended to be dominated by LSE academics, leading to tensions within the discipline over the distribution of funding. The grants, fellowships and research centres played a vital role in securing the institutional fortunes of social anthropology, providing students and staff with many opportunities to conduct research.

An empire of moral purpose? The Colonial Development and Welfare Act

The idea that the state could be an agent of reform had been gaining in popularity in Britain since the turn of the century. The writings of John Maynard Keynes provided an influential justification of state intervention, and the American experience of the 'New Deal' was also much discussed in domestic policy circles. This began to influence thinking within the Colonial Office, as did the increasingly critical public debate around the 'colonial question' during the 1930s. Gradually the colonial empire began to be seen 'more as a whole, and as a stage upon which more interventionist and generally applicable policies might be evolved, beginning with Colonial Development and Welfare' (Hyam 1999, 257). The Colonial Office began to see that the emerging consensus about domestic social and economic problems could also be applied to the colonies.

Prior to 1940, there had been virtually no government funding for British social science research in Africa. The first Colonial Development Act of 1929 was primarily concerned with reducing unemployment in the UK (Morgan 1980). This contrasts with the Australian National Research Council, which sponsored many of its doctoral students, including the first fieldwork trips of Raymond Firth, Phyllis Kaberry and Ralph Piddington, all of whom went on to play an influential role in British anthropology.

My story begins at a meeting in Whitehall at the Carlton Hotel on 6 October 1939. The war had just begun. Called by the Secretary of State Malcolm MacDonald, it was attended by a group of colonial reformers and writers associated with the Fabians. Present too was the larger-than-life scion of British imperial reform – Lord Hailey (Cell 1992; Wolton 2000).

The Secretary of State briefed the meeting. Dampening expectations of a major new policy shift, instead he raised three topics for discussion, of which ‘the policy with regard to land is fundamental to everything else’. He insisted that ‘we need more knowledge on this subject’. The best way forward, he suggested, would ‘be a series of local inquiries’ rather than a royal commission. Lord Hailey characteristically pointed out that the object of the meeting was ‘not to decide what policy ought to be adopted, but to explore what investigations it might be useful to initiate with a view to arriving at a policy’. The emphasis on careful planning and research was taken up by Margery Perham, an influential public spokesperson on colonial affairs and an important reformer, who commented that ‘land could not be studied in isolation’. She opined that ‘a new technique of study was needed. There might be three or four experts, each knowledgeable in a different subject, investigating the whole range of these subjects co-operatively.’¹ The stress laid on the importance of a carefully planned and prioritised multi-disciplinary research model echoed Hailey’s own views in the *African Survey* (Hailey 1938). The conversation went on to discuss the economics of this new colonial settlement and the major investment it would need from Britain.

What lay behind this new altruistic vision? Much of it was motivated by self-interest and self-image, and the difficulty of defending the colonial project to a much-needed political ally – the USA. As Hyam notes, MacDonald was anxious to make the colonial position in wartime unassailable. It was ‘essential to get away from the old principle that Colonies can only have what they themselves can afford to pay for’ (Hyam 1999, 275). A second factor was the influential Moyne report, compiled on economic and social conditions in the British Caribbean islands after a period of anti-colonial protest that the Colonial Office euphemistically called ‘disturbances’. Lord Moyne recommended that a ‘West Indian Welfare Fund’ be established to finance colonial development in those particular colonies.

The final piece in this new modernist jigsaw was Lord Hailey’s voluminous *African Survey* (Hailey 1938), a multi-authored Carnegie-sponsored report into ‘economic and social conditions in Africa’. A highly influential survey, it became an urtext for British colonial reformers and social scientists. Sally Chilver notes that within weeks of its publication it was ‘as familiar an object on the desks in the Colonial Office as ... the Imperial Calendar’ (Chilver 1957, 121), and that ‘there

can have been few books that have exercised such a direct influence on policy'. Key to its significance was its proposal for the wholesale reorganisation of colonial research and enquiry. Together these factors convinced MacDonald of the importance of including a special research fund within the planned Colonial Development and Welfare (CDaW) Act. They went to visit the Chancellor of the Exchequer together. He was clearly amenable, for the new act was passed in 1940, at one of most difficult moments of the war. It allowed for a spending of five million pounds annually for ten years, and more significantly, another £500,000 for research each year. It was an impressive sum of money.

With the publication of the *African Survey* Lord Hailey had come to be regarded as a principal spokesman for colonial reform and development. He was never an academic, and after an influential career in the Indian Civil Service, he had been chosen to lead the survey precisely because of his lack of African experience – he would bring a 'fresh eye' to the task, and be able to dispassionately assess the evidence.

He was first offered the opportunity to carry out the survey in 1933, but he had no idea that the task was so enormous, and would almost overwhelm him. Its ponderous compendiousness is both a strength and a weakness, providing what now might be described as a 'baseline' survey of colonial and African systems of governance in sub-Saharan Africa. It was written by a team of scholars, including Lucy Mair and Audrey Richards, who were seconded to the Colonial Office during the war. The experience convinced him that anthropology could play an important role in informing development policies (Hailey 1944). However, the report's emphasis is determinedly administrative, with chapters on 'native administration', 'the problems of labour' and 'African economic development'. In some ways the most influential part of this text was the conclusion. It was here that Hailey recommended a broad programme of 'research', a term he carefully gave a wide connotation, describing it as 'studies either of an abstract or (to use a convenient term) of a practical nature' (Hailey 1938, 1611). He demonstrated how previous research had been 'in response to an unrelated series of demands rather than as the outcome of comprehensive planning', and so made a strong case for 'liberal assistance from the British Treasury' (ibid., 1629) for a new fund. It was a view of the relationship between research and policy that was increasingly common in the USA. His ideas were taken seriously, and plans began to be drawn up for the coordination and funding of research as part of the CDaW Act.

The Colonial Research Committee and the CSSRC

After the CDaW Act was published on 20 February 1940, Secretary of State Malcolm MacDonald wrote to Hailey congratulating him on his role in its creation, and asking Hailey to be chair of a new Colonial Research Advisory Committee, for 'the general scheme for colonial research is so much your own project, and one which I know you have so deeply at heart'.²

A month later, the Secretary of State wrote again to suggest possible committee members, including up to fifteen scientists and even 'a business man connected with one of the big companies, such as Imperial Chemicals'. MacDonald went on to write that 'the selection of members to represent the somewhat wide field of sociological research may perhaps be a matter of greater difficulty'. This was a moment to express his reservations about these new sciences. 'The trouble of course, is that sociological research covers such a very wide and divergent field, and it will be very difficult indeed to get together a really representative and harmonious committee to tackle this work.' Then things get even more sticky:

One special point on which I shall wish to consult you on your return is whether an anthropologist should be included on the committee. I felt that I shall be pressed later on to include an anthropologist, but I gather that it will be rather difficult to find one who has not his own personal axe to grind, and I am told that in any case anthropologists, as a class, are rather difficult folk to deal with.³

Hailey duly responded to these thoughts. Diplomatically ignoring the implied criticisms of the social sciences, he pointed to those academic disciplines that MacDonald's proposed representatives might not be able to cover. Of these, he felt that geology and anthropology particularly deserved particular attention. Agreeing that 'it is true that anthropologists are difficult folk to deal with', Hailey pointed out that 'there are many people, not themselves professional anthropologists, who will constantly make it their business to remind you, that it is useless to provide for enquiry into the physical sciences, unless you consider also the human elements to which the result of these enquiries must be applied'. He went on to suggest that 'some of the colonial governments would feel the committee to be incomplete, unless this side of the enquiry were represented'.

In a veiled reference to nascent anti-colonial movements, he noted that both the Gold Coast and Kenya had recently admitted to 'grave gaps in their knowledge of the native social organisations with which they have to deal'. Hailey acknowledged MacDonald's concern regarding the 'limits within which support should be given to

anthropological studies by a body such as that which you are establishing.' 'Its aim,' he went on:

is not primarily to encourage academic study ... it is limited to discovering those things which our administration must know if it is to make the best use of its resources for the development of the people in the colonies. In looking for an anthropologist therefore, our main object should be to seek someone whose experiences enable him to help in estimating the social factors which must be taken into account if our technical or administrative agencies for development are to operate with success.

Insisting that the committee's focus should be on the social factors 'if our technical or administrative agencies for development are to operate with success,' Hailey put forward two 'very suitable' names, Edwin Smith (an ex-missionary, and one of the founders of the IAI) and Raymond Firth, and commented that the latter 'would perhaps be more acceptable to the younger school of anthropologists'.⁴ Firth, one of Malinowski's protégés, had published *We, the Tikopia* in 1936. He was shortly to be made Professor of Anthropology at LSE (London School of Economics). Whilst Firth wasn't appointed to the umbrella Colonial Research Council, his work as secretary to the CSSRC did much to secure research funding for the discipline within British universities over the subsequent two decades.

Despite the fanfare with which the Colonial Development and Welfare Act was launched, very little happened at first, given the seriousness of the war. The matter was raised again by the new Secretary of State Lord Cranborne in June 1942, as a result of a number of research funding applications to the Government from the social sciences. Without quick decisions, he foresaw a 'danger that the academic bodies concerned might feel obliged to proceed with their individual plans and that this might lead to uneconomical and inconvenient dispersion of activity in these fields of colonial studies'.

As a result the Colonial Research Committee was finally convened in 1943. A committee of seven began their work immediately, holding one meeting a month. Alexander Carr-Saunders, a demographer and the new director of the LSE, represented social sciences. Audrey Richards, a close friend of Lord Hailey, was brought onto the committee a year later, and had a good deal of influence over its subsequent development.

The Secretary of State Lord Cranborne addressed their first meeting. He suggested that the terms of the act referred to 'research and enquiry', in the widest possible sense, covering both pure and applied research, such that the committee's function in coordinating research was as important as recommending grants for expenditure. Despite his administrative background, Hailey revealed his increasing support for

academic research, stating his determination that the committee's goal should be the pursuit of scientific 'truth'. As the first annual report notes, 'the committee should not confine itself to examining proposals put to it by Colonial Government ... it conceives it as its duty to study the whole field of scientific inquiry; to distinguish the parts of it requiring attention, and to ensure that gaps in it are filled wherever possible to do so'.⁵ It goes on to express concern about the 'tendency for research problems to be dictated too exclusively by local and temporary interests, without due regard to scientific possibilities'. The comprehensiveness and scope of the committee's self-appointed remit inevitably echoed the ambitions and interdisciplinary self-confidence of Hailey's *African Survey* (1938) and Worthington's *Science in Africa* (1938).

Small groups of experts were commissioned during 1943 to review the present state of all the various scientific disciplines. 'The inadequacy of the data' from the social sciences and the 'difficulties dealing with this subject', were seen as problems, partly because 'there is no organisation acting for the social sciences'. The 'Colonial Social Research Group' report emphasised the inadequacies and lacunae in extant knowledge.⁶ One of its first recommendations was that 'the need for social research in the colonies is evident ... very few social surveys of general standards of living have been done, and of these hardly any have been in charge of trained investigators. In some colonies no general ethnographic surveys have been made, and there are no descriptive accounts of the chief ethnic groups.' Again, one senses the magnitude of the task foreseen, and yet the need for such detail for rational welfare and development planning. This group of 'experts' went on to recommend the founding of a Social Research Council. Recognising that 'the field of social research is so wide that it cannot easily be covered by any one expert', the group saw this new council as playing a co-ordinating role in the expansion of the social sciences. Again, the limiting factor was seen as the shortage of people with 'specialised training'. The presumption that 'knowledge of a particular language or residence in some particular area is sufficient' qualification for research was gently dismissed. Instead, anthropological expertise and skills in conducting large-scale social surveys were particularly prioritised.

The report of the Social Research Group begins ecumenically, admitting that 'we have found it impossible to fix any exact boundaries to the field of social research. Because "primitive" and "advanced" communities live "side by side" in the colonial territories research in the social field must be wider in scope than is usual in this country and investigators of more varied types may be required for it.' The report goes on to attempt to define anthropology as a broadly inclusive project. Later in the report there is, however, a steady slippage from

Hailey's call for a multidisciplinary approach to the narrower concerns of academic anthropology.

The recommendations went on to focus on employment and training issues, emphasising the shortage of trained staff to carry out the necessary research. Isolation, poor conditions, and short-term funding contracts were all seen as pressing problems. Rather than creating special departments of colonial studies or colonial anthropology, the report recommended the expansion of existing UK university departments in UK universities, as these topics 'can only be approached as part of the general study of the field'. Establishing departments of social science at Makerere or Achimota was also strongly supported, and seen as an 'integral part of future programmes in social research'.

Noting that 'in some cases trained anthropologists have been forced to take up other professions', the authors comment that 'it is therefore not surprising that there are now few anthropological field workers with the qualifications necessary to undertake the conduct of one of the large scale ethnographic or general social and economic surveys to which we give priority value'. It ends unequivocally with the importance of making provision for the expansion of anthropology and sociology departments 'if the increased demands for training envisaged' were to be met. At this point only Oxford, Cambridge and London had funded anthropology lectureships.

This document closely determined the evolution of the new Colonial Social Science Research Council (CSSRC) and its history over the next eighteen years. Formally appointed by government legislation in 1944, the CSSRC was the first government body to represent, organise and fund social sciences, and played a key role in financing and institutionalising these embryonic disciplines. At the very moment that social anthropology was converting from an informal 'band of brothers' into an expanding academic discipline, the CSSRC provided it with financial support and prestige.

The CSSRC at work: 'the right man in the right project at the right time'

With the change in wartime fortunes, partly because of the huge contributions in men and resources made by the colonies to the war effort, and the involvement of the USA, the Colonial Office became increasingly aware of its responsibilities to justify trusteeship and to plan for post-war development. The CSSRC wasted no time in developing its ambitions. Its first meeting was an informal one, called in June 1944 at the Colonial Office to discuss Audrey Richards's

impending fact-finding visit to East Africa. The particular issue at question was a proposal from the principal of Makerere College to develop a humanities degree with a social science component. Richards was in no doubt that, on the contrary, developing a research agenda should be the council's first priority. Citing the lack of literature appropriate for teaching, everyone agreed that 'emphasis should rest on research *ab initio*, as this would form the basis of the teaching of social subjects later'. Monica Wilson, widow of Godfrey Wilson, the first head of the RLI (Rhodes Livingstone Institute), was mentioned as a possible person to lead such research. Already the importance of the RLI as a model for a training ground for future generations of anthropologists had been recognised.

The first formal meeting of the council was held a month later, with Carr-Saunders of the LSE as chair, and Raymond Firth (now also an LSE professor) as the newly appointed secretary. As he had done the previous month, Carr-Saunders again laid out the extensive work facing the council. This time he also pointed to the lack of an umbrella organisation to represent the social sciences, leading him to call for the 'closest liaison' between the disciplines. Procedure was discussed, and it was agreed to rely not upon formal subgroups of 'experts', but rather on each member of the council representing one of the eight disciplines that had originally submitted reports. Firth later produced a discussion document on the council's general policy, suggesting that 'linguistic and socio-economic studies' should be 'major aims for systematic research in the first instance, covering successive territories'. Firth suggested that this would provide 'basic data for colonial governments and for research in other disciplines', and also rapidly secure 'a body of personnel with some knowledge of colonial conditions and local research techniques'. These surveys 'would offer opportunity for collaboration among several disciplines (sociology, anthropology, psychology, economics, statistics)'. Where would all this start? Given the importance of regional concentration, wrote Firth, 'Africa would appear to be an obvious first choice'.⁷

At this first meeting there was extensive discussion of both the research priorities and the training and employment of colonial researchers. Indicative of the discipline's dominance, the minutes noted that 'the council was of the opinion that the programme of ANTHROPOLOGICAL [*sic*] work was most important, as it formed a basis for so many of the other sciences with which it was necessary that it should be closely associated in field work'.⁸

There is little doubt that the members of the CSSRC saw themselves as intellectual pioneers, leading the way both in mapping out uncharted territories of African social research problems, and in trailblazing the new possibilities for a problem-oriented multidisciplinary social science.

The council met monthly in the Colonial Office, and gradually research priorities began to be established. The first annual report exhaustively lists the series of 'major' and 'urgent' research needs, including 'surveys of social and economic conditions in urban and rural areas', 'comparative studies of local government', the 'effects of migratory labour', 'political development in "plural" communities', not to mention studies of land tenure and colonial administrative law. The council accepted that the 'emphasis on the practical applications of research was appropriate'.⁹

As the council began to define its work, Firth circulated a memo to colonial governments. He listed the council's primary functions as:

the review of the organisation of research in the social sciences in the colonies (including the selection, training, and terms of service of research workers) ii) the scrutiny of particular schemes of research submitted for approval iii) the initiation of proposals for research in fields not otherwise covered and iv) advice as to the publication of results of research.¹⁰

Of these, the heaviest burden on council members was the administration and selection of research grants. The meetings constantly returned to the issue of the shortage of candidates suitable and willing to work in the colonies. For senior scholars, one problem was the difficulty of obtaining leave from universities. For more junior researchers, the perceived need was for specialist research training – at this time there were no government funds for anthropology research students.

In the very first meeting of the CSSRC, Firth proposed that he do an informal canvassing for possible candidates for research funding. This led him to come up with a list of thirty (unnamed) possibles, of which six 'had expressed willingness to undertake research work under the auspices of the colonial office'. None were considered of high enough calibre to be awarded one of the colonial research fellowships, and he suggested that 'they would work best in teams, or under the direction of a senior research worker'. Firth envisaged an 'emergency' training programme for this new cadre of young graduates, with metropolitan universities (he named Cambridge, Oxford and London) providing the main source of training, and the regional institutes providing social science research workers with 'special knowledge of local conditions'. Firth also advocated that the training should encourage 'the expansion of knowledge gained in one discipline into a broader social field ... the object here should be to weld on to the specialised equipment of the particular research worker a general knowledge of the principles of social and economic structure which will condition so much of the material he will be required to investigate'. Despite the

rhetoric of interdisciplinarity, the emphasis on 'social and economic structure' was unmistakably anthropological.

Under the terms of the Colonial Development and Welfare Act £500,000 annually was earmarked for research. This aspect of the act that had the most visible impact. Even though only a small proportion of this money went to the social sciences, it was enough to fund a major multidisciplinary research agenda, a large number of research fellowships, and four high-profile regional research institutes in Africa and the Caribbean. Grants were given to individuals for a whole variety of research topics, varying within anthropology from studies of African land tenure systems to linguistics, local economies and 'traditional' law. Support was also initially given to the International Institute of African Languages and Cultures to continue its extensive research surveys, including the *Handbook of African Languages* and the *Ethnographic Survey of Africa*. Funding for the regional institutes was less tied to particular research projects.

Two major concerns dominated council proceedings – the selection, training, supervision and support of researchers and the development of the regional institutes. Both led to conflict. Initially, there was disagreement within the council about the best use of its money. A question mark hung over formal research training schemes, and whether these fitted the council's remit. In 1944, Carr-Saunders expressed the non-anthropologists' concern about the youth and inexperience of the candidates, such that it 'would be quite impossible to allow most of them to go out to the colonies and work independently, even after a period of training'.¹¹ Raymond Firth responded by insisting that the programme would be strictly short-term, without which it would be 'impossible to meet the demands which would be made' for sociological researchers. A compromise was agreed: the scheme would be set up, but each application would be submitted to the council for consideration of details of pay, leave, length of training, etc.

In 1947 the council awarded the first twelve postgraduate studentships. These provided six to twelve months' research training, following which students 'were required to undertake a specific priority research project of about two year's duration, in one or other of the Colonial territories'. The training was deliberately not tied to a specific project in advance, for it was recognised that candidates 'may yet have no clear ideas of the exact field of research for which their aptitudes fit them, or of the locality or problem to which they would be most attracted'.¹² Amongst the first students were M.G. Smith, Edwin Ardener, John Middleton and Frank Girling, all of whom went on to make important contributions to social anthropology. The following year the scheme was extended, somewhat controversially, to allow

Fulbright scholars and other American students to receive training at British universities in preparation for 'field work in the colonies'. In total twenty-one students received these studentships, and, by 1951, the CSSRC annual report showed that the council was much more confident about the recruitment situation, noting that the 'field for junior appointments to regional Research Institutes appears to be fairly large'. The council awarded few further research fellowships, and by 1955 had announced that the funds available for individual research were 'severely limited', with priority increasingly accorded to the research institutes. Chilver suggested that 'it was felt that the discipline had been given a shove' and should now be developing its own resources.¹³

Throughout the 1940s, concern over recruitment and training continued to dominate discussion. As the 1947 CSSRC annual report noted in the gendered language of the time, 'the greatest difficulty may be summarised as that of bringing the right man to the right project in the right place at the right time ... [but] the chief difficulty is still that of finding the right man'. The same concerns were voiced by the Colonial Research Council itself, which voiced its concern over the 'danger of taking second-rate men because of the shortage of first-rate men [*sic*],' noting that 'it would be preferable to let the schemes grow as and when first class workers become available'.¹⁴

'Centres of research and learning in the colonies themselves'

The wartime deliberations of the Colonial Research Committee had dwelt on the 'special difficulties' of carrying out research in the colonies. This was blamed upon both the lack of UK departments 'responsible for the conduct of detailed investigations', and the non-existence of departments of social studies within colonial institutes of higher education. Audrey Richards had an innovative solution – building up an independent academic research capacity within the colonial territories. The issue of 'isolation and restricted opportunities for colonial research workers' would also be 'greatly mitigated if centres of research and learning could be developed in the colonies themselves'.¹⁵ This would also solve problems of independence and continuity. Richards was one of the keenest exponents of the development of regional research institutes in the colonies, and she joined the committee at this time. The biological and agricultural sciences already had established field-research stations, but for the social sciences the obvious precedent was the Rhodes Livingstone Institute (RLI) in Northern Rhodesia, an independent foundation

created five years earlier in 1938 (Schumaker 2001). It had been set up at the instigation of the colony's Governor, with funding from local mining companies, and a remit to apply anthropology to problems of social change. Makerere (in Kampala, Uganda) and Achimota (in Ghana), which at that point were government-funded technical colleges, were equally cited as places where research facilities could be built up, to go from being centres of 'vocational training' to 'real centres of learning in the Colonies'.

This development became the second key role of the CSSRC. It was a highly ambitious undertaking. Over the fifteen years the institutes eventually consumed more than a million pounds of the council's budget, twice as much as that directed at individual research projects. Even at the very first meeting of the council in 1944, Max Gluckman's plans to expand the RLI in Northern Rhodesia were discussed (Gluckman 1944, 1945). It differed from the other institutes, in having been previously set up (in 1938) as an independent social science research institute with no university attachment. Its importance lay in the precedent that it set for the council's own plans for further regional institutes in West and East Africa and the Caribbean. Yet the process of setting up regional centres for social studies was far from straightforward. To whom would they be accountable? Should they be part of the university colleges, or wholly separate? Should Colonial Development and Welfare funding go into buildings and infrastructure? Could 'high-quality' staff be recruited? I focus particularly on Makerere: plans for the East African Institute for Social Research (EAISR) were the first to develop, and were the most ambitious of the regional research institutes (Chilver 1951).

Establishing such an institute at Makerere College meant intervening in the fraught politics of regional colonial policy: this was a university college for the whole of East Africa but located in Uganda. Many Ugandans were against any attempt at creating an East African federation. There was also institutional politics to attend to. The 1944 Asquith Commission on Higher Education had unequivocally recommended the development of such institutes, but had strongly advised against the creation of a 'semi-independent' social science unit at Makerere, which might diminish 'the authority and prestige of the university'.¹⁶ Audrey Richards, the key council advocate for the institutes, was rather differently minded. After her visit to Uganda in 1944 she made a strong case for 'a separate institute, that the staff should be free from routine teaching duties, and that the Director should have power to frame research programmes'. Such dissension from official policy had to be justified, and the council developed the case that the effective teaching of social studies depended first on the accumulation of 'a body of knowledge on sociological and kindred

matters in East Africa'.¹⁷ Prioritising research was a way to avoid the danger that staff would have to 'devote an undue proportion of their time to routine teaching duties', whilst still allowing the eventual aim of merging the institute into the University.

One solution was to create dedicated research fellowships. Following the example set by Rockefeller and the IALC (International Institute for African Languages and Cultures), the idea of establishing research fellowships 'nominally stationed at Makerere or Achimota'¹⁸ was well received, and Hailey proposed that the Treasury be asked to fund five such fellowships a year for outstanding scholars. Max Marwick and Aidan Southall were the first social anthropologists to receive such fellowships, when the East African Institute was finally established. A lack of 'high-quality' candidates, partly because no funding was available for research students, led to a later focus on postgraduate training, and a studentship scheme was established, to the benefit of a generation of anthropologists, including M.G. Smith, Vernon Sheddick, Philip Gulliver and Jeremy Boissevan.

Richards's plans envisaged a well-resourced research institute, equipped with staff, dwellings and offices. Next, a potential director needed to be found. Raymond Firth recommended William Stanner, an Australian anthropologist who had been a member of Malinowski's seminar. Strong reservations about Stanner were expressed by the Governor of Uganda. These included the concern that he had no economic training, and that in 'view of the recent conversion to Christianity of Africans in Uganda it was also advisable that he should not be a militant rationalist'. He was nevertheless selected, and eventually paid a visit to Makerere in 1948 to report on the progress made with the institute. He made much of the political complexities of the situation, questioning even the wisdom of its implementation. This was exactly what the committee did not want to hear, and there was much discussion of other possible locations, and ways of surmounting the difficulties that Stanner seemed to be presenting. Max Gluckman was secretly pleased, and wrote in 1948 from Oxford in 'semi-confidence' to Clyde Mitchell to say that 'Stanner has recommended, we hear, that the Makerere Institute be dropped which leaves us unique'.¹⁹ Stanner eventually resigned in 1949 and was replaced by Audrey Richards, the initial architect of the East African Institute for Social Research (EAISR). Richards resigned from the council in order to successfully establish and run the institute for six years (Mills 2006).

She was replaced as director in 1957 by the anthropologist Aidan Southall, who had first lectured in social studies at Makerere (1945–48) and then been a research fellow at EAISR (1949–54) after doing a master's degree at LSE. Fluent in Lwoo, he was unusual in deciding to stay on in Africa rather than returning to the UK to pursue

his academic career. In 1957 he was simultaneously appointed to the first Professorship of Sociology and Social Anthropology at Makerere, part of the university's attempt to involve the centre more closely in the life of the university. When Uganda became independent in 1962, it continued as an influential centre of applied social research under the directorship of Derek Stenning until his untimely death in 1964.

Not everyone shared Richards's enthusiasm (e.g. Richards 1977b) for the research centres. As early as 1946, she expressed her concern to Raymond Firth about a seeming lack of commitment from anthropologists to the regional research infrastructures. Max Gluckman, she writes, 'wants to get all the research workers over to England with him – to give them what he calls training in Oxford, preferably for a year.' She goes on to suggest that his rationalisation for avoiding 'the trouble of going back to the field' is his view that 'local centres are bad, that short periods of fieldwork are better than long, that there is too much field work being done and not enough theory, and that anyone who works on a government project is betraying their science!'²⁰ She concludes:

I don't think the Council ought to agree to letting all the research fellows come back for a year to Oxford because Gluckman doesn't want to be in Rhodesia. It seems to me to be a bad precedent, and at present I even think that the Oxford atmosphere of 'down with applied anthropology' and their emphasis on the fact that the climate of Africa is dangerous would not be very good.

She ends by pointing out the significance of this move: 'I think it is important because it will mean, I fear, the abandonment of our whole local centre policy if we give in on this.'

These differences were openly aired in the next meeting of the council, attended by Max Gluckman. In response to the council's view of the importance of the regional institutes, he argued that this depended on the facilities available, and that at the RLI 'there was nothing which could be described as a University atmosphere, and in his opinion the function of a regional institute was to act as an advance base from which to conduct field work rather than as a centre of academic training or excellence'. The chair pointed out that the whole purpose of the scheme was to create 'regional universities comparable in standing to Universities at home', whilst Firth accused him of hoping to draw up a plan of local research by 'remote control'. Whilst arguing for increased funds for the RLI, Gluckman again insisted on the 'intellectual stimulus of the home universities' for writing up.²¹

The argument moved on to the relationship of research institutes to 'government planning in colonial territories and the problem of

applied research to which this gives rise'. Whilst everyone agreed that research institutes should concentrate upon basic research, and 'should not obtrude their advice unasked upon territorial governments', Gluckman disagreed with the council's view 'that if their assistance was sought in connection with particular investigations ... they could contribute much and should be ready to do so'. The issue was left unresolved. Despite the tensions, and the coded reservations expressed by Firth and Richards about Gluckman 'overstretching himself' in the rapid expansion of the RLI, it was agreed to support the RLI plans. No doubt the argument was partly motivated by the growing rivalry between Oxford and LSE, but also by fundamentally different attitudes to the role and future possibilities for anthropology in the colonies. In adopting a more avowedly anti-colonial stance, Gluckman saw little possibility for a higher education institute with the capacity for independent research amidst the tensions of the federation (Colson 1977).

The CSSRC and anthropology

The history of relations between the Colonial Social Science Research Council and individual anthropologists is fascinating. There was a direct tension between a metropolitan academic agenda and the council's concern with colonial 'social problems'. Given that many key disciplinary figures were on one of the CSSRC committees or involved with the regional institutes, the disputes also reveal political disagreements between individuals over the appropriate stance for anthropology to take in relation to colonial affairs.

Within the discipline, there was a growing perception that the CSSRC, given the prominent roles of Firth and Richards, was an LSE affair. A rift developed between LSE and Oxford, partly fostered by Gluckman's scepticism about a research programme too closely directed by the Colonial Office. Gluckman had already been frank in his expression of views in another public forum – the Association of Social Anthropologists. In a written response to a memo from Evans-Pritchard proposing this new association in the spring of 1946, he highlighted the 'grave danger that the demands of colonial governments for research workers may lead to an excessive concentration on practical problems, to the detriment of basic research, and to the lowering of professional standards and status which would lose the gains of the last 20 years'. He already knew of both Evans-Pritchard's long antipathy towards Audrey Richards and his suspicion of applied anthropology, both legacies, Goody suggests, of Evans-Pritchard's earlier animosity towards Malinowski (Goody 1995).

By the late 1940s relationships between Oxford and LSE as the leading anthropology departments of the day became steadily more strained. As well as differing theoretical positions and views on the importance of 'applying' social research, the perceived LSE bias of the CSSRC made many unhappy. The split was also intellectually motivated, by what Richards later remembered as the 'sudden break that we felt after the war when EP and his students were advancing the suggestion that social structures – in his sense – were the only things to be studied' and 'the sense we had of a battle being joined'.²²

The split was a vituperative one, with Max Gluckman in Oxford writing a stormy letter to Audrey Richards attacking the LSE 'mafia' in late 1946. While he subsequently retracted his outburst, her response is fascinating:

Only Max I do hope this is not going to be a personal quarrel. I disagree with some of the Oxford developments but Firth and I have strenuously stood against the idea that there are two camps. We won't let the students group themselves like this and we lecture on and discuss both Fortes and EP's material. We certainly kept up our personal relations at the ASA in July. So what is all this talk about 'smashing your influence'? For God's sake don't let us become like two sets of psycho-analysts who turn their scientific hypotheses into religious faiths that you must accept or perish and won't associate with unbelievers. We shall certainly hope that you and some of your ex-team will come up to seminars in London, and that any of our East African PhDs will come to Oxford and see you ... Perhaps I write with some tone of injury too. I think it is that both EP and Fortes have decided that Council is dominated entirely by the LSE and that they are shut out from it. They admit that they aren't prepared to go on the council or do the extremely heavy work that we have done these last years. (I reckon about 1/3 of my time goes on it in term time). But I don't think they give credit to the fact that if I hadn't fought in the Colonial Office for so long there wouldn't have been any money for anthropology at all. Hence when you make entirely baseless charges I suppose the WORM begins to turn though disclaiming that it has been prodded at all! Well anyhow, for heavens sake don't let us fight!²³

The disagreement spilled out in a heated debate in an Association of Social Anthropology (ASA) business meeting in July 1948. Discussed under 'Other Business' a stoutly worded resolution was passed that 'the present organisation for the expenditures of funds from the Colonial Development and Welfare fund on anthropological research is not in the best interests of anthropology and its application to colonial problems'. The minute continued that 'the interests of anthropology should be represented by persons nominated by the Association', and instructing the secretary to write to the Secretary of State to ask him to receive the President of the Association, 'who would put to him the

reasons why the Association had come to these conclusions'.²⁴ The motion was Oxford-led. When it was put to a vote the LSE contingent – Professor Firth, Dr Read, Dr Kaberry and Dr Mair – all abstained.

The association decided to seek a meeting with the Secretary of State Creech-Jones. It is indicative of the influence of the discipline's leading figures that their concerns were treated with great seriousness by the Colonial Office. A lengthy and uncompromising 'aide-memoire' was sent to the Secretary of State regarding the funding of anthropological research. The ASA document made a series of demands, calling first for 'a consultative panel consisting of the professors of anthropology of Oxford, Cambridge, the LSE and UCL [University College London] of London University, which should be consulted with regard to all projects for anthropological research'. It went on to ask that any 'research worker financed by the committee on social research should be attached to a university department of anthropology during the period of his training and research'. These demands served to consolidate not only the discipline but also its oldest departments. The final point was the most controversial, claiming an anthropological monopoly over methodological competence in colonial social research:²⁵

It is said that the majority of the research projects accepted by the committee on social research have in fact been such as can only be effectively carried out by anthropologists. It is desirable that in appointing any person to carry out such projects two things must be taken into consideration:

- a) that the person appointed should, before taking up research, have received a thorough training in general social anthropology
- b) that before being required to devote attention to some particular problem in which knowledge is required for administration purposes, he should be given sufficient time (in most instances a year) to make a general sociological study of the people or area with which he is concerned

In the meeting Radcliffe-Brown, as Honorary President, explained that the ASA's main concern was to have more professional control over funding decisions. The Colonial Office response was surprisingly amenable to several of the ideas, including that of a consultative 'subject' panel. It agreed that its members should be those of the council with a qualification in anthropology, plus those other professors at Oxford, Cambridge, LSE and UCL not presently on the CSSRC. As a result a Social Anthropology and Sociology subcommittee was subsequently set up, with Evans-Pritchard, Hutton and Fortes as additional members. This served as a model for other subject panels, replacing the previous regional committees of the council. Yet

anthropology's attempt to monopolise colonial research did not go unchallenged. The Colonial Office memo ended strongly, saying that it 'cannot accept the suggestion that the majority of the research projects accepted and recommended by the council have in fact been such as can only be effectively carried out by anthropologists', and that the 'final recommendations' would rest with the council, and not the consultative panel!

These new subject panels signalled the increasing power of British anthropology departments and metropolitan disciplinary agendas over the 'colonial social problem' focus of the council. The huge increase in post-war funding for British higher education assured anthropology the institutional security and confidence of an academic identity, even if Colonial Office patronage and the training offered by the regional institutes such as the RLI remained important for the reproduction of the discipline.

By the end of the 1940s, a huge volume of social research had been commissioned by the CSSRC, much of which was published by the government in its extensive Colonial Research Series. Ethnographic work in Africa included research on land tenure in Nigeria by Charles Meek (1957), in Zanzibar by John Middleton (1961) and in Basutoland by Vernon Sheddick (1954); work on native administration in Northern Rhodesia by Bill Epstein (1953) and Nyasaland by Lucy Mair (1952); and on the social organisation of the Nandi by George Huntingford (1950), the Hausa by M.G. Smith (1955), the Acholi by Frank Girling (1960) and the Tiv by Paul Bohannon (1954). Many of these studies were theoretically oriented, with little direct policy relevance. Inevitably many of these studies were holistic accounts of single ethnic groups. Where survey work was conducted, it was usually at the household or local level, for few of the anthropologists were equipped to carry out the territory-wide quantitative surveys that might have been more necessary for national planning purposes. Information coverage was selective and partial, and hardly served the comprehensive and strategic welfare and developmental blueprint originally envisaged by some in the Colonial Office.

In the 1950s the work of the council gradually changed, particularly as colonial governments were increasingly encouraged to organise their own research into local 'problems'. CSSRC funding concentrated on the institutes' own research programmes, and few individual grants were awarded. Yet conflicting interpretations of the council's remit continued. The new Anthropology and Sociology subcommittee began to meet in 1949, mostly to discuss applications for research fellowships. The initial chair was the historian Godfrey Thomson, but on his resignation in 1950 Evans-Pritchard became chairman. Audrey Richards, who was by this time running the East

African Institute for Social Research (EAISR) at Makerere, confided her fears about this with Sally Chilver, the CSSRC secretary of the time, writing, 'I am depressed because he is dead set against local institutes and has made no secret of that. He will vote and finally win his way of getting large grants to English universities, no questions asked and no results expected and those of us who have tried to play the Colonial Office fair will feel HAD.'²⁶

Quite apart from the institutional rivalry and Audrey Richards's sense that 'the ordinary rules of fair play don't work with him', her main concern was with Evans-Pritchard's dislike of the whole principle of the devolution of research agendas to the regional institutes like the RLI and the EAISR. Evans-Pritchard's view, echoed by Gluckman, that researchers should return to their 'home' universities in the UK for a six month break during fieldwork, negated one of the rationales for the institutes, as envisioned by Richards. By then, the RLI's work was increasingly academic in nature, partly reflecting Gluckman's new self-identity as an anthropological theorist (Gluckman 1948, Colson 1977).

The argument revealed not only methodological disagreements, but fundamental political conflicts over disciplinary priorities and what 'professionalisation' entailed. The very consolidation of the discipline in UK universities depended on a new generation of scholars receiving CSSRC funding for training and fieldwork, but then taking up UK academic posts.

Subsequent letters between Richards and Chilver highlighted continuing tensions in the council over the huge cost of funding the regional research institutes, especially as the West African Institute became caught up in regional politics and infighting. In a report from the Anthropology and Sociology subcommittee he chaired, Evans-Pritchard strongly recommended that any savings available should be devoted to 'independent schemes of research', in areas outside those covered by the institutes' activities or the 'special interests' they had developed.²⁷ Even the chair, the LSE director Carr-Saunders, began to turn against the idea of such institutes, and the view developed that these should be more closely integrated within the new universities. Richards, however, felt that EAISR could train students far more cheaply than otherwise, and that 'all the preliminary negotiations and muddles with governments which most other academic research workers have are avoided because we now have good relations with all three Governments'. In her letters she reflected acutely and wittily on council micro-politics and the likely sources of opposition, writing to Sally Chilver to proclaim: 'I have already told Perham that it is cheaper to finance an Institute than a Scarborough student [a new programme of Government-funded studentships], and I hope she is smoking that

in her pipe.' Richards had influential connections within the Colonial office, and she continued to win many of her battles. Despite the rivalry between them, both Margery Perham and Audrey Richards remained major public influences and scholarly advocates for colonial reform (see Perham 1962; Smith and Bull 1992; Kuper 1996b).

Conclusion

What were the long-term consequences of this colonial patronage for social anthropology and the other social sciences? De L'Estoile argues that the impact of what he calls a new 'field of competence' was transformative: 'This new academic discipline emerged as the result of a process of construction of a specific field of competence in the knowledge of those social phenomena that characterise "backward" societies, and the monopolisation of that competence by a group of professional scholars, at the expense of those whose claim to competence had formerly been recognised: the "practical men".' (de L'Estoile 1997, 373). Richards is equally emphatic, arguing that 'the suddenness with which considerable funds became available had ... dramatic effects which would not have been achieved by a series of small grants' (Richards 1977a, 186), evidenced by the huge volume of research produced (Jeffries 1964).

Anthropology's status within universities was immeasurably bolstered by the volume of research funding received from the CSSRC. By 1953 there were thirty-eight teaching positions in social anthropology in the UK, an impressive rate of expansion. Some of this occurred through the UGC funding for new departments at SOAS, Manchester and Edinburgh, but much also relied on Colonial Office funding of doctoral and postdoctoral research. More than fifty anthropologists benefited in some way from Colonial Office-funded research grants, studentships or its funding for the regional institutes (Richards 1955). As the Ardeners note, 'the "professionalisation" of the discipline for which the pre-war generation worked was overwhelmingly realised in the post-war "bulge"' (Ardenner and Ardenner 1965, 303). Important as university posts were, the reproduction and expansion of the discipline depended primarily on finding research funding for students.

In 1977 the LSE held a series of seminars revisiting the colonial experiences of British anthropologists. Richards, Firth and Chilver all presented papers (Chilver 1977; Firth 1977; Richards 1977a). They found themselves swimming against a dominant post-Vietnam perspective that, through a strong and overly simplistic reading of influential texts such as *Anthropology and Colonialism* (Asad 1973,

including Feuchtwang's (1973) valuable account of the CSSRC; see also Owusu 1975), viewed this earlier generation as having been 'handmaidens' of colonial states. Such a gendered caricature was, and remains, somewhat unjust. Whilst this was an important moment of critique and self-reflection, it collapsed very different historical moments and geographical concerns. It also did not capture the diverse involvement and contradictory political agendas described in this chapter – amongst anthropologists, those in the Colonial Office and within the colonies themselves.

The critics tended to underplay the major historical changes occurring at the end of the colonial period. As Richards notes, 'before the Second World War ... the colonial office ... gave no financial support to anthropological or any other kind of social science research and might almost be said to be famous for not doing so' (Richards 1977, 169). The focus of critiques, as Pels and Salemink note (1999), is on 'the colonial complicity of *academic* anthropologists' at a time when the 'academy' was not the only anthropological habitat. Even after the war, anthropologists like Gluckman and Evans-Pritchard were ambiguous and tactical in their relationships with colonial authorities. Some, like Richards and Firth, negotiated a multiplicity of roles.

At a general level Stocking is right in accepting that 'colonialism was a critically important context for the development of anthropology' (Stocking 1996, 368). Yet this is the start, rather than the end, of any explanation. As he notes, 'important groups within the world of colonial administration had shown themselves willing to accept the scientific status and the utilitarian promissory note of social anthropology' (ibid., 420). When it suited them, the various protagonists played down this utility, in favour of fundamental research. Anthropologists were not alone in this regard, and the CSSRC also played up this rhetorical opposition when it suited them. At one moment Richards describes the CSSRC as 'do-gooders trying to organise research which would increase the knowledge we felt to be helpful for "welfare and development"'. At the next she downplays the administrative appropriation of anthropological ideas, noting how young anthropologists involved in detailed studies 'were learning their jobs ... and had not the competence to pronounce on the problems of the colony as a whole' (Richards 1977a, 178).

Anthropology's pre-eminence within the CSSRC thus relied partly on its ability to reformulate 'social problems' as scientific ones. Its intellectual prestige depended primarily on the symbolic and financial support from the council, and its willingness to accept the discipline's research agendas and train its fieldworkers. Kuper's view that the 'winding up of the CSSRC did not have much impact' on anthropology

(Kuper 1996a, 117) does not fully capture the importance of this symbiosis. The end of empire was also the end of a complex set of political relations linking scholarly practice with the production of knowledge. It created a lacuna that was filled by a new cadre of 'development' experts, relatively few of whom had a background in anthropology. By working so successfully within the ambit of the CSSRC, anthropologists had developed a monopoly over a field of practice that suddenly disappeared. The discipline was never to be so confident again.

Notes

1. Perham papers, Rhodes House, File 685/2.
2. Brit Emp MSS 342, Rhodes House, MacDonald to Hailey, 18.3.40.
3. Brit Emp MSS 342, MacDonald to Hailey 18.4.40.
4. Brit Emp MSS 342, Hailey to MacDonald, 3.5.40.
5. First Annual Report of the Colonial Research Committee, 1943–44, Cmnd. 6535.
6. Perham papers, file 685/9, Rhodes House.
7. Firth papers, LSE, file 2/2.
8. CSSRC 1944 files, LSE.
9. First annual report of the CSSRC, p27.
10. CSSRC (44) 24, LSE.
11. CSSRC minutes of the 8th meeting, 1944, LSE.
12. CSSRC fourth annual report, LSE.
13. Interview, 6.3.00.
14. Colonial Research Council, first meeting of council, 1948, minute 2, LSE.
15. Colonial Research Council Progress Report 1942–43, LSE.
16. Asquith Commission, Cmnd. 6647, p17.
17. CSSRC, minutes of the 11th meeting, July 1945.
18. PRO 927/2/1 – CRC paper 1943 (57).
19. Mitchell papers, Rhodes House MSS 5/1 27 from Gluckman 1.7.48.
20. Firth papers, 2/3 Letter from Audrey Richards to Raymond Firth, 17.8.46.
21. CSSRC minutes of the 20th meeting re CSSRC (46) 27, LSE.
22. Firth papers, letter from AIR to RF, 8.3.76.
23. Richards papers, 16/19, LSE, letter from AIR to Max Gluckman, 4.11.48.
24. ASA papers, LSE A1/1.
25. CSSRC papers CSSRC (48) 62.
26. Richards papers, 16/7, LSE, letter from AIR to Sally Chilver, 14.11.50
27. CSSRC minutes of the 62nd meeting, autumn 1952.

TRIBES AND TERRITORIES

Introduction

The post-war years were expansive ones for British universities and the new social sciences. There was an unprecedented expansion of the proportion of the costs of higher education borne by the British Treasury, doubling from 31 per cent in 1938/39 to 65 per cent in 1951/52. This chapter describes the growth of social anthropology in the 1950s, during what Annan calls the 'golden age of the don' (1990, 337). Yet not every anthropologist welcomed this expansion. The creation of new anthropology departments was sometimes resisted by existing centres, leading to growing rivalry and rifts amongst Firth's 'band of brothers'. I describe the establishment of departments at the University of Manchester and the School of Oriental and African Studies in London.

The discipline faced two major challenges. One was to find new sources of research funding, partly achieved by astute lobbying by social anthropologists of two government commissions (the Clapham and Scarborough commissions) looking into the social sciences during this period. The other was how best to respond to the growing popularity and expansion of sociology, especially during the 1960s. I show how, with the appointment of Peter Worsley as a Professor of Sociology at Manchester in 1964, Gluckman fulfilled his vision for a joint anthropology and sociology department. Its subsequent acrimonious divorce in 1971 typified the growing rivalry between two deeply intertwined fields. Sociology's rapid expansion and growing institutional dominance, especially in the new universities, crystallised

the diverging methodological, political and epistemological 'slots' (see Trouillot 1991) apportioned the two disciplines. Once established, this divergence was difficult to reverse, despite the continued flow of ideas and individuals across the divide.

The post-war expansion of British Higher Education

In 1945, Britain's anthropologists returned to their metropolitan universities amidst a welter of plans for the future. Post-war reconstruction was spurred on by state funding, both in the empire and in Britain. A domestic example of this was the influential 1947 Clapham commission into the provision for social and economic research in the UK (Clapham 1947). Made up primarily of London School of Economics (LSE) professors, including the demographer and LSE Director Alex Carr-Saunders, the economist Harold Robbins and R.H. Tawney, the commission marked the first of several attempts to define, coordinate and support the nascent social science disciplines now emerging within British universities.

The first step, defining the social sciences, was by no means the easiest. Noting that its practitioners 'are by no means agreed on the precise boundaries of their subjects', they decided that it was not 'necessary to give exact definitions of the fields of research covered'. Revealing their LSE roots, they made much of 'the great practical value of knowledge in these various fields', but also bemoaned the fact that 'progress in social and economic research has been very seriously hampered by lack of adequate finances', and that 'the number of universities in which there exists continuous provision for research in social questions is still extremely small' (Clapham 1947,13).

Recognising the huge contributions of the Rockefeller and other private foundations to supporting the social sciences in the UK, the commission nonetheless insisted that, for the social sciences, 'picking up what they can by appeals to outside foundations, some of which draw their funds from abroad', was not a 'satisfactory state of affairs'. Recommending a 'permanent and routine' increase in resources for social science research, the committee proposed a sum 'of at least £250,000 or £300,000 per annum'. A specialist subcommittee was to advise on how this should be spent (on which Firth represented anthropology).¹ Such advice was translated by the University Grants Committee (UGC) into a series of 'earmarked' grants, eventually amounting to £400,000 per annum.

What were anthropologists hoping for in 1947? More teaching posts were seen as a high priority. At this point, there were fewer than a dozen permanent university posts. Only four British universities,

University College London (UCL), the London School of Economics (LSE), Edinburgh and Cambridge, offered first degrees in anthropology. Evans-Pritchard was in the Oxford chair, with three other lecturers. Hutton still held the Cambridge chair, with Raymond Firth in the chair at LSE and Daryll Forde at UCL, each with roughly one lectureship apiece. They were greatly outnumbered by staffing and departments in the 'dominions' awarding anthropology degrees, including Melbourne and Sydney in Australia, and Rhodes University, Witwaterstrand and Cape Town in South Africa.

Anthropology did well out of the Clapham recommendations, with funding earmarked for a professorship and senior lectureship at Manchester, two lectureships at Cambridge, and further lectureships at UCL, Durham and Leeds. Anthropology was thus to be taught for the first time at Manchester, Durham, Leeds and Edinburgh. Progress was only hindered, according to Firth, 'by the shortage of persons with the necessary qualifications for appointments in this field'.²

In 1951 the UGC suspended these 'earmarked' grants, as they felt they had served their purpose of 'pump-priming' the new social sciences. The newly formed British Sociological Association (BSA) decided to lobby the UGC. Such lobbying marked the increasing willingness and confidence of this new social science to articulate and defend its interests. The ASA telegraphed its support to the BSA's chair Professor Morris Ginsberg, stressing that 'provision was still urgently needed if the advances made in the past five years are to be consolidated'. An ASA meeting also urged Meyer Fortes to make a further submission detailing the special needs and demands of social anthropology, such as for longer-term research funding, and argued that 'departments should be large enough to permit at least one member to be away every year in the field', for, 'without such close contact with fieldwork, there is a danger of stagnation'. The letter raised the spectre of a new American hegemony over the discipline, pointing out that the overseas research institutes 'have been obliged to recruit American personnel and still have unfilled posts'.

Another Royal Commission also looked favourably on the discipline. The 1947 Scarborough commission on African and Oriental studies boosted research capacity by recommending that the Treasury fund postgraduate scholarships. Amongst those who were awarded Treasury scholarships between 1949 and 1952 were John Beattie, Kathleen Gough, Peter Lienhardt, Rodney Needham, Emrys Peters, David Pocock and Michael Swift. The commission also recommended the expansion of area studies, but the creation of a new anthropology department at the School of Oriental and African Studies (SOAS) was a contentious affair.

‘Keeping their people from having to go down to the School’

The rivalry between LSE and UCL anthropology, or more precisely between Bronislaw Malinowski and Grafton Elliot Smith, had existed since the 1920s. Within the university, an uneasy division of labour had developed between the two departments, with LSE jealously guarding its reputation as the centre of the new ‘school’ of social anthropology. As a result, the LSE was suspicious of Daryll Forde’s efforts to revitalise the UCL department after the war. His appointment to the college’s first Chair in Anthropology led some to fear a revival of Elliot Smith’s diffusionist approach. Followers of this UCL school were mockingly labelled ‘children of the sun’, their own name for the migrants who took ‘civilisation’ out of ancient Egypt. The theoretical approach was based on an archaeological reading of a single ‘archaic civilisation’ that then diffused throughout the world (Kuklick 1991). As Firth recalls of William Perry, one of Elliot Smith’s followers, ‘he tried to convert me over tea and buns to the notion of diffusionism, without success’, whilst all the time ‘protesting that it was General Anthropology’.

Whilst Forde had done a UCL Ph.D. in prehistoric archaeology, he had subsequently been exposed to social anthropology as a postdoctoral researcher at Berkeley. So Forde’s approach at UCL was to begin to ‘convert it into a type of social anthropology’, aware that this area was attracting most funding. ‘Like everybody else,’ remembers Firth, ‘he wanted to get his students into the field.’³ By 1950, Firth and Forde were working much more closely, and jointly chairing the University of London interdepartmental seminar, though this was the subject of some criticism. Writing to Daryll Forde about this, Firth admitted that there were criticisms ‘that you and I seem more bent on getting across our respective points of view than on lending ourselves to the common aim’.⁴

This rivalry almost scuppered plans to establish teaching in anthropology at SOAS. Any changes in teaching provision at the University of London had to be agreed by the University-wide Anthropology Board of Studies, and this led to a stormy meeting of the Board in May 1948. It was here that Turner, Principal of SOAS, put forward his plans for a proposed readership in Anthropology in the Department of South-East Asia and the Islands. Raymond Firth, the Chair of the Board, pointed out the importance of ‘co-operation in the use of available teaching strength’: there were already two anthropologists specializing in the anthropology of South-East Asia, and thus the new post was not a ‘high priority’. He and other Board members feared that the post would be counted as an anthropology

post by the UGC, imperiling the Board's wishes to expand the two existing departments.

Turner explained that, as it was a readership in the languages and cultures of the area, 'the subject would be taught with specific reference to the cultural background of language'. Reluctantly the Board agreed to the post, provided it was titled 'Readership in the Languages and Culture'. The understanding was that SOAS would not seek to develop expertise in 'general' anthropology, especially if it duplicated existing provision. This guarantee would ensure LSE's primacy as a centre for theoretical anthropology.

It was a difficult promise to keep. Whilst the first teaching of anthropology at SOAS was justified as being 'only to keep their people from having to go down to the School', Firth recalled that 'he suspected what was coming'. The following year he wrote an uncharacteristically robust letter to his namesake, the linguist John Firth at SOAS, declaring 'I would certainly object to the setting up of a department of anthropology which went in for teaching general anthropology of a non-regional kind', and that 'this was only reasonable since we have already in existence two quite strong Schools of Anthropology'. He went on to bemoan SOAS's 'lack of consideration' for the agreements that were reached in appointing anthropologists in 'fields that directly overlap ours'.⁵ Writing a few months later, UCL's Daryll Forde equally vociferously protested about the possibility of a third department in the university.

Firth's predictions came true. In 1949 SOAS created a department of Cultural Anthropology after an internal reorganization (Phillips 1967). The cultural anthropologist Christoph von Furer-Haimendorf, a student of Malinowski, was made reader in the same year, and founded the new department. The sense of competition and distrust between the departments returned. Despite the Board's 1949 compromise agreement on the relationship between the provision for 'general and regional teaching of anthropology at the University', in 1950 SOAS unanimously announced plans for several further anthropology lectureships with regional specializations, and von Haimendorf was appointed to a Chair in Asian Anthropology in 1951.

Whilst SOAS gradually built up a research profile and numbers of doctoral students, the London undergraduate degree syllabus and exam papers remained the product of all three departments, and the Anthropology Board of Studies continued to coordinate this process. As a result, undergraduate students continued to attend many lectures at the LSE. Studying between 1959 and 1962 at SOAS, David Parkin recalls attending both lectures (with Raymond Firth, Maurice Freedman, Paul Stirling, Isaac Schapera and Lucy Mair) and tutorials at the LSE.⁶ New department or not, undergraduate education at the University of London continued to revolve around the LSE until the late 1960s.

Undergraduate anthropology?

The ambivalence towards teaching undergraduates is exemplified in Evans-Pritchard's changing attitudes to the topic at Oxford. For the first four years after his appointment to the Oxford Chair, Evans-Pritchard and the other Oxford lecturers (including Meyer Fortes and Beatrice Blackwood) campaigned hard to establish an undergraduate final honours course in anthropology. Over an endless series of meetings extensive and detailed drafts of the proposed course syllabi were prepared. Working closely with the Professor for Archaeology and staff at the Pitt Rivers Museum, the proposed syllabus also recognised the contribution of biological anthropology, with compulsory papers on 'social evolution', the 'biology of man' and the 'comparative study of human institutions, and a whole variety of archaeological and anthropological option papers.⁷ The proposal strongly echoed a similar initiative led by Radcliffe-Brown ten years earlier, and placed itself within the long history of such attempts in Oxford to found an honours school, seeing it as providing a 'sound educational background ... for understanding man's place in nature'. Surprisingly, given the emerging post-war political settlement, it also played the empire card, noting 'the contribution in this important field which the university can rightly be expected to make to the future development of the Empire will be seriously curtailed if men and women cannot be attracted to it'. The proposal carefully argued that there were now enough teachers in the university to make the degree feasible.⁸

In considering the proposal, the university's General Board asked for the opinions of all the other faculties. Whilst some, such as psychology, welcomed the proposal, the *Literae Humaniores* faculty was unsupportive, dwelling on the likely demands on the professor and on tutors in archaeology of thirty undergraduates each year. They were also concerned that a 'new Honours school should only be founded if it will offer those who take it an education, and not merely the technical training'. Confident in their position as guardians of scholarship, the classicists felt that 'an Honours school in which less anthropology than in the school now proposed was combined with some study of civilised man would be one for which it could be more confidently claimed that it provided an education'. The General Board used this reasoning to justify turning down the anthropologists' proposal. The Board argued that 'it seems neither to provide a strict scientific training nor alternatively, a humanistic education'. As a result the 'material used in the school will hardly ever be first hand' such that undergraduates 'will be driven to rely on opinions expressed in lectures', and that 'it is not clear that a satisfactory education can be

obtained from a school so predominantly confined as that envisaged by the present plan to the study of man in a primitive or uncivilised state'. Instead the Board recommended that the 'course of study which the new school is intended to provide is essentially postgraduate in nature', and recommended that the 'co-ordinating and harmonising' of anthropological studies be effected at a graduate level.

Initially Evans-Pritchard expressed disappointment about the rejection of undergraduate studies, feeling that the role of social anthropology in giving people an understanding of social life was as important as the organisation of professional research. However, his freedom from undergraduate teaching commitments resulted in the creation of a dynamic research culture at the institute. By the end of the 1950s, Evans-Pritchard viewed the university's rebuff rather differently: 'I was always of two minds in the matter and now I am most glad the University refused our request. I am now convinced that ... in the present state of social anthropology, an undergraduate school is undesirable' (Evans-Pritchard 1959, 121). He went on to bemoan the fact that 'we have more students than we can adequately teach' and that 'a drop to half our present numbers would be most welcome'. In a swipe at over-formalised training, he went on to note how 'meaningless' he finds the question of 'how anthropology is, or should be, taught to postgraduates' (ibid., 121). 'Anthropology not being remotely like an exact science,' he added, 'you can do little more than tell him which books and papers you think he will profit most by reading' and 'make him feel that what he is doing is really worthwhile'. His views were influential, and led many to question whether the new discipline's ideas were appropriate for undergraduates.

The view from Dover Street

The story of Max Gluckman's founding of the Manchester department in 1949 reveals the swiftness with which anthropology's theoretical principles and academic practice diverged during this period. The University of Manchester advertised for a Reader in Social Anthropology in 1949 in the new Faculty for Economic and Social Science, specifying that 'candidates should be interested in both modern and primitive societies'. A glittering appointment panel of Manchester professors was set up. However after receiving applications, and on advice from Raymond Firth, they decided to interview only two people, W.H. (Bill) Stanner and Max Gluckman, though Gluckman had not actually applied for the job. Bill Stanner was an Australian anthropologist who had studied with both Radcliffe-Brown and Malinowski. Max Gluckman was a South African

who had studied anthropology at Witwaterstrand before coming to Oxford to do a DPhil. under Radcliffe-Brown. By 1949 he held a research lectureship in Oxford.

Gluckman agreed to have a discussion 'without prejudice'. Max Gluckman subsequently wrote to the Vice Chancellor, suggesting that the reason he had not originally applied was because he had felt the proposed readership 'was too ambitious, in that it seemed to integrate the new department with other branches of the social sciences too rapidly'. He went on to note that 'at least three years of straight teaching of social anthropology, as it has developed as a specialized study of primitive peoples would be required before branching out'. The Vice Chancellor hastened to assure him that his views were 'extremely sensible' and he invited Gluckman to discuss his position with the committee.⁹

The conversation was clearly productive, for, soon after their meeting, Gluckman wrote to thank the Vice Chancellor 'for the honour you have done me in raising the proposed readership to a chair', and accepted the position he had been offered. Aware of the risk of being seen to compete with established departments, he went so far as to reassure the Vice Chancellor of 'Evans-Pritchard's and Fortes' enthusiasm for the establishment of the chair', and to convey their view that it was 'one of the most important steps in the history of our subject'. Less grandiosely, he also began to plan for the department's future, writing at length about the 'patent shortage of social anthropologists', and the importance of appointing additional lectureships in order to make 'Manchester the centre in Britain for African problems'. His first choice was Elizabeth Colson, a Harvard-trained student of Clyde Kuckhohn, and by then Director of the RLL. She was appointed in 1951. He also made clear his desire to ensure that his students have 'a training in the technology of primitive and possibly modern societies' to ground the students by handling tools and understanding their mechanisms.¹⁰

Writing to one of his students a few months later, Gluckman had his own version of events:

I went to Manchester to advise them about a proposal to establish a Readership. I advised them too well and they offered me a Chair which after a tremendous struggle I wanted to reject because I feel it is early in my career and the subject cannot yet carry it. You can see how tempting it was. Finally EP and Meyer and I discussed it and we decided I ought to take it so I am off in October ... Beyond this Manchester is keen on developing the study of modern communities in England.

In another letter, Gluckman foregrounded his commitment to the discipline, saying, 'I did not want to take it but in the end was

persuaded by EP on the ground that though it might be bad for me undoubtedly it was good for the subject.'¹¹ Oral histories and the Vice Chancellor's files tell a more self-interested account. Gluckman was offered a Readership but managed to persuade Manchester to make it into a Chair.

The ebullience expressed by Gluckman in 1949 glosses over the growing intellectual differences within the 'band of brothers'. If everyone agreed on the need for more research funding, there was less consensus about expansion, or even over social anthropology's theoretical development. Gluckman's efforts to develop anthropology's interdisciplinary links were unique amongst his peers. His correspondence with his students gives a glimpse into the pedagogic cultures he created during this period. Max Gluckman was a charismatic teacher who inspired great loyalty amongst his students, such as the South African J. Clyde Mitchell (who had trained in sociology in South Africa before coming to work at the Rhodes Livingstone Institute in 1945) and the British-born John Barnes. He became godfather to both their children. Gluckman's extensive correspondence with Clyde Mitchell began as a way of advising him about his research after the former left Northern Rhodesia to take up a post in Oxford in 1946. Such letters were Gluckman's way of building his students' confidence, but also played the role of keeping the RLI camaraderie alive, and developing a similar aura around the new 'Manchester school'.

Despite having studied under Radcliffe-Brown before coming to London, first in South Africa and then in Oxford, Max Gluckman's advocacy of empirically grounded social science strongly echoed that of his former LSE teacher – Bronislaw Malinowski. He was very explicit about the importance of training and of remaining attentive to the power dynamics within the fieldwork situation (Schumaker 2001). When he was appointed to the directorship of the Rhodes Livingstone Institute in 1941, he built its reputation, and subsequently that of the Manchester department, partly through directly training his students. He advocated a team-based approach to field research and the importance of reanalysing earlier anthropological work. Max Gluckman's letters to his students often discussed the relative merits of his colleagues as teachers and lecturers, and asserted the importance of a careful and thorough period of research training.¹²

In his 1945 report as RLI director, Gluckman details his hands-on approach, noting that 'I was planning to take our new officers into the field for a short time, to introduce them to African life, and to show them certain field-research techniques.' These included 'an analysis of demographic data, budgets, and of labour migration figures' (Gluckman 1945, 70). The field site chosen was the Lamba reserve in

Ndola province, and, whilst Gluckman was quickly called away to meetings in Lusaka, his report noted that 'it has proved a most useful exercise in training us to collect quantitative data in a single scheme, and in developing a method of analysing such facts as matrilocality, divorce rates, type of kinship organisation within a village, on a quantitative basis' (ibid., 70). The research was later published, albeit with a critical commentary by Gluckman (Gluckman 1950; Mitchell and Barnes 1950). Mitchell acknowledged the fundamental importance of this field trip, and the training/data analysis that followed at Cape Town under the South African Isaac Schapera, writing that 'not only did the Institute provide the finances for academic and disinterested research, but it also created the framework in which a group of sociologists, of divergent interests and backgrounds, could work on common problems' (Mitchell 1956a, ix). Schumaker (2001, 109) describes how during this field school Gluckman impressed upon the group the 'necessity of collecting sufficiently detailed data that would enable one to analyse it later from angles not anticipated while in the field'. For Gluckman, the exercise posed 'problems of what data we can measure and how to measure them, and above all, of whether we are measuring the correct things' (Gluckman 1950, 18). He also felt it helped set lines along which 'the institute officers, as a team, can collect comparable data in their different areas' (ibid.). This focus on collaboration, and the resulting comparable and controlled statistical data collection is equally characteristic of the work of Mitchell, Barnes, Colson and even of the first half of Turner's *Schism and Continuity* (1957). It is a less well-remembered methodological contribution of the Manchester school.

Gluckman's letters to Mitchell from Oxford in 1948, show how he had to defend his close supervision and intellectual involvement with his students to Evans-Pritchard. 'EP said to me (after our third pint)', he wrote, "Don't you think you've done too much work for them?" and I replied with well-lit spontaneity "they've done more for me".¹³ The letter continues, stirring, 'remember that we are brotherhood with a tradition to respect and that those old blokes were not all pampoen [sic], so one day have a go at reading Tylor, Maine, Engels and the others'.

A subsequent letter to Mitchell reveals a growing difference of intellectual opinion between Gluckman and Evans-Pritchard over just what it meant to be a social anthropologist:

We've been having rather a battle this term in seminars with an idealistic wave – it started with Mrs Bohannan in a discussion of Malinowski's Argonauts saying that sociological theories were just attempts of the mind to bring order, and there is no way of testing between theories. Then EP, Lienhardt and others said there were no facts about a people, only what the

observer wrote in his notebooks. Meyer and I are fighting hard for our scientific attitude: the facts are public, DCs and Barotse read what I write about the Lozi and it has meaning for them, the facts are checkable in the subject so that I told Lienhardt that even if he wanted to lie about the Dinka he couldn't get away with it. That we have a series of propositions which are being tested all the time etc. And more and more I feel I am a social anthropologist, and I must stick to my last.¹⁴

Soon after arriving, Gluckman began to write about 'starting the new RLI at Manchester' as it had been 'an important experiment in sociological research' that needed to be furthered. He encouraged Mitchell to leave Northern Rhodesia and come and join him, admitting that 'though you would have fewer contacts with social anthropologists, you would have more to do with economists, political scientists, general sociologists. And they are all good men.'

Gluckman thought strategically about how to consolidate his position at Manchester and to begin to shape a new form of anthropology. In particular, he championed the case-study method that he had used in his influential account of the ritual surrounding the opening of a new bridge in Zululand (Gluckman 1958 [1940]). Skirting around its origins within Sociology and Psychology (Platt 1996), he gave the case-study method a distinctively anthropological history, repeatedly promoting it as a Manchester-based methodological innovation (Mills 2005b) within forewords to his students' work (e.g. Van Velsen 1964). He also took the task of promoting anthropology enormously seriously, involving schools and forging links with local teacher training colleges, but also with the cognate social sciences:

I'm well aware of the danger of degenerating into human relations as they have in the US. But I think our discipline is sufficiently clear in Britain for these tie-ups to be intellectually profitable. Beyond that as I see it we must have these links to make social anthropology a general teaching subject filtering through other subjects into the schools (particularly through educational psychology and geography and into general thought through law history etc.) ... I can argue that there are all these demands besides those of my own dept – hence I need a large staff though at the moment I've not got a degree or many students.¹⁵

By 1950 he was confident of a second chair in social anthropology: 'It won't be called that, but something like Social Studies, though I shall aim for a title like comparative sociology or experimental sociology.' In his letters to Mitchell, he went on to outline his vision for an anthropology of modern society:

It will be specifically for the study of modern communities (western family and kinship, neighbourhoods, factories etc) and will be a chair to provide

the academic background and research, with its equivalent of colonial administrators in the people we call social administrators – welfare workers, social workers, personnel managers etc ... These are not just idle dreams of mine, though they are uncertain in that to some extent university politics is keen on these modern studies, and my faculty colleagues want the new chair.

Gluckman increasingly voiced his dissatisfaction with the theoretical direction of the Oxford department, especially Evans-Pritchard's 'turn' to history in his 1949 Marett lecture, and his Third Programme broadcasts, which Max saw as 'full of contradictions'. In one letter he suggested that 'Oxford is moving into sterility.' His comments on the poor quality of the work of Evans-Pritchard's students led him to define his brand of anthropology as developed at the RLI and Manchester as increasingly occupying the disciplinary mainstream, building on the ethnographic empiricism of Malinowski himself (Gluckman 1961b, 1966), whose reputation he and others sought to restore (e.g. Firth 1957, Kaberry 1957). This became clear in his letters:

Now all we were doing was honest to god social anthropology without frills. Above all we were not trying to be historians, or students of comparative religion, or what have you, but to work within our distinctive discipline, acknowledge the limitations of technique data and problems, but dealing with those in the way we can best do so.¹⁶

The same letter is also full of derogatory remarks about the other departments, with Meyer Fortes (in a new Chair at Cambridge) viewed as being 'hamstrung by his staff, and will have a hell of a job', and London also 'useless and sterile'. Gluckman can then conclude that 'Manchester will become a centre for postgraduate study, preferred to London and Oxford and Cambridge by a number of people fairly soon.' Again he emphasises the interdisciplinary atmosphere, noting how 'I've now got on good and co-operative terms with the Professor of Psychiatry, historians, lawyers etc' and his plans for weekend schools to 'prepare training college teachers for giving courses on social studies for teachers'. His optimism is expansive. 'I can do the same in geography and psychology any day when I feel I can cope. I'm sure the university is ripe for the development and expansion of social anthropology.' By 1952 Max talks of the 'wide demand' for the subject, that anthropology had become compulsory for all doing an honours degree in politics and economics and how even the Vice Chancellor had told him 'he'd heard how much the subject was adding to the university'.¹⁷

This optimism is reflected in Manchester's expansion during the 1950s. By 1958 it had five posts, with a wide programme of undergraduate teaching. During the same period, established departments had increased their tenured posts by only one or two positions. Oxford went from five to six posts, Cambridge from five to six, UCL from six to eight, LSE from six to eight, Edinburgh from two to four. In 1951, a one-year postgraduate diploma in anthropology was launched, and the department continued to expand during the 1950s, with Tom Lupton (who went on to shape the new field of business studies) and William Watson appointed in 1957 as the first lecturers in sociology within the Department of Social Anthropology. It soon became a joint department, and by the end of the 1950s the sociology courses outnumbered anthropology courses. In 1957 Gluckman himself taught courses on industrial sociology, field sociology, the sociology of India and sociological texts and problems. The titles often belied anthropological themes. Gluckman was by this point Dean of Faculty, leading a growing but cohesive group of left-leaning students and lecturers that now included Emrys Peters and Ian Cunnison, both of whom had trained at the RLI.¹⁸

By the mid-1950s, the institutional and intellectual rivalry between Gluckman and Evans-Pritchard had become increasingly bitter. With the research base and access to the field offered by the Rhodes Livingstone Institute (RLI) being key to Manchester's success at attracting new students, Gluckman began to fear for its future funding, fearing that Evans-Pritchard was determined to sabotage the RLI. 'E-P has the will only to exploit the RLI for his students,' wrote Gluckman in 1955 to Clyde Mitchell, and 'would be pleased if the show broke up and was a failure – since he has publicly stated that research cannot be done well from the Institutes but only from universities. Since in fact the RLI is turning out far better work than Oxford, it would suit him to get a bad Director who would break the show up.'¹⁹ The same year Gluckman accused Evans-Pritchard of spreading a rumour about one of his students' political sympathies (making him therefore unacceptable as an employee to the RLI trustees). Max announced in one letter his estrangement: 'I've broken with E-P', declaring that 'he is spreading rumours that Bill Epstein carries a communist party card'.²⁰

Whether or not Evans-Pritchard was behind such a slander, Gluckman took the matter deadly seriously, writing at great length to the Northern Rhodesian Governor, head of the board of RLI trustees. His fear was that the rumour might be used as a pretext for wresting control of the institute from the anthropological community. He failed. Liverpool-born Bill Epstein was refused further research permission and subsequently passed over for the RLI directorship, in favour of a

scholar-administrator called William Fosbrooke. Fosbrooke had been a government sociologist in Tanzania, and had an authoritarian reputation. Meanwhile the Colonial Social Science Research Council (CSSRC) had been putting pressure on the institute to develop a closer relationship with the university. The institute's trustees used the events as a way of appointing a director who would be less independent and more amenable to government influence at a time of growing political unrest in the territory. Fosbrooke was the ideal candidate. Gluckman, Mitchell and others were dismayed, recognising that the academic freedom of the institute would be increasingly limited. As Mitchell wrote, rather despairingly, 'In a real test situation like this the CSSRC is powerless for the simple reason that the trustees decided that they would not consult the CSSRC – for obvious reasons: they knew it would recommend someone on academic grounds not on extraneous grounds – RLI – Rest in Peace.' Two years later, as Gluckman finally accepted that his influence had waned and that the 'RLI is going to become an adjunct to government', he decided to withdraw all support for and contact with the institute.²¹

By this time he had other matters to deal with. He had founded a highly successful and expanding department in Manchester, and many of his students were working on research projects within the UK. Gluckman now had less need of the RLI, even if he continued to make intellectual capital from its past. His success in getting research funding for UK-based research projects meant that the RLI steadily faded in significance for him. 'The development of the department in the industrial and other fields', he wrote in 1956, 'has turned me from a happy-go-lucky companion in research to a querulous and overworked business executive. I am asking for three more telephones to be installed.'

Though it was to end in recriminations, Gluckman continued to have an expansive vision for anthropology. His department was now a Department of Anthropology and Sociology, and his reputation had led to his appointment as President of the Sociology Section of the British Association in 1961. He was by now a regular fixture on the lecture and media circuit, and was asked by a number of the newer universities (including Belfast, Reading and Sheffield) for advice on setting up sociology or anthropology departments.

Too much can be made of his interdisciplinary ambitions. When Bill Epstein proposed doing an ethnographic study of a Manchester Jewish community in 1951, Gluckman apologised for his 'apparent lack of enthusiasm'. He regularly recommended to his students that they should cut their teeth on 'primitive societies'. Clyde Mitchell wrote to console Epstein, saying, 'Personally, I should say, go for modern studies every time. I think that social anthropology is disappearing as

a discipline and that the future lies in modern studies. However primitive society is so much easier and much more pleasant. Max is conservative and I shall tell him so.'²²

Gluckman's own view, as he later wrote to Mitchell, was of 'how important it is to work on these problems in Africa ... I am convinced that the complexity which we work out now on tribal systems derives from our attack on the problems of more complex societies'. For Gluckman, studies of industrial society served primarily to benefit and inform the 'real' work of the anthropology on rural African societies (Gluckman 1961a). Not everyone agreed. Many of his students went on to develop a field of urban anthropology, with first Clyde Mitchell (1956b) and later Abner Cohen (1969) making important contributions to the study of symbolic use of ethnic identifications in complex urban situations.

Frankenberg (1988) suggests that Gluckman was not the only one to be making links outside the discipline during this period. 'Firth and Gluckman in very different ways, both saw Social Anthropology as a firm base from which to co-operate with other social sciences. EP and Fortes, perhaps reflecting the situation at Oxford and Cambridge, turned more inwards.' Yet Manchester's proselytising approach was isolated. Ironically the department was by now becoming a formidable training ground for students who would subsequently find employment in sociology departments, including John Barnes, Peter Worsley, Max Marwick, Clyde Mitchell, Michael Banton, Tom Lupton and William Watson. No other department followed Manchester's example of involvement in teacher training colleges and schools. On the contrary, Evans-Pritchard's view that social anthropology should be a graduate subject was increasingly influential.

It is too easy to emphasise the differences, and to present each department as having a distinct intellectual atmosphere. The discipline remained a close-knit network of scholarly patronage and loyalty, bonded by the obligatory sociality of annual ASA conferences. The dominant view was that anthropology's moral relativism was risky for unformed minds. The intellectual security of the ASA and the funding buffer of the UGC led senior figures to disparage calls for change.

Not everyone agreed. At an ASA meeting in 1962, Gluckman emphasised once again the need for disciplinary expansion. 'No new departments had been created in Britain since those at Manchester and SOAS in 1949,' he pointed out, 'though individual lectureships in other types of departments had been set up.' The meeting only agreed to 'think over the position of "isolated" lecturers in the subject and see if the ASA could do anything to help them'. More significantly, he insisted that the meeting agree that 'the committee and all members should consider all ways of using their influence to get the subject

established in new universities, though it was clear that the ASA could not write officially to the Vice Chancellors of these universities about this matter'.²³

Sussex was one of the only new universities to prioritise anthropology, and its Vice Chancellor approached Evans-Pritchard for advice. He responded with a pithy summary of the discipline, acknowledging that it 'can be said to be a branch of Sociology', and that it increasingly attended to 'urban communities and to problems of industry and medicine' but that 'we have always been insistent on field studies'. As to establishing it anew in an institution, he declared himself in favour of 'starting it off at the lowest level and let it work its way up to a readership and a Chair in course of time rather than the other way around', the exact opposite of Manchester's approach. The advice was disregarded. The School of African and Asian Studies was established at the University of Sussex the following year, and Frederick Bailey, one of Max Gluckman's students, was appointed as Professor of Anthropology in 1964. With the blessing of Raymond Firth, Paul Stirling left the LSE to a professorship in the new Sociology Department at Kent in 1965.²⁴

Sociology and anthropology: we'll show them a real discipline

Did the two disciplines ever work together? There were several attempts at interdisciplinary dialogue in the 1960s, such as a 1961 joint British Sociological Association and ASA conference at LSE on 'Family and kin ties in Britain and their social significance' convened by Firth, and coming out of research he had done in Bethnal Green. Yet even Firth had to get permission to invite those sociologists who were not ASA members to this meeting. Collaboration was not the priority of every anthropologist. Peter Worsley recalls Meyer Fortes leaning conspiratorially towards him at the opening reception, saying 'We'll show them what a real subject looks like.' Given that Worsley had been told that as a card-carrying communist he would never be able to get a job in British anthropology, he was unsympathetic.²⁵

Subsequently plans were made to host another joint conference on teaching, and Paul Stirling was appointed to the BSA education committee. However, nothing came of this plan, or of attempts to publish ASA occasional papers in the *British Journal of Sociology*. The issue of disciplinary identity also arose at the ASA's own discussions about teaching anthropology in schools. Frankenberg recalls how one 'Mr Tyler from Bristol, who taught overseas teachers, was unwise enough to conflate social anthropology and sociology, bringing down

upon his head the polite but firm wrath of Meyer Fortes, who reiterated that difference was all, between societies as well as disciplines' (Frankenberg 1988).

The efforts by individuals such as Gluckman and Banton to forge interdisciplinary alliances had little lasting impact on intellectual debates. During the 1960s there was a boom in demand amongst undergraduates wishing to study a social science that addressed pressing issues of class conflict and political economy in industrial societies (Platt 2003). As sociology grew and became more diverse, its relationship with anthropology became steadily less important. Nor did all anthropologists see any strategic value in a closer alliance with sociology. As Barnes commented, 'the relationship between anthropology and sociology was far too sensitive a political matter to discuss openly – sometimes it was convenient to argue that one was a branch of the other, or that they were the same thing, or that they had nothing in common – it all depended on who was providing the funding'.²⁶ The differing fortunes of the two disciplines were also reflected in their hiring practices in the early 1960s. Social anthropology departments tended to staff themselves with other social anthropologists; sociology departments were staffed with whoever was available. As one commentator puts it, 'the issue of professional or disciplinary coherence was raised after – rather than during – the period of expansion' (Spencer 2000, 5). This is confirmed in the careers of anthropologists turned sociologists such as Peter Worsley, Michael Banton and Tom Lupton. Worsley reminisced that 'Sociology exploded: all the sociology departments did. I had five chair offers in one week. "Please can you come?" they would say.'²⁷ Whether despite or because of their shared origins as intellectual half-siblings (Peel 2005), sociology and anthropology increasingly developed antagonistic personalities.

As a result, Gluckman's commitment to a dialogue with sociology faltered. During the 1960s he backed away from his earlier efforts to persuade anthropologists to do research in industrial societies, and to do sociology in an anthropological way. Barnes described how 'Max's plans for me were to go to Norway and turn myself into a sociologist, but then Max found that there were no jobs for me in Manchester.' Barnes subsequently held the first Chair in Sociology at Cambridge, much to the consternation of existing sociologists.

Peter Worsley's personal recollections about his appointment as a Professor of Sociology in Manchester and his growing rift with Gluckman are revealing:

He appointed me as his pupil – the only concern he had was that Sociology wouldn't overtake anthropology. Three quarters of the faculty students wanted to do sociology – we had 700 students, and they only had 40. I agreed to a limit to expansionism, but Max pushed for more concessions. We came to a showdown and went to faculty. Other departments got

involved, and it was reluctantly agreed that we should split the departments. The compromise was to split the first year course, so students could opt for either option – but he still wanted to keep them separate. He was not prepared to recognise the decline of anthropology to a small enclave in sociology.

Worsley also described setting up a joint seminar with Gluckman, where they would invite speakers on alternate weeks: 'Gluckman first invited Lester Hiatt to a talk on spear-carrying amongst Aboriginals, and then I invited a biographer of Rosa Luxemburg. You should have seen the anthropologists glaze over.' Tensions mounted, and by the end of the 1960s, Manchester's Department of Social Anthropology and Sociology had split in two.

A rather different problem faced anthropology in Oxford during the 1960s. Sensing the growing importance of sociology as a discipline, a committee of Oxford intellectual glitterati (including Professors Ayer, Beloff, Berlin, Habbakuk and Trevor-Roper, as well as Evans-Pritchard) was convened to appoint a new Readership in Sociology. No one candidate could be agreed upon, and, after much infighting, Evans-Pritchard wrote to the Vice Chancellor with a proposal to remedy the situation. Acknowledging that the appointment was anticipated as being within the Department of Social Studies, he wondered 'whether it is understood how remote the sociology of today is from most of what goes on in Social Studies', and that "'sociology" has been taught in Oxford under the name of "social anthropology" since 1910' such that 'it would be very difficult to determine between what I and my five colleagues do in the Institute of Social Anthropology and what is called "sociology"'. In another memorandum, he was more explicit still: 'Why not a department of sociology the professor of which should always be a professor of social anthropology?'²⁸

He also revealed his deeper feelings about the new discipline: 'Since a lot of nonsense has been written in the name of sociology, sociology may have a bad name, and, after all, 'social anthropology is and always has been regarded as comparative sociology'. He bolstered his campaign by asking Max Gluckman to send down copies of his exam syllabuses and questions to show the affinities between the two disciplines as they had developed at Manchester. Yet his attempt to capture the professorship for anthropology came to nothing. Ten years later, tension between the two departments returned when, just before Evans-Pritchard's retirement, the Social Studies Board repaid the compliment by seeking to take over the Chair in Social Anthropology for Sociology.'²⁹

Conclusion

Since the 1940s social anthropology has depended on the patronage of the British state for its institutional survival. It repeatedly sought to emphasise its disciplinary uniqueness in order to bolster that funding. This uniqueness was reinforced by its endogamous approach to intellectual reproduction – no sociologists were offered jobs in anthropology departments, and anthropologists getting jobs in sociology departments were seen as ‘leaving’ the discipline. Partly because of a shortage of qualified staff, expansion into the new universities, most of whom were keen to establish sociology departments, was not actively pursued during the 1950s and 1960s. At Oxford and elsewhere, a continuing legacy of intellectual elitism ensured the prioritisation of postgraduate training and research over the development of undergraduate teaching.

Max Gluckman and Raymond Firth were exceptions to this rule. They did most to spearhead a ‘modern’ anthropology that sought a dialogue with its closest cognate – sociology, as opposed to history. There is little doubt that Gluckman’s vision had a major impact on the discipline (e.g. Parkin 1966). Yet his inclusivity had its bounds. As sociology became increasingly powerful at the end of the 1960s, he became increasingly embittered about the institutional divergence of the two disciplines, despite their shared intellectual heritage and concerns. Anthropology’s ‘expansive moment’ (Goody 1995) seemed to be at an end.

Notes

1. Clapham Commission Cmd Paragraph 2.
2. *Ibid.*, p127.
3. LSE Archives, Firth interview with Maurice Bloch, 1988, BLPES.
4. Firth Archives, 1/14 Letter to Daryll Forde 14.11.50.
5. Firth Archives, 1/14 Letter to Sir John Firth 5.11.49.
6. David Parkin, personal communication, July 2006.
7. The syllabus also had parallels with the human sciences degree, created in Oxford in the 1970s.
8. Myres MSS 81.
9. Vice Chancellors files (Vca), University of Manchester, Vca/7/87 Chair of social anthropology, Gluckman to VC 21.3.49, VC to Gluckman 22.3.49.
10. Vca/7/87, Gluckman to VC 21.4.49, 22.5.49.
11. Mitchell Archives, 5/1 f44 letter from Gluckman to Mitchell 17.9.48.
12. Clyde Mitchell Archives, Rhodes House, Oxford.
13. Mitchell Archives 5/1 f25.
14. Mitchell Archives, letter from Gluckman to Mitchell 5/1 38 12.2.49.
15. Mitchell Archives, 5/1 f127.
16. Mitchell Archives MG to Clyde Mitchell, 11.12.50.

17. Mitchell Archives, 5/1 letters from Gluckman to Mitchell 1951–52.
18. Commonwealth University Yearbooks, 1949, 1955, 1959.
19. Mitchell Archives, Gluckman to Clyde Mitchell 16.2.55, Mitchell MSS box 2 fo. 56, Rhodes House.
20. Mitchell Archives, 5/1 f72, letter from Gluckman to Mitchell 15.7.55.
21. Gluckman to Clyde Mitchell 8.7.1957, Mitchell MSS box 2 fol. 206.
22. MSS Mitchell, letter from Mitchell to Epstein 5/3 f11 28.5.51.
23. ASA Archives, BLPES, ASA committee minutes 1.1.
24. ISCA archives, Evans-Pritchard papers 2/2/11.
25. Interview, London, 8.5.00
26. Interview with John Barnes, Churchill College, Cambridge 23.7.00.
27. Interview with Peter Worsley, 8.5.00.
28. ISA Archives F2/9 Sociology, Oxford University Archives.
29. Ibid.

HOW NOT TO APPLY ANTHROPOLOGICAL KNOWLEDGE: THE RAI AND ITS 'FRIENDS'

Introduction

At the end of the Second World War, the Labour Party's election victory under Clement Attlee led to hopes of an economic renaissance. Key manifesto promises included the nationalisation of the coal and steel industries, and the application of scientific planning and management. Policymakers began to consider the role that the social sciences might play in understanding human behaviour within industrial settings. In this chapter, I describe social anthropologists' reluctance to 'apply' their disciplinary knowledge to such utilitarian ends during this period. The story reveals the distance that the discipline had travelled since Malinowski successfully marketed 'practical anthropology' to Rockefeller in the 1920s. Now that social anthropology had a secure institutional foothold within Britain's universities, its leaders could enforce a hierarchy that placed theoretical and 'pure' anthropology firmly above application. Evans-Pritchard evoked this new determination when he wrote, just after the war, that 'I doubt whether anyone can investigate fundamental and practical problems at the same time' (1946, 93).

Historically, British anthropology's attitude towards its application can be characterised as one of serial ambivalence. Anthropology both depends on and denies its utility. Early proselytisers like Bronislaw Malinowski made the case for anthropology's relevance and

applicability to garner funding from the major philanthropic foundations (e.g. Malinowski 1929, 1930). Once anthropology's institutional position was more secure, academic anthropologists could distance their practice from that of colonial administrators, gentlemanly amateurs and curious travellers, creating a disciplinary 'comfort zone' around their work. At this point, 'application' developed a different meaning and resonance. It became dependent on, and derivative of, academic and theoretical work. Pels and Salemink, writing about the colonial contexts in which anthropology was formed, suggest that the discipline is in the habit of back-projecting 'the self-image of twentieth century academic anthropology onto all ethnographic activities that played a role in the formation of the discipline' (1999b, 2). They suggest that many of its practitioners saw the 'real' history of anthropology commencing only when theoretical and research expertise were fused in the person of the professional fieldworker. It was a narrow view of the discipline's recent history – after all, Radcliffe-Brown's collaboration with two Australian scholars, the psychologist Elton Mayo and the sociologist Lloyd Warner, led them to influentially apply Radcliffe-Brown's ideas to understand American factory life (Mayo 1933, Warner 1952), and also to strongly influence the sociologist George Homans (Homans 1951). Nonetheless it strengthened the resolve of British social anthropologists to challenge the use of their discipline to solve industrial 'problems' in post-war Britain.

The RAI and its 'friends' 1947–55

The attempt by captains of British industry to court anthropology in this period is one of the more glamorous chapters in the history of a discipline already marked by its aspirations to join the British establishment (Leach 1984). Black-tie soirées at Claridges offered a convivial new milieu in which to woo social anthropologists. The key figure in this development was Israel Sieff, co-founder of Marks and Spencer. He was determined to show how anthropological methods could be used to study labour relations within large corporations. Sieff spent a great deal of time during the early 1950s convincing academics and his business colleagues that anthropology could solve industrial 'problems'. Misapprehensions about what the discipline could offer abounded, amongst both industrialists and anthropologists.

The new field of social anthropology had already proved itself adept at gaining funding from independent foundations (such as Rockefeller and Carnegie) and the British Colonial Office. However, commercially sponsored research was of a different order. In seeking to woo the

Royal Anthropological Institute (RAI), Sieff threatened the autonomy that academic anthropologists, through the creation of the rival Association of Social Anthropologists in 1946, had established around the discipline.

Israel Sieff, born in 1880, grew up in the same street as Simon Marks, the son of the founder of the retailer Marks and Spencers. After making a loan to Simon and joining him on the company's board in 1915, they worked closely for the next fifty years to build the company from its Manchester roots to its place as one of the UK's most powerful retailers. Renowned for his personal energy, Sieff used his position as a public figure to great effect. Soon after its founding in 1931, he became involved in Political and Economic Planning (PEP), an independent think tank to promote the science of 'national planning' as a solution to the economic and political crises of the time. It brought together an informal network of politicians, economists and journalists in working groups to develop practical strategies for tackling economic decline and mounting unemployment, and Sieff co-authored an influential report on industry in 1932. As he later modestly recalled, his influence ensured that the reports kept 'on a practical level ... theoretical and abstract approaches have always been above me' (Sieff 1970, 172). After the war, its activities greatly expanded. At the request of Sieff, a new PEP study group was launched to study human relations in industry. Given his experience at building a coalition of opinion, and his amateur interest in anthropology (he was already a member of the RAI), he decided to involve the RAI in this new initiative. Over more than five years, Sieff spent a great deal of time seeking to build links between anthropology and industry. It was here that the black-tie dinners came into their own, as he sought to bring together academics and his business colleagues.

The romance had actually been initiated by William Fagg, RAI Secretary between 1939 and 1956, and an important scholar of African art. In 1947 Fagg approached the industrial reformer Sir Robert Hyde, and asked him to come to speak to the RAI about the potential for 'cooperation' with industry. Hyde, born in 1878, had begun his working life as an ordained priest and warden of a Hoxton boys' hostel, and his experiences led him to set up the national Boys Welfare Association in 1918. Campaigning for better working relationships between managers and employers, and in particular for the provision of basic workplace amenities like lavatories, canteens and changing rooms, Hyde's hands-on style was highly influential. The organisation grew into what subsequently became known as the Industrial Society (which continues to this day under its new title 'The Work Foundation'). By the late 1940s Hyde had had a considerable impact on British industry and commerce, and his style of visiting and

spending time on factory shop-floors made him sympathetic to an anthropological approach. After speaking enthusiastically about anthropology's potential, he was made a member of the RAI council, he subsequently became a Vice-President of the RAI for a number of years, authoring a number of reviews and chairing the British Ethnography Committee in the 1950s (Hyde 1955, 1957).

Whilst nothing came of Fagg's efforts, an approach several years later by Lord Raglan, then RAI President, to Hyde's friend Israel Sieff had a more tangible outcome. Lord Raglan, the great-grandson of the famous Crimean War general, was a linguist and independent scholar with an interest in anthropology, and boasted an impeccable aristocratic pedigree and set of establishment connections. Sieff could hardly refuse, and invited the RAI officers to talk over lunch about the needs of the institute. Sieff was obviously impressed by what he had heard, and decided to host an informal dinner to bring together the RAI officers and committee members, including Meyer Fortes (then editor of *Man*), and Sieff's friends and business contacts.

Held in November 1951, little is recorded of that evening's conversation, but it was clearly a successful occasion, for soon afterwards Israel Sieff's secretary sent a number of cheques from Sainsbury's Ltd, Thomas de la Rue, Unilever and Lotus to the RAI. Of particular note was a £1,000 cheque Sieff forwarded from George Harris, Chair of Rowntrees, who had noted 'how impressed he was by what he had heard at dinner', and in particular 'by the long and interesting discussion he had with Meyer Fortes'. According to Sieff, Harris had felt 'it would be an excellent idea if Fortes visit York and tell his colleagues there something about the work of the Institute is doing'. Sieff immediately wrote to Fortes to suggest this, noting that his colleagues 'were much impressed with the great potential value of this work in relation to the major problems we are facing'.¹

Buoyed by this response Sieff discussed with Lord Raglan the potential for formalising the link, and proposed the creation of a body entitled the 'Friends of the RAI'. Soon after, Lord Raglan sent out a formal invitation to members of the RAI Council to meet Sir Robert Hyde over dinner at Sieff's flat, and to 'discuss a number of interesting aspects of the industrial problems today in this country, in which he feels that we may be of valuable assistance to Industry in general, both in this country and in their activities overseas'. Again, the response was positive, and the 'Friends of the RAI' were formally recognised at the next Council meeting. In the Summer of 1952 Council also agreed the formation of a liaison committee to expand this network of 'Friends', made up of twelve members, including Hyde, Sieff and Webster Plass as representatives of industry. As well as the RAI officers and President, Evans-Pritchard, Fortes, Forde and Leach were all

invited to join, as serving members of the RAI's executive committee. The report in *Man* of the RAI Council for 1952 presented the initiative to its membership in glowing terms – 'a group of industrialists who recognise the great potential importance of the results of fundamental anthropological research in the future of this country both at home and abroad'. Whilst acknowledging that the research would 'in no way be at the expense of existing methods and fields of research', the RAI Council was of the view that the Institute's role was to ensure that 'fundamental research should keep pace with developments in the application of research in social affairs'. This was to be a new and provocative twist to the relationship between anthropological theory and practice.

The liaison committee drew up a document for discussions with potential new Friends. Its rosy presentation of the RAI's past achievements and Panglossian potential for solving the problems of modernity deserves quoting at length:²

We firmly believe that the RAI, now hampered severely in its great work by a rise in operating costs out of all proportion to its income, deserves our immediate and effective assistance. The institute has gained a high international reputation and sponsored anthropological research from all over the world. Its forthright defence of the need to integrate human studies has played a vitally important part in the cross-fertilisation of ideas between specialisms, which to the detriment of our understanding of human behaviour, are growing steadily apart.

We live in a changing world, and new conditions require new methods of approach as problems of human relationship increase in magnitude and complexity, the political advancement of colonial territories, the economic development of backward areas, the increasing integration of world trade ... the growth of nationalised industries and vast industrial combines, new forms of labour organisation, a managerial class, all these are creating a new situation in human affairs about which we know very little and on which, relatively speaking, research expenditure is negligible. Many believe that the broad approach of the Institute, rooted as it is in empirical research, is becoming increasingly important to an understanding of the world, and especially industrial personnel problems.

'A strictly scientific attitude'

Determined to keep the momentum, Sieff planned a major dinner for the Friends of the RAI in the Summer of 1952. Again the guest list was illustrious, including the chairmen of ICI, GEC, Unilever and Metal Box. Whilst only five out of the twenty or so of such magnates attended, amongst these were Thomas Padmore, Under Secretary in the Treasury. Again, a number of anthropologists attended, including

Edmund Leach, who was later to become RAI President. Leach, however, was 'dismayed' at some of the extravagant promises that were made by some of the RAI representatives over dinner, and, in a characteristically long and colourful letter to Sieff, told him so. It was 'very natural', Leach agreed, that some of the financiers should ask, 'Of what practical use is this anthropology to us?' and it was inevitable in the circumstances that 'anthropologists should put forward claims for the practicality of their subject and the comprehensiveness of the RAI as an institution'. 'Nevertheless', he went on,

I feel this is a very bad basis on which to make claims for support. All the serious anthropologists I know have a strictly scientific attitude to their subject; they are interested in the structural relations of human society in much the same detached kind of way as physicists are interested in the structural relations of the atomic nucleus. The fact is that the anthropologist really does not know whether or not his subject has any important practical applications, but when anyone holds out a financial carrot he tends to invent them. This is not fair either to the anthropologist or to the financier.

This was a rather disingenuous account of a discipline that only a few years earlier had argued for the potential relevance of anthropology for understanding colonial 'problems', and received a significant amount of funding for doing so. Leach went on to dismiss as 'nonsense' the idea that anthropologists might act, as it were, as social planners, or that they could provide the data that would 'solve the industrialists' problems'. As far as he was concerned, 'Anthropologists are people who have made a specialisation of techniques for describing the behaviour and organisation of small groups especially those of "primitive societies", but it is *anthropological* information, and it is most misleading to suggest that it can be readily utilised or made available to anyone who is not an anthropologist.' Finally, Leach pointed out that until very recently, 'research work relating to "modern society" has been felt to be outside the Institute's province' such that 'the Institute is not now equipped to provide the background data for sociological studies'. Instead, he proposed, that these resources could be developed, and that the Institute should 'aim at being a focal point for all the fields of sociological study, not only the anthropologists but the sociologists and social psychologists as well'. The prime need was for the 'institute to occupy its own building and pay its staff without pinching and scraping', such that the 'immediate need therefore is for money for general purposes and not for money for specific research into this or that detailed problem for which the anthropologist may be ill equipped to deal'.³

Leach copied his letter to Fagg, who privately expressed the view that it showed Leach 'and the LSE generally are trying to wreck the whole thing'. To his credit, Leach did nominate Kenneth Little to the RAI Council, suggesting that the 'work going on under the sponsorship of Social Anthropology in Edinburgh is just the sort of thing that Mr Sieff and his friends might be interested in'.⁴ With Leach's encouragement, Little also wrote to Fagg, expressing his regret at having not been told about the committee earlier, given his interest 'in extending anthropological research amongst advanced societies', and noting that much research activity of his Edinburgh department 'is being carried on in this field'. In turn Little nominated Noel Stevenson to the liaison committee, given his experience as a colonial administrator. As a result, Fagg invited Stevenson to act as a point of liaison between the committee and the Friends to pre-empt an early souring of a relationship that Fagg felt was only beginning to develop. Unfortunately, Leach had already had a run-in with Stevenson, who had been involved in what Leach later called 'crazy cloak and dagger' intelligence work in Burma (Leach 1986, 377).

Stevenson threw himself into the role, and, with the help of the RAI, prepared a memo to the Friends detailing the RAI's financial outgoings, as part of another circular Sieff planned to tell 'his friends about the RAI and the way in which it – and anthropologists – might help industry'. The RAI Treasurer proposed a figure of £6000 to 'accomplish its functions', which he suggested, was only a matter of 'ten of our wealthy industrial friends' agreeing to covenant £400 each for seven years (the sum that Marks and Spencer had already committed), 'which they would not really miss in these days of high profit'.

As the momentum for the campaign developed, Sieff and the liaison committee prepared a new and more explicit appeal for research funds, emphasising the urgency of addressing social change. In a letter to Peter Rowntree, Sieff noted that:

Many of the causes of unrest, misunderstanding and unhappiness in industry are to be found in the past, and have their roots, not in present events, but in customs, traditions and group loyalties which no longer hold. The growth of business concerns, the disappearance of old personal relationships, the emergence of a managerial class, the breaking down of operations, are creating new situations in human affairs. We believe that this rapidly changing social pattern demands fresh methods of study and approach if adjustments in relationships, stability and integration are to be realised.

A draft fund-raising document proposed a 'research fund' for research into human relations in industry, strengthening the resources of the

Institute so that it could (1) establish an information service; (2) complete the reorganisation of its unique library; (3) undertake investigations into specific industrial problems; and (4) enlarge its publications on industrial research. Finally, the document proposed the establishment of a 'Museum of English Life and Industry', providing material evidence of the traditional way of life in England. This last proposal was included mainly at the behest of Hyde, who was also a close friend of Sieff. He felt that 'if we can bring in those who gathered around Sieff's table on some practicable issue such as this, it might lead to the development we all desire'.

Social anthropologists on the liaison committee were still not happy with the shape the fund-raising proposal was taking. Raymond Firth, new to the committee in 1952, decided to prepare an alternative draft. Ever sensitive to the need for disciplinary autonomy and new studentships, his proposal was to seek general financial support for the RAI and to establish a 'Research Fellowship' scheme. His proposal spent some time seeking to define the discipline itself:

The great importance of anthropology is in giving general clues to the understanding of human behaviour ... but it may also give some help in the solution of practical questions, provided that adequate study has been made of the problems ... this necessitates a great deal of basic research which, like such research in the natural sciences, is not by any means always linked to the solving of some practical problems.

This time it was the industrialists' turn to express their frustration. Robert Hyde, perhaps aggrieved that no mention had been made of his museum idea, felt that Firth's letter was 'too vague, and focuses primarily on problems', and 'fails to relate the work of the anthropologist to actual conditions prevailing in rapidly changing circumstances or to the wider influences that affect that relationship'. 'In my time,' he added, 'I must have read hundreds of documents addressed to employers and have found that when direct argument falters the writers fall back upon "problem" or "factor" as an easy way out.' For Hyde the solution was 'a more direct approach ... to the ordinary business man', ideally with a few extracts 'taken from that American journal of applied anthropology showing in what direct and practical ways anthropology could be of service to industry'. Drawing on his reading of this journal, *Human Organisation*, where US scholars were developing Lloyd Warner's seminal studies of factory-based networks, Hyde even proposed that the case for applied anthropology should be set out in a clear six point structure: '1 Study of basic human relationships and loyalties. 2. Interpret traditional anthropological method to modern industrial practice. 3. Fields of application in

industry. 4. Examples of successful applications. 5. Results to be expected. 6. Cost and return to industry.'

The following year Peter Rowntree, one of the Friends of the RAI, expressed similar misgivings about a memo prepared by Professor Forde. Writing to Sieff, he explained that having 'read this with interest several times, some parts of it I must confess I am unable to understand. Basically I think there is a difficulty of communication. It would seem that there is considerable difference between the nomenclature in general use in anthropological circles and in the business world.' For Rowntree, the solution was to try and 'translate what the anthropologist has got to say into language which the businessmen will be able to understand'.

As further appeals and fund-raising dinners were planned, and the network of sympathetic 'prominent industrialists' reached almost 100, the Friends increasingly sought to define the relationship in their own way. Yet another promotional document by Robert Hyde provided practical examples of the use of applied anthropology. He cited a 'pioneer study under the direction of Elton Mayo in a Western Electrical plant near Chicago' that 'demonstrated most forcibly that practical measures for improvements with regard to such matters as output, absenteeism, the understanding and acceptance of new instructions, depended in large measure on their adjustment to social necessities, both within the working unit and in the wider community from which managers and workers are drawn'. In his lecture to the RAI on the 'Application of Anthropological Knowledge to Our Industrial Society', Hyde pointed to the potential for anthropology to 'detect weaknesses in the industrial system which encourage discord rather than promoting harmony' (Hyde 1955). He went on to argue that awareness of the discipline's utility was currently limited to America. Citing with approval the foundation of the US Society for Applied Anthropology in 1941, he lay down a challenge to anthropological practice in the UK.

Rowntree also repeated his concern to 'bridge the gap' between the academic and industrialist view in a further letter to Sieff in April 1954, after another 'very excellent dinner and representative list of guests'. This time, he was not just concerned with anthropologists' use of language, but also about the need for a 'really practicable proposition as opposed to generalities'. He also felt that it needed to be shown 'why the contribution of the anthropologist can be useful and in what way his training and experience differs from that of the psychologist'. Ominously he went further still, suggesting that 'the biggest stumbling block to getting ready acceptance of the ideas which are so important is the use of the word "anthropology"', feeling that it was 'a word which immediately conjures up into the mind a detailed

study of foreign and primitive races, with particular reference to the study of physical attributes and trivial habits'. 'I believe', he ended, 'that when the time comes to collect money and to give publicity to the activities it will not be found that this can be done successfully for "Anthropology" or "anthropologists".'

Peter Rowntree and Robert Hyde were not the only Friends who felt that the onus increasingly lay on anthropologists to demonstrate their interest in this potential new research field. Minutes of a Friends meeting in June 1954 record one Sir Frank Shires pointing out 'that it was not only the industrialists who must be convinced of the value of Anthropology, but also the anthropologists who must be shown that one of the most fruitful fields of study lay in industrial organisations. A research project, supported by the fund, might achieve both these aims.' The Friends suggested lobbying the new Department for Scientific and Industrial Research (DSIR) to include anthropological studies. Speaking at a liaison meeting about the prospects for DSIR support, its representative felt that 'there were already a certain number of studies that might be considered as anthropological in nature.'

The DSIR representative may have been referring to the work that Max Gluckman had initiated at Manchester. In the early 1950s, the Harvard sociologist George Homans – having just published his influential *The Human Group* (1951) – spent a year as a Visiting Professor at Manchester. Influenced by his work on the social norms created within a group of workers, Gluckman's students studied shop-floor relations in a number of different factories over a period of four years (e.g. Lupton 1963; Cunnison 1966.). The work became known as the Manchester 'shop-floor ethnographies' (Emmett and Morgan 1982, Cunnison 1982). Thomas Lupton went on to shape the emerging field of organisational behaviour. His work had minimal influence within anthropology, possibly because it drew more on social psychology than on sociology.

Why were these new ethnographies not discussed within the RAI at the time? One reason may have been Max Gluckman's increasing estrangement from Leach and Evans-Pritchard. Another was that this work, like the other community studies initiated by Gluckman's students – such as *Coal is Our Life* (Dennis et al. 1956) and *The Village on the Border* (Frankenberg 1957) – had not yet been published. As a result there were few monographs on industrial issues, and the first report of the joint DSIR/Medical Research Council committee on human relations in industry is silent on anthropological contributions. There was another angle too. When the Friends criticised the lack of anthropological involvement in this joint committee, given the obvious shortage of trained investigators, Firth bluntly pointed out that the

DSIR projects specifically stated that the work must increase productivity. Firth was adamant that anthropology was not in a position to agree to such demands.

With Friends like these ...

Dissent within the liaison committee grew during 1954. Despite Sieff's success at gaining donations from Friends, there was no consensus about how the funds should be used, particularly in relation to 'commissioning research into industrial problems'. Once again, anthropology was put on the defensive. At a meeting in July, Sieff felt that there was still need for practical evidence of anthropology's potential contribution, whilst others asked why funds raised for sending anthropologists overseas were never used to send them into industry, and what anthropology offered that psychology had not already offered. Fortes sought to defend the discipline as a 'young science', asking for a five-year experimental period to develop 'a new side to the old tradition of devoted work with a high purpose', whilst Firth felt that the anthropological method of 'going to a society as a member and living like its people might be difficult to put into practice in industry ... by going into an organisation within his own community a man would most likely be "taped" in accordance with the class from which he comes, and so cut off from his fellow workers'. For Firth, anthropology needed a chance to 'experiment' in industry, and not to be judged on a narrow burden of proof.

Gradually Sieff realised the limitations of the task he had set himself, in the face of a ponderous and slow-moving RAI, and the striking distance between the utilitarian concerns of his fellow business leaders and the scientific values espoused by the anthropologists. Raymond Firth sought to mollify Sieff's growing frustration at how little was being achieved, noting after one meeting that 'it was rather a wearing occasion, but I think it was worth it ... You sounded a little disappointed at the end. It seemed to me however that this was perhaps as much as we could have hoped for in realistic terms. ... Wilson, our treasurer, told me that one of the Scottish distillers has promised £500 under covenant.' Firth went on to note that another industrialist had 'started our conversation by being entirely sceptical, but in the end admitted that the anthropological case did make some sense ... he might be willing to put our case to the English Electricity Board'¹⁵ This was not the progress Sieff had hoped for.

By 1955 the liaison committee was replaced by yet another new RAI 'Committee on Anthropology in Industry' that aimed to plan a programme of research. Yet the last thing that business

representatives wanted was to be dominated by academic research concerns or a lumbering RAI bureaucracy. Barnes wrote to Raymond Firth in May 1955 to record his 'slight progress' with the Friends, after he had drawn up a new draft letter of appeal. As he recalls, 'Castle liked the draft, Rowntree thought it was hopeless and should be completely rewritten. I heard nothing from Sieff. Then about ten days ago, Sieff apparently called a meeting to discuss a draft which Miss Bradney [an independent researcher who later carried out an ethnography of Selfridges] had drawn up ... It is not the sort of document that I would have written, but there is nothing outrageous in it.'⁶

One of the Friends' last initiatives was to propose a semi-autonomous 'Industrial Relations Research Group' in order 'to avoid involvement in the internal business of the Institute' and to 'make a greater appeal to industry'. Their letter of appeal to employers used the blunt language of self-interest:

As a modern employer of labour, you will know that full employment, high wages and the provision of first-class amenities and working conditions have not been the complete answer to restlessness, dissatisfaction, high labour turnover or poor standard of work. Within your own factory you may have had these difficulties to face, and the incidence of unofficial strikes is sufficient indication that these are deep-seated problems. Our purpose in writing to you is to enlist your support for a method of scientific investigation and enquiry which, in our view, can help employers to find out root causes for labour stresses and strains within their organisation, not explicable by the ordinary criteria of wage levels and conditions of work which have been principal yardsticks hitherto.

The letter went on to extol the resources of the RAI, 'which is the recognised and long-established society for promoting the study of man himself in tribes, social groups and modern industry'. Such a straightforward promise of anthropology's benefits was not received well within the RAI, and there was strong disapproval of this latest proposal. At one committee meeting the President himself felt obliged to reiterate 'that all anthropologists depend upon the Institute, and that the main purpose of the appeal must continue to be the strengthening of the general purposes of the Institute'. This was unanimously agreed, but left Sieff, Rowntree, Castle and the others more isolated.

Caught in the middle, the RAI found itself unable to manage the growing rift between sceptical academics and impatient business folk. When Fagg circulated a draft Annual General Report of the RAI declaring that the creation of the committee on Anthropology in Industry was 'an earnest indicator of your Council's intention to

encourage by all means in its power the anthropological study of British industrial communities', many senior anthropologists reacted with disquiet. Meyer Fortes was of the view that 'some reference to the traditional interests of the Institute should go into every report', whilst Forde felt that it 'may give the wrong impression' and 'hamper the collaboration in prospect'. John Barnes felt it important to tone down such a statement to one simply 'encouraging anthropologists to take an interest in the study of contemporary industrial society' lest it 'raise false hopes'. He added wryly that 'it is not lack of funds that has prevented sufficient attention being paid to this in the past, it is merely that most people, quite rightly, think that the Bongo Bongo are more attractive than Mancunians'. The mood hardened. When Barnes wrote to Raglan at the end of 1955 asking if the committee on Anthropology in Industry was likely to play an active role in future, the response was frank: 'as long as we have any hope of money from Marks and Spencer or any other industrial concern we must keep the Committee for Anthropology in Industry, otherwise they cannot legally give us any money. Whether we can still hope for such money is quite another matter.'⁷ The romance had ended.

At this point Sieff lost patience, and turned his energies to other projects. There were to be no further soirées at his Kensington flat or dinners at Claridges. Writing to Lord Raglan in January 1956 he apologised for having not been able to continue with his work of strengthening the Friends, and noted instead that 'the situation with regard to the economic problems of the State of Israel has compelled me to devote practically the whole of my leisure time to the work of the various Zionist organisations and funds for the Jewish Agency and world Jewish congress here and abroad'. He had become Chaim Weizmann's personal secretary after Weizmann stepped down as the first president of Israel. Sieff was spending a great deal of time in Israel, and was also still actively involved with PEP.

Sieff attended one final RAI reception in June 1956 with eighteen other industrialists and Firth and Forde to celebrate the completion of an ethnographic study of Selfridges. The occasion was used by Firth to reiterate the singularity of the anthropological approach. Unlike industrial psychology, he insisted, it was not based on the interviewing of people, 'but preferred to be among them as they worked, seeing how they behaved, and from that, building up its patterns'. For Firth, it was an 'observational science' that 'functioned on a long-range planning basis; it did not promise quick solutions of industrial problems but sought rather an understanding of what led to the existence of such problems'. Welcoming the report, Sieff was frank in his assessment. He agreed that it 'would have been quite impossible for a sociologist or an industrial psychologist to have made a similar study', and that 'social

anthropology could go further afield in this country'. However, Sieff made clear that social anthropology 'should be used by industry, not in place of, but in addition to, industrial psychology as both skills had their different contributions to make'. Sieff was a better judge of this than Firth, as he had continued to be highly active in PEP, publishing influential policy reports on industrial relations, the press, and the film industry. Deeply frustrated by his dealings with the RAI, Sieff resigned from his role as Vice-President of the RAI in 1957, ending a thirty-year association with the Institute.

Without Sieff's energetic leadership, the Committee on Anthropology and Industry fell apart. Marion Wesley Smith, an accomplished American anthropologist who moved to London in the 1950s, was appointed RAI Secretary in 1955, and tried hard to reinvigorate the committee. As well as writing to Sieff to persuade him to reconsider his resignation, she used her contacts to invite Dr Margaret Mead to come and talk to a meeting of the Friends. Mead was advertised in the circular letters as being 'in close touch with recent developments in anthropology in America which have proved useful to business men both in management and in their overseas contacts', and she talked on the importance of cross-cultural awareness within business. Whilst the Report of the RAI Council in 1956 again emphasised the number of lectures and publications now focusing on the anthropology of Britain, demonstrating the 'ever enlarging scope and significance of anthropology', the moment for collaboration had passed. Marion Smith attempted to follow up links with DSIR. Meyer sent her a letter with a note about two BP directors who might be interested in a closer relationship with the RAI – though noting that Sieff had 'had a crack' at ICI, with little success. Further attempts to organise events came to nothing. After Smith's untimely death in 1961, her successor Anthony Christie put his energies elsewhere.

If little came of the original ambitions of the Friends, a good deal of money was raised for the RAI. An annual covenant of £1,000 from Marks and Spencer for seven years helped the Institute through a difficult financial period. Through his contacts and support, Sieff also secured the extensive library of the explorer and Orientalist Sir Richard Burton for the Institute. Only one company is ever recorded as having approached the RAI for advice over industrial welfare issues. Booker Bros McConnell and Co. Ltd approached Firth to see if research could be carried out into the 'lack of communication between management and workers and other difficulties, despite liberal loan provision for workers to buy their houses' and 'the general problem of skilled labour'. Firth characteristically replied that such research 'would be of a fundamental kind without necessarily yielding any results which could be analysed by industry', and suggested a five-year

time span for such research. They also discussed a potential anthropological study of the labour situation in Ankole, Uganda, where a large sugar estate was planned. One Mr Caine from the company attended a couple of RAI seminars, and Firth lent him Oscar Lewis's book *Group Dynamics in a North Indian village*, which he returned, with a polite note saying that it had provided 'another insight into the methods and purposes of anthropological research'. No further communication is recorded.

The story of the Friends is marked by the repeated misrepresentations of anthropologists and their willingness to study contemporary industrial issues, by both the RAI and industrialists. Given the discipline's history, fields of expertise, and sense of moral purpose, this should not be surprising. Little ultimately came of Sieff's efforts, something of which he was perhaps aware. In his own memoirs, Sieff devotes twelve pages to his involvement with PEP, noting that it was one of the things in his life he was most proud of (Sieff 1970). He makes no mention of anthropology or the RAI.

Conclusion

This was the first time that social anthropologists had turned down financial patronage, a mark of their newly won autonomy. In the 1940s anthropology had depended on the funding and prestige that Colonial Office patronage offered. By the 1950s there were a growing number of academic posts and research fellowships available. The discipline had no need to sell itself to the highest bidder. Despite internal differences, and some persuasive arm-twisting by industrialists, influential figures within the RAI remained sceptical. This was partly because Sieff's initiative threatened the independence also nurtured within the Association of Social Anthropologists. The association's members had only just been able to distance themselves from the RAI, and social anthropologists had no wish to see the RAI represent or dictate the shape of academic research. Yet the creation of a second scholarly association made it hard for the discipline to speak with a single voice. The cases demonstrate the misunderstandings and different styles of organisational working that exist not only between scholars and practitioners, but even within a single discipline.

This is not quite the end of the story. In the USA, the field of applied anthropology grew and prospered. Back in Britain, some of Max Gluckman's students, who had conducted ethnographies in a variety of different industrial settings in the 1950s, completed their doctorates and left anthropology. Tom Lupton went on from studying social relations in a steelworks to an influential career within management

studies (e.g. Lupton 1963). He and others, like the sociologist Tom Burns, wrote ethnographically-informed studies of business practice and innovation (Burns and Stalker 1971). Sociologists ruled where anthropologists feared to roam.

This situation finally changed in the 1980s, as the wheel of disciplinary evolution turned full circle. A growing number of anthropologists, unable to find academic posts, came together in a group inspired by Paul Stirling, entitled the Group for Anthropology in Policy and Practice. Holding regular meetings, they sought to shape the policies of the ASA and to promote the discipline's contributions to different fields of social policy (e.g. Stirling 1983). In the 1990s, an influx of career professionals seeking to study master's degrees in development anthropology and medical anthropology continued this process. The application of anthropology has once again become a recognised disciplinary field, and a subject of intellectual debate in its own right. With the disciplinary field no longer in thrall to a narrow theoretical agenda, these departures served both to revitalise older debates and to launch new sub-disciplinary fields (Grillo 1994, Pink 2006, Wright 2006).

Notes

1. This and all subsequent references draw on A57 'Friends of the RAI', RAI's House Archives, unless otherwise noted. Grateful acknowledgement is made to Sarah Walpole, RAI archivist, for her help with this work.
2. A57.
3. A57.
4. RAI Archives A95/31/11.
5. Raymond Firth's personal correspondence, in Firth A9, LSE archives.
6. Ibid.
7. A95/39/13.

ANTHROPOLOGISTS AND 'RACE': SOCIAL RESEARCH IN POST- COLONIAL BRITAIN

Introduction

The United Kingdom of the 1940s and 1950s was shamed by a sudden and vociferous explosion of racial prejudice. The first wave of post-war Commonwealth migration provoked a widespread, if informal, 'colour bar' within many parts of society. Yet social anthropology had little intellectual interest in engaging with this post-colonial racial politics.

Kenneth Little, one of Raymond Firth's students, was an iconoclastic exception. He began his career as a physical anthropologist, and then carried out innovative ethnographic research on the black community in Cardiff. Recruited to launch a new department at the University of Edinburgh, the work he championed was at the forefront of research and public debate into 'race relations' in the 1950s. Despite the important parallels between this work and that being carried out by some of Radcliffe-Brown's students in the USA, the department established no real theoretical research 'school'. The work remained marginal to disciplinary concerns, and was largely ignored by government policymakers.

In order to understand some of the challenges Little faced, one needs to understand both the legacy of Victorian racial science and the threat posed by Nazi Germany in the 1930s. Describing his achievements in setting up the Edinburgh department, I document the

challenges presented by the politicised nature of 'race-relations' research, and explore the institutional, as well as intellectual, reasons for social anthropology's disengagement.

Race and its Victorian legacy

For British anthropologists of the 1930s, 'race' was not a new topic, but one with an embarrassingly tarnished past and a highly charged present. The history of Victorian anthropology's entangled understandings of race and culture still hung over the discipline (Stocking 1968, 1987). Establishment figures like the biologist and RAI president Sir Arthur Keith and the soldier-collector Captain Pitt-Rivers used the RAI journals to expound what Pitt-Rivers called his 'ethnogenic' theory of the race-culture complex. The political appropriation of such 'scientific' debates, in the light of the history of the eugenics movement and Nazi pronouncements about the superiority of the Aryan race, made scientific clarification of the issues increasingly important.

In 1934, the first International Congress of the Anthropological and Ethnological Sciences (ICAES) was held in London. For the organiser Professor John Linton Myres, a prominent Oxford classicist, archaeologist and influential supporter of social anthropology, the aim was 'free and friendly discussion, between men of good will from all nations, of the numerous questions as to Race and Culture which are continually arising and giving occasion for serious trouble, if they are not handled as scientific problems' (Myres 1934, 81). In the same year, the RAI itself convened a committee with the Institute of Sociology that aimed to 'consider the significance of the racial factor in cultural development' (RAI 1935, 2). It explicitly saw itself as a scholarly response to political claims 'for the intellectual pre-eminence of Germans'. The views of the committee, which consisted primarily of biological anthropologists like Sir Arthur Keith, juxtaposed with social anthropologists like Raymond Firth, diverged. A loose consensus emerged on what people actually meant by the word 'race', and a woolly definition was agreed, but substantial disagreement emerged over 'how far particular races and populations are actually linked with particular cultures or culture elements' (ibid., 4). Only Firth grasped the nub of the problem, that the 'wide gap between the definition of race acceptable to a biologist or anthropologist and the current idea of "race" in political discussion should be made clear' (ibid., 19). For Firth, 'the interest of the concept of race as a 'sociological phenomenon' was the force given to its institutional expression, and the fact that these 'institutions embody discriminatory treatment of individuals sharing the same geographical environment'.

The issue remained live. Later that year, Cambridge anthropologist A.C. Haddon joined two prominent public intellectuals – the biologist Julian Huxley and demographer Alex Carr-Saunders – to publish *We Europeans: A Survey of 'Racial' Problems* (Huxley and Haddon 1935). It was a direct response to growing Nazi propaganda and the threat of 'racial science'. Abhorring the misuse of the R-word, the writers suggested its replacement with 'ethnic type'. The work concluded that 'racialism is a myth, and a dangerous myth at that'. Such proposals were later supported by the campaigning physical anthropologist Ashley Montagu (Montagu 1942, Lieberman 1994).

Given the ambivalence about the very utility of a term like 'race', and the destructive ways in which it had been used, it was no wonder that there was what Stepan later called an 'anti-race' reversal within anthropology and the sciences (Stepan 1982). Social anthropologists no longer saw any analytical use for a term like 'race'. Racism became a similarly taboo subject.

Their fears were well founded. In 1936, the RAI became concerned about the activities of the Scottish Anthropological and Folklore Society, and in particular the activities of its editor Robert Gayre of Gayre, self-styled laird and Scottish nationalist. The Edinburgh-based society had just announced the creation of an 'Institute of Anthropology' for teaching and research in collaboration with the Free Church College of Edinburgh. It also invited the RAI Council to send a representative to a proposed international congress at Edinburgh to further a 'scheme of co-operation in Northern European folk-lore and ethnology'. Gayre was to be the congress's British representative. After the *Edinburgh Evening News* exposed its links with Germany, the RAI council feared that the whole Congress was funded by the Nazi government. Charles Seligman, about to retire from the LSE, travelled to Sweden to meet Professor Herman Geijer, one of the organisers of the proposed conference. Geijer admitted that the German government was funding the new congress, but was sure that 'many Germans were quite independent of political influence and could be trusted to work scientifically'. Another representative denied that the society, with its roots in Swedish folk-lore research, had any interests in 'racial exclusiveness'. Unconvinced, the RAI withdrew its representative, and the Congress was never held. Robert Gayre continued to expound his racist views for many years in a journal called *Mankind Quarterly*.¹

Edinburgh and the new politics of race, 1940s–1950s

Having led the retreat from the racialising logic of Victorian science, anthropologists in the 1930s repeatedly insisted that ‘race’ was simply a biological, and not a social category. Thus reclassified, it could not be a topic of social anthropological research. The discipline’s purpose, they might have added, given its history, was to engage with the broader comparative questions thrown up by in-depth ethnographic study of ‘simple’ societies, and not to respond to the latest political and social ‘problems’ facing a complex metropolitan society. It was a fragile defence – the rationale contradicted exactly the promises that Malinowski had made as he gained funds for the practical application of anthropology from the Rockefeller Foundation.

A young anthropologist called Kenneth Little was determined to challenge this consensus. Little studied at the LSE under Raymond Firth, completing his PhD in 1944. For its time, his was a progressive and highly unusual doctorate. It had begun as a project of physical anthropology, as he had taken over some of the teaching work of the physical anthropologist J.S. Trevor at Cambridge on the outbreak of the war. In 1940, on the back of his results in the Cambridge Tripos, this had involved him travelling to the black community of Tiger Bay in Cardiff to study the physical characteristics of what he called the ‘Anglo-Negroid’ cross, publishing his first article in *Eugenics Review* (Little 1942). As Little developed close relationships with Cardiff residents – Banton refers admiringly to his ‘personal chemistry’ – he became aware of the impact of the ‘colour bar’ on people’s lives (Little 1943). He later admitted that his immediate experience in Cardiff ‘was a slice of the reality about which my African friends in Cambridge had told me, and so I decided to pursue my research in a sociological form’ (Little 1972, vii). Politically principled, and feeling that there was very little academic interest in race relations, which was seen simply as ‘the colour problem at the port’, he embarked upon what became a wide-ranging piece of scholarship into the issue.

Within the UK, racial prejudice was seen as a pressing concern during the war. Kenneth Little was virtually the only anthropologist willing to address these issues. He wrote an unsigned briefing paper entitled ‘Memorandum on the “Treatment” of Colour Prejudice in Britain’ for the Colonial Office. Strongly worded, and based on his research in Cardiff, he insisted that the ‘implications of inferiority, mentality, unintelligence etc which are attributed to coloured people and more particularly the Negro ... are passed on to a greater or lesser extent by every cultural medium of our society’. In it he talks of the problem of ‘social psychology’, and the need to alter ‘the nature of the

stereotype of the Coloured Man', as 'popular reactions towards a coloured person are reactions towards the "stereotype"'. Aware of the media's powerful role in shaping social prejudices, he felt the solution lay in 'correcting popular ideas', and in exercising 'a far stricter censorship over absurd and undignified representations of the Coloured man and of scenes of so-called "Native life".' Some of his advice now seems rather quaint. As well as suggesting that newspapers no longer used the term 'native', he went on to further recommend that 'Missionary societies tone down the implications of inferiority of any form whatsoever attributed to "native" peoples in their appeals to the public for financial assistance (Booklets should not be entitled "My friends the cannibals"!).' ² Little also makes recommendations as to legal means to tackle racial discrimination, urging that it be treated as interference with the national war effort. It was his strongest statement on the topic of racial discrimination, and demonstrated his perceptive understanding of the risks of well-meaning condescension, and the media's role in legitimating these attitudes.

Next he wrote an educational pamphlet entitled *Behind the Colour Bar* (Little 1950) for school students. In it he describes a number of different national histories (e.g. South Africa, the USA, Brazil) in order to demonstrate the variety of racial relationships. He begins by insisting that the expression 'colour problem' is 'invidious' as it could equally well be described as a 'white' problem (ibid., 3). He goes on to challenge the fallacy of biological explanations and concludes that 'fundamental to the present problem is the enormous difference in relative prosperity between Western peoples and the rest of the world' (ibid., 17). No other anthropologist at this time was so involved in public debate and education on race and racism. He did much of this from his position as an LSE lecturer, and was appointed to a Readership in race relations in 1950, just before moving to Edinburgh.

Kenneth Little's thesis was unusual, as it deftly combined history, demography, literary analysis and sociology and a wide range of archival and documentary sources to understand the experience of the Cardiff coloured community and the larger history of the black presence and racial attitudes in Britain. Having Raymond Firth as his supervisor gave Little's work credibility, and the thesis was published as *Negroes in Britain* in 1948. The book was groundbreaking for its recognition that popular racial 'attitudes' had long outlasted an academic dismissal of Victorian racial science. Little uses the term in two very different senses, both to refer to the 'biological categories' of race and in discussing the history of social stereotyping. *Negroes in Britain* (Little 1948) opens with Little's frank admission that his interest in the topic stemmed from his 'sympathy for the victims of this prejudice'. In the preface to his book, he also saw his aim being both 'to

draw the attention of anthropologists in this country to the possibilities of applying their discipline more positively to the study of urban society', and to argue for 'scientific attention to be given to the study of racial relations in its own right' (ibid., xi).

Little felt that it was vital that a study of the 'social structure and sociology' of the Cardiff community be understood 'as part of the surrounding matrix of the larger British society'. Yet the exact shape of this relationship is not elucidated, and the book makes little real analysis of the social structure of this 'larger matrix', even within Cardiff itself. Despite his ambitions, Little ends up studying a bounded social setting. The book's academic reception was also hampered by its lack of engagement with British anthropological scholarship, despite his interest in social structure. His theoretical conceptualisation of discrimination was problematic, suggesting that such attitudes 'do not become markedly antipathetic or antagonistic unless some form of actual social or physical contact with a coloured person seems likely to take place' (ibid., 245). This ambivalent depiction was later adopted by Banton, but was heavily critiqued by American sociologists for its analytical looseness.

Racial tensions continued to mount after the war. The arrival of SS *Empire Windrush* in June 1948 took the debate about black–white relations in England into a new realm. A telling survey into public knowledge about the colonies (Evens 1948) conducted at the same time found that only half of those interviewed could actually name a colony. A confidential government report published three years later brusquely concluded that 'antipathy to coloured people in this country is probably considerable amongst at least one third of the population', and that this 'antipathy is unrelated to actual personal contact with coloured people'.³ The results were never made public, to avoid attracting adverse criticism in the colonial press. The report ended by recommending an immediate programme of action to reduce such 'racial' antipathy. No action was ever taken.

In 1950 Kenneth Little was appointed as Reader in Edinburgh, a post to which he had been 'warmly invited' to apply by Ralph Piddington, its previous incumbent (Little 1987).⁴ It was a timely moment to promote his brand of social anthropology and a programme of research into race relations. A £40,000 UGC grant to Edinburgh University funded several anthropology research posts within a newly established interdisciplinary Social Sciences Research Unit.⁵

Piddington's support for Little was no surprise. Social anthropology at Edinburgh began four years earlier (if one excludes the opportunism of Robert Gayre) with the former's appointment to a Readership within the Department of Mental Philosophy. An exotic figure in the

staid world of post-war Edinburgh, he was awarded an LSE Ph.D. in 1932 for work on the Australian Aboriginal community, under Radcliffe-Brown. Yet Piddington too had a history of engaged scholarship and speaking out about racial indiscrimination, incurring the wrath of the Australian funding council by doing an interview with a Sydney newspaper discussing examples of gross racial discrimination, and claiming that this 'absolute indifference to the sufferings of the native' was typical of state affairs in Western Australia (quoted in Gray 1994, 219). As a result of this challenge to the cosy relationship between academics and administrators, Piddington was censured and never again worked in an Australian university.

Little's first years in charge went very well. Little was successful in obtaining grants from the Noel Buxton Trust and the Nuffield Foundation to do research on race relations, having argued that an anthropological technique was particularly 'well adapted to the study of the enclaves of coloured people in this country' (Nuffield Foundation 1953, 42). The grants enabled him to employ a team of young researchers to tackle the topic of race relations, particularly amongst students. These included Philip Garigue and Alex Carey to explore attitudes towards colonial students in London and elsewhere (the latter was a Ph.D. student in the department), Eyo Bassey N'dem to study voluntary associations amongst students in Manchester (N'dem 1953, 1957), Sheila Webster to look at the experience of black students at Oxford and Cambridge (Webster n.d.), and Violaine Junod to look at 'coloured' elites in London. Subsequently, a grant was awarded to Sheila Patterson to carry out research into the West Indian community in Brixton. He also obtained other grants, including one from the Buxton Trust, with which he employed Michael Banton as a research student to do a study of the 'Coloured Quarter' of Stepney. By this point Sydney Collins, an assistant since 1948 in Edinburgh, had also begun research on two different communities on Tyneside, one 'Moslem' and one 'Negro', and their differing relationships with the 'host' society, supported by a grant from the Carnegie Trust.

So who was Kenneth Little, and what kind of research environment did he create in Edinburgh? Whilst politically committed, he had little of the energetic, overbearing charisma of Gluckman or Malinowski. Edinburgh also faced disparaging comparisons with LSE and Oxford from newly arrived staff. Not everyone was negative. Mary Noble's memory of Little, for example, was of someone 'who managed to get across the subject and enthuse about it ... he didn't need to have a school around him – but people were spellbound by him, and he was great at communicating the importance of the discipline'. Little suffered mood swings, both from undiagnosed cerebral malaria and

from recurrent depression. Whilst the university gave significant autonomy to its professoriate, Little's abrasive personality discouraged some from working with him. Despite this, he established a significant research programme into urban change in West Africa (involving Michael Banton, Jimmy Littlejohn and other anthropologists) during the 1950s, a field that was more professionally recognised than that of race relations (Little 1960, 1969).

Little's most developed theoretical position was his notion of the 'colour-class consciousness' (Little 1948). As he notes, "'colour" has the same socially inferior connotation as English spoken ungrammatically, or without the "correct accent" or of wearing a muffler instead of a collar and tie' (ibid., 203). His subsequent work did not really develop these ideas. Perhaps the most prominent commission Little received, a recognition of his position in the field at that time, was to write a UNESCO-sponsored textbook, called *Race and Society* (Little 1952). He did negligible further anthropological writing on 'race' in Britain after the 1950s, partly because of his developing research interests in West Africa and his ethnographic research on the Mende (e.g. Little 1965), though he did remain 'passionately committed' to the topic, and was involved in government race relations committees until the 1970s. As Noble wrote in her obituary, 'This was the start of Kenneth Little's love affair with Africa. There he saw the pace of change and felt that this was a matter that could not be ignored by social anthropologists' (Noble 1991, 12).

Of the research into 'race relations' that Little initiated, a common theme was the attention to the cross-cutting hierarchies of colour and class. Several studies stand out as being both theoretically innovative and politically sensitive. One, entitled 'Negroes in bluebrick' – a study of racial attitudes amongst Oxford students – was carried out by Sheila Kitzinger (née Webster), now a well-known campaigner for 'natural' childbirth. Conducted as part of a postgraduate degree, it involved more than 200 interviews and case studies. If studying modern society was seen as questionable within anthropology, then studying colour prejudice at Oxford University was even more marginal, and the work was never published (Webster n.d.).

Her closest colleague whilst at Edinburgh was Sydney Collins. She found it hard to talk about the issue of racial prejudice with her other Edinburgh colleagues, who, despite their research interests, would 'pretend that if prejudice existed, it was something very distant from the lives they lived'. Collins's work is important for its careful comparative analysis of race relations between different areas and communities, for its demonstration of the heterogeneity of the coloured communities, and for insisting that 'studying the immigrant community by itself is an inadequate approach to the problem'

(Collins 1957, 15). Neither Kitzinger (1960) nor Collins adopted a simplistic model of 'assimilation' or 'integration' that was prevalent at the time. Whilst the texts are of their time, they do not engage in the 'cultural pathologising' that Alexander (1996) suggests is characteristic of such studies. Both make extensive use of empirical ethnographic research, with Kitzinger quoting lengthy conversations to explore the racial prejudices of the students she interviews.

The one member of the Nuffield research team who went on to an influential academic career was Michael Banton, recruited as a Ph.D. student by Little from LSE. He did his Ph.D. research in Stepney, in an area he described as having 'no coloured ghetto but a depressed working-class neighbourhood sheltering residents of many races and nationalities' (Banton 1954, 13), and published it with the title *The Coloured Quarter* (ibid.). Like Little, Banton provides an empirical analysis of the area in a larger political and historical context, at the expense of a detailed ethnography or contribution to extant anthropological theoretical debates. It explores recent migration and settlement patterns, and issues such as employment and intermarriage, and makes a number of policy conclusions, challenging any dispersal policy, insisting that assimilation was a two-way process. Max Gluckman reviewed *The Coloured Quarter* approvingly, and commented that 'I wish, after he had finished this study, Dr Banton had moved to investigate the receiving British community ... it is to this point surely, that students of Negro immigrants must now shift their attention ... the main problem, both for anthropology and for social workers, lies on the other side of the colour line' (Gluckman 1956, 164).

A final recruit to the Edinburgh-based Nuffield research project in the 1950s was Sheila Patterson, who did research in Brixton, and went on to edit the journal *New Community* and play an active role in the IRR (Institute of Race Relations) and the field of 'race relations' policy, but her work (e.g. Patterson 1964, 1969) was heavily criticised by anthropologists, and provided other critics with a reason for dismissing anthropology *in toto*. Alexander (1996) rightly suggests that Patterson's (1964) portrayal of a Brixton community 'racked by 'cultural doubt, dislocation and inauthenticity' set a precedent in its ascription of 'cultural and social pathologies' to black communities that became characteristic of many subsequent studies, especially those sponsored by the IRR.

Little made no real attempt to synthesise and develop the evidence and ideas around 'race relations' that his team of researchers – many of whom were still postgraduates – had developed. Banton (1983, 558) notes that 'we all met together only once, for two days in 1952', such that it 'was difficult for us to develop any common scheme of

interpretation'. By comparison Gluckman maintained the RLI camaraderie for years by keeping up a voluminous correspondence with research colleagues such as John Barnes and Clyde Mitchell. In retrospect Little tried to suggest that the department had followed a 'parallel course' to that of Max Gluckman (Little 1987, 4), but its impact on the discipline is far less obvious. Edinburgh became notorious for not advancing its students, and according to Noble 'we weren't expected to write papers etc. I never had any notion of working as part of a team – the Ph.D. was just something one did.' Another factor may have been disparaging comments about what was 'unkindly called the "Negroes-in-Britain industry"' (Banton 1973). The research 'team' lost its momentum. Banton recalls that whilst Little 'wrote and published 'about matters that were considered politically important', he never had the 'sense of his trying to create a school in the way that Gluckman created a school. There's no comparison. He wasn't even trying to do that.'⁶

Later in life, Little expressed the view that Edinburgh's strength had been to offer 'something additional' to avoid being 'subordinated' to the 'well-established centres in England' with their 'anthropological elite' (Little 1987). The title of his inaugural lecture, 'Social Anthropology in Modern Life', neatly summed up his ambition to turn anthropology 'to the study of contemporary developments' (Little 1965).

Engaging with 'race-relations' policies in the 1950s

Ever reluctant to apply their ideas to policymaking, few anthropologists made links between the social problems affecting the colonies and those in the metropole. Into this vacuum came the 1950 proposal from member of the Colonial Social Science Research Council (CSSRC) Harry Hodson, a prominent Conservative intellectual and editor of the *Sunday Times*, for a Commonwealth Institute of Race Relations. Arguing that the Commonwealth was indeed 'at risk', and that current academic work on race relations was being done in a 'piecemeal' fashion, he proposed an independent academic body 'devoted to study and research, and precluded from propounding or propagating policies'.⁷ In public, he phrased it rather more colourfully, linking it to the threat of communism to suggest that 'this was one of the most dangerous problems of the contemporary world', which without a solution would see 'our civilisation in sharp and increasing peril' (Hodson, 1950). He was convinced that 'the subject could be isolated from others for scientific study'.

The Council's cool response reveals the academic distaste for such public pronouncements about 'race relations', with its implicit understanding that 'races' could be distinguished and studied. Carr-Saunders, LSE Director and chair of the Council, initially suggested an alternative university-based institute. But, when the antipathy of the others became clear, Carr-Saunders hardened his position. For him 'the term "race relations" was a very indefinite concept' such that 'anthropologists and other scientists working in this field, would, in his belief, have to do a good deal more work on the origins of the different races before any really scientific study could be undertaken on the question of their relations'.⁸ Turning down the £6,000 annual funding request made by Hodson for such an institute, Carr-Saunders commented that the proposal was 'too vague at the moment and involved too many different disciplines to be possible of fulfilment', and instead proposed supporting a single study if 'some specific aspects of races relations could be put forward'. He was not the only one with reservations. Banton recalls how Little was not 'very supportive, he saw this as his territory, and he was a bit suspicious of these other people muscling in' and 'doubted if they would do what he would regard as quality work'.⁹ Raymond Firth shared this disdain, and, according to Mason (1984, 18), was sceptical about entrusting this work to anyone who wasn't a trained anthropologist. But Hodson was an All Souls Prize Fellow and had allies in powerful places. He managed to set up the Institute under the Royal Institution of International Affairs in London (Chatham House). Its elite establishment connections enabled him to garner together financial donations from more than a dozen Southern African mining companies, including Anglo-American, De Beers and Oppenheimer, to support its work. This was a time of increasing anti-colonial protest and activism, particularly on the Copperbelt in Northern Rhodesia, and it was here that 'relations between the races' (Mullard 1985, 14) were seen to be most at risk. The implicit assumption was that race relations in Britain were not a concern.

This corporate involvement confirmed the suspicions of many scholars about 'race relations' as an academic topic for research. The appointment of Philip Mason, an ex-colonial civil servant, as director of the new Institute for Race Relations (IRR) also ruffled many feathers, especially after the publication of his populist 'Essay on Racial Tension' (1954). Seen as 'not at all academic', it was criticised for its 'superficiality' and 'inadequate knowledge', which 'risked bringing Chatham House into disrepute'.¹⁰ Writing to Firth in 1958 about the IRR, Little again expressed his doubts 'about whether a non-university body can claim adequately to cover the field for purposes of research'.¹¹ Mason recalls defending his vivid writing style to Firth, who responded

'if you want to write like that, you shouldn't be writing about race relations, but about something else, perhaps about love' (Mason, 1984, 40). The dispute over its pragmatic and policy-focused approach led Firth and Lord Hailey to leave the Institute's Board of Studies soon after. The IRR did little for the reputation of the academic 'field' of race relations, but the Institute went on to publish a great deal of (often corporate-sponsored) work on international race relations (Mason 1970; Hanley 1971; Hodson 1976).

Given the weakness of this field, anthropologists in the 1950s were well positioned to speak publicly about issues of race. Yet few did so. For a while, Max Gluckman became anthropology's public intellectual with his newspaper articles and talks on BBC Radio's Third Programme. He already had a public profile, having clashed with Sir Philip Mitchell, ex-governor of Kenya, on the pages of the *Manchester Guardian* newspaper. Challenging primordialist explanations of Mau Mau (Mitchell 1954), he insisted that the 'conditions with which we have colonised Africa, and not pagan Africa, have given birth to Mau Mau' (Gluckman 1954). Yet this hardly made him an outspoken critic of colonial rule, and indeed he was increasingly of the opinion that academic work and political opinion should be kept firmly separate. Several years later, Gluckman did a radio talk and an article for *Race* entitled 'How Foreign are You?' (Gluckman 1959). Asked by the BBC to discuss 'the values which people defend by treating others as aliens', he situated his own sense of 'foreignness' as a South African Jew, before talking about the ways in which everyone becomes a foreigner at times, noting how 'membership of one group makes us foreigners in another', and that whilst watching Manchester United, 'I am at one with the host of other United supporters of different ethnic origin, of different religious persuasion, and of different occupation' (ibid., 19). He was famous for his authoritarian insistence that the whole Manchester anthropology department turn out en masse to support the home team. His conclusion, that 'the enmity of some British to coloured immigrants is peripheral to the main divisions in this country', avoids addressing the way that skin colour was being naturalised as a marker of 'foreignness'. His views that 'foreigners occur whenever there is group life', that 'hostility is an essential part of group existence', and that 'dislike of the foreigner may be realistic in that he is a danger' all risked making ethnocentrism acceptable (ibid., 21).

The anthropologist Maurice Freedman (1960) challenged this view of coloured people 'merely as one kind of stranger'. For him it seemed 'to leave out of account a "racial factor" which often enters into the social perception of coloured people'. By a 'racial factor' he was referring to 'an element which ascribes the social and cultural differences of coloured people to their biological endowment'. In this

way Maurice Freedman, like Little, articulated the importance of studying the social perception of race at a time when this was being dismissed by others as too vague a topic for research. Again, he was an isolated voice.

Indicative of anthropology's public and private ambivalence about tackling a politicised topic like 'race relations' was the ASA's response to the Sharpesville massacre in South Africa in 1960. At the ASA's Annual Business Meeting the leftist anthropologist Clifford Slaughter proposed that the Association should make a public protest about the events. Whilst there was much support for this view, it was concluded that in 'view of the stated scientific objectives of the Association no public protest be made in the name of the association'. Instead the meeting resolved to put together a statement challenging the scientific validity of the racial arguments on which the policy of apartheid was justified. Once drafted, this was sent as a letter to *The Times*. The paper turned it down for publication on the grounds of space. Slaughter subsequently left the ASA and academia.

In the summer of 1958 there were a series of attacks on black communities in Notting Hill and in Nottingham, which were labelled as 'race riots.' They served to increase social consciousness of 'race' as a source of conflict, and led to a significant media interest in the topic. A small number of anthropologists began to follow Little's lead and comment on race-relations issues, including Freedman and, increasingly, Banton.

If the ASA was ambivalent about the study of race as an academic topic, then the RAI was less circumspect, and in 1959 it approached the new Institute of Race Relations with a proposal to hold a joint conference on the development of thinking about race since Darwin. An important set of papers were produced (IRR 1960, Mason 1960), a number of which pointed to the Notting Hill riots to emphasise the importance of analysing racial attitudes. The sociologist Donald McRae's comment that 'the majority of sociologists in this country have thought of this as being a subject both irrelevant and distasteful' probably also captures the view of social anthropologists. In McRae's view, 'it was distasteful because it had associations with the racial theories of the Nazis ... and irrelevant because the debate about the influence of biological differentiae on society had long gone stale, and both sociology and social anthropology had made great strides on the assumption that such differentiae are sociologically altogether unimportant.' Yet he, unlike many, recognised the paradox that 'Race, by explaining too much, too easily, by being susceptible to neither proof nor disproof, explained nothing,' whilst at the same time 'the ideology of race remained as a social fact' (McRae 1960, 76).

A follow-up conference the following year met to consider a list of significant research projects in race relations. Attended by, amongst others, Michael Banton and Audrey Richards, a list of around twenty possible research projects were discussed, of which the first was 'more systematic data on British behaviour towards other distinctive minorities' (IRR 1960). The meeting was the precedent for a number of subsequent joint IRR–RAI conferences on the topic of domestic race relations during the late 1960s. The titles of these conferences – 'The Absorption of Minorities' and 'Incipient Ghettos and the Concentration of Minorities', 'Integration' – reveal the integrationist policy perspective of the Institute. Debates at such conferences were increasingly polarised. In 1969 there was a 'palace coup', in which Institute employees, led by the librarian A. Sivanandan, successfully challenged the Council's insistence on the IRR's original mandate to not 'express an opinion on any aspect of relations between different races' (Mullard 1985, 26). This more radical group of academics fundamentally reshaped the IRR's purpose and mandate towards a very different understanding of racism and the black community in Britain (Mullard 1973). The final conference was entitled simply 'Conflict and Race'. This time the discussion was about structural change, institutional racism, and the 'need for the white specialists to learn from blacks about the needs, problems and goals of the black communities' (Kushnik 1971). The politicisation of the field was acute.

The Edinburgh legacy: Michael Banton and the field of ethnic and racial studies

Kenneth Little left the task of bringing the empirical work of the Edinburgh scholars together and giving it some theoretical substance to Michael Banton. This proved to be the stimulus for Banton's *White and Coloured*, subtitled *The Behaviour of British People towards Coloured Immigrants* (Banton 1959). In this book he draws extensively on the work of Webster (Webster n.d.), Collins and N'dem (e.g. N'dem 1953, 1957), writing chapter-length synopses of their work. In doing so he uses their ethnographic studies to elaborate his influential distinction between discrimination and prejudice, in order to focus solely on discrimination, attempting to compare it with anthropological notions of custom.

But this work also drew on the rather abstract psychological concept of the 'stranger'. During his career he was the recipient of much scholarly criticism, whether for his refusal to use terms like 'racism', for his generalisations about 'White' perceptions (Kirkwood

1985) for his 'underlying bitterness towards the radical camp' (Corlett 1984), or for his attempts to apply rational choice theory to group relations (Pierce 1980; Bagley 1985; Porter 1989). His continuing, and sometimes lonely insistence that the languages of politics and of theory had to be separate weakened his position in theoretical debates.

Despite this, Michael Banton was the most prominent and influential intellectual descendant of the Edinburgh 'school', the first director of a Social Science Research Unit on Race Relations, and in many ways doyen of the field of 'race relations'. He has little time for disciplinary classification, and has never seen himself as an ethnographer, which explains his lack of influence within anthropology.

One puzzle in this story is why Little and his students did not draw on the work of either the Rhodes Livingstone Institute or those writing about 'acculturation' in the colonial context. In many ways, Max Gluckman's work in the field defined anthropology's post-war theoretical agenda. His 'Analysis of a Social Situation' (Gluckman 1958 [1940]) offered an important precedent for anthropological research into 'race relations.' His careful exegesis of the ceremonial opening of a new road bridge in a Zululand reserve offered a useful and innovative theoretical framework for anthropologists wishing to study race relations at this time. Of course it had its problems, such as its conception of conflict as somehow standing for cleavage within a shared moral order. It was also his interpretation of the social forces at play, rather than that of the social actors themselves. His student Clyde Mitchell went on to write about the reinvention of 'tribal' identities in a class-conscious urban environment (1956b, 1960, 1966). Gluckman and Mitchell's work stands out for its engagement with racial politics at a time when many shied away from it. However their methodological and theoretical approach was not taken up in the work carried out under Little's leadership. Banton recalls discovering Gluckman's famous case-study article after reading *The Kalela Dance* (Mitchell 1956b), but he doubted that Kenneth Little or others at Edinburgh knew of it or appreciated its relevance.¹² Up till that point, he recalls that he had read nothing of the work produced by the Rhodes-Livingstone Institute. The neglect was mutual. Gluckman did not encourage his British students to explore issues of 'race' in their own work, and was even wary of students doing their PhDs on 'complex societies'. Not everyone obeyed. Scholars such as Abner Cohen carried out research on youth groups in Israel, and later wrote about Notting Hill carnival.

Anthropological ambivalence about theorising 'race relations' continued into the 1960s. Banton continued to reject any analytical use of the word 'racism', saying that it was a 'sponge' word that could take on all sorts of meanings, used by some as an 'explanandum'

rather than an 'explanans'. He was right to insist on a degree of scholarly level-headedness, but his resistance to talking about race left him ploughing a rather lonely intellectual furrow, as he worked first on his theories of social distance, and later on theories of group competition. Neither gained a significant following within anthropology. This lacuna played into the sceptics' hands. At a BSA race-relations group in 1970, the anthropologist M.G. Smith once again raised the question as to whether a 'clearly articulated analytical framework for the discussion of race relations' actually existed (quoted in Rose 1970). Smith instead drew on an alternative history of theorising coming out of Furnivall's notion of the 'plural society', describing 'a society comprising two or more social orders which live side by side, yet without mingling, in one political unit' (Furnivall, quoted in Morris 1957). For those still uncomfortable with a term like 'race relations', the notion of 'pluralism' offered an alternative theoretical paradigm that avoided addressing perceptions of race. Anthropologists like M.G. Smith developed this work (e.g. Smith and Kuper 1968; Smith 1998), sharing the feeling of Morris and others that the 'use of the criterion of race confuses the analysis' (Morris 1957, 124). The sociologist John Rex later developed an influential critique of these plural society theorists, pointing to examples where 'the dynamics of the society turn upon the involvement of men of differing ethnic backgrounds in the same social institutions, viz. the slave plantation' (Rex 1973, 261).

The work of the two different Birmingham 'schools' increasingly overshadowed Banton's contributions. The sociological study of race led by John Rex, and the work in cultural studies inspired by Stuart Hall's leadership, both of which directly addressed issues of conflict, interracial tension and white domination, replaced the more 'neutral' scholarly focus espoused by Banton.

'Race' from the present

Despite dominating public debates, 'race relations' continued to be analytically taboo for many disciplinary academics in the 1960s. A number of anthropologists, including Banton, Firth and Little, were authors of the SSRC 'race-relations working party' report in the late 1960s, which adopted an urgent tone about tackling this taboo, and which gave rise to a new SSRC Research Unit on Race Relations in 1969. Echoing Little, the report acknowledged that 'serious racial tension' lay ahead, for, if 'first generation immigrants have been bitter about discrimination, second generation immigrants may be more bitter'. Banton, first director of the Unit, saw its task as 'stimulating

research in the various social science disciplines' (Banton n.d.). Yet the politicisation of the field meant that scholars such as Sandra Wallman, Roger Ballard and more recently Prina Werbner, worked more with sociologists than with anthropologists.

There have been a number of reappraisals of this history. Benson (1996, 48) suggests that one of the reasons for the discomfort with the idea of 'race' amongst post-war anthropologists was the unease at the 'absence from the communities of certain key features – an ordered kinship system, religious institutions, collective structures of social control'. This led, she suggests, to Afro-Caribbean ethnic minorities being portrayed as problematic objects of investigation, in comparison with Asian ethnic minorities being 'proper objects for anthropological study'. She suggests that only 'in the late 1970s can we detect the emergence of a specifically anthropological voice', naming the publication of Watson's *Between Two Cultures* (1977) as an example of this new anthropological genre. Werbner (1987, 176) does recognise these early anthropological contributions to the debate, but summarises them as the 'study of immigration'. Both oversimplify and underestimate the contribution made by the Edinburgh scholars.

Of course, there were problems and limitations to this work, methodological, conceptual and theoretical. One of these was the failure to seriously grapple with the larger system of economic relationships of which these dynamics were a part. In 1948, Little commented that the 'colonial problem of yesterday is taking on a fresh shape as the racial and national problem of today', yet his work never really sought to analyse the social, economic and political links between Britain and the ex-colonies. In a proposal to Nuffield Foundation in 1952, Little did suggest that the 'West African research could obtain data on the complex social and economic factors which give rise to emigration and direct it to Britain'.¹³ Exactly this type of 'multi-sited' ethnography has become fashionable half a century later, but it was not a proposal that he followed through. But there are also important positives about all this work: its focus on urbanisation was novel, as was its attention to inter-group relations, and its ability to draw careful distinctions between race as a biological and as a social category.

Have things changed? Despite much debate within the American discipline on the topic (e.g. Moses 1999), difficult questions remain about the best way to study 'race' and racism. Cowlshaw (2000, 101) argues that the 'rejection of the analysis of race as an operative principle in the social world' is a 'deeply embedded characteristic of liberal scholarship that restricts attention to the ubiquitous power of race'. For her, 'while speaking of race may appear to reproduce racial

categories, not speaking about race allows racial differentiation to flourish unchallenged' (ibid.). The conundrum remains.

Conclusion

Why did the work at Edinburgh leave relatively little mark on the discipline? Disciplinary identity politics has to be part of the explanation. With the growth of sociology, disciplinary boundaries grew progressively clearer. The question of whether this work was sufficiently 'anthropological' constantly resurfaced. In 1962 disciplinary divisions were not seen as important, but only a few years later there was a very much clearer division of intellectual labour, leading to the creation of a sociology department at Edinburgh.

Theoretical 'schools' need leaders. Whilst Kenneth Little had an eclectic set of interests, he did not champion a particular approach or seek to reproduce his agendas in his students' work. He was better at getting grants than influencing others through his writing. Indeed, the work of many of Little's colleagues was limited by their methodological catholicism and lack of theoretical development. This was not helped by their lack of engagement with classic debates in social anthropology, such as the literature around acculturation, or the work being published under the auspices of the RLI. The analytical relationship between culture and class that Webster (n.d.) and Collins (1960) began to explore was overtaken by a diverse and sophisticated corpus of sociological work on the articulation of different forms of social and economic relations (e.g. Rex 1981; Miles 1993; Hall 1996 [1980]). Little's burgeoning interest in African social change also took him away from the field of racial and ethnic studies. The lack of ethnography in Little and Banton's work also won them no favours in a discipline that prioritised the importance of detailed empiricism.

There are also institutional issues to consider. The majority of the team were research students, and either never published book-length works or moved on to other research interests. Edinburgh anthropology was also marginal to the discipline as a whole, and a long way from the densely populated seminar rooms of LSE and Oxford. It was also not the most obvious place to expect the field of race relations to flourish, given the city's overwhelmingly white populace. There was also no distinct university department of anthropology until the early 1960s, hindering its sense of unity and coherence. Social anthropology was initially in the Faculty of Arts, part of the Department of Mental Philosophy. Little felt heavily constrained by the institution and its conservative structure.¹⁴

It was not just Edinburgh's loss. In 1960, one prominent commentator, the Oxford Professor of Race Relations argued that the 'important' work of Little and his associates on 'contemporary problems of race and colour' had been largely ignored by administrators. 'It is my conviction', he wrote, 'that if greater heed had been paid by policy-makers and responsible administrators to the research papers, and the implicit and explicit warnings of men [*sic*] like these, we might have been spared some of the troubles of Nottingham and Notting Hill' (Kirkwood 1960, 96).

Kenneth Little remained passionately committed to improving race relations, and continued to serve on Home Office race-relations committees in the 1970s. Yet opportunity's window is unforgiving. The policymakers and administrators were never asked to 'take heed' by anthropologists. A tightening of disciplinary boundaries, and the growth of ethnic and racial studies within sociology led others to take up the theoretical, political and empirical challenges of studying 'race' in Britain.

Notes

1. RAI Archives 109/5.
2. Unpublished memorandum on the 'Treatment of Colour Prejudice in Britain' dated 1942, Kenneth Little archives, University of Edinburgh, Box 1.12.b.
3. 'The public's knowledge of the Colonies and its attitudes to Coloured People'. Confidential draft report by B.M. Osborne. Kenneth Little archives, University of Edinburgh.
4. Gregory Bateson was also a candidate, after Little had suggested he be approached. At interview, Bateson's marriage to Margaret Mead was explained to the University Principal. He is alleged to have responded, 'Well, if he stole another man's wife, we can't appoint him.' In his inaugural lecture, Little pays great tribute to Piddington's enthusiasm and planning both for anthropology and the social sciences at Edinburgh (Little 1965). Much later Little attributes Piddington's success to his 'skill' at 'University politics', and to the fact that his wife was a 'leading light in the Ladies' tea club' (Little 1987).
5. Cmnd. 8875 University Development; Report on the years 1947–52 in Vol. 17, Reports.
6. Interview with Michael Banton, Dyfed, July 2001. Unless otherwise cited, subsequent references are to this interview.
7. CSSRC 53rd meeting, 1950 PRO CO901/9.
8. Chatham House IRR Archive: 3/6n g/3 IRR – General Correspondence 1949–57.
9. Interview with Michael Banton, Dyfed, July 2001.
10. Board of Studies of Race Relations, Lord Hailey's file, Chatham House Archives.
11. Sir Raymond Firth Archive, LSE.
12. Personal communication from Michael Banton, October 2001.
13. Nuffield Foundation, 7th Annual Report 1952.
14. Tony Cohen, personal communication, October 2003.

DISCIPLINE ON THE DEFENSIVE?

Introduction

The 1960s found British social anthropology nursing a post-colonial hangover. The unseemly scramble out of Africa at the end of empire had meant a sudden end of CSSRC funding. The new generation of academic anthropologists could no longer be sure that their students would get research funding, or even research access, and found themselves negotiating increasingly politicised fieldwork environments. Many turned their attention to research questions 'at home'. But this too had its challenges and compromises, such as the hidden agendas of would-be corporate sponsors of applied research, or the challenge of studying racism. Even the discipline's professional associations turned against each other, as they tried to adapt to the expectations placed upon them in this uncertain world.

In this chapter, I show how the demise of funding meant the loss of epistemological security, and doubts over the discipline's constituency, purpose and future. The growing numbers of trained social anthropologists employed outside the discipline added further discordant voices to the debate about social anthropology's changing place in the order of things. I begin with the final years of the CSSRC, and describe anthropologists' efforts to influence two major British higher education commissions – those led by Lord Robbins (Robbins 1963) and Lord Geoffrey Heyworth (Heyworth 1965). In each case 'Britishness' was again an issue, but this time anthropologists had to make the case for funding international research, in an atmosphere of increasing attention to pressing questions of domestic social policy.

A similar debate about the discipline's place in the world was refracted in the more quotidian debates over membership criteria of the Association of Social Anthropologists. Should membership of the association be restricted to people trained within 'British' social anthropology, or should it be opened up to citizens of the new imperial power – America? The debate exemplified the growing tension between disciplinary stewardship and popularisation, a recurring theme in the history of the discipline (MacClancy and McDonough 1996). It was memorably described by Paul Stirling as the 'mandarin/missionary divide'.

As the discipline sought to redefine itself, it began to grapple with questions of pedagogy and reproduction. How important was an 'authorised' disciplinary textbook, expanding into new universities, or introducing anthropology into schools? I describe the ASA's abortive attempts to produce a collective introduction to social anthropology, and the constant concern about recruiting and training students. Despite the changing times, the views of the 'mandarins' continued to prevail. Despite the best efforts of disciplinary proselytisers, anthropology never became an A level subject, and only established its presence in a handful of post-Robbins universities.

The end of empire and the last years of the CSSRC

The anthropological project was particularly vulnerable to the shifting political winds in Africa and elsewhere in the empire, even though British universities were experiencing a funding boom times in the 1950s and early 1960s. The gathering strength of anti-colonial sentiment overtook the bureaucrats' longer-term agendas for withdrawal, leaving the Colonial Office in London seem increasingly irrelevant. The voices calling for independence – including anthropologists among their number – made it impossible for defend colonial funding for independent social research in these regions. The valiant efforts of Audrey Richards to protect funding for social research in these new nations made her an isolated voice.

The post-war development consensus unravelled surprisingly quickly (Porter and Stockwell 1987). By 1947, Attlee's struggling post-war government preferred to see the colonies as sources of raw materials and resources to help aid British economic recovery. Colonial economic policy began to develop a 'neo-mercantilist' character, more reminiscent of the 1890s (Butler 1999). In a climate of economic stringency, British interests took precedence. The Colonial Office's progressive philosophies and 'colony-centred' approach were increasingly criticised and overruled by the Treasury and other parts of

government. Even the promised financial allocation amounted to less than £2 per person amongst a population of 60 million. Much of this was never distributed. The sums were far outweighed by the territories' payments on outstanding loans from London. Colonies found it increasingly hard to raise the necessary capital funds to match new loans for capital, and the growing austerity strengthened the anti-colonial movement.

Leftist calls for decolonisation, based on the rights to self-determination of those who had contributed to the war effort, began to receive a hearing. There was also an increasing awareness that the 'benefits of empire' had gone to only a small strata of investors and businessmen. George Padmore was one of the more famous critics of empire (e.g. Padmore 1936). His wholesale critique of the CDaW policy, commissioned by the Pan-African Congress, viewed it as a 'sop to the coloured races of the Empire', and as evidence of a new form of 'Economic Imperialism' (Padmore 1948: 9, 157). He pointed out that 'Africans had no say in drawing up the plans' and that 'to carry out the schemes the Government have had to recruit from Britain an elaborate staff of high-salaried officials to supervise and direct the operation of the Plan at all levels'. In the case of Nigeria, he felt that the ten-year plan 'amounts to nothing more than a series of disjointed projects drawn up by various government departments and co-ordinated for the purpose of budgeting' (ibid., 162). The Gold Coast riots of 1948 demonstrated the growing demand for change, and the increasing divergence between Colonial Office planners and African aspirations.

If the strength of anti-colonial resistance had been hard to predict, so too was the speed with which independence was granted to many African countries. Riots in Kampala in 1949 and the Mau Mau rebellion in early 1950s Kenya upset the cautious timetables for gradual self-government. As Sally Chilver, Secretary to the CSSRC, commented, the attitude in the Colonial Office began to shift, with the view developing that one had to get out 'without getting one's tail caught in the door'.¹ Yet these momentous changes were not reflected in the research proposals and programmes put before the Council. The single-focus ethnographic and sociological surveys continued as before. Audrey Richards was herself a case in point. She kept a careful diary of the political intrigue surrounding the British Governor's expulsion of Buganda's *Kabaka* (king) to exile in London, and his subsequent triumphant return to Uganda. Yet she published little on the topic. Her Carnegie-funded 'Leadership' project at Makerere continued, and her contributions to Lloyd Fallers's *All the Kings Men* (Richards 1964, Fallers 1964) on the Buganda polity made no mention of the complexity of nationalist and anti-colonial politics. Only by the late 1950s did the Council's attitude begin to initiate a

programme of 'comparative studies of election procedures'; involving Lucy Mair and others.

Instead a significant proportion of the remaining Council funding went into supporting the production of regional histories. These included the multiply authored three-volume *Oxford History of East Africa* (the last of which was edited by Low and Smith 1976). The Council's energy was also directed towards incorporating the research institutes more closely with the university colleges. During the same period, a new Applied Research Unit was established at the East African Institute for Social Research, funded by the Ford Foundation. American funding and policy interests became increasingly visible in African Higher Education.

In December 1956, Audrey Richards, back on the Council after five years at Makerere, received an ominous letter from Professor Sir Arnold Plant, retired LSE economist and by then Chairman of the CSSRC. 'What I have been trying to decide', he begins, 'is whether there is a special case for continuing Treasury finance for Colonial research in this field.' His letter revealed how the Council was increasingly being perceived. 'I have been thinking about the special problem of ex-dependencies which are attaining independence. I expect the Colonial Office and the Treasury will be very concerned to avoid involvement in continuing finance. They may feel that any finance ... [is] likely to be misinterpreted as interference if the UK government puts up the money'.² His concern that CSSRC research funding was being viewed as trying to correct 'misinterpretations of nationalism' reveals the growing suspicion of colonial-sponsored social research in many African nations. He went on to note how in other cases Treasury funding was being routed through the British Academy. All sides feared being accused of political interference.

Audrey Richards was unwilling to accept the full implications of this new political landscape. In her responses to Plant she reiterated the importance of ensuring the continuity of research in the social sciences, calling for a 'rather energetic re-examination of the whole position of colonial research'. She also had strong views on state disregard for academic autonomy in newly independent African states, as she wrote in November 1956:

There are already signs that the new Governments wish to control research in the cultural and historical fields and that they sometimes have objectives beyond those of pure scholarship. Nor are they yet aware of the qualifications needed for directors of research schemes. By continuing to make grants for even a skeleton staff of local administration of research, some measure of control of appointments of this sort would remain in the hands of persons academically qualified, whether in this country or overseas, and a tradition of scholarship might be established.³

Finally cognisant of the threat to the CSSRC, Richards immediately began mobilising her contacts. One of her main arguments in favour of continued colonial funding of the research institutes was that this would preserve their academic freedom. She wrote to her old friend Andrew Cohen, by now Governor of Uganda, expressing her concern that about the demise of grants to the institutes:

The point is that the social sciences are new in the colonies. They have no government department which understands what they are doing, such as the medical research workers, agriculturalists etc have. They are liable to be concerned with questions of social policy which are controversial and which it would be tempting for the governments of newly self-governing territories, such as the Gold Coast, to try to run.

The irony of this statement, given the history of colonial control, seems lost on her. Richards goes on to give the example of a Nigerian director 'whose motives seem entirely political'. In a separate informal note to Cohen, she is blunt in her assessment of post-colonial governance: 'the universities want local autonomy over social research and to have this research under their own aegis. This means in effect that they will keep their autonomy but do no research.'⁴

Ever energetic, Richards fired off numerous letters, and began to develop an idea for an organisation to replace the CSSRC. She also lobbied for the CSSRC to take a stronger position against its likely demise. A widely circulated 1961 memo expressed the council's great concern at the likely 'break' in the field of social science research, and proposed continued support 'for UK scientists to undertake research in the social sciences in colonial territories that were newly independent'. The council firmly recommended its reconstitution as an advisory body to the new Department for Technical Co-operation. The consequences of not doing so included the veiled threat of increasing American and Soviet academic dominance of the research field, with 'the leading position and international influence of the UK in the study of the social sciences ... lost to other countries'. The memo went on to suggest that the institutes would be under heavy pressure to meet research needs 'of immediate practical interest' at the expense of dealing with 'the fundamental problems of underdeveloped countries.' This was hardly the moment at which to emphasise the institutes' detachment from their applied roots. Despite the strong words, little came of the proposal, and the council gradually wound up its affairs.

Robbins and university expansion in the 1960s

The new politics forced the discipline to seek new sources of support. Despite the expansion in the number of UGC-funded posts for social anthropology, this had not been accompanied by funds for research and training. Social anthropology's past reliance on the Colonial Social Science Research Council became all too visible. Raymond Firth raised the issue at an ASA committee meeting in March 1956. He proposed that the ASA made a public pronouncement on the situation, 'perhaps in a letter to the Times', 'an interview with the Chairman of the UGC', or a 'meeting with heads of the new Colonial University Colleges'. Nothing came of his ideas, and even an approach by Firth to the Nuffield Foundation to fund twenty PhD studentships was rebuffed. Four years later, Firth sounded the alarm again suggesting that 'British scholarship is now being seriously threatened' by the lack of a British Social Science Research Council, and called for the British Academy to administer funds for postgraduate students.⁵

The concerns of anthropologists paled against the general optimism felt within universities, and the upbeat findings of the first ever government commission on higher education, led by Lord Robbins. The commission marked a major shift towards a mass higher education system. It was also a response to a crisis in student places following the post-war population 'bump', and applications from a number of county towns to create new universities. A total of 82,000 places in universities in 1954 increased to 118,000 in 1962, even before Robbins recommended the creation of six new universities (Halsey and Trow 1971). Keele was the first to gain its statutes in 1961, closely followed by Sussex, East Anglia, York, Essex, Kent, Warwick, Lancaster, Strathclyde, Heriot-Watt, Dundee and Stirling.

Predictable concerns over funding for research and training informed the discipline's submission to the Robbins commission. This was coordinated by Max Gluckman and the eminent demographer David Glass on behalf of Section N (Sociology) of the British Association for the Advancement of Science. When the British Association finally decided in 1958 to include sociology within its remit, the proposal for a common section to be entitled 'Social Science' was rejected, with Glass arguing that 'sociology should not be brought into the BA simply as an appendage to any of the existing associations'. Glass instead proposed a separate sociology section. Max Gluckman became the first Chair of the new section, and was seen as the ideal face of a politically united set of social sciences, 'in order to encourage anthropologists to take more part in the British Association'.⁶

The Glass-Gluckman memo, as it became known, was entitled 'The Social Sciences in British Universities'. It stressed the common interests

of the three social sciences, including social psychology, and focused primarily on 'the lack of consistent policy in developing the Social Sciences within the University framework'. Keen to emphasise their scientific contribution, they bemoaned the 'lack of research funds which would make it possible systematically to build up knowledge and valid generalisations in their respective spheres'. A special case for anthropology was made in reference to the threat of American dominance, where 'the study of developing societies is becoming increasingly the preserve of US universities – hardly surprising when a single US research grant given in this connection may be larger than the total annual expenditure of all UK universities for anthropological and sociological research in less developed countries'. Glass and Gluckman also viewed 'with grave disquiet the tendency of the Government and of the UGC to put their main emphasis upon undergraduate teaching', and instead emphasised the importance of a proper postgraduate training programme. Repeated stress was put on 'catching up with the older disciplines', along with the recommendation to establish 'a Human Sciences Research Council to match the other existing councils'. They described the constant search for grants for young scholars as leading to 'senior teachers having to spend a large proportion of their time searching, cap in hand for small amounts of money from diverse sources' (Glass and Gluckman 1962). Firth, as ASA Chair, lent the association's support to the memorandum, and, in his own letter, equally emphasised anthropology's increasing interest in industrialising societies.⁷

Not everyone supported the idea of a joint submission with sociology. Edmund Leach, perhaps because of growing theoretical differences and increasing personal rivalry with Gluckman, was distinctly unimpressed. Gluckman had strongly disagreed with Leach's emphasis on conflicting individual interpretations of social models and collective political processes. (Kapferer 1987). Leach saw the memo as presenting social anthropology as 'an appendix to sociology', and assuming that the interests of the two disciplines were 'always identical'. He went on to suggest that in some universities 'social anthropology needs independent support', and made a special plea for the Royal Anthropological Institute and the co-ordinating role it could play within anthropology. In so doing, he brought the discipline's infighting over the role of its professional associations full circle, for he held a rather different view of the discipline and its future. He was appointed President of the RAI the following year.⁸

Gluckman responded suggesting that 'Leach has misunderstood the enquiry' and pointing out that the key issue facing anthropology was that the new committee 'says that it is only concerning itself with research in Britain'. 'What I feel we must do,' he went on, 'is point out

that the substantial body of experts and knowledge built up in Britain about overseas countries should be contained in the future. We are competing on very unsatisfactory terms in this respect with other countries.'

The Robbins report was sympathetic to the nascent social sciences, calling for broader first degrees, and an increase in funded postgraduate study. The inevitable downside was the social science's increased reliance on British government funding. As Shattock notes, 'Robbins represented the essential watershed between the period when university autonomy was taken for granted and when its gradual reduction became part of a litany of university complaint against the government of the day' (Shattock 1994, 107).

The Heyworth Report and the founding of the SSRC

If Robbins envisaged an expansive future for universities, the future of anthropological research was more directly shaped by the 1965 Heyworth committee on social studies. Despite the findings of the Robbins committee, many in the Conservative Government were strongly resistant to funding social science research. Lord Hailsham famously saw the whole field as a 'happy hunting ground for the bogus and the meretricious' (quoted in Nicol 2001). After persistent lobbying, he was forced to concede 'not quite another inquiry, but an inquiry into whether an inquiry was required' (Nicol 2001, 10). When a committee on social science research was finally convened, the appointment of a corporate figure like Lord Geoffrey Heyworth, then Chairman of ICI, ensured a highly utilitarian and policy-focused vision for the social sciences.

Predictably, Lord Heyworth drew heavily on his business background. His terms of reference saw the relevance of the social sciences as reaching far beyond universities. For the first time, 'users' of social research, including the government, were included in the consultation, with an implicit corollary that social research could and should be policy-oriented. Whilst its terms of reference were 'to review the research at present being done in the field of social studies in Government departments, universities and other institutions', the commission's own appendix called for submissions that looked 'for evidence concerning the application of these subjects' in 'administration, education, employment and industry, government, law, medicine and social services'.⁹

Was Heyworth only interested in research in Britain? The committee's view was that they were concerned with 'research abroad as part of a study which includes research in this country (e.g. a

comparative study)'. The ASA decided to challenge this narrow interpretation. Stressing the comparative nature of their discipline, Banton and Gluckman insisted that it was 'impossible to have a social anthropology of British life unless it is set in the general perspective provided by the study of human societies throughout the world'. They described this British focus as 'meaningless in the case of our subject as it is founded upon social comparisons and cannot be forced into a national straight jacket [*sic*]'. 'British' social anthropology was, in their eyes, international by definition.

They went on to note that 'any interpretation of the committee's terms of reference which virtually excludes social anthropology would be bizarre in the extreme'. Finally, they pointed out that the USA, the USSR, France, Belgium, Holland and Italy were putting more money for research in less developed countries, and 'that the international reputation of British work in these fields is such that American institutions have drawn away some of our best young anthropologists'. A second submission from the ASA sought to cover all corners by also emphasising anthropology's 'contribution to understanding British social and industrial life'. In a similarly strategic move, the British Sociological Association also made a two-page submission, written by Michael Banton, to the committee entitled 'Statement concerning Under-developed Societies', and emphasising that British sociology 'should not become parochial or limited in its perspectives to industrialised nations alone'.

The secretary to the Heyworth committee invited representatives from the RAI and the ASA to do an oral presentation, in order for 'representatives of the profession to speak in unison'. Leach's opposition to the Glass–Gluckman memo and his counter-proposal to make the RAI the funded centre of British Anthropology made unanimity unlikely, and an anxious correspondence followed. The ASA secretary went as far as to suggest that 'some of our members would feel there was a better case for our appealing jointly with the British Sociological Association than with the Royal Anthropological Institute'.¹⁰

The Heyworth committee held a number of consultation seminars, including one in Manchester on 'Research into problems of regional and urban development'. The meeting was dominated by a professor of planning, who suggested that a new research council should get involved in regional planning for the North West. Gluckman's response was crisp and revealing. 'I would have thought that for a research council to get involved in this kind of project would involve it in so many political complications that its effectiveness would soon be destroyed ... What I think is important is to distinguish academic from practical research.'¹¹

Disciplinary special pleading dominated the consultation. As a result, the final report attempted to try and define the disciplines, ostensibly because 'outside universities people are often unfamiliar with their technicalities', but also to reiterate the scientific potentials of the social sciences. Yet this was easier said than done: 'Sociology is perhaps the discipline which people find the most puzzling of the major social sciences,' the report began. (Heyworth 1965, 3). As the Heyworth report noted, 'Sociology has recently grown with explosive force from two or three centres to practically every university.' The report argues that the difference between the two disciplines is primarily one of method, but also highlights the discipline's 'useful' qualities: 'In addition to their traditional work in underdeveloped countries, anthropologists have been able to make a number of highly useful observations about British life by setting it in the general perspectives provided by the study of human societies throughout the world' (ibid., 3).

Whilst the committee's recommendation to establish a Social Science Research Council (subsequently renamed the Economic and Social Research Council in 1981) was broadly welcomed by anthropologists, its focus on the rational application of social scientific knowledge to national social planning was far less palatable. The final report stated that 'basic research and applied research both have to be carried out simultaneously; neither can advance without the other' (ibid., 28). The list of research priorities, such as 'What is the most effective form of classroom organisation' or 'What are the most effective forms of organisation of industrial enterprises' all adopt a narrowly functionalist and utilitarian rationale for social research. It encouraged policy-makers, and many academics themselves, to view the social sciences in positivist terms. The one international research priority – the social and cultural legacy of colonialism for newly independent countries – was one that anthropology had not really begun to address.

This rhetoric put anthropology in a double bind. On the one hand, its survival as a distinct intellectual discipline depended on state funding. On the other hand this patronage was accompanied by and linked to a constant negotiation and conflict over what counted as useful, as opposed to merely important, social knowledge.

Empires old and new

Whilst the long-term future of social sciences was in the hands of these high-level commissions, the discipline itself faced more immediate problems. The Association of Social Anthropologists

continued to face the challenge of whether to become more inclusive in its membership policies or to maintain its role as mouthpiece for a distinctive 'British social anthropology'.

The first problem that emerged in the 1950s was that of graduates from British anthropology departments who 'then took posts in foreign countries' (here 'foreign' needs to be read as non-Commonwealth countries). Though they were not then eligible for ASA membership, the committee recognised that 'it might be fruitful to link them in some way with the ASA', especially as they 'were working in the tradition of British social anthropology'. Once again, national and academic identities were conflated. Social anthropology was never described as a Commonwealth intellectual tradition, even though many of its key protagonists hailed from New Zealand (Piddington, Firth), Australia (Kaberry) and South Africa (Fortes, Gluckman, Schapera). 'British' became code for the imperial context and networks within which social anthropology had developed. During the course of his peripatetic career, Radcliffe-Brown held teaching posts in Sydney, Cape Town and Chicago, creating cohorts of influential students, and later also in Yenching, São Paulo and Alexandria.

Yet those in the 'dominions' faced a difficult logistical challenge if they wished to attend the twice-yearly meetings in England. When the Professor of Anthropology in Sydney wrote in the summer of 1952 proposing that an Australian subgroup be formed, to be called 'ASA (Australia branch)', the ASA's own committee responded by changing their organisation's title. They published a short piece in *Man* announcing that its new title was the 'Association of Social Anthropologists of Great Britain and the Commonwealth'. The Australian branch was welcomed, but their members were asked to continue their individual membership of the umbrella association.

The following year John Peristiany, an Oxford lecturer who specialized in the Mediterranean, pointed out the corollary problem of international students who had done postgraduate work in this country. A fuzzy modification to the rules was agreed in 1960 – that 'persons who are not nationals of the Commonwealth should have received "a substantial" part of their training in a commonwealth institution'. In the same meeting the eligibility of 'persons working in the industrial field' was raised. This was seen as far less controversial, with both Gluckman and Firth pointing out that the discipline was defined not by its object of study but by its methods. Gluckman, however, was the only senior ASA member to have a significant number of students doing research on 'complex societies'. Most of these subsequently became employed in sociology departments, though many also joined the ASA.

The rules governing membership got steadily longer and more difficult to interpret. On the other hand, the small size of the association in this period ensured both scholarly familiarity and a shared focus on a cohesive set of theoretical questions. The bureaucratic ritual of scrutinising applicants served to 'beat the bounds' of disciplinary discussion. Did the applicant share the 'tradition' of 'British' social anthropology? Were they one of us?

The Americans are coming

The great unmentionable in these debates was the ASA's relationship with American anthropology. The 'Britishness' of social anthropology increasingly began to be defined in counterpoint to a Boasian tradition and the 'culture and personality' approach of Ruth Benedict and others. Yet both Radcliffe-Brown and Malinowski had taught in the USA, and their students became increasingly influential. Radcliffe-Brown was Professor at Chicago between 1932 and 1937, and a number of his postgraduate students, including Fred Eggan and Sol Tax, took up his theoretical interests. By ASA rules, they were not permitted to join because they did not hold positions in 'social anthropology' departments. Meanwhile, the American Anthropological Association, through its conferences and journal, continued to expand its international influence. It was in the *American Anthropologist* that George Murdock attacked the narrowness and ahistoricism of the new British school of social anthropology in 1951 (Murdock 1951). Whilst Murdock's call for 'comparative or cross-cultural validation' hardly represented all American scholarly opinion, his caricature was influential. The article was entitled 'British Social Anthropology', and the label stuck.

Not everyone in the USA was unsympathetic to social anthropology. In 1958 Fred Eggan, one of Radcliffe-Brown's students at Chicago, nominated Firth and Fortes for fellowships at the Centre for Advanced Study in the Behavioural Sciences at Stanford. Once selected, they were joined by the American scholars Lloyd Fallers, Clifford Geertz, Cora DuBois, Fred Eggan, George Murdock, Melford Spiro, Dozier and Greenberg. During their stay there was an extended discussion over the need for a journal of social anthropology. Recognising that 'every other major branch of anthropology has at least one journal to cater for its needs', a title was tentatively proposed: 'Social Anthropology: A Journal of Society and Culture'. On his return to the UK, Firth sent the proposal out to ten 'British leaders' in the discipline. Responses were mixed, to say the least. Evans-Pritchard responded that he was 'unenthusiastic about the proposed new journal ... I would myself like to see a British journal of the kind, but not an Anglo-American one ... I just don't care

for their kind of writing.' He added that it would be 'unwise to start off with a purely Anglo-Saxon set up and then ask people like the French to come in later. The French are sensitive in such matters.' On the question of national influence, Leach added that 'quantitatively the American contributions to the new journal would heavily outweigh all the rest put together and this would lead to a debasement of the standing of British Social Anthropology'. With more than a touch of irony, he went on to acknowledge that 'the phrase "social anthropology" is becoming OK in America though very few American anthropologists know what we mean by this expression'.¹³

During 1961 disagreements over expanding the ASA beyond the 'Commonwealth' divided the committee. In a lengthy memo, Firth, then ASA Chair, questioned the restrictive membership policy. He felt that both the issue of the Australian branch and the forthcoming 'Anglo-American' conference (later known as the first ASA decennial) made the issue of membership particularly urgent. Firth had obtained US National Science Foundation support for American scholars to travel and participate in a conference that he envisaged would address 'divergences between British and American Social Anthropology'. In a strongly worded response, Chairman-elect Gluckman rebuffed these anxieties:

What is the crisis in ASA affairs? There is no indication that the Australian branch wants to withdraw, despite the difference of opinion over one rule. On the major issues, I don't see how we, as a strong association are threatened by the possibility of Americans forming an association of social anthropologists. I think we are more likely to be swamped if we continue to open the doors to Americans, for where do we draw the line between social anthropology and other forms of anthropology?¹⁴

Gluckman went on to discuss individual possible candidates, so revealing his view of the brand of comparative cultural anthropology propounded by George Murdock and others:

In Chicago there are three ASA members – Fallers, Schnieder and Pitt-Rivers ... Obviously Eggan – but then? Are we to take in Murdock, White, Aberle etc etc? They would swamp us, and I would not recognise Aberle as a social anthropologist, having just had him here for a year. The whole operation looks fraught with difficulties to me, and I consider that we had better continue along our own road. We should explain to Barnes why we restrict membership to people in the British Commonwealth trained there – it is not xenophobic, since we would welcome people like Eggan, but because we have to think about the Murdocks and the Herskovites. Secondly, if we change the rules of membership, we alter completely the character of the ASA: instead of growing steadily on the basis of personal

association of one kind or another, it would with a jump in size become a different sort of body altogether.

Firth quickly responded in a conciliatory fashion, agreeing that there was 'of course, no crisis in ASA affairs'. Reassured, Gluckman was in turn more amenable. 'My main worry about throwing the whole association open, is that we would then have ourselves to take on the very invidious job of selecting among foreign taught applicants those whom we thought were social anthropologists rather than other kinds of anthropologists'.¹⁵ Gluckman carefully avoided mentioning that it was precisely this 'invidious' process of selection that had always characterized the ASA.

On the whole, differences were kept within the committee. In 1963 the committee agreed that the Anglo-American meeting would be the ideal place to discuss the 'internationalisation' of the association. In public, however, it was left to Jack Goody to propose that the word 'Commonwealth' be deleted, and that all candidates be admitted on their merits, putting the association on an international basis. Yet he had little support amongst members, many of whom felt that the association was already expanding too fast.

The 1963 conference was seen as a success (Eggan 1987), and led to the publication of the first four ASA monographs (Banton 1965a, b, 1966a, b), though they faced the usual drawbacks of all edited collections. Frankenberg (1988, 3) remembers how one US speaker proclaimed his pleasure at being invited to 'the joint meeting of the British Empire and the University of Chicago'. The issue of internationalisation was not raised at the conference itself, but three months afterwards the committee nonetheless proposed offering membership to a number of American anthropologist (including Fred Eggan, Clifford Geertz, Ward Goodenough, Marshall Sahlins, Melford Spiro and Eric Wolf) and 'to allow them to nominate others so that an American branch could be formed'. However, Fred Eggan, one of the conference chairs, thought it would cause great embarrassment if only a select few known to be sympathetic to the aims of the ASA were invited to join, and dissuaded Max Gluckman from doing so.

This was one of several attempts to set up an American 'branch' of the ASA. Raymond Firth again mooted expanding the association in 1969, proposing an 'International Association of Social Anthropologists'. Again, the committee demurred, feeling that 'it might prove embarrassing to have to define a social anthropologist' and that 'it was hard to see what such an association might achieve'. The outcome of this proposal was simply that it was agreed to hold another large conference, 'to which social anthropologists from other countries would be invited', a proposal that led to the holding of the second decennial in 1973 and the setting in train of a precedent for a ten-yearly conference.

Unresolved, the 'Commonwealth' label continued to stir up anxieties over self-definition. Relations with the Australian branch worsened. Far more students were receiving anthropology degrees in South Africa, Australia and India than in the UK, and yet the focus of the ASA was almost entirely on metropolitan concerns. Led by John Barnes, the Australian branch asked to be able to elect their own members, and for the membership criteria to be expanded to include all four fields of the discipline. The ASA committee was unanimous in rejecting this proposal. Rebuffed but not cowed, the Australian ASA disassociated from its 'British' parent and formed an independent association in May 1969.

'The most useful kind of textbook'

The early history of the ASA is marked by repeated concerns over self-definition. When the ASA committee members were not revisiting their membership criteria, they seemed to be concerned with teaching and the production of a representative textbook. On the ASA's founding in July 1946, the first objective of its draft constitution was 'to promote the study and teaching of social anthropology'. Early meetings dwelt repeatedly on the idea of producing either a journal or a textbook for scholars and students that would capture the concerns of this new discipline. One of Evans-Pritchard's initial aims for the ASA had been 'a journal devoted solely to social anthropology'. 'Such a journal', he went on, 'would publish only contributions to theory and methodology, and not ethnographic fieldwork reports'.

In Evans-Pritchard's initial plans, drawn up in the autumn of 1946, he proposed a journal modeled on *L'Année Sociologique*, with a number of extended review articles and memoirs. His draft listed the contribution of each: Radcliffe-Brown – General Sociology; Firth – Economics; Fortes – Social Psychology; Schapera – Political Institutions; Gluckman – Law; Evans-Pritchard – Ritual; Forde – Marriage and Kinship; Nadel – Culture Change; Audrey Richards – Applied Anthropology (Firth 1986). Evans-Pritchard even had a title: 'Annals of Social Anthropology'. The journal, he felt, would legitimise this new intellectual school, and be a way of inspiring and informing potential students.

Evans-Pritchard was quoted £500 for the publication of a 500-page journal, a huge sum of money for an association with only £6 in hand. The committee resolved to seek support from the Carnegie Foundation. Carnegie were not convinced, and instead Firth proposed to publish the volume in an LSE series. The growing rivalry between Oxford and LSE meant that this was not greeted with enthusiasm

either. By the following summer the committee agreed to focus solely on a textbook with the provisional title 'Advances in Social Anthropology', and a sub-committee was formed. Seigfried Nadel and Max Gluckman were appointed joint editors, and a text of 150,000 words was envisaged, with publication in October 1950.

This plan also ran into problems. American colleagues recommended that such a text should 'include anthropology generally', but this was rejected by a now divided ASA committee. Eventually the editors resigned from the task, though Gluckman, at Evans-Pritchard's instigation, relaunched the initiative in the summer of 1951. Gluckman wrote a lengthy memo on the developments thus far for those new to the ASA: 'Primarily we should aim at a book, or series of books, which would evaluate the development of British social anthropology into a specific distinctive discipline.' Focusing on the developments in the 'technique of intensive field research by Radcliffe-Brown and Malinowski', he adds, 'I consider that it is possible to isolate the new technique as producing a new discipline.' The aim was the 'most useful kind of textbook, since an unformed discipline is best taught through its historical development'. 'Britishness' was invoked as a key – if undefined – marker of this new intellectual development. A new editor, John Peristiany, was appointed, and again members were urged to submit papers.¹⁶ But soliciting written contributions again proved impossible, and there was a growing recognition that the association had neither the financial means nor the critical mass to produce its own journal. Even plans to stenograph copies of talks made at ASA meetings never materialized. Instead, individual anthropologists began to publish their own introductory textbooks (e.g. Piddington 1950; Nadel 1951, Pocock 1961).

In 1956 Jack Goody returned to the idea of 'a collection of articles representing the more theoretical achievements in Social Anthropology'. The twist was that the articles were to be selected by younger members of the association, 'on the grounds that they are in a better position to assess the achievements of the senior generation'. It too became mired in the controversy over who would select such articles. According to one critic, 'it divided the ASA into seniors and juniors when at meetings all were equal'. Goody's idea did, however, inform the structure of the 1963 'Anglo-American' conference, which sought to highlight the work of younger scholars. Despite almost two decades of effort, the first official ASA publications were the monographs from this conference. Edited by Michael Banton, and with an introduction by Gluckman and Eggan, the contributions clustered (not always synergistically) around four major themes of social anthropological study – political systems, religion, the relevance of models and complex societies. Whilst hardly the textbook originally envisaged, the four volumes represented a final compromise

between the ASA's initial pedagogic ideals and the limited market for the discipline.

Pedagogic mandarins or educational missionaries?

Despite Firth's failure to get the Nuffield Foundation to fund Ph.D. studentships, not everyone was worried about finances. Fortes, secure at Cambridge, announced himself 'relieved that Nuffield did not give money', insisting that 'more money is about than good people to take jobs'. He instead noted that despite a decade of expansion and advance into teaching and research, there had been 'no opportunity to pool experience' around pedagogy. He proposed an ASA meeting to consider 'undergraduate teaching and patterns of training and research'. Backed by Audrey Richards and Barbara Ward, plans were made for a major ASA meeting to discuss teaching.¹⁷

The meeting, simply entitled 'The Teaching of Social Anthropology', was held in September 1958 at Kings College, Cambridge. It was the first annual ASA 'conference', replacing the twice-yearly meetings that had been held since 1946, meetings that were now increasingly poorly attended. Instead of the usual one or two speakers, seven papers were presented in four separate sessions – 'Social Anthropology for the Non-professionals', 'The Teaching of Undergraduates', 'The Teaching of Graduates and Training for Field-work', and 'Training for Fieldwork in Retrospect'. Twenty-nine senior ASA members attended, though, after earlier unhappiness about the presence of uninvited graduate students, no students were present.

Barbara Ward – an ex-teacher and a Cambridge-based anthropologist of China – opened the conference with a discussion of anthropology for 'non-professionals'. She argued that a 'comparative and structural approach to the study of human relationships could add to the moral equipment of educated people' and advocated the wider dissemination of the discipline's insights. She used the precedent of history, which at the turn of the century had equally been faced with 'a dilemma similar to our own', namely 'whether to give research priority over all other considerations or to broaden their approach'. She described how her own research had found a widespread ignorance amongst beginners about the discipline, and argued for a 'professional' committee to focus on the problems of 'teaching anthropology to non-professionals'. More controversially, she noted that teaching was often 'neglected or even despised' in the research universities. Kenneth Little, another disciplinary reformer, suggested that there was value in schoolteachers having knowledge of the discipline for handling children, and that it would be a 'good thing' if they understood the anthropological approach

to race relations. But these positions caused a good deal of controversy. Leach, in particular, thought the proposal of 'selling' social anthropology to be 'preposterous', and that 'if the subject was any good it would sell itself'.¹⁸ He continued to take a strong position against social anthropology in schools (Leach 1973), and popularisation in general.

Should teaching be an individual matter, or was promoting the discipline's contribution to 'general education' a priority? The tension between the two approaches dominated the conference. The importance of a textbook of social anthropology was again raised, with Freedman pointing out 'that if we had accepted this view earlier we would have produced the basic textbooks which were so woefully lacking'.

During the conference, undergraduate teaching provision at Cambridge, Manchester and LSE were compared in some depth, to the extent of comparing set readings and course options. The conference proceedings provide an excellent record of the different departmental approaches to teaching. Fortes, for example, advocated that 'current controversies should be excluded' in the teaching of undergraduates, and that 'no thorough treatment of the subject's history was needed at this stage'. The place of history and sociology in the teaching of anthropology was constantly returned to, with Fortes suggesting that 'were one to stick to social anthropology as an educational subject without bearing in mind these links it could have a narrowing influence'. On the other hand, the Manchester honours course involved 'one sociological problem examined in detail'. A good deal of anxiety was expressed about the 'mediocre' quality of anthropology students, for which Evans-Pritchard blamed 'the lowly place accorded to social studies in the this country', though he also acknowledged that 'we have not yet attained the high standards of rigorous scholarship an exacting student might require', and also 'to some extent lost touch with other fields of learning'. Gluckman, on the other hand, took the position that the 'lower seconds' were 'good, able and valuable'.

There was a good deal of debate about research training, particularly in the two sessions devoted to fieldwork preparation. Debate hinged around the appropriate length of graduate training, the necessary prerequisites for Ph.D. registration, the contents of pre-fieldwork seminars, and appropriate forms of supervision. Firth asked whether 'supervision should be as loose and informal as appeared to be the case in Oxford', whilst others suggested that the fieldwork should be learnt through the 'do-it-yourself' style of apprenticeship, or placed emphasis on the value of informal peer-tutoring from those engaged on 'writing up'.

Summing up the conference, Forde concluded that ‘the association should put expansion into the field of general education as one of its objectives’, and the meeting agreed to set up a sub-committee ‘for the study of the presentation of social anthropology to non-specialised audiences’.¹⁹ The 1958 conference set a precedent for a disciplinary concern with pedagogy, but from then on the focus was on ‘educating’ others about anthropology, rather than on how it was taught within universities.

‘People who know what they are talking about’: teaching anthropology from above

Plans for a regular meeting went into abeyance, only to be resurrected by Paul Stirling in 1964. That December, six years after the first, another conference on ‘The Place of Anthropology in General Education’ was held at the LSE, funded by Wenner Gren.²⁰ This time it focused on the teaching of anthropology in schools, further education and teacher training colleges. In his provocative call for papers, Stirling acknowledges the tensions within the association about its pedagogic role:

This topic has so far divided us into the Mandarins (social anthropology for professionals and mature minds only) and the Missionaries (social anthropology has a message for everyone). We are meeting to discuss how to help, not whether to help, and cannot afford publicly to divide amongst ourselves. There are plenty of problems, – what is to be taught, how, to whom, by whom, and from what books.²¹

This second conference attracted many senior figures. It began with a ‘closed’ session, where Stirling polemically announced that teachers and pupils were ‘pathetically not to say dangerously ignorant about society and societies’. ‘Have we not’, he asked, ‘a clear moral duty to propagate the truth as we see it?’ He went on to declare his reluctance to ‘sit by and see schools teach a sociology in which social anthropology is scarcely represented’. In the same session Gluckman talked in pragmatic terms of the actual services the association could offer to schools and education authorities, and raised the question of how far it would be possible to use ‘professionally trained’ staff. This was followed by a discussion on how best to present the discipline to the guests, and what people considered the ‘five main strengths’ of the subject when taught as a school subject. These, it was finally agreed, included ‘eroding ethnocentricity’, ‘a solid respect for facts’, ‘the limitations of all languages and cultures’ and the ‘general principles underlying the uniqueness of all societies’. There was very much less

consensus on 'the very great practical difficulties' of what to teach. One of the key subtexts to the gathering was whether sociology and social anthropology should be taught as A level subjects. Some welcomed the proposal, whilst others were 'strongly opposed'. The proceedings note that 'some of the opposition agreed that it was better to bow to the inevitable and cooperate'. All agreed that anthropology should only be taught by 'qualified people', and must not become 'woolly or doctrinaire'!

The open session on the following day, with around twenty-five invited guests – primarily teachers and policymakers – began with presentations by Mary Douglas and Frankenberg on 'What anthropology has to offer'. Mary Douglas, supporting Stirling's position, argued that 'neither knowledge nor even admiration of other cultures is harmful to faith or morals, unless illuminated by a falsely romantic halo'. Frankenberg discussed sociology's relation to anthropology, noting that 'some people find sociology vague, general and abstract' whilst 'social anthropology is specific and detailed', and also has the 'advantage' of the 'shock and interest of the unfamiliar'. The day also included papers by practising teachers, for the Manchester department had developed a close linkage with a teacher training college, giving advice on teaching the subject in schools.

As ever, the theory-practice dichotomy was cause for dispute. Some of the teachers reacted to the aura of abstraction by challenging anthropologists to 'provide solutions to specific practical problems'. On the other hand, several anthropologists 'expressed serious misgivings about a general policy of encouraging anthropology students to do practical local research as part of an initial training', noting that 'not even universities attempt this'. Others voiced strong objections to the students carrying out practical research as part of their initial training. The complex relationship between sociology and social anthropology also confused many outside the discipline. This was hardly surprising given the mixed messages they received from academics. Fortes argued that anthropology had to keep itself distinct and that 'the awareness of differences between societies was the crucial point', whilst Tyler, for tactical reasons, taught anthropology but called it sociology.

This problem of definition was not only a problem for the 'guests', but also a key dilemma for the final closed ASA session, and indeed for policy developments in subsequent years, during which the teaching of sociology at university and school expanded rapidly. As the 'overlap between sociology and social anthropology is so great and our interests so close, it would be absurd to appear publicly as rivals' went the argument recorded in the proceedings, whilst also recognising that 'complete identification with sociology is unacceptable both to them

and us'. It was agreed to act independently, but in the 'fullest possible co-operation' with sociologists.

With no publications or public record, the 1964 conference is little remembered in contrast to the first ASA decennial – the 'Anglo-American' conference – the previous year. It deserves better. The dialogue was a diverse but productive one, and in a final 'closed' ASA session it was agreed that there was a strong case for anthropologists to 'take more interest' in non-university education. The general enthusiasm for action led – perhaps inevitably – to the creation of another high-powered committee. Convened and chaired by Stirling, this was grandly entitled the 'Committee on Anthropology in General Education', consisting of the national ASA office-holders and representatives from all the main university departments. Each regional representative was also envisaged as setting up their own committee, as 'we seem unanimous that more about anthropology should be made known to colleges of education and school teachers'. 'Furthering the cause' involved responding to requests for help with syllabuses, offering talks to sixth forms and doing school visits. As the request to the departments ends, 'we all stand to gain from a wider dissemination by people who know what they are talking about, and the more interest we take ourselves, the less will it be left to non-anthropological amateurs to lead each other into the ditch'. Whilst such efforts at outreach continued into the 1970s, before being taken up by the RAI's own Education Committee, they remained largely marginal to disciplinary preoccupations.

One of the decisions of this committee was to cooperate as closely as possible with the British Sociological Association (BSA) in organising future conferences. However this was easier said than done. The BSA was already divided between senior research-focused academics and a 'teacher's caucus' within its ranks. Such internal feuds limited ASA/BSA cooperation. The Sociology Teachers Section of the BSA did subsequently hold their own conference on teaching sociology in colleges jointly with the ASA in 1966, and in 1968 an informal working party sought to put together an A level syllabus that was presented to the Oxford and Cambridge exam boards in 1970. Nothing came of the joint initiative. The reluctance of many anthropologists to consider the possibility of teaching anthropology as an A level was also not shared by sociologists, and the growth of sociology in schools buttressed its expansion in the new universities.

Conclusion

With hindsight, the 1960s was a crossroads in social anthropology's development in the UK. The end of empire forced it to find new sources of funding and to reflect on its organisation, its structure and the training of its students. The ASA continued to play a central role in the discipline. It could have been a strong advocate for change, reform and popularisation. But the rifts and tensions of the 1930s were still raw, and some did not want to be reminded of the discipline's colonial legacies by allowing administrators and amateurs to become members. The discipline was caught between the past and the future.

Despite the desire of some to proselytise on the discipline's behalf, intellectual coherence came first. The debate over who counted as 'one of us' continued to dominate the association's proceedings.²³ This selectivity came at the price of declining public influence, despite the media profiles of Max Gluckman, Mary Douglas and others. However, anthropologists were far from marginalized. Firmly tenured within the major research universities, they could begin to put their long-awaited disciplinary autonomy to use. From Firth's role in founding the new Social Science Research Council in 1965 to Rothschild's robust defence of the discipline in 1982 (Rothschild 1982), anthropologists had a wealth of political experience and social networks from which to protect and develop their intellectual legacies. Difficult folk they may have been, but canny too.

Notes

1. Interview, 6.3.00.
2. Letter from Arnold Plant to Audrey Richards, 4.12.56, Richards papers, 16/11, BLPES.
3. Letter to Arnold Plant, 12.11.56, Richards papers, 16/11, BLPES.
4. Letter to Sir Andrew Cohen, 3.12.56, Richards papers, 16/11, BLPES.
5. Firth, paper given to the British Association of Advancement of Science 1960, Firth Archives, BLPES.
6. Firth Archives, LSE Firth 3/3, Minutes of British Association executive committee meeting, Nov. 1958.
7. Firth Archives 3/3, BLPES.
8. ASA Archives 6.3, BLPES.
9. ASA Archives 1.2, Terms of reference for Heyworth committee, and correspondence from Banton, 4.11.63, to Cherns, secretary of Heyworth committee, BLPES.
10. *Ibid.*, 1.2.
11. Committee on Social Studies, Oral evidence transcripts part 1, Seminars 1964, PRO ED 144/5.
12. Firth Archives, 3/2 Firth to Fortes 9.6.59, BLPES.
13. *Ibid.*, LSE 3/2 Leach to Firth 10.6.59, BLPES.

14. *Ibid*, 3/9 Gluckman to Firth 1960, BLPES.
15. *Ibid*, 3/9 Firth to Gluckman 11.12.61, Gluckman to Firth 1.2.62, BLPES.
16. Firth Archives, File 3/1, BLPES.
17. ASA Archives E4/1, BLPES.
18. ASA Archives A2/1, BLPES.
19. ASA Archives, A2/1, BLPES.
20. Wenner Gren had just funded a series of international seminars on teaching anthropology (Mandelbaum 1963).
21. ASA Archives, A2/2, BLPES.
22. Titmuss papers, 2/187, BLPES.
23. In the succeeding forty years there have been many further revisions of the ASA's membership criteria.

THE USES OF ACADEMIC IDENTITY

I became intrigued by anthropology for two reasons: its ideas and its iconoclasm. Later I realised that it also offered a closely-knit intellectual community, with the benefits of both status and a vibrant disciplinary identity. This is still its appeal for many postgraduates who aspire to academic careers, despite the paucity of tenured posts, decreasing autonomy and meagre professional rewards. Disciplines like anthropology continue to attract recruits because they offer rich and fertile traditions for thinking and debate. At best they offer a theoretical ‘triangulation point’ from which to make sense of new horizons and new fields in the world beyond. Well-mapped intellectual landscapes continue to offer up unexpected new riches or perspectives. The less appealing corollary to such images is that of stale scholasticism (Bourdieu 2000), pedantic turf wars and intellectual dead ends.

Can one be too ‘disciplined’? Can a discipline’s seeming strength and coherence be a weakness, limiting what it is possible to imagine, to think and to say? In this book I have explored the origins of one particular disciplinary terrain. I have described its creation by a few highly ambitious individuals, their dependence on institutional patronage, and their determination to mark out and defend an intellectual patch and methodological approach. Is this story still relevant today? Some are predicting a post-disciplinary episteme for the social sciences. Others urge that we value the academic role of stewarding and reworking disciplines and professional identities. In this final chapter, I reflect on the curious role of disciplinary affiliations (di Leo 2003) in a university sector increasingly governed by the rationalities of the market and the corporation.

Learning to be undisciplined

More than two decades ago, Clifford Geertz (1983) argued that disciplinary boundaries were dissolving and intellectual genres were blurring in the social sciences. A decade later, the Gulbenkian Commission, led by Immanuel Wallerstein, again called for the 'opening up' of the social sciences, questioning what they saw as the outmoded nature of disciplinary knowledge (Wallerstein 1996), building on Gibbons et al.'s influential challenge to discipline-based 'mode 1' knowledge (Gibbons et al. 1994). These were the first salvos in a debate that has attracted increasing attention from academics, policymakers and funders. Early visions of a neatly demarcated map of intellectual territories, each inhabited by its own disciplinary tribe (e.g. Becher 1989), have come to seem increasingly static and inaccurate.

There are many benefits to a disciplinary affiliation. As is revealed in the battles fought by Malinowski, Radcliffe-Brown and their students, an intellectual specialisation can act as a motor for the development of new approaches and fields. Secure in their disciplinary identity, our protagonists were able to champion a particular theoretical paradigm that threatened, and redefined, an existing field of knowledge or practice. One could argue that such disputes are a measure of the vitality of the field. The checks and balances offered by conflicts between rival schools of thought, together with the archival logic implicit within disciplinary journals and books, all serve the vital function of curating, preserving and defending humanistic knowledge. By dint of their ways of working disciplines can protect a space for the unexpected, the tangential and the elusive. In an age of obligatory innovation and seeming standardisation, this remains one of the lesser sung virtues of disciplinary practice.

The conservative philosopher Michael Oakeshott wrote a powerful defence of the art of disciplinary specialisation. In an attack on the planners' rush to make universities more utilitarian and 'relevant', he argued that 'each true *techne* is, or involves, a particular manner of thinking, and the notion that you can think but without thinking in any particular manner, without reference to some definite universe of discourse, is a philosophical illusion. Every true *techne*, profoundly studied, knows something of its own limits, because it has some insight into its own presuppositions.' He went on to challenge the possibility that the 'the world of knowledge' could be 'integrated by a Summa', and that those who urged for a generic approach to education and training were 'unreliable guides whose immoderate thirst has conjured up a mirage' (Oakeshott 1989, 134). By this logic, disciplines enable learning, and the more profound the attachment to

one's disciplinary identity, the more one will learn. Disciplines are their own pedagogy, their own rationale.

The very strength of one's disciplinary calling changes over time and place. Some senses of belonging are more powerful than others. Many academics embody more than one identity, and are at ease with the double consciousness that can sometimes result. Social anthropologists tend to be particularly attached to their discipline, perhaps because of its size and distinctive history. In the UK, if not in the USA, the discipline has sought to retain and defend an intimate and close-knit community of scholars. Marked theoretical differences are tolerated because a discipline of small size can easily unite behind the flag of institutional vulnerability. Sociology's identity derives from a more inclusive and reformist history, even if its rival moieties often seem to be perpetually feuding. Cultural studies has loudly advocated an anti-disciplinary approach to knowledge creation, whilst gradually transforming itself from a radical theoretical school to a highly successful institutional 'brand', at the risk of dissipating the political and theoretical energy that drove the original intervention (Appadurai 1996), as I discuss below.

What problems might result from having 'too much' of a disciplinary identity? This is not a question one could have asked even fifty years ago, when the social sciences hardly existed as an institutional presence. Disciplines can risk becoming closed intellectual talking shops, where funding, prestige and influence tend to circulate amongst a narrow group of peers. A curatorial approach to knowledge can lead to professional gatekeeping, where certain epistemological challenges and critiques are kept firmly off-limits. A narrow interpretation of affiliation also serves to reinforce narrow hierarchies of academic status – within the UK the William Wyse professorship at Cambridge remains the pinnacle of disciplinary achievement in social anthropology. A finely gradated ranking of institutional prestige will be an increasingly pervasive aspect of a competitive higher education landscape. Many students and temporary lecturers struggle to get a permanent post that acts as a guarantee of professional status and disciplinary affiliation, at the risk of being exploited along the way.

How are such self-imposed restrictions sustained? Part of it comes from the appeal of all imagined communities. Given the choice, academics, like most people, tend to surround themselves with colleagues who share their predilections and habits. The intimate feuds that result are reassuringly commonplace, even if the self-referentialism of intellectual debates is less appealing or rewarding.

There are other material factors that reproduce the epistemological order – including discipline-based funding, teaching and organisational

structures within universities. Higher education policy is rarely a topic for disciplinary scholarship, unless one is an educationalist. There is much to learn from an ethnography of this policy field, and from what one might call the 'meta-professional' scaffolding that supports and surrounds academic life. For this reason, the nitty-gritty of national and institutional micro-politics – of scholarly associations, funders and universities – plays a prominent role in this retelling of the discipline's history. There remains more to do in understanding the changing nature of disciplinary identities ethnographically, situating this complex of social practices within particular institutional worlds.

I am under no illusions that a set of critically historicised insights into disciplinary pasts should necessarily loosen people's attachment to these affiliations. A sense of intellectual belonging is precious and to be cherished. At best, I hope this work demonstrates the provisional and relational aspects of these identities, highlighting the conditions through which they emerge and get institutionalised, and their political role today in an increasingly hierarchical and globally-stratified university sector.

Interdisciplinary knowledge and its discontents

The rise and rise of interdisciplinary work within the social sciences, and between the arts and sciences more generally, are the subject of constant comment, dispute and research. For some, this marks the coming of age of a set of social sciences that are increasingly comfortable in a post-disciplinary institutional landscape. For others, such forced commingling and explicit hybridity has damaging consequences.

These are not new debates. The expansion of the British university sector and the social sciences during the post-war years meant an explosion in the number of departments, with all the potential for conflict that lay therein. For one extant commentator, 'departmental organisation often reaches a condition of monstrous hypertrophy, falsifying the academic map, and bringing about the herding of teachers into pens surrounded by fences' (Carr-Saunders 1961, 8). For Briggs, 'duplication and dispersal of effort, lack of planning and co-ordination, rivalry and occasionally friction, boundary disputes and far from splendid isolation are familiar features in the twentieth-century university world ... in the modern map of learning within the universities, students and teachers in science and the humanities, literary and social studies all too often figure as inhabitants of separate continents' (Briggs 1964, 73) It was exactly this sense of 'separate

continents' that lay behind the plans for Sussex University, established in 1962, to create a series of interdisciplinary schools (Daiches 1964).

Wallerstein's work represents one pole in the debate over disciplines and their roles. Since chairing the Gulbenkian Commission's report *Open the Social Sciences* (Wallerstein 1996), he has consistently challenged a disciplinary order of things (e.g. Wallerstein 1999), claiming for example that 'the social construction of the disciplines as intellectual arenas that was made in the 19th century has outlived its usefulness and is today a major obstacle to serious intellectual work' (Wallerstein 2003, 454). In order to renew the social role of the social sciences, and to make them more useful, he has argued for major restructuring of universities. Wallerstein identifies the expansion of higher education within what he calls the 'world university system' as challenging 'dubious' disciplinary boundaries in a number of ways. He is dismissive of disciplinary 'originality' that is driven by 'academic poaching' (ibid., 455) as different subfields borrow each other's ideas and concepts. He sees disciplinary reward structures as a curb on innovation and change, limiting wholesale reforms.

Wallerstein offers the reader a thought experiment. If all the existing social science academics were merged into one large faculty and then left to regroup according to their research interests and approaches, he suggests that subdivisions would still be likely to occur, especially between those more committed to a 'nomothetic' epistemology, seeking to build general quantitative laws and rules, and those more committed to an ideographic, descriptive approach. Yet, as he points out, the social sciences are unlikely to be left to themselves. Administrative and financial rationales are increasingly driving decisions about intellectual work as departments are merged, closed or restructured. These are often driven by local and short-term agendas, such as the need to recruit students or to compete for funds. This for Wallerstein, 'militates against the emergence of the kinds of institutions that would facilitate the maintenance of world communities of scholars' (2003, 457). His grandiose and Marxist-inspired vision is for a wholesale reconstruction and reinvigoration of what he calls the 'historical social sciences', with the long-term aim of creating a 'singular epistemology for all knowledge'.

An even more influential challenge to the self-evident nature of disciplinary knowledge and practice has come from those who have identified an inexorable move to what has been called 'mode 2' knowledge production. They have advanced the hypothesis that traditional academic disciplinary 'mode 1' knowledge is increasingly irrelevant in the face of applied, trans-disciplinary and publicly engaged 'mode 2' knowledges (Gibbons et al. 1994, Nowotny et al. 2001, 2003). Whilst the strong version of this thesis has been

dismissed as somewhat over-simplistic, Manichaeic and apolitical, it has become a self-fulfilling funding prophecy. It is particularly appealing to those national governments keen to harness academic knowledge to promote national economic competitiveness.

Coming from a very different perspective Marilyn Strathern and others have challenged the assumptions that interdisciplinary work is in itself a 'good thing' (Strathern 2004). Highlighting the 'interlocking, scale-crossing complexity' of social and policy controversies driving the social sciences, she points to the way these problems increasingly ramify and spread across boundaries of discipline and skill, appearing to necessitate multi- or interdisciplinary expertise. Wallerstein's proposals for restructuring are, Strathern argues, already being carried out, driven by interventionist funders and urgent social policy concerns rather than by autonomous institutional rationales. As a result, she suggests, experts are now positioned rather disingenuously as a 'representative of his or her own discipline', and as a source of 'specialist wisdom assumed to be already in place' (*ibid.*, 2004, 5). The rush to be interdisciplinary, urged on by funders and policymakers, has for her overshadowed the invaluable internal debates and the important 'traditional' ways that disciplines evolve through theory-driven dialogues. Building on anthropology's own strength at conducting a critical self-analysis, Strathern points to the 'need to conserve the division of labour between disciplines, if only because the value of a discipline is precisely in its ability to account for its conditions of existence and as to how it arrives at its knowledge practices' (*ibid.*, 5).

Yet can academics, even in an avowedly reflexive discipline like anthropology, account for every aspect of their 'conditions of existence'? Whilst funding and policy priorities have driven changes within the disciplines, are there limits to the self-awareness and self-accounting that Strathern see as key to disciplinary practice? For the sake of academic propriety, are some things best not discussed? After all, disciplines are emotional embodiments as well as rational demarcations. Loyalty runs deep. Wallerstein points to the very real material interests – disciplinary honours, journal editorships, major research grants and public recognition – that those at the apex of disciplines have struggled hard to achieve. There are many forces that militate against change. Disciplines remain powerful organising identities. This is not just because they offer a valuable epistemological framing for intellectual work. They also represent particular conjuncture of political interests and social fields. The question about self-accounting needs to be rephrased. Can disciplines like anthropology acknowledge the way these material and structural factors shape the intellectual work that results? Can they combine a sense of intellectual provisionality and defend a necessary scholarly

autonomy? More challenging still, can they reach out beyond their institutional homes to engage a diversity of publics? The precedent set by cultural studies is one place to look.

Cultural studies and the future of disciplines

Debates about disciplinary belonging in the social sciences can learn much from cultural studies, an intriguing intellectual interloper that has reshaped both anthropology and sociology (see Peel 2005). In discussion about the origins of cultural studies at Birmingham University in the late 1960s, one of its early cohort of students, Paul Willis (see Willis 1977), laid great stress on the anti-disciplinary rhetoric being espoused by its co-founder Stuart Hall. He recalls his interview for a place at the Centre for Contemporary Cultural Studies (Mills and Gibb 2001):

I was also *very* attracted by Stuart and by Stuart's emphasis on multi-disciplinarity. As he put it, 'I'm not interested in whether you're a sociologist or an English person or whatever, Paul. What I *am* interested in is that you want to look at youth culture and music and at how young people live now.' And that seemed like a liberation compared with the very restrictive experiences I'd had at Cambridge.

Within the many histories and mythologies that have grown up around the Birmingham 'school', there is little doubt that, in its earliest incarnations, it constantly sought to fashion itself as a radical anti-discipline, rejecting the accoutrements of disciplinary practice. A key aspect of this self-fashioning was its productive juxtaposition of academic work and political activism. The aim was to insist on the politics of theory, on trying to do scholarship that 'made a difference' in the world.

Stuart Hall repeatedly shied away from writing an 'official history' about the Centre and its work. However, he has acknowledged that a key aim was 'to produce some kind of organic intellectual political work which does not try to inscribe itself in the overarching meta-narrative of achieved knowledges, within the institutions'. Yet he also admitted that 'there is all the difference in the world between understanding the politics of intellectual work and substituting intellectual work for politics' (Hall 1992, 298). Whilst the work at Birmingham was internationally groundbreaking, and has ensured that the study of 'race', gender and the social experience of class has been placed firmly on the academic agenda, it is less clear how cultural studies interventions have reshaped the political landscape of

twentieth-century Britain, or even redefined academic structures themselves.

Part of the problem was the ambitious and ambiguous conceptualisation of 'the political' by cultural studies. At best, its debates developed alongside, and in dialogue with, broader debates around feminism, multiculturalism, the democratisation of knowledge and the need to move beyond narrowly class-based politics. At worst, cultural studies took universities and their potential to enable social change too seriously. From the lofty perspective afforded by ivory towers, it was easy to over-estimate the importance of academic insights. Calls to make the social sciences more relevant, or to apply or popularise anthropology, risk a similar misplaced arrogance if they assume that university academics have a privileged understanding of social realities. Employed to teach and research within universities, social scientists are ultimately defined by their relationship to these institutions.

The social sciences today

The political economy of the social sciences in the UK has changed profoundly since the 1980s. Along with an expansion in staffing, there has been a major increase in international and part-time students, driven by a huge growth in taught master's students. In the UK, the RAE (Research Assessment Exercise) has strongly reshaped institutional and departmental priorities – often leading to a more strategic and explicit approach to academic practice and disciplinary consciousness. Repeated regularly since 1986, the RAE has led to a growing imbalance in the dual-funding model, with increasing pressure on academics to acquire grants and publish quickly. The influence of institutional financial and management protocols on academic practice is growing. Whilst institutions have also responded to funding council initiatives to enhance teaching quality, the 'research game' continues to reshape disciplinary and institutional agendas (Lucas 2006). Such policy dirigisme is the inevitable consequence of increased government funding of the sector over the last fifty years, but has led to a political culture of *ressentiment* and defensiveness within the social sciences. These attempts to measure and define academic work in utilitarian and functional terms have challenged strongly held academic vocations, even if disciplines have not always been willing or able to resist these new audit cultures. Corridor conversations can be full of frustration about the involvement of government, its funding councils and university management itself

in academic affairs, and the challenge this poses to imagined ideals of academic autonomy (Strathern ed. 2000, Shore and Wright 1999).

Other challenges to disciplinary self-identity have a demographic origin. There is an increasing pattern of postdoctoral migration between disciplinary fields in the social sciences (Mills et al. 2006). Academic staff trained in what one might call 'exporter' fields like economics, sociology and anthropology find employment in 'importer' fields such as education, management and business studies that have stronger connections to policy and practice. Such trends link to a hierarchy of disciplinary purity and status, where relatively 'closed' research fields are seen as the most prestigious. This has important implications for the funding and content of research training across the social sciences – should doctoral students be prepared for careers in their own disciplines, or for a range of possible disciplinary or post-disciplinary futures? Meanwhile, all the social sciences become 'importers' as they recruit increasing proportions of graduate students – and academic staff – from the EU and elsewhere in the world. This is particularly visible in research-intensive universities, and also challenges established disciplinary imaginaries and practice, in anthropology and beyond.

With the growth in Ph.D. production, more social scientists conduct research for non-university funders, producing knowledge that is an increasingly valuable aspect of the much-touted 'knowledge economy'. The visibility of the qualitative research methods adopted in these multidisciplinary situations is changing the public profile of social research. These changes mark a whole new stage in the debate about the application of disciplinary knowledges. They have profound implications for training, for public responsibility and for understandings of disciplinary autonomy. Other factors are also at work. A directed higher education policy environment is seeking to transform the nature of doctoral training in the social sciences to make it more relevant to the 'needs' of non-academic employers. As part of a broader shift from disciplinary pedagogy to what one critic calls 'perpetual training' (Rose 1999, 160) universities and research funders are increasingly directive about the form, content and purpose of the Ph.D. This sits uneasily with more conservative models of scholarly 'apprenticeship' that tacitly inform approaches to training, stewardship and creativity in the humanities and the social sciences.

There are limits to academic self-reflexivity. Despite the best efforts of Pierre Bourdieu, being part of the sociological object can be extremely hard to acknowledge. We can't assume that a process of making disciplinary structures and literacies more 'explicit' will in itself make academics reflective about the power they hold over students or junior colleagues. New unspoken assumptions replace

older ones. Silence may be a necessary aspect of knowledge production.

Conclusion

The evidence for an emerging post-disciplinary episteme in the social sciences is far from conclusive. The expansion of higher education within a managed market, increasing funding dirigisme, and the new policy fashion for interdisciplinary research are all affecting disciplinary cultures. Yet academics still place a great deal of value in disciplinary knowledge formations and their authorisation of intellectual traditions and possible futures. In this strange new world, disciplinary affiliations are best worn off the shoulder, intellectual garments not to be taken too seriously. A generous dose of ironic self-regard never harmed. Too much ontological security distracts attention from the material conditions of intellectual production in a rapidly changing and increasingly stratified university system.

Is it possible to predict a future for the discipline? Social anthropology's distinctive theoretical contributions look set to continue, and its networks within the UK academic establishment will help it defend its interests. On the other hand, its small size will leave it vulnerable to the whims of the managers and rationalisers of a globally-connected knowledge economy. In this corporatist model, universities and their staff will have to be increasingly entrepreneurial, and not shy away from strategic interdisciplinary engagements when the opportunity arises. Because academic communities hold the key to universities' success, reform-minded scholars are well placed to both defend and redefine their institutions and disciplines. As they do so, social anthropologists will continue to return to questions of relationships, affiliation and belonging, and to ask, 'What kind of knowledge is it?' Their role as 'difficult folk' lives on.

DISCIPLINING THE ARCHIVES

In the course of my research for this book, I stumbled across social anthropology's sacred vaults. Deep beneath the old Museum of Mankind in Burlington Gardens, London, passing through a prison-house of locked doors and passageways, lay a dozen grey metal cupboards in a bare corridor. It was not how one imagined a treasure trove to look. The corridor, painted in regulation magnolia and decorated with heating pipes, was a thoroughfare for museum staff. My guide, the archivist, was clearly embarrassed at the impoverished setting for such historical riches. For this was the archive of the Anthropological Institute (later to be granted a 'Royal' charter), founded in 1876. She respectfully approached one of the cupboards with another set of keys from the rusting sweet tin, and gently opened the doors to reveal the pride of the collection, the original Council minute books. Now more than a hundred years old, they had just been rebound in sumptuous red leather. 'Only to be read wearing dust-gloves,' I was told. I murmured my respects.

These beautiful volumes were only the first surprise offered up by this unprepossessing corridor. In the subsequent cupboards were some of the most perfectly conserved archives that I have seen. Each letter, memo or manuscript had been interleaved with acid-free tissue paper, and then inserted, a few at a time, into a protective inert polythene wallet, before being placed in a high-quality acid-free archive box. This history of anthropology would last for ever. Hardly surprising that its guardians felt that it was almost too precious to let scholars get their hands on. 'What if someone', she asked, 'found a letter that would now embarrass its writer, or that might portray them in a negative light?' I was at a loss for words. This was history as treasure: precious, sanctified and guarded. The vault doors were closed once more. There are many shelves of

material that remain to be prepared and catalogued. To their credit, the archivists have are also assembling an impressively detailed and indexed catalogue of the archive, eventually be made available online.

Such dedicated history-making presents contemporary social anthropology with provocative questions. On the one hand, it is flattering for the discipline to realise that the production of ethnography is also the production of history, as the anthropologist George Marcus (1998) notes. By this logic, every ethnographic fieldnote, every personal diary, is potentially a historical document. Yet this challenges anthropologists' strong sense of the private sphere of research. Very few have been willing to deposit such materials in their own lifetimes (Darnell 1995). Will such valuable resources survive?

This particular anthropological archive, in its very perfection, raises difficult questions about the whole purpose of disciplinary archives and their status. Archives are the key portal to historical knowledge, but how much attention do we pay to their organisation? As Derrida notes, 'every archive ... is at once institutive and conservative' (1995, 7). The questions are particularly acute when what is being archived is hidden disciplinary knowledge itself. If a scholarly discipline already acts as a form of living archive – through its published papers, its journals and monographs – what sort of 'meta-reflexivity' is required of those who create this additional level of self-knowledge? As I hope this book has shown, one's understanding of a discipline's public identity is challenged and deepened by the hidden archives of personal correspondence, administrative records, and unpublished work.

The epistemological status of the archive has received little anthropological attention. All too often the archive has been viewed as the province of the historian, umbilically linked to that discipline's sense of identity and expertise. Historians are often not ashamed to display a proprietorial attitude over 'their' archive's contents, occasionally describing the characters 'uncovered' within the archive as 'their tribe' (Bradley 1999). Yet as the concept of the 'archive' expands, it becomes more unstable, and more demanding.

Let us begin with the filing. How many of us, with an eye to our own posterity, view our filing as a significant and important intellectual practice? Do we ever consider that our variously half-hearted efforts at storing and discarding papers might one day be key ledgers in history's accounts? Derrida's work on the archive forces us to attend to the technologies and practices that go unmentioned, and how 'archivisation produces as much as it records the event' (Derrida 1995, 11).

Derrida chooses to focus as much on the power of the archive's organising principles as on its interpretation by the privileged few. Once again warning us against 'originary thinking', he is paying attention to the way that the archive is never simply memory 'as

spontaneous, alive and internal experience' (ibid., 8). Ironically, however, Derrida doesn't engage with the 'originary' thinking practised by archivists themselves. Archival practice does indeed emphasise 'original order' and the principle that the records should be maintained in the order in which they were originally kept when in active use. This risks presuming a moment which it is the archivist's role to attempt to recapture, with the archives faithfully mirroring lived experiences, treating organisations as functional 'going concerns'. Such principles, intended to prevent archivists from indulging in their own transformational re-readings of the archive's order, inevitably have to gloss over the ways in which filing and re-filing are interested and strategic acts. There is rarely one 'original order', particularly when records have been revisited and reappraised, kept or discarded on a whim.

In carrying out this research, I have had the good fortune to not only 'discover' a lost archive (a little less romantic than it sounds), but also to act as an amateur archivist myself. The Association of Social Anthropologists of the UK and Commonwealth (ASA) archive that I collected and deposited at the British Library for Political and Economic Studies at the LSE was originally a travelling archive. Few records remain from its earliest years when Edward Evans-Pritchard was its Chair and Secretary-General – he had little time or patience for paperwork. This changed with the appointment of Max Gluckman as Secretary in 1952. Gluckman was one of those invaluable fastidious people who wore his administrative responsibilities with pride. His files were carefully edited, ordered and comprehensive. One of the oldest files in the ASA archive is his doing, carefully bundled and tied together. Treasured letters come first – including a letter from Henry Levi-Bruhl, accepting honorary membership of the ASA in 1947, and a scrumpled, much reread airmail from Radcliffe-Brown in 1954, apologising for his absence from the forthcoming ASA meeting in Durham, as he did not wish to be the 'skeleton at the feast'!

The earliest records of the ASA have been cherished by its officers – collected minutes of committee and business meetings, memos about a planned reader, envisaged journals and the intractable questions of membership criteria. They have also been regularly revisited, with files from the early 1980s occasionally mixed in with those from the early 1950s. They also contain an extensive set of ephemera, from endless letters about subscription matters to chequebook stubs and bank slips.

By 1960, with the appointment of first Steven Morris and then Forge as Treasurer, a pattern had been established for the keeping of administrative records. With the appointment of each new office-holder, the previous incumbent would – perhaps with a sigh of relief – hand over his or her own papers, together with those accumulated

and passed down by his predecessors. The ASA records have never had a permanent home but always been on the move, travelling from one volunteer office-holder to another. As the files were parcelled up and sent separately around the country, two quite separate ASA archives began to develop. The two paper-chasing offices of Secretary and Treasurer produced their own records and their own filing systems. Even the packing and posting were a logistical feat, as by the early 1980s each archive consisted of several boxes' worth of papers. Eventually, John Comaroff proposed that an 'archive' of the earliest papers be created, to protect them and save them from continual transit. They were deposited in a big metal trunk in the basement of the Royal Anthropological Institute, described as the 'official archive' in correspondence between the secretary and chair, but then left, forgotten until recently. This echoes Barbara Pym's comments (1987) about one such academic treasure trove being the 'untidy old cupboard in the librarian's office'.

The fusty minutiae of this sort of administrative history are key to any archive. The documents make most sense when the contexts of their production, use and filing are understood. If there was no single ASA archive, each office-holder made sense of the records for their own purposes. Again and again, the files would have been unpacked, shelved, pruned, and reordered, before eventually being packaged once again and sent on. The complexity of the materials and the history of their production makes decisions over how to archive more difficult and more important. Yet, when I turned to the archiving profession for advice, I encountered a policy of archival realism. Seeking to try and put the materials I was cataloguing in some semblance of 'useful' order, I was firmly discouraged by several archivists. Pointing out the value of archiving principles such as provenance and original order, they felt that the materials, even in their present confused and multiply layered state, reflected the workings of the association, and were best left alone. This neo-functionalism purism at least spared me decisions about the historical significance of cheque stubs or bank paying-in slips.

Such conservatism is supported by current developments in computer searchable databases, which make it no longer necessary to physically order the materials in a logical or intuitive way. Indeed, the archive no longer even needs to be stored in one place. The index now becomes the archive, as it provides 'epistemological order' to the materials catalogued therein. At this point archival politics now focus on the design of such a database, and the way in which the catalogue is put together. To this extent, Derrida's claim that the 'technical structure of the archiving index also determines the structure of the archivable content' (1995, 11) would seem to have some validity. If an entry is not included in an index or handlist, it is no longer in the

archive. The deceptive ease of this form of archiving, indexing and searching can lead to the deposition of more and more materials, but also to the satiating image of this archive as being an ever more complete and total record of a particular institution or history. As I was reminded when doing this research, the archive could only tell one side of the story, and needed to be supplemented by oral histories, interviews and secondary sources.

Controversial initiatives to create online repositories and archives create different problems. The UK Economic and Social Research Council has realised that much qualitative research 'data' is not being 'properly' archived, and was therefore in danger of being lost. A catalogue has been created (www.esds.ac.uk/qualidata), and funded researchers are expected to offer their materials for deposition. However, despite claims (Silverman 1995) that anthropologists have a moral obligation to deposit their field-notes, very little has been deposited. Part of the problem is their ambiguous status as 'data'. If 'fieldnotes' become a recognised part of a research 'public sphere', would they then continue to be used for confessional intimacies or personal asides? Is there no boundary between the personal and professional? Anthropologists, all too aware of the power of hidden and secret social knowledge, are unenthusiastic about depositing such research materials, even if they were to be restricted or closed for years to come.

The official position is rather different. The American Anthropological Association has also adopted a 'Resolution on Preserving Anthropological Records', one part of which states that 'Anthropologists should take steps to care for the unpublished material in their possession and to make arrangements for the appropriate disposition of those materials' (quoted in Silverman 1995, 25). Silverman goes as far as suggesting that, in future, one's failure to make available research materials may well be used as a way of judging one's final results.

Marcus (1998, 57) argues for the multiple potentials of the 'once and future ethnographic archive' in its widest sense. He suggests that, in the 'realist' ethnographic archive of the present, ethnographic monographs act as the primary sources for the comparative work of others, whilst in the 'relativist' archive of the future the 'messy, constructed nature of ethnographic knowledge' becomes more obvious and open to critical reappraisal. It was in the middle of this dilemma, he suggests, that the 'authority constructed in ethnographic research and texts was caught and shredded in the 1980s critiques' (a reference to Clifford and Marcus 1986). Yet he also sees this future potential as key to a revitalised ethnographic project. Not, he hastens to add, in the positivist approach of creating cumulative comparative knowledge, embodied by the Murdock Human Relations Area Files project. Rather, Marcus points out

that anthropological obsession with record keeping, is inspired partly by professional norms and partly by the traveller's desire to register experience and observation. These are our private archives, highly personal, an extension of our anthropological selves, and rarely exposed to others. Once deposited, such personal archives 'become potentially subversive sources in relation to the claim of prestige and authority for published ethnographic scholarship' (Marcus 1998, 53). Here he is thinking of Malinowski's diaries, or the dispute over Margaret Mead's work (Freeman 1996, di Leonardo 1998).

This historicisation of the discipline, through re-studies and the use of personal archives, Marcus suggests, demolishes the authority of published materials as the sole disciplinary archive. This 'reconstituted, more complex and unwieldy' archive challenges the notion of ethnography's 'singular disciplinary achievement', and instead becomes 'a record that cannot be authoritatively ordered for any one particular vision of a discipline's knowledge quest' (Marcus 1998, 57). By this argument, seeing and working with 'field-notes' – such as those of Paul Stirling – provide invaluable insight into the anthropological method, even if the concept of the 'raw' field-note is highly problematic (Sanjek 1990).

For archival enthusiasts like Marcus, the ethnographic archive would enable the discipline to be increasingly opened up as an object for critical historical research, whilst ensuring that it remains a living scholarly community. The deposition of research materials and personal records – even if closed for many years to protect individual confidentiality – might subvert 'official' disciplinary narratives, to the benefit of those seeking to understand the conditions of academic knowledge production. Yet this sort of development may in turn, have its own risks. The very process of deposition, preservation and labelling objects as aspects of disciplinary knowledge can result in their acquiring disproportionate historical significance. This expanded 'ethnographic archive' of diaries, journals and ephemera could, against Marcus's predictions, serve to reinforce disciplinary identity. This sort of warts-and-all archive might paradoxically reassert distinctiveness and difference, objectifying and congealing the discipline as an autonomous and separable intellectual project.

The urge to archive and conserve is an important one. This book depends for its existence on numerous institutional and personal archives, and the guidance offered to me by their archivists. Yet an archive is never innocent, never neutral. The future of the university as a place for critical thinking depends on its students' understanding of and engagement with the conditions, politics and history of academic knowledge production. Such a future depends on the structure and contents of the archives that we deposit, as much as on the way they are read.

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