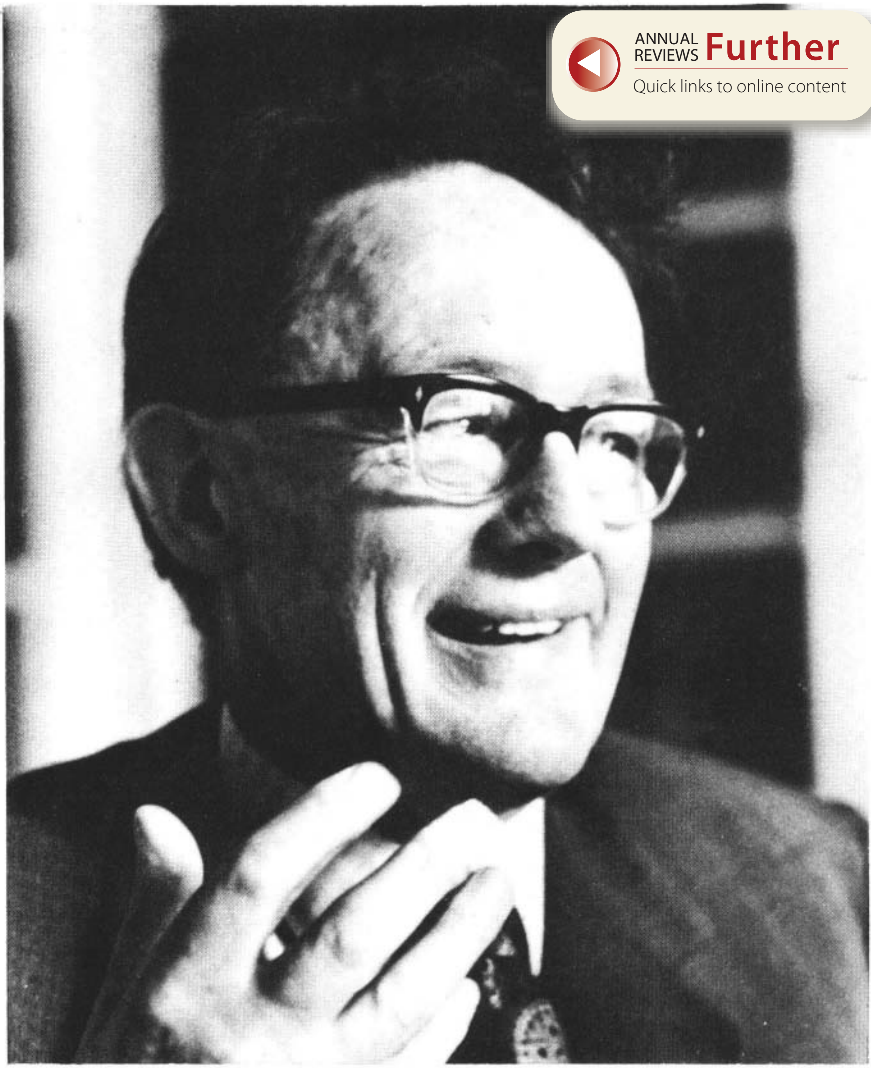

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GLIMPSES OF THE UNMENTIONABLE IN THE HISTORY OF BRITISH SOCIAL ANTHROPOLOGY

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> "Leach is one of the few British anthropologists of the prewar vintage with a 'conventional' upper-middle-class background."
>
> Kuper (15, p. 155)

It has become an established feature of the *Annual Review of Anthropology* that the opening essay should be written by a retired senior practitioner on the theme of "Anthropology in my time." I have been personally acquainted with most of the previous authors of these autobiographical essays, and two of them, Raymond Firth and Meyer Fortes, were my teachers and closest associates throughout my academic career.

This poses obvious difficulties. Fortes was my senior by only four years, Firth by nine; so if I were to stick to the standard pattern by recording my recollection of the bare facts, there would be an intolerable level of repetition.

Besides that, Firth and Fortes both started their autobiographical reflections with a reference to the charisma of Bronislaw Malinowski. Firth reported, "It is almost exactly 50 years since I decided to become a professional social anthropologist. With Ashley Montague, Evans-Pritchard, and a few others I helped in October 1924 to form Malinowski's first seminars at the London School of Economics" (8, p. 2). Fortes stated: "It was a chance meeting with Malinowski in 1931 in the home of J. C. Flugel, the eminent psychoanalyst, that eventually brought me into anthropology" (12, p. 3). It would be a perfectly legitimate autobiographical gimmick if I were to follow exactly the same line. Like Firth (an economist) and Fortes (an experimental psychologist), my initial training had nothing to do with anthropology. At Cambridge

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I had read mathematics and engineering. My first personal encounter with Malinowski was in 1937 and, on the face of it, the consequences of the resulting conversion experience were just as dire as they were for Firth and Fortes. So again there would be repetition.

But in my case that wasn't really how it happened. There was no point at which I decided to become a professional social anthropologist, nor could it really be said that my meeting with Malinowski "brought me into anthropology." It did and it didn't. Even in 1946, when I was demobilized from the army, although I had already had links with professional anthropologists for nearly ten years and had already had extensive and varied experience of anthropological field research, I was still very much of two minds as to whether to pursue the subject any further.

A valid biography of my anthropological *persona* would have to start much further back. I had already encountered Malinowski in print while still an undergraduate. This was a result of reading Russell (31), which led me, by way of Calverton & Schmalhausen (2, 3), to "Parenthood: The Basis of Social Structure" (26), which Malinowski had then described as: "the first full statement of my theory of kinship, the result of over 20 years' work on a subject to which I have devoted most of my attention."

But that too was not a critical beginning. No doubt my undergraduate experience at Cambridge had a formative influence on what I subsequently became, but this was trivial compared with influences stemming from my family and social class background, which are much harder to spell out.

One very relevant fact is that throughout my life I have almost consciously endeavored to follow in the footsteps of my great uncle, Sir Henry Howorth, K.C.I.E., D.C.L., F.R.S., F.S.A., author of a famous five-volume *History of the Mongols,* sometime President of the Royal Archaeological Institute, Trustee of the British Museum, Member of Council of the Anthropological Institute, Member of Parliament, art collector, etc, etc. But few readers of this journal are likely ever to have heard of my uncle Henry, and it would be absurd to extend the autobiographical elements in this essay to include features of that sort.

All the same, that kind of point needs to be made. As should be apparent to anyone who pays close attention to the details of David Lipset's highly perceptive biography of Gregory Bateson (23), differences of social class played a critical role in what happened in British anthropology during the first 40 years of this century, yet the two most recent monograph-scale histories of British twentieth century anthropology, by Langham (16) and Kuper (15), distinguish the protagonists only according to the qualities of their research and publications, their theoretical attitudes, and their direct academic influence upon one another. We are thus provided with only the barest minimum of information about the geographical, ethnic, family, and class background of the individuals concerned. Nor is the reader given any feel of the major intellectual innovations of the period as they were generated by such contemporary titans as Bertrand Russell and Sigmund Freud.

Such deficiencies are serious. Most of the argument about what happened in British social anthropology between 1900 and 1936 is concerned with events that took place in metropolitan England within the triangle Oxford-Cambridge-London, a region only about one third the size of the state of Massachusetts. On such a minuscule stage the relation of the actors to their social surrounding deserves close attention. Once we consider such matters, it is immediately apparent that very few of the leading characters were born in the British Isles and fewer still belonged to that exclusive "upper and upper middle" social class whose members were then alone in feeling themselves fully at home in the vacuous conservatism of Oxford and Cambridge universities. This circumstance had discernible consequences for how matters developed over time.

Being, relatively speaking, an insider, I was at first greatly tempted to try to put the record straight on this particular issue and to bring my commentary down to the present day. But I have come to see that it is simply not practical. British academics are still far too sensitive about such matters. It would cause too much offense. So the first point to be noted about this essay is that it is very incomplete. I do not refer to any living British social anthropologist who is younger than I. Nor am I at all frank about just where I fit into the social scene which I am discussing, though some of my prejudices will be obvious enough. Yet the point that I am making is far from trivial. I am saying that the sociology of the environment of social anthropologists has a bearing on the history of social anthropology.

At world level, academic anthropology has developed as a consequence of the interaction of prominent individual scholars and the cross-fertilization of their leading ideas. But these "prominent individual scholars" were ordinary human beings who had private as well as public life histories. Whatever they did or said as anthropologists was simply a "structural/metaphoric transformation" of what they did and said in quite nonanthropological contexts. There is a continuity in such matters and the particular style of an individual scholar's anthropology is meshed in with other aspects of his/her personality. Such continuities are difficult to demonstrate directly, but they sometimes show up in unpredictable consistencies in behavior. That is what this essay is about.

My purpose is autobiographical (after a fashion), but I am, quite emphatically, *not* attempting a survey of "social anthropology in my time." I ignore my juniors and even as regards my seniors and immediate contemporaries I am highly selective. I pay detailed attention only to those who have not only been my close associates but whose influence on my thinking I can clearly recognize. The arena of British social anthropology is a small world, but it is not quite as small as all that!

As a case in point it will be noted that I ignore the work and touch only briefly on the person of A. R. Radcliffe-Brown. There are two reasons for this neglect. First, I did not have any close personal contacts with R-B until the very end of his life; second, a great deal that is highly misleading has already been written about him. His case fits very well with the general thesis presented in this essay, but in order to show that this is the case, I would have to engage in a preliminary exercise of deconstruction which would here be inappropriate.

Before we come to the matter of the continuities between public lives and private lives, let me fill in some of the background including bits of my own. As to the issue of social class, I can give some indication of what I might have been writing about my contemporaries by referring to the dead rather than to the living. In this context I shall use the emotive term "aristocracy" in two different senses.

First there is what Annan (1) calls "the intellectual aristocracy," the members of a small group of closely intermarried families who came to dominate the affairs of Oxford and Cambridge (especially Cambridge) from about the middle of the nineteenth century. Members of these families are still prominent in British academia. A Huxley, who lives in Cambridge, is the current president of the Royal Society, and he is unlikely to be the last. The president of the British Academy is a Chadwick; he is a former Vice Chancellor of the University of Cambridge and Emeritus Regius Professor of Modern History. His brother, who is Regius Professor of Divinity at Cambridge, was formerly the Dean (Head) of Christ Church, Oxford. They are both scholars of the utmost distinction.

This intellectual aristocracy was never a part of the titled aristocracy in a formal sense. You do not find it spelled out in such reference books as *Burke's Peerage* or *Burke's Landed Gentry*. In origin it is mostly "upper middle class"; its original affluence derived from the Industrial Revolution of the late eighteenth century; it tended to be evangelical in religious attitude.

I shall also use the term "aristocracy" to denote the sort of people whose names *do* appear in *Burke's Peerage*. As far as the Universities of Oxford and Cambridge are concerned, the two kinds of aristocracy are not wholly distinct. And indeed, at the beginning of this century, the interests of the intellectual aristocrats who ruled the universities and of the titled aristocrats who ruled the Empire were almost identical.

Although it has been asserted several times by Fortes (11), Quiggin (29), Langham (16), and others that Cambridge academic anthropology was triumphantly established between 1898 and 1925 as the result of close collaboration between A. C. Haddon and W. H. R. Rivers, the facts, as I see them, are exactly the opposite. The most remarkable feature of Cambridge anthropology during this period was that Haddon and Rivers failed to establish anything at all.

They certainly tried hard enough. After years of campaigning, Haddon was appointed in October 1900 to a nontenured lectureship in ethnology on the princely stipend of £50 per annum; nine years later (when he had seemingly threatened to resign) this lectureship was converted into a personal readership at £200 per annum. It was the only salaried "anthropological" post in the university. In 1925, when Haddon first submitted his resignation, it was still the only such post even though the university as a whole had meanwhile been expanding rapidly in all directions.

There were complex reasons for this failure but one factor was social class. Haddon's voluminous correspondence (of which much survives) shows his limitations as a university politician, but it seems to me obvious that his most serious handicap was that he was "not a gentleman" and that he was sycophantic toward those who were.

Likewise Rivers, though himself a powerful and evidently attractive personality, was not even a graduate of either Oxford or Cambridge (he had an external medical degree from the University of London); worse still, he was the son of a speech therapist and the nephew of the notoriously ungentlemanly James Hunt, for whom the Darwin/Huxley contingent would have had no use at all. Rivers' own stammer was a constant reminder of these social deficiencies. He also had homosexual leanings, but in the Cambridge of that time that was *not* a handicap.

Although Rivers was brought to Cambridge to teach psychology (under the strange title of the "physiology of the sense organs") in 1893, his position as a university lecturer was not confirmed until 1897, and even then a commentator in the university senate said that the appointment was "a ridiculous superfluity." Haddon's appointment to his readership provoked a similar outburst; it was declared to be "the most reckless and culpable waste of money that could possibly be imagined." Rivers was not elected into a fellowship in St John's College until 1902, despite the fact that at this period nearly all university teachers were Fellows of colleges. In this respect Haddon fared rather better. He had been a member of Christ's College as an undergraduate, and he was made a Fellow as soon as he was appointed to his university lectureship.

Eventually Rivers came to be greatly respected in his college, and he was a well-known man of affairs in the public arena in London where he dabbled in left-wing politics. He was recognized as a distinguished scientist, but he never had a significant influence on developments in Cambridge University. During the Rivers era "psychology" fared little better than "ethnology."

No doubt the "aristocrats" were polite enough in public; indeed, Horace Darwin (the engineer son of Charles) collaborated with Rivers in research into visual illusions. But somehow, when it came to university politics, nothing ever happened. And in Oxford also, despite the honors accorded to Tylor in his old age, anthropology remained a nonsubject. Tylor was not a gentleman.

But at the London School of Economics, an upstart institution created as a platform for radical Fabian ideas, Edward Westermarck began to teach a rich combination of theoretical sociology and fieldwork-based social anthropology as early as 1904. After that all the significant developments in social anthropology which occurred in Britain during the first quarter of this century were focused around the L.S.E. And, by feedback, the greater the successes of L.S.E. anthropology [or, for that matter, University College (London) anthropology as sponsored by Elliot Smith and W. J. Perry] the less likely it became that the conservative Establishment in Oxford and Cambridge would touch the subject with the end of a barge pole.

Cambridge anthropology survived between 1900 and 1925 only because Haddon's extremely marginal post was attached to the Museum of Archaeology and Ethnology, which was created and patronized by a group of very wealthy, very "upper crust" amateur collectors of ethnographic curiosities led by the redoubtable traveler, Baron Anatole von Hügel.

What I have said here is not intended to belittle the historical importance of Rivers' contributions to both anthropology and psychology. My point is simply that any history of British developments in these fields needs to take into account not only the overwhelming dominance and academic prestige of Oxford and Cambridge but also the conservatism and social arrogance of those who were effectively in control of these two great institutions during the early part of this century. Haddon and Rivers were fighting to gain recognition in a most hostile environment and they were losing the battle.

Unless this background is taken into consideration, the self-advertisement that is scattered through the private papers of the principal protagonists is likely to be quite radically misconstrued. In the case of social anthropology this is exactly what has happened.

I could cite a long list of highly cogent examples, but one will suffice. Langham (16) purports to demonstrate that there was a "Cambridge School of Social Anthropology" created by Rivers and Haddon which, particularly in the period 1920–1926, made a series of major contributions to the general theory of kinship. One member of this supposed school was T. T. Barnard, whose academic prowess was vouched for by Haddon and who held office as Professor of Social Anthropology in Cape Town from 1926 to 1934. He died in 1983.

Besides numerous cross-references, Langham devotes a special four-page section of his book (pp. 208–12) to Barnard's work. The content of these pages derives almost exclusively from Barnard's personal reminiscences. And why not? Surely the recollections of a former holder of the Cape Town Chair can be treated as a contribution to the history of social anthropology? Langham does not tell us anything about Barnard's background, which he perhaps considered to be irrelevant. But even by Cambridge standards, the level of Barnard's aristocracy was remarkable.

Through his father he was a direct descendant of the great eighteenth century Prime Minister Sir Robert Walpole, first Earl of Orford; his mother was a Lambton from County Durham, "one of the oldest families in Britain," with a lineage stretching back into the shadows of the eleventh century. His wife was a Byng, a descendant of Torringtons and Straffords. Barnard himself, besides being an officer in the Coldstream Guards in both world wars, had been educated at Eton, Christ Church (Oxford), and King's College (Cambridge). None of this can mean much to those who do not know the system. Let it suffice to say that where titled aristocracy rates as an asset rather than a liability, Barnard's qualifications would be hard to beat!

From all accounts Barnard was a very nice man and an enthusiastic amateur botanist, but he did not know any anthropology. He only published two very minor, very derivative papers in the course of his whole anthropological career. When he got the job in Cape Town he hadn't published anything at all. But he had disqualified himself from becoming A.D.C. to the governor of South Australia by getting married, so he was made professor in Cape Town instead! How could this happen?

Its seems simple enough to me. Jan Smuts, the Prime Minister of South Africa, was an Honorary Fellow of Christ's College where Haddon held his fellowship. Smuts had established Radcliffe-Brown (then plain Mr. Brown) as Professor of Social Anthropology in Cape Town in 1920 on Haddon's advice. He would have consulted Haddon again when Radcliffe-Brown left for Sydney. Isaac Schapera, who later succeeded Barnard in the chair, was then a graduate student just about to leave for London to take a PhD under Malinowski. Schapera lent Barnard his notes on Radcliffe-Brown's lectures, and Barnard used these as the basis of his own lectures for the next eight years.

It may be argued that gossip of this sort, however well it may be attested, contributes nothing at all to our understanding of what happened in Cambridge anthropology in the 1920s. But I do not agree. The astonishing rapidity of Barnard's promotion (on the basis of ultra high social class status but almost zero knowledge) as compared with the nonrecognition accorded to the work of Rivers and Haddon speaks volumes about "the Cambridge system." It also serves to negate the whole of Langham's argument about the intellectual distinction of his posse of Cambridge graduate students.

I do not want to be misunderstood. This is in part an autobiographical essay. I am not myself an aristocrat of either variety. I am not trying to argue that my titular distinction is no more than a reflection of social class background. Rather I am saying that the history of British social anthropology as viewed by participant observers is quite different from the same history as viewed by nonparticipant observers, and further, that even among participant observers there are several different categories. The "insiders" and the "outsiders" participate in quite different ways.

After 1920 things began to change but only very slowly. From 1924 on almost all the Oxford and Cambridge graduates who, for one reason or another, found themselves interested in "social anthropology," migrated to London to sit at the feet of Bronislaw Malinowski. They included Edward Evans-Pritchard, Camilla Wedgwood, Audrey Richards, Monica Hunter (later Wilson), and Gregory Bateson.

I find it significant that three of these individuals, Wedgwood, Richards, and Bateson, belonged to the "intellectual aristocracy" and that two of them were women. Their migration reflected not simply the attraction of Malinowski but their cumulative aversion to the stifling Cambridge social atmosphere. Lipset (23, p. 132) cites contemporary evidence for Bateson's case; other evidence is circumstantial. It is on record that Wedgwood was considered to be one of the liveliest members of Malinowski's seminar. At Cambridge she had been a disciple of Rivers and had become fascinated in the kin term systems of Melanesia. After graduation Haddon gave her work appropriate to her female status; she was employed in measuring skulls and writing labels for museum artifacts! In London she was treated as a human being.

At this period Cambridge had an official policy of complete sexual segregation. There were two women's colleges and the women undergraduates took the same courses as the men, but their names were always listed separately and the degrees which they obtained were not formally recognized. Some members of the teaching staff refused to lecture if women were present.

But this system was under strain. In 1925 J. B. S. Haldane, an ultra aristocrat in both my senses, was dismissed from his university readership for committing adultery with his future wife (4, pp. 73–77). After much publicized legal proceedings he was reinstated. The case had the long-term consequence that the six guardians of the university's morality (the Sex Viri) now number seven (the Septem Viri) and have never subsequently been required to adjudicate a case! But at the time, Haldane's supporters were in the minority.

All this bears on my problem for, in this regard, as in others, Cambridge anthropology, such as it was, had been consistently conformist. Although "fertility" is one of the central themes of Frazer's *The Golden Bough* (14), human sexual intercourse is mentioned only as a magical procedure for improving the crops! But Malinowski had published *Sex and Repression in Savage Society* in 1927 (24), to be followed by *The Sexual Life of Savages* in 1929 (25). Both books were promptly classified by the Cambridge university librarian under "ARC," which meant that they could not be read without a special authorization from a senior college official! This further encouraged college tutors in their common belief that anthropology was not a proper subject for undergraduates to study at all.

I was an undergraduate at Cambridge from 1929–32. The majority of my contemporaries, not only in my own college (Clare) but in other colleges also, had been selected from a very limited range of private schools on the basis of personal recommendation rather than any obvious merit. Their common quality was that they were undistinguished and indistinguishable, though the more intellectual among us were almost all of a radical, near communist, political persuasion. We were already coming to hate the social rigidities of the system in which we had been reared, the injustices of which were visible on every side. By comparison with the present generation of Cambridge undergraduates, we were very politicized. We had no use for compromise.

The fact that in 1929–1932 Hitler was just coming into power added another dimension. We thought that we could recognize the encroachment of a "fascist mentality" in every aspect of British life, as evidenced, for example, by the reaction of the ruling class (to which we ourselves belonged) to the General Strike of 1926. Some became activist leaders of the political left. A few years later many of my contemporaries joined the International Brigade in the Spanish Civil War. At least one communist scion of the Cambridge intellectual aristocracy died in that vain defense of socialist democracy as we believed it to be. Recent revelations about the Russian recruitment of spies from among upper class Cambridge undergraduates in the 1930s is part of that same story. J. B. S. Haldane (see above), who by then was an avowed Marxist, was a kind of culture hero.

Official attitudes notwithstanding, the sexes were not effectively segregated, but the goal of sexual liberation and the permissive society was a novelty and in high fashion. Commentaries on the work of Freud, Jung, Adler, and other renegade psychoanalysts were to be found on the bookshelf of every undergraduate who saw himself as a member of the intelligentsia. A clandestine study of Malinowski's writings could be a part of that pattern along with the liberationist propaganda of Bertrand Russell.

Only a minority took their academic studies seriously, but those who did so did not bother to investigate "soft options" such as ethnology and social anthropology. I doubt if I even knew that such subjects were available for study. I myself read mathematics and engineering, ungentlemanly but tough. I ended up with a First Class Honours degree. I never subsequently practiced as an engineer, but my engineering background has influenced all my anthropology. I tend to think of social systems as machines for the ordering of social relations or as buildings that are likely to collapse if the stresses and strains of the roof structure are not properly in balance. When I was engaged in fieldwork I saw my problem as trying to understand "just how the system works" or "why it held together."

In my own mind these were not just metaphors but problems of mechanical insight; nor was it just make-believe. To this day, in quite practical matters, I remain an unusually competent amateur mechanic and retain an interest in architecture which is much more concerned with structural features of design than with aesthetics. The contemporary fashion by which the mysterious "cognitive" relationship between mind and body is modeled as a complex pattern of computer programs all running in parallel may turn out to be nonsense, but it is very congenial to my way of thinking. I had learned to work with binary arithmetic before I had ever heard of computing or of Saussurean linguistics. I recall that when, in 1961, I first encountered Jakobson's system of phonological distinctive features my inner reaction was: "Ah! I have been here before!"

My engineering background also effected the way I reacted to Marxism. Marx had used an architectural metaphor for the structure of society. He wrote of *Basis* and *Überbau* where his English translators have "infrastructure" and "superstructure." But Marx was not an engineer. His metaphors disguise the fact that even with perfect foundations *(Basis),* the long-term stability of the "superstructure" of a building may be highly precarious.

My concern with design stability does not mean that I am unmoved by the aesthetics of great architecture, but it adds a dimension which less numerate observers probably miss. My private use of the concept of "structure" in social anthropology is thus different both from the usage developed by Radcliffe-Brown and Fortes (where it simply refers to the skeletal framework of society without any consideration of design features) and from Lévi-Strauss's transformational usage, which borrows from Jakobson's phonology, though my engineer's viewpoint is much closer to the latter than to the former.

But in becoming a rude mechanical I did not cease to be a snob. George Homans (himself an American aristocrat by birth; his mother was an Adams) once explained the peculiarities of the Boston Unitarians by saying that while all sects of Calvinist origin assume that God has ordained a predestined distinction between the Elect and the Damned, the Unitarians are so certain that they themselves belong to the Elect that they never bother about the Damned. And that has been, very broadly, the position of the academic inhabitants of Oxford and Cambridge Universities throughout my lifetime. We know we are the Elect. What happens elsewhere is of no importance whatsoever.

Today there are 46 universities in Great Britain and Northern Ireland. Of these, the universities of St. Andrews, Glasgow, Aberdeen, and Edinburgh were all established before 1600; University College (London), King's College (London), and Durham are all "pre-1840" foundations. Yet the unique standing accorded to "Oxbridge" persists. It goes far beyond self-esteem; the preeminence is taken for granted.

The staff and students of "lesser" institutions are simultaneously both contemptuous and envious of the "the two senior universities." The contempt appears repeatedly as a form of words; the envy is shown by deeds. Although the social class composition of the "Oxbridge" intake is now entirely different from what it was formerly, a huge proportion of the star performers among all young people applying for university entry will still put an "Oxbridge" college as their first choice among the alternatives on offer. And the same attitude prevails among academic staff.

In 1900 Haddon resigned from a full professorship (in zoology) in Dublin in order to take up his ill-paid, nontenured lectureship in ethnology in Cambridge; 50 years later I, too, gave up a readership in London University in favor of a lectureship in Cambridge, a change involving a substantial drop in salary. The pattern persists. Every post advertised either by Cambridge University or by a Cambridge college is likely to attract applicants who are already in positions which are better paid and notionally superior in status to that which is on offer.

The behavior of Malinowski's L.S.E. coterie must be seen against this background. In the 1920s and 1930s the L.S.E. was a very low status institution. Up to a point it was proud of its radical unorthodoxy, but as a part of its efforts to achieve respectability (which were ultimately very successful), the politics of the place were steadily moving to the right. Most of the genuinely British staff, for all their posturing, would have dearly liked to be able to transfer to Oxbridge, but the "British" connection of most of Malinowski's pupils was tenuous.

With varying degrees of enthusiasm and varying degrees of success, Malinowski, Firth, Schapera, Fortes, Nadel, and the other "foreigners" who were mainly responsible for the high prestige that was attributed to "British" social anthropology in the 1950s and 1960s (at least in the assessments made by anthropologists from other parts of the world) eventually assimilated themselves into the life style and cultural conventions of Oxbridge academics, but they remained "outsiders" with a highly ambivalent attitude toward the values of their adopted academic milieu. This ambivalence is both reflected in and a reflection of their approach to the study of anthropology.

Meyer Fortes can serve as an example. He never for a moment sought to repudiate his basic social identity as the son of an impoverished South African Jew of Russian descent, yet in reaction to the social class hierarchy of British Jewry, he frequently made the improbable claim that his family were of Sephardim origin. This is quite consistent with the fact that, except for a period during World War II when he returned to West Africa, he was associated with the faculty of either Oxford or Cambridge from late 1939 until his retirement. For the last 31 years of his life he lived in Cambridge as a Professorial Fellow (later Honorary Fellow) of King's College, an institution founded in 1442.

When he arrived from Oxford in 1951, King's College was still a bastion of British upper-class values of the most archaic kind. Every detail stood in glaring contrast to the mixture of values to which Fortes had been acculturated in his South African homeland. Yet right from the start he was delighted. He gave the impression that he "venerated" King's and Cambridge. The fact that, as of right, he now had a College Fellowship meant that that at long last he had a definable status in what he most admired, an inflexible and enduring "social structure."

But if "veneration" is the right word, it was the veneration of an outsider. In relation to the College, he was, as he had been during his fieldwork in West Africa, an acute "participant observer." But fieldworking anthropologists do not ordinarily seek to intervene in the affairs in which they participate; and so it was with Fortes in King's. He was much liked and respected by his colleagues, but he never played an active executive role in College affairs. Indeed, to a quite disconcerting extent, he never seemed to understand how the system really worked or just why such an archaic construction should have failed to collapse long ago. He was *not* an engineer!

For the last 22 of those 31 years I was also a Fellow of King's College and for 12 of them I was, as Provost, head of the institution. At first I too was viewed by the Old Guard as an outsider; I had not been at King's as an undergraduate but at Clare College, which is immediately next door! However, after a while, I was allowed to come over the wall.

It has been argued with some justice that the changes that took place in King's College during my provostship were more drastic than any that had occurred during the whole of the previous 530 years of its history, the most notable being that the College began to admit women. Those changes were only marginally of my making, but certainly I was much more than just a participant observer; I was actively involved in what was going on. I could be actively involved in this way because, unlike Fortes, I was (more or less) an "insider" not an "outsider," and because, most certainly, I did not in any way "venerate" the archaic rigidities which I and my coconspirators were seeking to undermine.

I believe that the differences to which I have here drawn attention are reflected in the respective styles of social anthropology adopted by Firth, Fortes, and myself and other prominent "British" social anthropologists as well.

First of all there is the very general point that the British-born were trying to get away from a homeland which they found archaic, whereas the "foreigners" were looking for a new, idealized homeland that would offer a kind of stable respectability which their own original homeland lacked. Schapera, Hunter, Fortes, and Gluckman were all from South Africa. Is it too fanciful to suggest that the prominence that several of these authors were later to give to the notion of homeostatic social equilibrium and to the belief that social structures persist even when there are drastic changes in cultural appearances derived from their personal need for a stable homeland?

But let me be more specific and more personal.

Firth was a New Zealander. The New Zealand of his youth was certainly not a "socialist" society, nor was it in any way "unstable." It had the merits of a quiet provincialism. The values were egalitarian, do-it-yourself, rational; but it was out of the swim. When Firth first came to England at the age of 23 he was clearly fascinated by the aesthetic resources of the metropolis and of Europe generally, but the irrational snobberies of the English upper-middle class must have seemed both alien and bizarre. Yet for New Zealanders of that time Great Britain was still "home"; a homeland of tradition; cosy, perhaps out of date, but still worth preserving in a fossilized state.

Today, at the age of 82, Firth is the unchallenged "senior elder" among British social anthropologists. Over the years his influence upon how the subject has developed has been immense, but it owes nothing whatever to any "Oxbridge" connection.

Firth has never involved himself in British national politics, but his general stance has been consistently that of a moderate conservative. As in his anthropology, he displays a formal interest in the way society changes over time but tends nevertheless to view such changes as superficial. Certainly he has never shown any enthusiasm for change for its own sake.

In academic argument he has been consistently skeptical about all forms of reductionist generalization. He has never allowed himself to use ethnographic detail simply to exemplify a proposition which he has arrived at by a priori reasoning; the argument grows out of the evidence which is presented in massive detail. The enthusiasm for "theory" isolated from empirical evidence which is often displayed by Oxbridge academics and by their Parisian counterparts arouses Firth's undisguised contempt. Here at least he shows himself a true follower of Malinowski. Firth actually lived in Cambridge during much of World War II (when the L.S.E. had been evacuated to Cambridge), but he kept his distance.

This is all of a piece with his private passion for Romanesque art and architecture, a style in which extreme intricacy of decorative detail is fitted to structures of almost brutal solidity and which stands in sharp contrast to the mathematical elegance of the gothic architecture of later centuries.

On the other hand, despite Firth's avoidance of the abstract rhetoric of national politics, he has been a lifelong do-it-yourself politician of quite another kind. He has consistently displayed a deep commitment to the preservation and development of the academic discipline of social anthropology, and his achievements in that area have been very remarkable. From the 1940s to the 1960s he had a wide variety of personal, but quite informal, ties with senior civil servants in key positions. He used these contacts with outstanding skill.

At the beginning of the century, Haddon had had similar objectives, but he wasted his energies organizing futile official delegations from the University of Cambridge to Ministers in Whitehall (which produced no results at all except some platitudinous remarks from Prime Minister Asquith about the potential value of anthropology for the future of the Empire). By contrast, Firth went behind the scenes and talked with the people who really mattered. He got results. Considering the tiny scale of the whole enterprise in Britain of the 1950s, the central funding of social anthropological research was quite disproportionately generous. It was a phase which only endured while Firth was at the helm at the L.S.E. All of which, it seems to me, belongs to the same pattern as Firth's contributions to the subject matter of social anthropology itself.

Firth's published output has been very large. It is widely dispersed and covers many fields, but by far the major part has been devoted to meticulously detailed accounts of the ethnography of what Firth himself calls "traditional" Tikopia society, though the observations thus recorded were made at various dates between 1928 and 1973 and some of them in places other than on Tikopia itself (9, p. 219). This use of the word "traditional" reflects an underlying presumption, shared by nearly all anthropologists of his own and earlier generations, that until the coming of the white man, primitive society everywhere had been in a state of Arcadian stability if not of Arcadian bliss.

It has always seemed to me that Firth has rather similar feelings about England. "Traditional" England was what he first observed when he arrived from New Zealand in 1924. In objective terms it was a society undergoing rapid disintegration, but Firth seems to have perceived this total mess as an intricate variety of cultural detail grounded in foundations of great stability. He still gives the impression of believing that the changes that have taken place subsequently are only on the surface.

Even more striking is the way that Firth's performance as an academic politician fits in with his anthropological commitment to the concept of "social organization." This expression was the title of a well-known posthumous textbook (30) by Rivers (heavily edited by Perry), which first appeared in print in 1924, the year in which Firth "decided to become an anthropologist" (see above), but in Firth's private academic language it acquired quite a new meaning.

The expression recurs throughout his writings, though the personalized version only emerges in Firth's 1949 paper (7) which formed part of a *Festschrift* offered to Radcliffe-Brown on his retirement from the Chair of Social Anthropology at Oxford, of which he had been the first holder.

At this stage in the development of British social anthropology, the "structural-functionalism" of Radcliffe-Brown had been given a great boost by the formal simplicity with which the basic theory of segmentary lineage structures (which was barely distinguishable from Durkheim's concept of mechanical solidarity) had been exemplified by Evans-Pritchard (6), by Fortes & Evans-Pritchard (13), and Fortes (10). Firth's paper is a thinly disguised attack on this "Oxford" position. He is saying that real life behavior is not determined by formal structural arrangements and segmentary oppositions. Real life is a matter of ad hoc improvisation, of getting things fixed up by your friends regardless of what the formal rules may say.

In Firth's usage the "social structure" (in this Oxford sense) provides no more than the formal stage upon which social action takes place. If we are to understand what actually happens, we must take account of the "social organization," that is to say, the way in which individuals play the game of local politics in order to maximize their individual private satisfactions.

If my own approach to social phenomena has been that of an engineer, a concern with how the machinery works, Firth's style has been consistently that of a laissez-faire economist: each individual, in competition with every other individual, is presumed to be acting so as to maximize his/her personal satisfactions, subject only to the formal constraints imposed by locally accepted cultural conventions.

It may be that personal rivalries were in the background, but I find it significant that this opposition between Evans-Pritchard and Fortes on the one hand and Firth on the other should first have become explicit as an antithesis between an Oxford and an L.S.E. approach to matters anthropological.

Later, when Evans-Pritchard broke away from Radcliffe-Brown's "structural-functionalism" in favor of an idealist style derived from Mauss and Hertz and Van Gennep (which was not really very far removed from the structuralism of Lévi-Strauss), the rift in the Oxford-London axis became even more marked. But by this time it was a triangle, Oxford-London-Cambridge, rather than a bipolar opposition. But that must be a story for some other occasion. For the moment let me stick to the contrast between Firth and Fortes.

Firth was the senior by five years, but they were both introduced to the English academic scene as outsiders through the medium of Malinowski's L.S.E. seminar. In both cases this introduction was prior to their respective engagement in field research in Tikopia (Firth) and Taleland (Fortes). The social adjustment that then ensued was in itself a kind of "dry run" of a field research experience.

If that is a fair statement, then it is clear that the two men reacted to their initiation in very different ways. Firth fell in love with the L.S.E.; he joined the staff at the first available opportunity (in 1932) and remained there for the rest of his career. He was quite unimpressed by the pretensions of the Oxbridge setup. Fortes, though briefly on the L.S.E. staff after his return from the field, moved on to Oxford at the first opportunity, even though prospects of secure employment were very bleak. Even at that date (1939) he seems to have been attracted not only by the formality of Radcliffe-Brown's highly abstract concept of social structure but by the archaic rigidity of the Oxford academic environment, though one ironical consequence of this rigidity was that Fortes was never offered a College Fellowship.

The "social structure/social organization" debate reflects this difference in basic attitudes as does the contrast in style between Firth's writings about the Tikopia and Fortes's writings about the Tallensi. Neither author had the gift of literary elegance, but whereas the reader of Firth finds himself in a trackless jungle of ethnography which abounds in botanical rarities of the most exotic kind, the ethnographic terrain inhabited by Fortes's Tallensi appears as a product of well-ordered landscape gardening from which most of the botanical exotica have been excluded.

From Firth we learn a vast amount about the details of the "traditional" Tikopia way of living, but somehow the ordering of Tikopia society remains obscure. From Fortes we learn a great deal about the formal ordering of Tallensi society, but only in bits and pieces do we get an impression of what "traditional" Tallensi life was really like.

In terms of my engineering metaphor, Fortes describes the social machinery and its component parts but is unconvincing when he tries to explain how the system works. Firth gives us an instruction manual for operating the machinery, but he does not tell us what the bits and pieces would look like if we took it apart. Or to pursue my art and architecture model: it is wholly appropriate that Firth should be entranced by the highly decorated solidity of the Romanesque Cathedral at Conques and that Fortes should have been overawed by the symmetrical Gothic fragilities of King's College Chapel. At both levels my personal taste has repeatedly led me into conflict with both my teachers.

There can be no point in pursuing any further this comparison by nuance, but what I am getting at is that if we consider the membership of Malinowski's L.S.E. seminar and the leaders of British academic social anthropology who emerged from it, then in various ways the individuals concerned, notably Malinowski himself, Firth, Evans-Pritchard, Schapera, Richards, Fortes, and Nadel, present us with an interesting spectrum of personal characteristics which are reflected in their respective contributions to anthropology. One of the component variables in this spectrum was "Englishness."

At one extreme stands Evans-Pritchard, a very English Englishman despite his Welsh name, educated at the ultraprestigious Winchester College (founded 1387) and Exeter College, Oxford, where he read history. At the other extreme stands Nadel, British only by naturalization, remaining always at heart a very typical Viennese Jewish intellectual whose wide-ranging interests in musicology, psychology, and philosophy were prior to those in anthropology. In between we have Richards, true English but handicapped by the prevailing prejudice against women academics; Firth, near English but a "colonial"; Fortes and Schapera, both South African Jews; and Malinowski himself, a permanent Central European who was very much aware from the start that his potential audience was much wider than the inward-looking parochial community of British academia. He liked to claim that he was a Polish aristocrat.

Evans-Pritchard was a "true scholar" in a typical British sense, never seeming to be altogether serious, making a point of carrying his very considerable learning lightly. Nadel was equally a "true scholar" but typical in the Germanic sense. He was at all times deadly serious.

Where Evans-Pritchard eventually came to think of social anthropology as an art rather than a science, Nadel was deeply committed to the view that anthropologists are engaged in a scientific discipline which is capable of revealing facts of (social) nature in much the same way as experiments in physics reveal the facts of physical nature. In this antithesis I am quite emphatically on Evans-Pritchard's side, just as my "Englishness" is unqualified by either gender or birthplace.

Nadel's ethnographic studies of the Nupe and the Nuba still stand out as landmark contributions to the field, but it is surely most unlikely that anyone other than an historian now tackles *The Foundations of Social Anthropology* (27) or *The Theory of Social Structure* (28). Nadel himself tells us that he had originally planned that the former work should bear the title *Prolegomena to the Study of Society: Being an Inquiry into the Nature of Sociological Knowledge*. Presumably his publisher had rightly diagnosed that any English reader would be inclined to treat the whole idea as a joke!

Between the arts man and the theoretical scientist we have the no-nonsense fieldworkers for whom "theory" must always be subordinated to direct observation. The crucial feature which linked Malinowski with his closest followers was a down-to-earth empiricism and a marked skepticism concerning any kind of generalization which might suggest an antipositivist idealism. In the background were behaviorist theories of psychology as viewed through the philosophic attitudes of Bertrand Russell and the logical positivists.

It was not until the mid-1950s that, under the influence of the later Wittgenstein and Oxford linguistic philosophy, British social anthropologists began to show a serious interest in ideas rather than in behavior. This shift was initiated by Evans-Pritchard though the presence of Louis Dumont in Oxford for several years was also relevant, as was my own heretical idealism which had a variety of sources. It was certainly tied in with my early grounding in mathematics, though mathematicians are not necessarily of an idealist inclination.

So perhaps it is time that I turned to autobiography. Needless to say, I hold that I have been misunderstood. I must admit that Kuper's recent book (15), which is a substantial revision of an earlier work, provides on pages 155–65 a perceptive summary of my theoretical standpoint regarding the empiricist/idealist dichotomy, but even so, he is only the most prominent among a number of commentators upon the post-1945 British social anthropological scene who have claimed that I was responsible for introducing into British social anthropology a novel style of "structuralism" which derived from Lévi-Strauss rather than Radcliffe-Brown. This is not how it has seemed to me.

Derivations are never clear, but my article on Jinghpaw kinship terminology (17), which Lévi-Strauss himself recognized as having some kinship with his own work (22, p. 525), was completed in wartime Calcutta in 1943 at a time when I had never heard of "structuralism" of the French variety; nor had I encountered the work of Roman Jakobson. But Jakobson's subsequent fascination with Norbert Wiener's *Cybernetics* (34) and my own fascination with Jakobson's phonology represent a convergence of ideas. I never had the makings of a true mathematician, but I was mathematically literate. I learned about "transformational" theory (in the form of advanced algebra and the nineteenth century developments of projective geometry) several years before I entered Cambridge as an undergraduate. If some of my anthropological work is "structuralist" in style, it is for that reason.

Moreover, when I switched from pure mathematics to engineering as an undergraduate, I developed a bias against abstract theory as an end in itself. It is quite true that later on, as an anthropologist, I was often exasperated by the obsessive empiricism of Firth and Fortes and several of Malinowski's other pupils, but I never came close to sharing Lévi-Strauss's view that theory is the only thing that matters, and that if the ethnography does not fit, it can simply be discarded.

It was precisely on that issue that my own relationship to French structuralism became defined. I first became interested in the work of Lévi-Strauss because the Kachins of North Burma provided his type example of a "generalized exchange" marriage system (21, Chap. 15, 16). I was intrigued by the fact that while his theory, in an odd sort of way, fitted some of the facts on the ground (with which I was intimately familiar and Lévi-Strauss was not), there was a very wide discrepancy between the details of the ethnography and what Lévi-Strauss had supposed to be the case. Afterward I was shocked to discover that he himself was in no way put out by these discrepancies; he blandly assured his readers that my ethnography must be wrong! From then on I knew that when it came to the crunch I was just as much an empiricist as any of my British colleagues.

My book *Political Systems of Highland Burma* (18) was published in 1954. It was certainly my most influential work, though much that it contains has been seriously garbled by my critics. I had moved from the L.S.E. to Cambridge in the autumn of 1953, though the book had been written some while before that. Its "idealist" standpoint provoked hostile comment in both localities. Firth's anxieties about the direction in which I might be heading are apparent in his "Foreword." The notion that the persisting element in social relationships is a patterned structure of verbal concepts (which are open to very diverse interpretations) rather than a patterned structure of empirically observable "groups" knit together by mysterious "ether" called "kinship" was as repugnant to Fortes in 1980 as it had been in 1954.

My next fieldwork monograph, *Pul Eliya: A Village in Ceylon* (20), did nothing to improve matters on the home front, for while it conformed to structural-functionalist dogma in providing a great deal of formally organized ethnographic detail, it argued quite explicitly that kinship is not a thing in itself. I argued that, in the case of my Sri Lankan peasants, the material that Fortes and his pupils were wont to discuss under the heading of kinship was simply a way of talking about rights in land and water.

This book appeared in 1961, which was for me a kind of watershed. In the same year I brought out a book of essays, *Rethinking Anthropology* (19), which showed much more clearly than anything I had produced previously just how far I had distanced myself from my teachers. They had been interested in the particularity of other cultures and other societies, both of which were thought of as existing in the plural.

Malinowski and Firth had both taught and written as if it might be possible to describe a cultural system as a unique self-sufficient, functioning whole. Such a description would specify the Trobrianders or the Tikopia as *different* from all other people. Likewise, Fortes had followed Radcliffe-Brown in holding that whole societies are distinguishable as species types and classifiable as such in a kind of Linnaean taxonomy. In the lead essay of *Rethinking Anthropology,* which had been delivered in lecture form in December 1959, I denounced all such approaches to the data of anthropology as "butterfly collecting" and urged that what anthropologists ought to be doing was searching for generalizations for which cultural boundaries were quite irrelevant. In that same essay, to the mystification of most of my audience, I referred to the potential significance of binary arithmetic and computer machine code as devices for modeling sociological process (19, pp. 6–7). Another key point, about which I was also quite explicit, was that my use of "function" derived from mathematics and not from biology or psychology, as was the case with the followers of Radcliffe-Brown and Malinowski. Consequently, from my point of view, there was no inconsistency between "functionalism" and "structuralism" (in its then novel continental sense).

I spent the academic year 1960/61 at the Palo Alto "Think Tank" where I met Roman Jakobson and thus began to understand what I had been playing at ever since I ceased to be an engineer. Jakobson was concerned at that time with the search for linguistic universals. Our problems were of essentially the same kind. I began to see that my deepest concerns were with what is now discussed under such grandiose labels as semiotics and cognitive science. Everything that has happened in the subsequent 22 years has reinforced my belief that those insights were ones that I could stick with.

All this ties in with my later obsession with Vico. Vico, in his own way and in his own time, was likewise interested in structuralist transformations as generalized products of human thinking. His key perception in his *New Science* (33) was that only the maker of an object fully understands its nature; e.g. a carpenter understands why the chair he has made does not collapse. But human society was made by man, so man should be able to understand society, in an engineering sense, e.g. why it holds together and does not collapse. Behind this there is the further perception that all the artifacts (including human society) which man thus "makes" must necessarily be projective transformations of what the human brain already "knows." This implies, to use computer terminology, that social products are generated by "software programs," operating through but limited by the computer-like machinery of the human brain. The "software" comes from our cultural environment; the "hardware" derives from our genetic inheritance.

Many social anthropologists think that model making of this kind is just verbal eyewash. I find it valuable, and it has dominated my thinking now for nearly a quarter of a century. I reject the notion that I have swung back and forth between being a functionalist and being a structuralist; I have quite consistently been both at once. But both my functionalism and my structuralism derive from my grounding in mathematics and engineering. Furthermore, I have an engineer's interest in design, in how local regions of complex unbounded systems "work." Indeed, I have consistently maintained that the social systems with which anthropologists have to deal are not, in any empirical sense, bounded at all. To discuss the plurality of cultures is for me nonsense.

But what about my social, as distinct from my educational, background? Here, as I said at the beginning, I decline to be frank, but the relevance is obvious. Here is an example.

Kuper (15, Chap. 6) pairs me with Max Gluckman. We were more or less contemporaries. Although Gluckman was a year my junior, he had read anthropology as an undergraduate at Witwatersrand, so his grounding in the subject was earlier and much more thorough than my own, but he had missed out on other things. As was the case with Fortes, Gluckman's family background was Russian-Jewish-South African and, like Fortes, he ended up with an irrational devotion to stable systems in general and to Oxford in particular. Fortes and Gluckman were close personal friends over many years. Gluckman and I first met very briefly at one of Radcliffe-Brown's Oxford seminars in the winter of 1938/39. I took an instant and lasting dislike to the whole setup and to Gluckman in particular.

If anyone had asked me then or later what I thought of Gluckman, I would probably have said that I considered him to be an uncivilized and fundamentally uneducated egocentric whose attempts at theoretical generalization were of quite puerile incompetence. My views of Radcliffe-Brown were not all that different, though perhaps I would have qualified the uneducated.

Since Brown was at Trinity College, Cambridge, in several distinguished capacities, from 1901–1910, it might perhaps be thought that, in terms of the general argument of this essay, I should have found in him some mental affinity. In fact, I was constantly offended by the characteristic which Stanner (32), in a highly laudatory assessment, mentions as: "he (Radcliffe-Brown) was somewhat given to instructing other scholars in their own subjects." Our very first meeting was marked by just such a contretemps in which R-B gave me a lecture about a branch of mathematics in which I happened to be expert and about which he clearly knew nothing whatsoever.

In the latter part of his life, Radcliffe-Brown tried to give the impression to the more gullible members of the non-English audiences that he was usually addressing that he was, by lineage and upbringing, an English country gentleman. The fact that in 1926, on moving from Cape Town to Sydney, he should have taken the trouble to change his name from Brown to Radcliffe-Brown gives an indication of the value that he attached to such matters. In actual fact he was not born into the English social class which reacts favorably to hyphenated names. His education started at the Royal Commercial Travellers School at Pinner in Middlesex.

Such arrogance and prejudice on my part reflects no credit on me but, if I am honest, I have to admit that I feel today as I felt then. But in the case of Gluckman it was a radical difference of social background rather than any fundamental disagreements concerning social theory that lay at the roots of our mutual antipathy. Marx, Durkheim, and Freud were palpable influences on Gluckman's thinking (see 5); even a casual glance at my library shelves would suggest that they must have been powerful influences on my own.

So where does that lead us? One persistent tradition in British social anthropology is indicated by the frequency with which the practitioners refer to their discipline as a "science." They mean many different things by this word, but few of them mean what Vico meant.

One common model is that of Radcliffe-Brown, who thought of social anthropology as somehow analogous to a very primitive kind of nineteenth century taxonomic zoology. Brown took over this idea from Haddon, who had started out as a zoologist. In this model it is supposed that the "facts" with which social anthropologists have to deal are somehow "out there" and that they can be discussed and analyzed as objects and species types without reference to the prejudices of the observer or to the fact that the process of "participant observation" which has somehow become the hallmark of professional (as distinct from amateur) social anthropology must necessarily distort whatever it is that is being observed. The majority of contemporary social anthropologists will not, if pressed, seek to defend such an attitude, yet it seems to underlie all varieties of social anthropological empiricism.

At the opposite extreme are those who believe that the phenomena discussed by social anthropologists exemplify "science" in the way that Newton's laws of motion exemplify science. There are mathematical principles that will explain everything; the ideal ethnographic monograph would be one which consists almost entirely of mathematical equations and graphical representations of social vectors. There are not many authors who have as yet managed to persuade their publishers to put out monographs in this form, but one suspects that the number who would like to do so is considerable. In this case, as in the zoological model, there is an underlying assumption that the ethnographic "facts" recorded by anthropological observers in the field have some kind of objective reality.

The implication of my present essay is quite the reverse; the data which derive from fieldwork are subjective not objective. I am saying that every anthropological observer, no matter how well he/she has been trained, will see something that no other such observer can recognize, namely a kind of harmonic projection of the observer's own personality. And when these observations are "written up" in monograph or any other form, the observer's personality will again distort any purported "objectivity."

So what should be done? Nothing. Anthropological texts are interesting in themselves and not because they tell us something about the external world. When we read anthropological texts we can read them in two quite different ways. In the first case text is text just as the Bible is text. It may be interesting in itself; structured in discoverable ways; full of hidden "meanings" both intended and unintended. But we cannot assume that what is discussed in the text corresponds to any kind of "reality." In the second case we can read a text with the set purpose of discovering projections of the author's personality, of finding a record of how he or she reacted to what was going on.

Some might say that neither of these approaches to the published evidence has anything to do with social anthropology. I am not so sure of that; but of one thing I am quite certain. Unless we pay much closer attention than has been customary to the personal background of the authors of anthropological works, we shall miss out on most of what these texts are capable of telling us about the history of anthropology.

At the back of the "sense" of social anthropology, there is also the "non-sense" of social anthropology. The huge gaps in my story give some indication of how difficult it is to investigate this "non-sense" even when it is part of one's personal experience, as, in one way or another, must always be the case.

As a final coda for the curious . . . While my taste in architecture would put Romanesque ahead of Gothic, I tend to prefer the Baroque style to either. From an engineering point of view, it tackles much more complicated problems and solves them in a wider variety of different ways.

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