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Excerpt

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CHAPTER I

*Anthropological fieldwork
and the context of belief*

I

Those who are not social or cultural anthropologists must be surprised at the deep metaphysical significance we attach to fieldwork in exotic settings as a necessary condition in our formation and continued existence as practicing anthropologists. Malinowski's patron and the first regular teacher of our subject at the London School of Economics, Professor C. G. Seligman, did not exaggerate when he eloquently pleaded for research funds with the words "Field research in anthropology is what the blood of the martyrs is to the Church."¹ If only the one had flowed as freely as the other! It makes us what we are: Without it we are nothing. So, if you ask most students why they want to study anthropology, you will be told disarmingly, "To do fieldwork." And if you are so ill-advised as to attend a staff meeting in any university anthropology department, you will be amazed at the rancorous clamor that breaks out as soon as we reach that perennial agenda item, our competing claims to apply for leave to carry out further research overseas.

Fieldwork thus is our Utopia, our millennial dream. In such an atmosphere mere "library research," as it is disparagingly described, is virtually taboo – a possibility, perhaps for lame ducks and rainy days. The ultimate test of the professional anthropologist's standing remains the successful completion of a major piece of field research in exotic surroundings. It is necessary also for the researcher to display an impressive command of the relevant vernacular and present the results of

his heroic endeavors in an appropriately detailed and scholarly monograph where they can be carefully scrutinized. The more difficult, dangerous, and inaccessible the terrain, the better. Similarly, the longer the period of fieldwork, the greater the student's merit. So the number of years and months (and sometimes even weeks) notched up under the sun is studiously logged and scrutinized by our recording angels. These statistics are carefully preserved and figure prominently in that fascinating ethnographic document, *The Register of Members of the Association of Social Anthropologists of the Commonwealth*.²

In this tradition, the most damaging criticism that can be lodged against a colleague concerns the quality of his fieldwork. Did he *really* get the facts right? Did he *really* speak the local language with sufficient proficiency for us to accept his complicated interpretation of their symbols? Do his findings represent a view biased by his close association with only one section of the community he studied? Suspicions of this sort dog the reputations, and sometimes threaten the careers, of even the most dazzling theoreticians if they have not also given acceptable proof of their scholarship as fieldworkers. Here Lévi-Strauss's case is particularly interesting. As he himself sometimes admits and as his warmest British advocates acknowledge,³ his field research is patchy and thin and falls far short of the standards Malinowski established. Indeed, most of his theoretical writing relies heavily on information collected by other anthropologists and is in this sense markedly dependent on secondary sources. Yet he proclaims the crucial role of fieldwork for our subject and exhibits highly ambivalent attitudes toward it. Replying to a spirited attack by the French sociologist Gurvitch, Lévi-Strauss accuses the latter of misunderstanding and exaggerating the aims of anthropology. We do not, he retorts loftily, attempt to reach as complete a knowledge as possible of the societies we study. And he continues, "The disparity between such an ambition and the resources available to us is so great that we might be called charlatans, and with good reason. How would one penetrate the dynamics of an alien society after a stay of a few months,

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knowing nothing of its history, and usually very little of its language?"⁴

This interesting riposte, to which it is tempting to rejoin, "Speak for yourself," contrasts sharply with another passage by the same author. Here, again in reply to Gurvitch, Lévi-Strauss parades by name the various ethnic groups he has visited in South America and protests indignantly, "All these names are associated with men and women of whom I have been fond, whom I have respected, whose faces remain in my memory. They remind me of joys, hardships, weariness and, sometimes, dangers. *These are my witness* [emphasis supplied], the living link between my theoretical views and reality."⁵ And in a later passage, he announces even more insistently, perhaps even defiantly, "I am an anthropologist who has conducted fieldwork."⁶ This is not the image of Lévi-Strauss with which we are most familiar, but it is one of the many facets of his intriguing situation to which I shall return later. We may also note here how one of the currently most influential schools of American anthropology makes a similar and more direct fetish of fieldwork through its slogan, "New Ethnography." By this title it seeks to appropriate the prestige that traditionally attaches to the most minutely detailed and scrupulous collection of field data.

As further evidence of the anthropologist's obsession with fieldwork in alien cultures, the peculiar position of the anthropologist who investigates his own culture merits attention. As one would expect, orthodox opinion holds that such research is of dubious legitimacy. The distance between the observer and the observed is not considered sufficiently wide to generate those sparks of intercultural inspiration so essential to the anthropological imagination. Yet is this always and necessarily so? Few societies are truly homogeneous. Most contain significant and to some degree mutually incomprehensible cultural distinctions. This is one reason why it is perfectly possible for a British patriot to carry out authentic anthropological research in the United Kingdom. Ulster is perhaps a rather extreme example of such a setting. There is also the consideration that the autochthonous anthropologist

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in a sense often does his fieldwork in reverse. The imaginative leaps in such cases occur not so much in the actual collection of ethnographic data as in the analysis and writing-up.

Finally, if fieldwork is crucial in our subject, different methods of doing it should produce different kinds of anthropology. In a sense, this has always been a dominant assumption, one that underlies the great importance we all attach to Malinowski's influence. In this vein, Audrey Richards⁷ has pointed out how the contrasting characteristics of French and British anthropology reflect their different modes of fieldwork. The light-footed French descend in droves like locusts upon their terrain, making a succession of short, sharp probes. These may be continued at intervals over a decade or more and sometimes culminate in a sudden final climactic confrontation when the peoples they study at last submit to this relentless goading and expose their hidden souls. How different we British anthropologists are! Our approach is solitary, more intensive and concentrated as we doggedly attempt to unravel and record for posterity the dominant features of a whole culture in eighteen to twenty-four months of uninterrupted work and total immersion in the world of our hosts. We share the same Muse as our French colleagues, but we practice her rites in a different style.

Through this romantic quest for knowledge and illumination from the uncharted peoples of the Third World – those for the most part traditionally neglected by more conventional European scholarship – social anthropology can legitimately claim to stand as the authentic founder of black studies, the original academic discipline devoted to the study of alternative cultures, institutions, and beliefs. We are thus true transcultural transvestites, professional aliens, cross-cultural voyeurs. And fieldwork is primarily a matter of observation and detection, so that abundant natural curiosity is an essential attribute of the successful research worker. Whether he likes it or not, every anthropologist is in practice a private eye, prying relentlessly and often irritatingly into the affairs of his foreign hosts and so intruding savagely on their privacy. No wonder most anthropologists, whatever their intentions,

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political opinions, or connections, are regarded at some stage in fieldwork as spies. (There is a curious harmony between this sinister aspect of the most empirical part of anthropology and the current fashion that terms such as “hidden messages,” “codes” to be “cracked,” and other jargon borrowings from the communications industry enjoy in that rarefied world of symbolic analysis known to its intimates, a little unfortunately perhaps, as semiology.) Such indeed is the field-worker’s privileged intimacy with his informants that, if we are to follow some authorities, it automatically rules out marriage as well as other, less formal romantic attachments. So, according to Margaret Mead, one of the particular hazards likely to arise in studying what she somewhat old-fashionedly calls “high cultures” is the danger that the anthropologist might actually want to marry his or her informants!⁸

II

Fieldwork is for us also a voyage in a different sense, since it is the royal road to the anthropological calling. The would-be anthropologist trains for fieldwork, disappears into the field, and finally returns to write up the research, collect a Ph.D., and embark upon a professional career. As a number of anthropologists have noticed, and none more perceptively than Rosemary Firth,⁹ we see here the familiar tripartite structure common to most transition rituals. In the first phase, the neophyte prepares to surrender his old status and to shed ethnocentric assumptions. In the second, “liminal” phase, he retires to learn the new culture of his hosts. In the third phase, he reemerges to be readopted within his own academic culture in a new role as professional anthropologist. Each stage has its attendant traumas, which have to a certain extent become conventionalized, the most taxing being those of the actual field research and subsequent writing-up. Here, however, there is a deep paradox. In its factual and theoretical concerns, anthropology is a cumulative discipline in which new discoveries give rise to new theories and these in turn engender novel research projects. Yet, the actual field

experience is for each new recruit a unique and often regressive personal drama. This can be prepared for, but its full impact can never be precisely estimated in advance, nor can one say with complete confidence what the outcome will be.

These considerations, and the clearly critical significance of what we now know as “culture shock,” naturally invite comparison to similar experiences in psychiatry and psychoanalysis.¹⁰ There is certainly a valid sense in which “the field” is our equivalent of the analyst’s couch, and the analogy between fieldwork and a training-analysis is obvious. It is also not difficult to see that the process of participant observation implies at least partial assimilation and internalization of the morality and assumptions of the alien society under study. S. T. Kimball, for instance, records how he encountered a ghost in the course of his fieldwork in Ireland, and how this helped to convince his hosts that he was a normal human being. I remember, too, how impressed I was when Paul Baxter told me how he had become so thoroughly imbued with the spirit of the Boran pastoralists he was studying in northern Kenya that he began to have Boran dreams. Similarly, Rosemary Firth reports how on the eve of her departure from Malaya, where she had been working with her husband, Raymond, she had a terrifying nightmare. She dreamed that she had become “a Malay peasant, a woman crouching over the fire to blow up the embers for an evening meal.” She awoke, she records, “in terror, momentarily confused about her own identity – Malay woman or English scientist.”¹¹

Much of the terminology employed in the more reflective writing about fieldwork carries the same allusions. Evans-Pritchard (my teacher at Oxford) tells us that only the anthropologist, and not his hosts, can make the “transference” that is essential if fieldwork is to be successful.¹² The burden of anthropological discussion on this point, however, seems rather to follow Lévi-Strauss¹³ in regarding the anthropologist as the equivalent of the analyst or psychiatrist, and the people studied as patients. Thus the onus of transference is shifted from the anthropologist to the people under study. But we

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need not be too perturbed by these strange transformations, for even psychoanalysts now officially recognize what they engagingly call “countertransference.” This is the well-known process by which the analyst develops feelings towards the patients that are as ambivalent as their own toward the analyst. We also know, of course, that the boundary between patient and practitioner is, in the psychiatric field especially, not always clearly defined, and indeed is readily crossed in either direction.

These facts are more explicitly recognized in that older deviant cult that anthropologists call spirit-possession or shamanism, which I find a more illuminating model for both the psychoanalytic and the anthropological professions. Consider the parallels.¹⁴ Like shamans, anthropologists go on trips to distant and mysterious worlds from which they bring back rich stores of exotic wisdom. They mediate between their own group and the unknown. They speak “with tongues” that are often unintelligible at home, and they act as mediums for the alien cultures through which they roam and which, in a sense, they come to incarnate. Like so many shamans or psychiatrists, they regularly occupy marginal positions in their own cultures as well as those they visit in search of knowledge.

Again, as was Malinowski, anthropologists are typically summoned suddenly and dramatically from other occupations and impelled to assume their true and final calling. Malinowski’s whole system of graduate anthropological training was based on the shamanistic assumption that all his students were converts, inspired by Providence to enter into their true destiny. And this method was transferred lock, stock, and barrel to Oxford, where it was administered first by Radcliffe-Brown and then by Evans-Pritchard. This shamanistic tradition survives in the influentially held view (which I do not entirely share) that social anthropology should only be taught at the graduate level to mature students who have already been trained in another discipline.

Pursuing my parallel, we should note that in internalizing the culture of the alien hosts (as their client) the anthropologist becomes possessed by them. True, in common with

shamans the world over, the anthropologist may dissemble and claim that he “possesses them,” speaking suspiciously often of “his people.” But do not be deceived. The real situation is that they possess him. Or rather, as in other possession cults, the situation is in reality ambiguous, each participant possessing the other (as in sexual intercourse). Finally, as the anthropologist proceeds to analyze and write up his findings, he externalizes his experience and gradually disengages himself from his informants. He reestablishes the original distance separating the two cultures. This process is a form of exorcism. And, as one would expect, as in shamanistic cults, the anthropologist rarely achieves a final and complete separation from his Muse. He instead remains in a permanent state of bondage, periodically seized and impelled to further bouts of exorcistic writing. Some, alas, are denied even this relief and remain so completely enthralled that they defy all attempts at exorcism, subsisting in what is virtually a perpetual state of mute communication with their muse.

III

It is now time to begin our careful scrutiny of the treasures that the anthropologist brings back from his shamanistic voyages. First, what psychiatrists would doubtless identify as our “primary” gains. If he had not acquired new information about a hitherto unrecorded or underrecorded culture, the anthropologist would have little to say. For, of course, we are all plagiarists, *bricoleurs*, living parasitically upon our ethnographic sources. Where would Malinowski have got without the Trobriand Islanders, Evans-Pritchard without the Nuer and Zande, Firth without the Tikopia, or Forde without the Yako? All our pronouncements carry conviction and authenticity only when we anchor them in the lives and thoughts of those we study. Our ethnographic sources provide our indispensable claims to legitimacy; they establish our credentials: “The Nuer do that, you say”; “How odd, the Somali don’t”; and so on.

This ultimate referring of all our ideas to the ethnographic

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“facts” elucidated by painstaking field research is now so deeply engrained in our subject and so taken for granted that we tend to forget how dependent we are on our sources. Acutely aware of the subjectivity of our research techniques, we tend now to overestimate our part and to underestimate that of our informants. Strange as it may seem, the same tendency toward a position of ethnocentric superiority seems to me implicit in the rather overworked view of anthropology as the spoiled child of Western imperialism. Here those anthropologists who carried out research in the colonial territories are depicted as the deliberate – or, more charitably, unwitting – instruments of imperialism. These circumstances, it is held, have led those who undertook their fieldwork during that period to misunderstand the real position of their subjects and thus to have produced inevitably distorted analyses of social structures and cultures.

I find the condescending implications of this interpretation repugnant. It assumes that the people we study, or have studied, possess no vigor of their own; that they have insipid lackluster cultures with no intrinsic dynamism, no *élan vital*, no capacity for self-expression; and that such articulate representatives as anthropologists encounter are by definition not representative but, on the contrary, brainwashed slaves and Uncle Toms. Those who paint this misleading picture, it must be acknowledged, have usually not carried out actual fieldwork and tend to overgeneralize from the deplorable treatment of the American Indians in North and South America.¹⁵ But they are also, I think, victims of a wider ethnocentric tendency to exaggerate greatly the force and finality of European culture and its implicit as well as explicit assumptions. Whatever Durkheim may have said, the people we study are *not* robots. Unlike artists’ models, they do not posture pliantly as the omniscient ethnographer wields his brilliant brush. Cultures do *not* pose as “models” for the anthropologist in the servile, manipulable fashion that artists typically expect of their subjects. Excessive preoccupation with so-called theoretical models conceals, very conveniently, our own lack of originality and the contrasting richness of the

peoples we study. It is *we*, not *they*, who are the puppets. Let me offer some evidence to support this heterodox view.

First let us take the instructive case of Lévi-Strauss. There exists a large and often tedious exegetical (I almost said liturgical) literature on this brilliant man's thoughts and their intellectual pedigree (much of which he himself has disowned).¹⁶ But whatever the obvious influences of Hegel, Marx, and Freud on the development of Lévi-Strauss's ideas, there is also a quite remarkable concordance between his key concepts and the salient characteristics of the ethnographic area in which he has actually carried out field research. As is well known, the colonial history of the American Indians is a sorry tale of rapacious exploitation and extermination. Those Indians who were not killed outright by the conquerors had their numbers dramatically reduced by exotic new diseases and related calamities. The surviving remnants of indigenous culture were then further impoverished and diluted by foreign accretions, with which in some cases they combined to produce bizarre hybrids. The presence today of Indian themes in the increasingly popular syncretic cults of the American continent bears impressive witness both to the vitality and persistence of Indian culture and to the centuries of intense pressure its authentic bearers have been forced to endure. Counterculture is a much older phenomenon in America than is generally supposed.

From the anthropological point of view, the colonization of America thus produced a bewildering assortment of ethnographic jigsaw puzzles. This encouraged those who sought to solve them to concentrate on what Robert Murphy has called the "cultural residues of the mind," as they attempted to wring out the last pieces of remembered custom from the oldest Indian informants they could find. As Murphy observes, it also prompted the publication of a series of monographs that, in their exhaustive cataloguing of disparate and often disjointed cultural elements, read like laundry lists.¹⁷ These circumstances strikingly reflect Robert Lowie's famous definition of civilization as a "thing of shreds and patches."¹⁸ They also, I suggest, directly reflect Lévi-Strauss's conception