

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 embrace 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.