

Jeremy  
MacClancy

# ANTHROPOLOGY IN THE PUBLIC ARENA

HISTORICAL AND  
CONTEMPORARY CONTEXTS

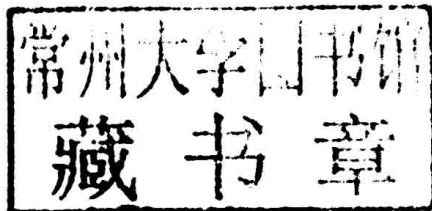


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# Anthropology in the Public Arena

## Historical and Contemporary Contexts

Jeremy MacClancy



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# Anthropology in the Public Arena

To Peter Hacker,  
and towards the memory of Rodney Needham (1923–2006),  
with my thanks

*I don't think anybody would question that it's a valuable thing to write histories of disciplines and of disciplinary concepts.*

Charles Stafford

*What do you mean nobody would question it! People have been questioning it all the time! Violently!*

Adam Kuper (1999: 12)

# Acknowledgments

I have been working on this material, in a spasmodic way most of the time, ever since my BLitt thesis in the late 1970s. My debts are extensive. I sincerely thank all those below for their generosity, though in some, lamented cases my statement of gratitude is in fact a memorial tribute rather than an acknowledgment.

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Earlier versions of chapters were given at seminars in Oxford Brookes University, the University of Sussex, the University of Oxford, the University of East London, and the ASA Decennial Conference held in Oxford. I thank their audiences for their comments.

If you think mention of anyone above implies they agree with what I have written, you are incorrect.

In 1974 Peter Hacker, the senior philosophy tutor at St. John's College, Oxford, agreed to take on a medical student who had belatedly realized doctoring was not for him. In 1976 Rodney Needham accepted a recent graduate as one of his three students for that year. To a keen but callow young man, each was, in his own way, an exemplar of academic endeavor and productivity, and of commitment to his students. Even though I was a faltering philosopher at best, and an aimless anthropologist for a protracted period, both scholars did their best to train me to think, critically.

Do not take this book as an indicator of how successful, or otherwise, they were as my teachers. Better to regard it as a prolonged statement of thanks.

Jeremy MacClancy



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# Beating the Bounds of Discipline?

## *Innovation at the Margins and Beyond*

*Social anthropology, in the course of this century, has behaved like some shops – Boots the Chemists, W. H. Smith & Son the newsagents and bookseller, for example. It has expanded, diversified, and shifted its alliances and boundaries, so that what it was first known for no longer indicates the range of commodities it stocks.*

(Lienhardt 1997: 63)

Anthropology has no bounds. It has no limits. So long as something appears to fit, however vaguely, however polemically, within “the study of man,” it can be called anthropology. That is all the etymology of the term, first used in the late sixteenth century, requires: from the Greek, *anthropos*, “man,” and *-logia*, “study of.” Let us ponder the consequences of this for a moment.

To begin with, this is not a new point. In 1903 one of the very first professional anthropologists in Britain, Alfred Haddon, stated:

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. The practical difficulty of drawing a dividing line between the legitimate scope of Anthropology and that of other studies is so great that we are often told there is no science of Anthropology. This lack of definiteness adds a charm to the subject and is fertile in the production of new ideas. (Haddon 1903: 11)

Haddon’s general point is easily demonstrated. In mid-century Britain those within the Ethnological Society of London, founded 1844, drew upon archaeological and ethnographic data in order to elucidate a single common origin for humans. In contrast members of the Anthropological Society, founded 1863, stressed

polygenism and the value of physical anthropology. These learned bodies were not mutually exclusive: a significant minority, especially medics, were members of both. In 1871 they tucked away their residual differences to form a broadly based Anthropological Institute of Great Britain and Ireland (Stocking 1987: ch. 7; Ellingsen 2001: 235–330).

In the next century, thanks above all to the efforts of Malinowski and Radcliffe-Brown, the subject taught in most British universities came to be known as “social anthropology,” to differentiate it from evolutionary, archaeological, biological, and pre- or anti-functionalist approaches. This adjectival innovation also served to draw a transatlantic line between this UK variant and its North American counterpart, “cultural anthropology.” The latter, for many, for many decades, was to be sited within “four-fields anthropology,” which included physical anthropology, linguistics, and archaeology as well. Students of Malinowski and Radcliffe-Brown were so successful at developing a distinctive approach that in 1951 the US anthropologist Murdock felt able to conclude that “British social anthropologists,” as he collectively dubbed them, “are actually not anthropologists, but professionals of another category: . . . primitive sociology” (Murdock 1951: 471–472). In response Firth seemed content with this relabeling, especially if it emphasized that the primary connections of he and his colleagues were “not with the human biologists who study physical anthropology, nor with the students of primitive technology . . . nor with the archaeologists” (Firth 1951: 477).

“Ethnology,” however, though unfashionable, did not completely disappear. It was just pushed deeper into the margins. The Pitt-Rivers Museum, Oxford University, continued to teach an unpopular Master’s in the subject until its title was changed in the late 1980s to “Museum ethnography.” When, in 1976, as a neophyte anthropology postgraduate I asked an Oxford social anthropologist what “ethnology” actually meant, he replied, to my great surprise, “To be honest, I don’t know” (P. Riviere pers. comm.). If social anthropologists allowed ethnology to drift off in an indefinable way, many of them appear to have turned their backs on folklore, though the ethnographic overlap may be very marked. In a good number of countries many anthropologists today neglect folklore studies or simply treat it with haughty disregard, to the anger of folklorists (e.g., Azcona 1984). Japan appears to be an exception (Kuwayama 2006).

In the interwar period, “physical anthropology” could mean very different things, depending on location: the German and Austrian versions had “only very little in common with its counterparts” in Britain, France, and the USA (Gingrich 2010: 375). By the end of World War II, “physical anthropology” had become so tainted with racist and eugenicist excesses that the subject was left in the far margins of academic desuetude. “Biological anthropology,” which appears to have later replaced it, exploits a far broader range of approaches; it is revealing that some of “the most interesting work” within this field has been carried out by researchers who do not call themselves biological anthropologists (Huss-Ashmore and Ulijaszek 1997: 82; MacClancy and Fuentes 2011: 17).

Stocking, a hundred years after Haddon, has written eloquently on this variability of anthropology, which he terms "multifariously constituted, variously denominated, nationally diverse" (Stocking 2001: 313). However, the phenomenon extends far wider than the West European and American examples he looks at. For instance, in revolutionary Russia anthropology was branded bourgeois, and so replaced by "Soviet ethnography," nationally bound and geographically oriented (Gingrich 2010: 358). Fascists in Italy left anthropology dormant and, for political reasons, reinvigorated folklore studies, renaming it *popolaresca* in the process (De Simonis and Dei 2010: 79). In newly independent Cameroon anthropologists, to avoid a charge of neocolonialism, presented themselves as historians or philosophers (Fokwang 2008: 133). In Titoist Yugoslavia anthropology was neglected for the sake of ethnology which, though regarded as a very general science, lacked a comparative dimension, while in post-Communist Slovenia the turf-wars over who controlled the term "cultural anthropology" became, in the words of one local practitioner, "rather comical" (Boskovic 2008a).

Eric Wolf (1964) called anthropology "a discipline between disciplines." On the evidence of the above, it appears more an evolving assortment of activities coasting among disciplines. Moreover, at this point it seems difficult to study our central theme without preempting ourselves: how to research a highly contested, fragmented, and vaguely defined pursuit without our own working definition excluding some historical dimensions, especially if their investigation might otherwise have proved fruitful? My image for this conundrum is not of a dog chasing its tail, but of a passage down a glazed labyrinth: much effort, little progress, and a lot of banging into oneself.

Perhaps the least worst compromise here is to take a plural approach: follow the work done by those who are today regarded as precursors of modern anthropology, whether or not they called themselves "anthropologists"; scrutinize the writings of those who called themselves "anthropologists," "ethnologists," or closely associated contemporary terms, whether or not they are today viewed as ancestral figures of modern anthropology; be prepared to review the work of anyone/any institution whose activity appears relevant to work done by members of either the above approach or whose work appears to fit within a definition of "anthropology." To some this would be a messy, uncoordinated style of historical method; to others it might seem a discerning eclecticism, open-ended to suggestive possibility. The point is not to make a premature decision either way, but see where researching the material leads. I wish to follow my nose, not be led by it.

As the opening examples suggest, the liberty to decide the content of the term is an apparent freedom all too open to exploitation, even abuse, by those with their own agendas, however laudable or innocuous those aims may at first seem. The logical sequitur is that if we are to gain a more exact idea of what our common pursuit is and what it might be, then the questions we need to ask, in any instance, are who is deploying what conception of anthropology, how, for what ends, and to what effect. For instance Kempney, who has written on the

history of Max Gluckman's time as the postwar head of anthropology at Manchester, states that he did not worry whether the work done by his colleagues were labeled sociology or anthropology "as long as" (and that is the illuminating qualifier here) "the research was done in accordance with the tradition that dominated the department" (Kempney 2005: 189). Similarly Meyer Fortes, who held an even more powerful position (the chair at Cambridge), propounded a nakedly self-serving definition, shortly after helping set up the Association of Social Anthropologists, which laid down professional credentials of the pursuit: "Social anthropology is what social anthropologists do" (quoted in Hart 2003). That is to say anthropology is performatively defined, by the example of institutionally powerful definers with a declamatory style. It's a closed circle.

Knowledgeable anthropologists, even those within the relatively pacific Anglo-American traditions, are very well aware that the chronicle of their scholarly practice is not one of steady development, or even saltatory evolution, but of constant dispute as rival camps seek to persuade others (colleagues, prospective students, funders, the public) of the value of their distinctive definition or approach. These intellectual protagonists strive to take advantage of anthropology's vagueness, by trying to fill it with the content and style they deem most appropriate or promising. In these circumstances we cannot speak of a simple-minded scholarly progress over time for anthropology, rather a stormy muddling through a learned terrain whose terms, divisions, and destinations change as we attempt to potter on.

In this book I seek to explore key consequences of this integral vagueness for our conceptions of what it is we do, why, and whether we should revise our practice. For, as far as I can see, failure to recognize this integral lack of limits cramps our idea of what anthropology has been, is, and could be. And an easy, effective way to demonstrate this point is through a historical investigation, very broadly conceived. In particular, I focus on otherwise neglected figures, movements, and topics within anthropology, in order to highlight just how straitjacketing the conventional histories of our practice are. I have to stress that I am not hauling the previously marginalized onto center-stage for the narrow sake of some arcane historiographical end. Rather, my aim is that reevaluation of these slighted characters, organizations, or themes may well nudge us to reconsider the frames and nature of anthropology.

This historical re-view is key because those British anthropologists with an uncritical sense of the past of their pursuit tend to be relatively unaware of why we have ended up with the discipline that we have and that our present predicament does not have to be this way. For many, the anthropology of 1950s Britain remains a "golden age," regarded as both exemplary and worthy of a mild nostalgia. What is not being taken on board here is what an unusual period that was in the chronology of our discipline and, just as importantly, in British academic life more generally. Mills, in his well-received political history of UK anthropology, sites his work into this period as an investigation into the formation of an academic

discipline (Mills 2008). My work has a related, but broader remit: to query the deployment of a notion of “discipline,” to expose the limitations protagonists attempted to impose, to urge the serious consideration of anthropologists usually thought beyond the pale, and in so doing, to recognize the fertile plurality of our common pursuit, in and beyond universities.

A common query by colleagues about this kind of work is, but what is the theoretical payoff? This sort of question starts the conversation on the wrong foot, immediately prejudicing the direction of the conversation, and thus the possibilities of positive response. It is too restrictive, prohibiting from the very beginning the consideration of theoretical approaches other than those already accepted by the interrogator. For I wish to suggest that we should contemplate the potential benefits of stepping outside the usual theoretical bounds, and be prepared to envisage the benefits of other anthropologies than the conventional. Also, it was suggested to me that an extra-academic anthropologist, such as Hocart, would be a better candidate for study than say Layard or Gorer because some of his work is still regarded as illuminating reading for anthropology students. This again is to prejudge the issue: Hocart’s work on kinship terms, for example, is considered worthy of inclusion in a course of anthropology precisely because it fits into the presently construed parameters of social anthropology. This sort of attitude, however, only confirms the contemporary bounds of our subject; it does not question or extend them. In contrast, the work of Layard, Gorer, Graves, and Mass Observation, to each of which I devote a chapter of this book, goes beyond the conventional limits of anthropology and can make us query the aims and modes of our pursuit. It is precisely for that reason that they are included.

Similarly some modern-day anthropologists, blinkered by contemporary delineations of the discipline, sideswipe the issue by a definitional sleight of hand. All those who practiced the pursuit before the emergence of a modern, university-grounded anthropology are termed “pre-anthropologists” or “proto-ethnographers,” as though the only practitioners worthy of the title are those born after the mid-nineteenth century and trained and accredited by university departments of anthropology. Herzfeld, for instance, pointedly refers to the “apparent anachronism of calling pre-19th century scholarship anthropology” (Herzfeld 2010: 290). These more blinkered of our brethren have already created their own terms of debate, without justifying their maneuver. For them, anything not recognizable as academic in today’s terms is not to be discounted, just ignored from the very beginning.

If there are key terms here I pay especial attention to they are the academic and the popular. These two may be, at different times, an opposed pair, a deeply dovetailing couple, or more simply two amorphous terms distant in some ways, overlapping in others. In places throughout this text, I examine different varieties of popular anthropologies, complementing and contrasting their contemporary academic variants, to see what the popular might offer us, to check whether these extra-mural styles should make us rethink the purposes and methods of

anthropology, broadly understood. In the process we may come to regard anthropology in a more open, plural, richer manner, of benefit to us all.

For some, the remarkably successful establishment of the history of anthropology as a worthy subfield of our pursuit was a further sign of the discipline's intellectual exhaustion. These skeptics took an academic turn into our own past to be a damning statement of our contemporary irrelevance: nothing to say about today, so let's look at all our yesterdays. Of course, it is only themselves they damn, for, in Santayana's original formulation, "Those who cannot remember the past are condemned to repeat it" (Santayana 1905: 284). A skeptic might reply that that is precisely the point of history of anthropology as conventionally taught. The obvious response is that if the past is narrowly conceived, the lessons learnt will be equally narrow; if our chronicle is highly structured, then all the more difficult to think outside those strictures.

Since the 1980s anthropologists have been only too well aware of the real threats to the survival of their discipline: swingeing university cutbacks; the rise of new disciplines seen as competitors, especially cultural, queer, and media studies; following the death of Margaret Mead in 1978, the sustained dearth of anthropologists as public intellectuals; and so on. In consequence, recent years have seen the rise of "critical," "engaged," and "public" anthropologies (e.g., Marcus 1998; Eriksen 2006; Borofsky 2011). At root all three have a common concern in reintroducing anthropology to debates broader than those which only interest colleagues. Eriksen and Borofsky are both deeply aware of the potential power of our subject to inform issues of the day in a knowledgeable, analytical, revelatory manner (e.g., MacClancy 2002a). Both are also very conscious that an essential ingredient in crossing academic boundaries is writing in an unpretentious, clear style. Otherwise there is little hope of turning public into popular anthropology. And a public activity without a populace does not make much sense.

This recent "turn to the public" by mainstream anthropologists is part of a more general shift toward making our pursuit more accountable to the taxpayers who foot our bill (e.g., Strathern 2000; Brenneis, Shore, and Wright 2005). This shift has both been imposed by government policy and welcomed by those practitioners who were always unhappy by the image of anthropologists as gathered in a close huddle and only talking to one another. This book is, I hope, an indirect contribution toward that shift, a demonstration that anthropology can be profitably practiced by amateurs and not just by the university-trained. It is at the same time a reminder that there have always been more versions of anthropology than the ones imposed by its hegemony.

Of the many different factors at play in the production of academic and popular anthropologies, two are crucial. First, organizational: universities are deliberately structured to ensure the reproduction of their key staff. Departments of anthropology reproduce themselves by producing, among other things, academic anthropologists. Popularizers do not have this recourse, but arise afresh with each new bidder for book sales and non-academic success. There are no



courses in popular anthropology, and no preexisting paid positions for them. They are, to this extent, lone stars. The second key differentiating factor is commercial: the market for academic writing is usually tiny; it rarely extends beyond colleagues, students, maybe a few others; other than the comments of academic reviewers, they are usually left to get on with the job alone. In stark contrast, the potential sales of successful popularizers are huge. They are thus frequently assisted and cajoled by a host of involved others: literary agents, friendly critics, interested publishers, and a range of other mediators who know how to gauge the market. The literary agent I worked with on a popular book about the anthropology of food (MacClancy 1992) forecast, "It'll sell OK in Britain, do much better in the States; we might get it translated into two or three languages. If it comes out in Japanese, that'll sell as much as the rest combined." He was exactly right.

In this opening chapter I present my understanding of the development of anthropology. I pay particular attention to the interaction between supposedly professional anthropologists and anthropologically informed writers, especially in the years surrounding the appearance of a recognizedly academic version of the pursuit. This may be seen, at different points, as a pitted contest or a mutually enriching collaboration between a literary intelligentsia and an intelligentsia devoted to anthropology. A similar debate was played out over the same period between men of letters and social theorists in the intellectual arena whose academic dimension came to be dubbed sociology. As its chronicler, Lepenies, points out, the consequences of that debate "are still visible today" (Lepenies 1988: 1). Almost identical comments can be made about anthropology, for the parallels between this pair of historical fora are highly suggestive.

More generally, this book should be also sited within the broad historiography of British social science, which has recently started to appear. The key questions here go beyond conventional intellectual histories and strive to discern the interlocking dynamics of rising disciplines and the social contexts within which they are arising. Historians engaged in these tasks seek to understand: how practitioners of different disciplines competed for public prominence; the evolving encounters between academic practice and contemporary literature; the usually parallel processes of professionalization and popularization; the roles of publishers and editors, and the effects of novel publishing technologies, modes of transportation, and the levels of literacy (e.g., Thomson 2006; Lightman 2007; Beer 2009; Savage 2010). It is misleading, within one's account, to prescribe in which direction the causal arrows should point: quite simply, it seems best to flesh out the effects of these developing interrelations as they appear to emerge from the data. Like the historian of Victorian popularizers, Bernard Lightman, I practice a discriminating eclecticism (Lightman 2007).

A properly rounded account of popularization would also discuss different modes of communication: books, periodicals, films, lecture tours, radio, television, museums, exhibitions, etc. (e.g., Starr 1893; Loizos 1980; Benedict 1983; Coombes 1994; Caplan 2005). In this book I have restricted myself almost exclusively to



printed modes. I accept this limitation, grounded on considerations of publishing space and personal expertise, may edge me slightly toward the elitist, i.e., toward studying those with sufficient education to read fluently, and the leisure time to indulge their habit. In mitigation, I point out later in this text that publishers made great efforts to produce cheap books as the rate of literacy grew and the numbers of people with no disposable income whatsoever declined. Further, my focus on print over other means of communication may appear somewhat arbitrary. In defense, I plead that the broader consequences of my work may transcend the circumstances of its production.

## **Anthropology for Beginners**

To make my text accessible to non-specialists, I summarize the most relevant schools of thought.

Evolutionism, which arose in Victorian times, propounds that human societies have evolved from simpler to more complex social organization. Evolutionists equated greater complexity with a greater degree of civilization and, for some, with moral superiority. Taking contemporaneous societies as evidence of these stages, evolutionists ranked different social groups around the globe. Australian Aborigines were usually placed on the bottom rung, and educated Protestant Westerners on the top one. The intermediary stages ran from hunter-gatherer societies to nomadic and agricultural ones and thence to industrialized ones. Evolutionists propounded a unilinear model: all societies had to pass through the same series of stages. They ignored the facts that some seemingly simple societies may well have evolved in their own manner, and that superficial simplicity may mask layers and modes of great subtlety. They also blithely passed over the caveat that any style of explanation which puts its protagonist and readers at the top of its tree should immediately be viewed with deep suspicion.

From the 1920s on, modern anthropologists promoted functionalism. Here a society is viewed as a complex whole. Each of its integral parts fulfils a social function. Linked together their maintenance ensures the continued reproduction of society. One of its earliest proponents was Bronislaw Malinowski, an expatriate Pole who turned the London School of Economics into a world center for anthropology during the interwar period. Thanks to his promotional efforts, long-term intensive fieldwork became the distinctive research method of anthropology. Malinowski at first proposed a biological functionalism, where each integral part was supposed to meet a biological need, intuitively derived. Obvious objections here are how does one specify these purported needs, and how can their links to social activities be persuasively demonstrated?

During the interwar years, the main competitor to functionalism was diffusionism. Led by Grafton Elliot Smith, at University College London, its proponents