Reports

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Ten Questions Put to Claude Lévi-Strauss[1]

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Claude Lévi-Strauss. (Photo E. Garrigues.)

MA: In several past interviews, you have always responded very willingly. However, the range of your work is such that the interview form not only demands great forbearance on your part but also, I think, suggests that your interrogator must be to some degree naive, pretentious, or obtuse. So, the first question I would like to put to you is: What do you think about this kind of exercise? I can focus the point more sharply by adding subsidiary queries: Are you not irritated by apparently being requested to supply "potted ideas"? Are you not being called upon, as it were, to be your own first critic, your own best-authorized reader, so as to spare the people to whom your works are addressed the arduous but fundamental task of engaging directly with your work even if, in so doing, they are in danger of not understanding, or of definitely misunderstanding?

CL-S: Believe me, I decline far more interviews than I accept! And when I do accept, it is with the feeling that people have been asking me the same questions for the last 40 years and that I always give the same answers—or rather, through weariness, reply less and less adequately to hackneyed questions that no longer correspond to the present state of my thinking.

I may add that the interview form is not something to which I am naturally suited; I suffer from *l'esprit de l'escalier*. In talking, my first response—in writing, my first draft—is never satisfactory. Behind every published text there have been three, four, sometimes even five revisions. And when I am sent the text of a recorded interview, I usually tear it up and start afresh. The basic weakness of the interview form is that it lures you into restating very badly something you took great pains to express a little better in writing.

This being so, you may say, why not just refuse? There are constraining circumstances. An author has a responsibility to his publisher, who has taken a risk. One cannot systematically avoid the pressure exerted by the publicity manager. One may be charmed by the personality of a female interviewer. Ambiguous motives also come into play. From my academic seniors I learned the rather cynical maxim that if one has an idea that seems neither very sound nor very convincing, one can always turn it into an article for a *Festschrift*. Similarly, in an interview, one can hazard rather rash statements or express new, untested ideas that one doesn't intend to follow up for the time being. I might never have written about painting if I hadn't been made to think about it by questions that Georges Charbonnier put to me nearly 30 years ago. More recently, I was persuaded to supply a certain amount of biographical information to satisfy the curiosity of Didier Eribon, although I have never been tempted to write my memoirs, nor indeed think myself capable of doing so. Rightly or wrongly, I have pleasant memories of two long interviews with Raymond Bellour, during the years when the volumes of *Mythologiques* [1964–71] were appearing. He prompted me to clarify certain of my leading ideas and to explain my method of working more clearly, I think, than I had ever done before.

Such occasions are infrequent, and, on the whole, I share your reservations—especially when some daily or weekly paper asks for an interview in connection with a recently published book as an easy way of getting the author himself to give an account of it instead of entrusting the task to a competent critic, on the fallacious grounds that no such person exists. The publicity granted one by the press is a substitute for the mental effort it shirks. This is a sign, and a major one, of the decline of the critical spirit.

On the whole, and all things considered, the interview is a detestable genre, to which the intellectual poverty of

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the age obliges one to submit more often than one would like.

MA: My second question is a restatement of the first in another form. You yourself have said how remote you feel from your books once they have been published. Is it not rather embarrassing for you to be faced with questions relating very specifically to lines of argument to which—while not wishing, in any sense, to disown them—you may rightly think you have nothing more to add? Here again, I can introduce alternative formulations: Do you consider that all your work can be seen in the same light and from the same point of view (does it form a "whole," in the sense in which it has been said that the Revolution forms a "whole"?), or rather that it includes different "periods" (in the sense in which the term is used of painters) and even divergent theoretical tendencies?

CL-S: As soon as a book has been written, it becomes foreign to me, and I feel awkward about being expected to make it seem present when it already belongs to the past. Not that I am convinced of having made definitive statements to which there is nothing to add; far from it! But the things I ought to add, the corrections I should make or the errors I should rectify, are not present to mind at the particular moment. They will materialize only in another book. And even supposing that I have already started on that book at the time of the interview, the points are still not clear to me, because my first rough draft will require several rewritings. It follows that the interview takes place either too early or too late. The only suitable time would be coincidental with the book itself, and in that case, why paraphrase? People might say, "Summarize it, bring out the essential points," but that is precisely the difficulty.

You are right to ask the question about the "whole." In what you call my *oeuvre* (a term I prefer to avoid, as being pretentious if used by myself), there are two distinct periods, the first devoted to marriage rules and kinship systems and then, from 1950 onwards—but not excluding frequent returns to my early interests— a very much longer period when I was concerned with myths and religious concepts.

No doubt, in my first books I was guilty of some rash formulations. There are things that I would not write today, or that I would put forward more cautiously. But, that having been granted, it seems to me that I have always been concerned with the same problems and have always endeavoured to solve them in the same way. In the case of both marriage rules and mythical concepts, I tried to discover a principle of intelligibility behind an apparently chaotic jumble of arbitrary and irrational practices and beliefs—or, if not chaotic, only to be explained, in either case, according to a separate procedure. From *Structures élémentaires* [1949] to *Mythologiques* there is no shift in theoretical approach.

Consequently, to summarize the spirit of this book or that would be to go on repeating the same thing. Their interest, if they have any, depends on the specificity and concrete diversity of the material analysed, and that does not lend itself to summary.

MA: I have heard a colleague say, about the volume of conversations with Didier Eribon [1988*a*], that "it is a very good book for students." I would say that it is very useful for the educated, non-specialist public, the 20th-century "common reader," at the same time as for specialists well acquainted with issues that the book, of necessity, mentions only in passing. However, as far as students are concerned—at any rate, students of ethnology or, more generally, the social sciences—their first priority should be to read the actual works so as to acquire a better appreciation of the scope and importance of these issues.

CL-S: I don't know if the conversations are of any interest to the educated public, as you call it, but I agree with you in thinking that they can hardly be of much use to students. I share the anti-biographical approach expressed by Proust in *Contre Sainte-Beuve*; what matters is the work, not the author who happened to write it; I would say rather that it writes itself through him. The individual person is no more than a means of transmission and survives in the work only as a residue.

Why did I agree to do the talks? For two reasons. First, I felt a debt of gratitude towards Eribon for having enabled me to hear Georges Dumézil's voice from beyond the grave, in the volume of conversations published by Gallimard [Dumézil 1987]. This was the last occasion on which the great scholar, to whom I owe a lot, was able to express his ideas. My second reason had to do with curiosity. What questions would I be asked, what aspects of my life and work would interest a young writer who might have been my son, or even my grandson? It was amusing, I confess, to discover how many of the events in which I had been involved or had witnessed had already taken on a legendary colouring for someone of a later generation. And so I made it a rule not to evade any of the questions, even if they did not accord with the angle of vision from which I myself would have looked at the past.

If I had had a greater liking for autobiography or had given more extensive answers (they were constantly held in check by my fear of being self-indulgent or of magnifying the tiny events of my existence), the conversations might have developed into a modest contribution to the intellectual history of the present century.

MA: It is not my ambition (in any case, the intention would no doubt be meaningless) to get you to make any original comment on your work. At the same time, I know that the ethnologists of my generation (and many other people too, but I restrict myself to ethnologists) all define themselves in relation to your work, whatever tendency they may belong to. Is there, in this sense, a "Lévi-Strauss generation," and what have you to say about it in general?

CL-S: For the reasons I have already given, I am probably the person least fit to comment on my work. As regards the rest of your question, I am rather at a loss to reply. In what is sometimes referred to as my influence, the differences seem to me more striking than the resemblances. It would appear that Foucault in various interviews said he had been influenced by me, but also, he added, by Lacan. I sense immediately that there must be some misunderstanding. Very likely the generation which came after mine was prepared to accept some of my ideas, but never, I think, exclusively. I certainly did not found a school, and I think that is just as well.

MA: On the philosophical level, you frequently quote Kant and Marx as points of reference. How do you reconcile these two influences, from the theoretical and methodological points of view?

CL-S: I mentioned the two great names as biographical landmarks, but this should not lead to a misconception; it is a good 40 years since I last looked into Kant or Marx. However,—and this was all I meant—I was deeply marked by reading them as an adolescent. You ask what I think they have in common. I would say, with hindsight, that both made me understand that reality exists in two modalities, between which we have to establish a hierarchy. I have no wish, of course, to see an analogy between Kant's contrasting of the world of the *noumenon* with the world of appearances and Marx's opposition of infrastructures and superstructures; this would be absurd, especially since Kant maintains that the *noumenon* is beyond our ken, whereas for Marx it is precisely the world of infrastructures that has to be studied in order to explain superstructures. But whether one says, like Marx, that the obstacle to knowledge is the fact that the consciousness is not honest with itself or, like Kant, that our mode of apprehension of inaccessible reality is conditioned by our mental constraints, in both cases what is being referred to, and given primary importance, is the mediatory function of the mind. In Kant's view, this mediation occurs through the *a priori* forms of sense experience, the categories of the understanding, and schematism, all of which are common to mankind in general. According to Marx, the world of superstructures depends rather on particular constraints, such as occur at a certain historical moment, in a given culture or society. Each culture and society has its forms of sensibility, its categories, its schemas; or, to put it more accurately, each—in accordance with its environment and its history—directs and modulates the way the human mind functions, always and everywhere.

As Mauss taught us, the essential task of anthropology is "to draw up the most extensive possible catalogue of categories." As for Marx, he rarely asked himself how, in each concrete case, the relationship between infrastructures and superstructures is established. A famous passage in the preface to the *Critique of Political Economy* raises the issue in connection with Greek art, without providing any answer. So, between Kant and Marx, there remains an area still to be explained; there is room, if you like, for a critique of ethnological reason.

MA: Is the double determinism that you recognize (that of the brain and that of the infrastructure) compatible with the definition of history as pure contingency? If so, is it incompatible with the definition of the human subject as being free?

CL-S: Your question implies an objection that would be valid if I claimed that these two determinisms covered the whole of human experience. My interest is in exceptional cases, in those rare instances where we are able to see a number of mechanisms at work, study their operation, and break them down into their component parts.

In a saturated solution, certain areas crystallize more or less quickly. Others fail to crystallize. Various factors are responsible: local temperatures, degrees of concentration, the presence or absence of impurities or of a precipitating agent. . . . In addition, no "set" is a closed entity, even though we may try to think of it as such. Each is under the influence of other sets, which are not of the same order of magnitude and whose repercussions on the scale of the set under consideration are aleatory. The tentative crystallizations of order occurring among the sets are constantly disturbed by what you call—and I call—the contingency of history. The important thing, from my point of view, is that certain of these tentative crystallizations of order are no sooner destroyed than they tend to reconstitute themselves in the same, or different, forms in the same place or nearby and, it must be said, with equally uncertain prospects.

As for the liberty of the human subject, forgive me, but I don't know the meaning of the expression and have always supposed that it disappeared from the vocabulary of philosophy after Spinoza's time. At the "molecular" level—if I may call it so—at which I study phenomena, there is no subject. I have been amply criticised for this! For my part, I don't deny others the right to choose a level of observation at which the subject becomes a reality again. It is up to them to raise the question of freedom, if they are so minded.

MA: You have always refused to engage in debate about the relationship between ethnology and psychoanalysis. It is fairly obvious, of course, that the unconscious structures referred to in *Structures élémentaires* or *Mythologiques* are not the same as those with which the psychoanalyst is concerned. However, in your "Introduction à l'oeuvre de Marcel Mauss" [1950], you wrote: "strictly speaking, it is the person of sound mind, as we say, who is alienated, since he agrees to exist in a world definable only by the relationship of the Self to the Other." In saying this, were you not initiating, however momentarily, a parallel between the two disciplines, and so hinting at the possibility of a "theory of practice"?

CL-S: The sentence you quote was part of a lengthy discussion in which I was arguing against the idea—still quite widespread at the time—that the personality of the shaman and the phenomena of trance and possession were to be seen as psychopathological. I was saying that we cannot apply to cultures different from ours the criteria we ourselves use to distinguish between the normal and the abnormal. Whatever organic factors may be at the root of mental illnesses, each society has a different understanding of these illnesses, draws their boundaries differently, modulates their respective frequencies, and influences both their form and their content.

For this to amount to the rudiments of a "theory of practice," one would have to conclude from it that mental illnesses could be abolished by changing society or—and here I am only slightly satirizing the tenets of a once-modish "New Psychiatry"—that there would be no mad people if only the so-called sound of mind were willing to seek treatment. This is an absurdity. Changing society would redraw the map of mental illnesses; it would not cause them to disappear.

My point was that, in every society, a variable percentage of individuals inevitably fall outside the system. Each society expresses its nature and reveals its contradictions by the forms of behaviour it considers sane and those it declares to be pathological. This way of looking at things, which is more widely accepted now than it was 40 years ago, does not point to any particular method of treatment.

MA: Taking a stand in direct opposition to Murdock's approach, you have said that generalisation is the basis of comparison. But did you not move on from *Structures élémentaires* to *Mythologiques* or *La voie des masques* [1979] by setting cultural limits to the structuralist method? Do you not now consider the concept of cultural constants valid only (or at least operative only) within certain clearly defined cultural and geographical areas? Is it out of the question to revive Sir James Frazer's universalist ambition?

CL-S: By "generalisation" I mean the preliminary procedure which consists in reducing empirical differences, in detecting what apparently disparate phenomena have in common and so can legitimately be compared. Let me give an example: I showed in a recent article [1988] that customs as different in appearance as circumcision and the wearing of penis-sheaths, which in the 1950 edition of *Notes and Queries on Anthropology*—the anthropologist's bible for three-quarters of a century—were dealt with under two different headings ("Personal Care," in the one case, and "Clothing," in the other), are similar in that they significantly modify the penis, the first by the removal of a natural part, the second by the addition of a cultural part, with the result that both parts can be defined as a "foreskin." On the formal level, this procedure is identical with that used in *Les structures*, which enabled me to understand the diversity of marriage rules as different modalities of a single phenomenon, the exchange of women, or, 30 years later in *La voie des masques*, to see objectively dissimilar masks as the successive states of a single transformation. This generalising intuition is an act of the intellect. Whatever the temporal or territorial distribution of the data under consideration, neither their form nor their nature is affected by changes of scale. It follows that generalisation should not be confused with comparison. Only the latter is more or less ambitious according to the cultural limits it chooses or that are assigned to it.

In *Les structures*, I felt able to make large-scale comparisons because marriage rules and kinship systems had already been studied for a century. The task of generalisation had already been performed by my predecessors, who had evolved a generally accepted vocabulary for the description of these phenomena. On the basis of their achievements, I was able to move on to the next stage: comparison.

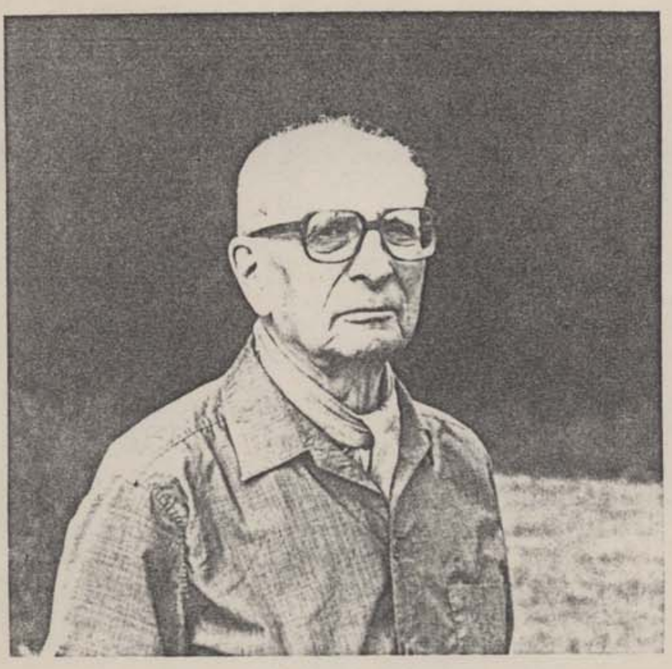
It was the opposite in the case of the myths. Fanciful comparisons had been made between superficial appearances, so that I had to delve down to a deeper level at which comparisons would have a meaning. And, in order not to risk relapsing into the vagaries of comparative mythology as it was still being practised at the beginning of this century, I had to set up a kind of protective barrier, formed by a common historical past and a spatial continuity that gave an objective basis to the work of generalisation.

Dumézil told me (and he said the same thing to other people, who reported back to me) that it was *Mythologiques* which prompted him to write the three volumes of *Mythe et épopée* [Dumézil 1968–73]. But it was from Dumézil's previous work that I myself had learned, when I was planning to write *Mythologiques*, that it would be wise to copy him in keeping to a geographical area definable in terms of a common past.

As to whether or not it is conceivable that Frazer's ambition will one day be revived, I have no idea. As a preliminary condition, all the mythological systems in the world would have to be subjected to the process first of generalisation, then of comparison, that I initiated in the case of North and South America. I suspect, however, that as the aim widens, generalisation will reach ever deeper levels, to the point that, in the end, it will be apparent that the deepest do not belong to anthropology.

MA: On the subject of kinship problems, you have been given credit for having replaced the lineal problematics instituted by the English Africanists with a more comprehensive problematics of alliance. But now other specialists have come along who try to "deconstruct" alliance theories by suggesting that there is no necessary connection, or at least no real link of complementarity, between forms of filiation, kinship terminologies, and forms of marriage alliances. The attempt to link them is said to result from the illusion of primitivism, as criticised by Adam Kuper in his latest book, *The Invention of Primitive Society* [1989]. What do you think about this "return to Square One"?

CL-S: Two criticisms have been levelled at me in connection with kinship problems, and if they were valid they would prove me to have been remarkably inconsistent. In the first place, *Les structures élémentaires de la parenté* has been attacked on the grounds that it presents filiation as being exclusively unilinear, which is quite untrue. Even in that early work, I warned against any



At home. (Photo Matthieu Lévi-Strauss.)

such error. Forgive me for quoting my own words, but they are to the point: "My schema of interpretation implies neither the existence of stable institutions nor the establishment of any particular rule of filiation or residence" [1967:154]. As I emphasized in the conclusion: "The marriage of cross-cousins can exist independently of any unilateral theory of filiation" [p. 506]. The concept of exchange, on which my whole theory was based, requires only that the society think of itself as consisting of what I might call *ad hoc* units, formed for a shorter or a longer time, as units qualified to make exchanges according to criteria of filiation, residence, interest, spiritual or physical heredity, or any other criterion whatsoever. I added that it was not necessary "to postulate any precise type of institution" [p. 154].

I have said this again and again in all my books, in the two volumes of *Anthropologie structurale* [1958], in *Le regard éloigné* [1983], *La voie des masques*, and *Paroles données* [1984], but the people who make this type of criticism blandly ignore everything I have written since *Les structures*, as if I had stopped thinking in 1949. And yet, during the 22 years I spent at the Collège de France, I devoted lecture courses to marriage rules and kinship systems with special emphasis on cognatic societies.

Other critics, or sometimes the same ones, have charged me with ignoring filiation in favour of alliance. Actually, the "return to Square One," to use your expression, involves something more serious, which I see as a regression to empiricism and an abandonment of theoretical reflection. As I am not afraid to admit, the primary and principal value of the alliance theory is precisely that it is a theory—in other words, an explanatory system, worked out on the basis of a hypothesis, and one which copes not only with the initial problem it set out to solve but also with others relating to apparently different aspects of social life that, as is eventually realized, can be dealt with by means of the same interpretation. The value of a theory is that it unifies, and solves by the same means, more problems than had previously been brought under the same heading; however, its value is always provisional, since each theory is eventually replaced by another, representing a comparable improvement in relation to it as it did in relation to its predecessors.

I don't underestimate the value or the interest of the work done by the enthusiasts for filiation. Their essentially empirical approach has led them to describe and analyse kinship terminologies in extreme detail. Their findings will have to be taken into account by any future theorist. For instance, from now on it will hardly be possible to deal with the Australian systems without making use of Scheffler's *Australian Kin Classification* [1978] or with the so-called Crow-Omaha systems without reference to *Two Crows Denies It* by R. H. Barnes [1984]. But do these books contain anything more than empirical descriptions? Do they increase our understanding of how these particular societies function?

As for the primitivistic illusion under which the theoreticians of kinship and marriage are supposed to labour, the reproach amuses me, since, during the last six years of my teaching and in numerous articles, I have exhumed the concept of the house from the past of our own societies as a means of interpreting customs that had long puzzled specialists in American, Oceanic, and African anthropology. I thus showed that modes of social life and types of organisation that are well documented in our own history can throw light on those of different societies. In respect of kinship and marriage, the distance between the so-called complex societies and those wrongly dubbed primitive or archaic is, then, much less than might appear. Such is the conclusion to be drawn from the most recent research in this area. Far from setting up "ethnographic" societies as separate worlds, the study of peasant traditions or of the marriage customs of noble or royal families establishes all sorts of interconnections between those societies and ours that no one would have suspected only 20 years ago.

MA: Is an ethnology of the modern world possible? I believe that, here again, the question can be broken down according to whether we are thinking of the object of study or the method. Is the globalisation of culture, in the sense of a tendency towards uniformity, inevitable? If so, will anthropology be restricted to working from documents, or will it have something to say about the new social forms? How do you envisage its future?

CL-S: To imagine the future of ethnology would be to imagine the future of civilisation. I am too convinced of the contingent nature of history to go in for this kind of speculation, which could only consist of hazardous guesswork.

Let us restrict ourselves to the present. There are a number of things in the modern world that we anthropologists are not best qualified to study; they belong primarily to other disciplines, such as history, sociology, demography, economic science, political science, and social psychology. We can only make a contribution.

But, in this confused mass of phenomena stretching beyond our scope, there may survive, or come into being, limited areas where ethnological research feels able to operate because it finds there conditions which correspond to its needs: relative continuity in time, contiguity within a space, and direct communication between people. The site may be a country village, a district within a town, or even the point of intersection of two or three streets in a metropolis of thousands or millions of inhabitants; these are all places where spatial proximity gives rise to habits or encourages their continuance.

My first field of ethnological enquiry was the town of São Paulo, at a time when, as a young teacher, I was influenced by the urban sociology of the Chicago school. I have also read and reread the nine volumes of *The New Survey of London Life and Labour*, an inexhaustible source of instruction and suggestions for ethnologists. And I am proud of the fact that, in 1949, under the aegis of Lucien Febvre, I initiated what I think was the first study of a French village conducted in an ethnological spirit; it eventually resulted in a book by Lucien Bernot and René Blancard, *Nouville: Un village français* [1953].

But above all, let us focus our attention on the hundreds of inadequately studied societies in which traditional modes of life and forms of thought still survive, even though they may now have to be looked for in restricted areas of social life or collective activities. Just over 30 years ago, R. Gordon Wasson, who had no ethnological, or indeed any scientific, training—he was a banker—invented an entirely new branch of our discipline, ethnomycology, with sensational results. Other doors are waiting to be opened, other locks to be forced. What is lacking is imagination; there is no lack of material.

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Human Colonisation and Change in the Remote Pacific[1]

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The prehistory of the remote islands of the Pacific displays two very general patterns—one relating to colonisation and the other to the later diversification of island populations—that have predictive value for local situations. Colonisation began rapidly, and its advance was directed to the survival of voyagers. The rate of exploration, usually thought to have slowed, in fact increased, and the only perceptible variations conformed to changing natural circumstances. Many of the human patterns of the remote Pacific described at the time of European contact were due not simply to the diversification which occurred with colonisation but also to the circumstances of post-settlement history. In particular, changes in voyaging frequency systematically affected island prehistories.

Remote Oceania has been distinguished from Near Oceania (consisting of New Guinea and its nearer Melanesian neighbours) because east of the Solomons the islands become generally smaller, much further apart, and increasingly impoverished in natural resources (Pawley and Green 1973). The boundary between the two marks a major physical discontinuity, and it was a cultural one too in the sense that Pleistocene settlers probably could not cross it. Most Pacific archaeologists agree that the first people to reach the remote Pacific carried a culture called Lapita (Allen 1984, Bellwood 1978, Green 1979, Kirch 1988), which evidently arose somewhere in a voyaging corridor including Near Oceania and extending back to mainland Southeast Asia that was characterised by chains of intervisible islands and monsoonal reversals of wind and current within a sheltered equatorial zone between the northern and southern belts of tropical summer cyclones (Irwin 1989). These were ideal nursery conditions for the development of maritime technology until people had learned how to deep-sea sail and survive.

The spread of Lapita culture, some 3,500 years ago, across the western Pacific from the Bismarck Archipelago to western Polynesia was, given that sampling and dating are imprecise, archaeologically almost instantaneous. There is, however, considerable debate over the

Indian sociology and the cultural other

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Background

Indian sociology,[1] as discussed in the pages of *Contributions*, original and new series, raises some unavoidable issues about its own identity as it tackles Western approaches and perspectives on India. The issues of the West's cultural other variously influence the content, form, history and development of Indian sociology.[2] In the context of major researches conducted since the mid-1950s, Indian sociology, I shall argue, must still more fully investigate the burden of its Western ancestry. Such an examination requires developing more interest in the sociology of knowledge in India, with attention to the roles of Indian cultural logic and reasoning. The whole issue, I argue, demands a closer study of the West as India's 'cultural other' from different Indian cultural vantage points, illustrating Indian sociology's approach to the universal and the relative.

Our discussion of the subject will be informed by cultural critiques of anthropological representation on the one hand, and issues in critical

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[1] By 'Indian sociology' I refer mostly to distinct developments discussed in *Contributions*, the original and new series, since the mid-1950s. I prefer the term 'Indian sociology' over 'sociology of India' since it helps me underscore the original emphasis that Dumont and Pocock (1957, 1960) envisioned for their project. Historically, I distinguish between 'Indian sociology' that develops from within, reflecting the changing Indian intellectual temper, and that which reflects the colonial and/or Western value assumptions.

Meenakshi Thapan's (1988) excellent paper was an encouragement to me in writing mine. She made my task easier by pointing out a central issue of Indian sociology: its limited ability, so far, to deal with the cultural other within India, and outside. I also thank Professor T.N. Madan for making editorial comments on the paper. As always, he was incisive and thought-provoking.

[2] Widely different forces produce socio-cultural otherness within a society and one could examine them from different theoretical positions. I confine myself (as do the scholars I review) to India's cultural discourses from within and their analyses. One could obviously take other positions to view India's internal strains.

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philosophy and hermeneutics, on the other.[3] We will purposely take a critical view of modern Western epistemology and its universalist claims, especially when it adopts a rigid position against other civilisations and their rigorous, time-tested epistemologies. Similarly, we suggest that Indian sociology must move beyond its conundrums of status quo—e.g., where the modern, universal West must confront traditional, localised India (for early formulations in *Contributions*, see Dumont 1966; Dumont and Pocock 1957, 1960; Madan 1966).

Indian sociology needs to launch a sustained critical discourse on itself, and on Western thought (for an initial formulation of such a position, see Uberoi 1968, 1978). However, such an exercise demands a better reading of India's own cultural past, and a critical understanding of others' accounts of India over time. Such exercises will also help Indian sociology discover the crucial roles the Indic (i.e., Hindu, Buddhist and Jain) systems of knowledge and interpretation play in constituting Indian reality.

Etymologically, the word 'other', related in Old High German to *andar*, and in Sanskrit to *antara* (*Websters Dictionary* 1985: 835), refers to relationships of plurality, addition, diversity, opposition, exclusion, and the temporally former. Our usage draws on all these meanings by context, rather than subscribing to a simple opposition between inside and outside. But an India-West sociological comparison must confront a basic issue: Can the social sciences lay claim to genuinely universal truths? Is a 'truly universal social science' not mostly about dominating alternative systems of knowledge and their truth values? Is it not often the case that rationalists, relativists, and orientalists end by pointing fingers at each other's 'blind spots'? More useful for Indian sociology may therefore be a study of changing historical conditions and relationships between the dominant Western and the subdued non-Western epistemologies. It may focus on the role played by the educated Indian's ambivalent handling of the colonial past and its mechanisms of cultural control via the antagonistic politicisation of caste, language, region and religion. As an 'insider-outsider', and often a jumble of the colonial-nationalist-traditionalist temper, the modern Indian intellectual carries his own blind spots. Retrospectively, Dumont and Pocock may have insufficiently recognised the continuing influence of colonial thinking on Indian studies.[4] Indian sociology must therefore much

[3] The literature on the subject is vast and varied in Western philosophy, with some recent notable additions. For a wide-ranging and conventional discussion of the issue of 'other minds' in philosophy, especially for reflecting Wittgenstein's influence, see Wisdom (1956). For an anthropological analysis of the issue, see Fabian (1983); Marcus and Fischer (1986); Whitten (1988). For a critique of traditional Western positions on value, taste, judgement, and justification, see Barbara Smith (1988). She develops an 'alternative framework' by criticising such recent thinkers as Derrida, Habermas, Northrop Frye, and Richard Rorty.

[4] This has been so despite the best Western liberal tradition (and its intentions) that Louis Dumont and David Pocock may have represented. Recent studies alert us to the lingering colonial temper shaping the thinking of Indian and Western scholars of the period (see Nandy 1983).

more carefully reconstruct India's many—familiar and alien—faces from within.[5]

But this requires viewing India neither selectively, nor in ways that stifle Indian sociology. It also means discovering how India develops its 'science of appropriate distinctions'. Such studies can neither be 'reactionary', nor automatically opposed to all that is non-Hindu, Western, modern, and universal. Similarly, 'indigenous' thought has to be neither archaic, parochial, underdeveloped, or incomprehensible. Such labels are often red herrings employed to maintain the unique supremacy of Western intellectual tradition, and to deny the possibility of multiple centres of equally authentic knowledge.[6]

Within the prevailing Western epistemic paradigm, India or any other major non-Western literate tradition can only become a traditional other (as in Dumont's India-West oppositional schemes; see Dumont 1977, 1980); or a significant other (e.g., in Marriott's *plural* cultural or 'ethno' social sciences 'of other lands', providing 'an expanded, multicultural set of sciences' to evolve 'that "*universal* significance and value"'; see Marriott 1989: 3; also Marriott and Inden 1977). However, such initiatives help us review the development of Indian sociology for the instructive markers it provides on India's otherness to the West in the pages of *Contributions* (e.g., Ahmad 1972; Bailey 1959; Dumont and Pocock 1957, 1960; Jaer 1987; Kantowsky 1969; Madan 1966, 1981; Saberwal 1983; Uberoi 1968; Venugopal 1986). Though not restricted to the original Dumont-Pocock theoretical programme, the overall emphasis in *Contributions* remained on the socio-cultural distinctness of India in Western sociological terms. With the review and criticism of Dumont's essentialist-comparativist cultural view of India, Marriott's 'monistic' approach, especially since the 1970s, evolved more rigorous standards for studying India from within.[7]

[5] My argument underscores the necessity of not reducing India's multiple cultural faces over time and region, for the sake of a simple theory or a 'system'. However, such attempts should also not overlook the internal devices of cultural unity.

My proposal emphasises the role of indigenous forms of 'cultural reasoning and worldviews', especially of the three long-standing major Indic traditions—Hindu, Buddhist and Jain. These major players have long been involved in what Gilbert Ryle (1954: 1–14), in another context, called the 'litigation between [alternative] theories or bodies of ideas', shaping both the common sense and common knowledge of India—and the Indian.

[6] However, toward the end of this century, the West's universalist position encounters difficulty when a protesting knowledge system (e.g., Islamic or Chinese) disputes the ultimate epistemic supremacy and authority of the West, and proclaims itself to be a separate anchor of ideological and cultural universalism.

[7] In *Contributions* during the 1970s and 1980s (as in other major journals), Marriott's ethnosociology could only receive insufficient intensive analysis and comparative evaluation (though it was frequently alluded to). This difference should be analysed as an issue of disciplinary intellectual history, putting a requisite distance between the living personalities and our attitudes toward them. Looked at this way, ethnosociology has enjoyed only a limited general appeal and influence. *Contributions* also, accordingly, devoted its special issues first to evaluating Dumont's and Srinivas' work.

Let us now consider some general approaches to studying the cultural other.

The other: Essential or non-essential

We face a crucial question: Is the cultural other ultimately dissoluble by an epistemological universalism that modern man and his scholarship produce? Or must it remain a subject of only contextual interpretations? (For a recent but inconclusive round of philosophic discussions, see Larson and Deutsch 1988; for a review and a continuing debate, see Rorty 1989a, 1989b; Taylor 1989.) The Western philosopher's position on such questions remains rather unhelpful. He is either a rigid rational universalist or a relativist. On the other side, Indic philosophical discourses (e.g., the Brahmanic, Buddhist and Jain) differ. Indian sociology therefore might do best to deal with the issue in terms of the 'lived culture', where cultural ideals, social contexts and historical forces must contend with the cultural other.

However, learned Indic texts remain a part of such inquiry. We cannot deal with the Hindu's otherness without grappling with different notions of self, soul and the universal soul (Potter 1965).[8] For example, the vedantic Hindu treats all forms of otherness (a sign of plurality) as a manifestation of illusion (*māyā* or *prakṛti*). For him, only self (the absolute *atman*) exists; his ideal is the total dissolution of alienating self (the ground of 'I-ness' and 'I'). Some of his philosophical ways of expressing such an essentialist position are: 'parts are unreal', 'effects pre-exist in cause', and 'difference is non-grasping of similarity'. At the opposite end stand the sceptics (Cārvāka) and Buddhists who see the other as an irreducible part of reality. Here the whole becomes unreal; effects do not pre-exist in cause; and similarity is non-grasping of difference. This non-essentialist position disputes the all-encompassing ideal and its reality.

However, such a story of Indic philosophies remains incomplete until we include those with 'middle positions' (Jains, Mīmāṃsikas), where both self and the other remain real and distinct. Everything is found to be both same and different, and equal and unequal under a philosophy (*anékāntavāda*) which strives to avoid taking either of the two extreme positions. Others colour self (as self colours others) moment to moment, but moment is denied any essentialism of its own. Both self and the other could thus be viewed from endless standpoints, with differing messages and meanings.

For sociological purposes, we may distinguish four general values or 'faces' of otherness: (*a*) the rational other (pursued by modern scientific universalism), (*b*) the critical other (evaluating the modern), (*c*) the contextually relativised (and transforming) other, and (*d*) the unique other (usually discovered via the history of a specific human culture and civilisation). We will briefly consider below each of these 'faces' in a schematic form, to help us better interpret Indian sociology's treatment of the cultural other—so far.[9]

The rational, scientific approach reduces all forms of cultural otherness to such scientific universals as causality, impartiality, symmetry and reflexivity (Hollis 1982: 68). The other (or any non-other universal) cannot have an independent or irreducible 'essence and existence' outside such a rationality. The cultural other can produce only particular and conditional knowledge. It is fully 'explained' and best represented by modern canons of logic and rationality (see Hollis and Lukes 1982).

The second 'face' of the other appears when such a conquest of modern reason encounters criticism from both within and without the West, yielding not only to 'post-modern' and 'post-structural' critiques, but also to debates on the effectiveness of modern reason in today's world (e.g., Clifford 1988; MacIntyre 1981; Overing 1985; Said 1978, 1983). Not merely a secondary, verifying example within this discourse, the other launches a critical evaluation of modern universals and their limitations and failures. However, this position of the cultural other still has to discover ways to avoid regression into the simple relativism of earlier decades (see Hatch 1983), and it must make sense of its own diversifying critical debates (e.g., Smith 1988).

The third face of the other appears when self, the other, and its otherness are discovered to be without any ultimate essence.[10] The 'particular other'

Though undoubtedly Marriott's approach is far more refined, accurate, and rigorous in cultural terms than Dumont's, his style of presentation remains inaccessible. His 'Hindu science' and Western formal scientific method continue to be a misfit within ethnosociology. Marriott's formalism (i.e., his transactional jargon, 'scientific' logic of parsimony, set theory, and multidimensional geometric representations) fetters him in approaching the multifarious (*vividha*) being and becoming of the Hindu world. The Hindu for him essentially becomes a transactional—porous—'body', with no room for a soul or a feeling self (or just a *jīvātaman*). The feeling-faithguided-aesthetic-intuitive-experiential world of the Hindu can only be transactional (hence 'fluid') within such an ethnosociology.

[8] Potter (1965) presents what major Indic philosophies, in profile, have to say on such major issues as parts and the whole, causation, and the notions of 'freedom' or liberation. However, the anthropologist must suitably adapt such information for his use. Thus, for example, the otherness issue may relate to a host of classical conceptions of self, soul and universal soul, from the upanishadic period; see Hume (1985: 23–32).

[9] We should at this point consider how Indian sociology in the pages of *Contributions* has (consciously or unconsciously) employed different models and meanings of the West's other as its own. However, we presently lack a suitable background study. Meanwhile, as we have already remarked (see note 7), the Western other very often enters via sociology's formulations, even if the adopted view is emic. A.K. Saran, for example, had very early remarked that Dumont's 'Indian sociology' remains alien to India (see Dumont 1966).

[10] Whatever fleeting cognition of these is admitted, it is simply to yield to ever-dissolving momentary perceptions. In this context, the Buddhist philosophic 'deconstruction' of self or I-ness (*anātmavāda*) is most radical. But even such a position cannot deny the everyday struggle with otherness (for the monastic lives of Buddhist monks see Carrithers 1983).

and the 'particular universal' can coexist here but only as conditional products of a multi-sided and ever-changing human cultural reality. (Compare this with the preceding summary of the Jain and the Mīmāṃsa schools of Indic philosophies.) Since the claim to an absolute, single universal is given up as either untenable or impractical, any approach or methodology that still hopes to reach the unconditional universal is subjected to criticism. Some dispute the very possibility of knowing the other.[11]

The fourth face asserts that all cultural otherness is in some sense unique (and irreducible). It is integral to people's cultural and moral perceptions of themselves. Though not beyond translation, it can only be incompletely translated, generalised and appropriated by another culture (Overing 1985: 1–28; Parkin 1985). India, China and the West are thus culturally distinct from each other. However, when deeply probed, they may disclose a long-standing ground for pursuing convergent (but independently-reached) similarities in reasoning and confirming reliable knowledge (Staal 1988: Introduction).

An anthropological account is likely to pursue, by context, all the four values of the other. In addition, it develops a distinct interpretation of otherness as a part of post-modern knowledge, usually in two phases. In the first phase, the anthropologist uses the other culture simply as a sounding board for viewing and re-viewing his own culture, usually with an uncritical acceptance of modern epistemology. The second phase criticises modern epistemology in order to open it to the existence of major alternative epistemologies. Though rare, such an attempt may still aspire for some kind of universalisation, often by intertwining emic and etic epistemologies. Marriott's 'construction' of ethnosociology perhaps exemplifies such an effort.

Four approaches and their characteristics

Let us now discuss the other in the context of some major developmental phases of Indian sociology—pre-Dumontian structural-functional approach, Dumontian 'structural sociology of India', Marriott's 'ethnosociological' approach, and the recent critical and interpretive explanations of *aspects* of Indian society and civilisation. These phases implicate the four general values of otherness as they render India to be the West's cultural-other (or vice-versa).

[11] Comparatively, some in Western philosophy have long debated the problem of knowing about the thoughts, feelings and dispositions of 'other minds'. For philosophers investigating other minds is not altogether the same as knowing about other knowledge by analogy, translation or other tropes (see Wisdom 1956).

Structural-functional approach

As the 19th century Indian cultural renaissance culminated in Independence, many prominent West-educated Indians (of whom Mahatma Gandhi and Jawaharlal Nehru were later examples) increasingly viewed India in terms of its own cultural history, disputing or dismissing the coloniser's otherness that the British introduced in their accounts (for psycho-historical studies of such 19th century conflicts among observers of India, see Chatterjee 1989; Nandy 1980, 1983). Most British writers recorded facts as they saw them, often for administrative and political goals of the Raj. Out of such general pools of data developed the 'empirical' social science field studies of India, especially after the Second World War. They fostered among the researchers a West-inspired 'scientific' intellectual temper, within which anthropologists and sociologists (Indian and foreign) viewed Indian society as an 'object' of study. These researchers mostly mapped, catalogued, classified and evaluated India's basic cultural units (i.e., villages, castes and tribes) with a studied political purpose. Succeeding them appeared numerous 'village studies' and caste and kinship accounts based on fieldwork, providing a 'scientific' basis for discovering India's 'social reality'. Objectivity here translated as a sort of aloofness (even for those Indian social scientists who studied their own villages or caste groups).[12]

Within this phase of logico-empirical research, India showed two distinct values, first as the 'cultural other' (evident most and best to a foreign scholar), and second as the scientifically produced 'objective other' (subscribed to by both Western and Indian scholars). Objectivity was considered both a necessary and sufficient condition to reach scientific truth, the analyst's ultimate quest.

During this phase, as the anthropologist collected maximum field data on the chosen subject, he consistently tried to remain aloof from the subject, projecting himself as subtly superior and enigmatic. He rarely felt the need to share *his* own field experience with his 'subjects', much less to accord *their* judgments a serious place within his scientific explanations. His informants were almost always passive 'producers of facts' rather than truth-knowing participants. The major analytic reasoning sought, discovered and established in such an approach, rested on the investigator's notions of 'scientific' observations and social science theory. People's own voice and reasoning remained indirect and muted even within careful field reports (e.g., Marriott 1955).

However, some of the best attempts, over time, tried to break away from such constraints. M.N. Srinivas' work, for example, successively

[12] For recent reviews, see Singh (1983) and Srinivas and Panini (1973). However, Yogendra Singh's term 'Indian sociology' refers to all that has been studied sociologically in India in recent times from various theoretical positions.

expanded the range and connotation of sociological description and characterisation in India as it dealt with changing caste groups and villages on the one hand, and as it suggestively depicted the fullness of Indian village life and its cultural sensibilities, on the other (Srinivas 1964, 1976). Adopting the precarious (insider-yet-outsider) stance of a social anthropologist, Srinivas perhaps best exemplified a carefully crafted 'sociological' approach suitable to a newly independent non-Western nation—his own.

Structural sociology of India

With the rise of structuralism, such Western categories as subject and object, ideology and practice, inside and outside, parts and the whole, and the individual and the collective, acquired centre stage, and they yielded, in turn, a West-complementing India. The proposal for pursuing a distinct 'Indian sociology' thus starts, as is well-known, with Louis Dumont's programme of studying such binary (logical) oppositions as high and low, purity and pollution, status and power, and hierarchy and holism (formulated after certain classical Hindu notions; see Dumont 1980, especially Postface).

The significance of Dumont's research scheme on India must, however, still be thoroughly investigated for its overall theoretical grounding in certain modern Western social philosophies.[13] First, it remained very close to the 19th century British-and-Brahman-pundit resurrected 'India'. Despite his initial announcements to establish a genuine 'Indian sociology', his 'ideological' approach severely limited his ability to embrace the diversifying, vociferous India of the 20th century. Second, Dumont's sociology instead chose to deal with one social quality (hierarchy) of the Hindu world and its cultural consequences. Third, it kept a glaring distance from the long-recognised and rigorously-worked studies of Indic cultural logic, epistemology and reasoning, available in texts, and often reflected within everyday life. While Marriott's work (1976a, 1989) has dealt with the second issue, the first and third points still need careful study and critique (for another discussion of some of the repercussions of the original Dumontian proposal, see Thapan 1988).

Ideologically contrasted to the historical, egalitarian and modern West,

[13] Dumont's research programme on India should be examined in terms of Dumont's reading of the modern ideologies of France, England and Germany. His view of India remains firmly grounded in the Cartesian 'ideology' of the modern West (with occasional critiques of its deformities during the world wars). In this way (while excluding the internal critiques and failures of post-war modern Western thought), he characterised India only as the West's perfect other—a society without the modern Western individual and a genuine politico-economic history. Ironically, however, this India (retrieved from a mixture of selective ethnography and Indological texts) appeared before the West only to reflect the West's own colonial archaism.

India provides a perfect counterfoil for Dumont to demarcate the West's own cultural boundaries, and a better self-definition, especially after the trauma of the Second World War. Dumont's study of India remained Europe-centred in many ways, and to depict India as the West's 'total' other (in the Maussian sense), he freely equated the Indian caste system with the Hindu world, and the latter with India (for observations on India as the non-European other, and the role of the 'anthropological community' within it, see Dumont 1977, 1986; and for a review Khare 1989).

In such a 'structural' view, Dumont found India without true history, genuine secular power, real economic motive, and the enduring moral individual. At the level of ideology, Dumont could not detain himself to deal with complicating alternative Indic models of parts and the whole (e.g., for a summary see Potter 1965: 103; see also Khare 1983, 1986). He implicitly assumed a confirmation of Durkheimian 'holism' within the vedantic view of 'the one over many', in order to contrast it with modern European individualism. We thus entirely miss in Dumont's work any attention to Indic (or even Brahmanic philosophic and epistemological) 'logics' to explicate the issues of the universal and the particular, or the self and the other.

Dumont's India, as some have commented, may have barely moved beyond the orientalist's notion of the 'dependent other' (see Appadurai 1986 for a reappraisal of Dumont's approach; for crisis in anthropological representation, see Marcus and Fischer 1986). However, Dumont's approach has had a rather persistent and diverse influence on the succeeding sociological studies of India. For example, anthropologists focused on 'traditional' India for its major (West-contrasting) cultural principles and categories. Since Dumont has himself selected and critically evaluated a whole crop of such studies in his revised and complete version of *Homo hierarchicus*, the reader is best referred to this book for his 'Preface to the complete English edition', 'Postface', and the corresponding citations in 'notes'. Such Dumont-inspired contributions displaced earlier village and social change studies of the 1950s. The latter were found weak in 'theory'. The 'new' approach also rendered historically-situated India spurious because history simply stood outside the 'structural' ideology of the traditional Indian caste system. Correspondingly, the Dumontian field worker concerned himself with discovering hidden category oppositions and their significance within social conditions. Ironically, such a scholar, though studying India from within, remained preoccupied with verifying his structural theory, and again remained elusive and distant to the informant. He rendered India's villages sociologically secondary, and India's modernity ideologically spurious (see, for example, Srinivas 1976, for disputing Dumont's position on the Indian village, and Khare 1989, for a study of modern India's 'otherness' to the West).

Ethnosociology

This leads us to another major attempt to study India from within. It is McKim Marriott's 'ethnosociology' that grew out of another series of attempts, a counter-theory of sorts, spelled out over 30 years. With his early preference for 'interactional' over 'attributional' theories, Marriott (compare 1959 and 1989) claimed to provide an 'alternative' approach to study and understand India. Though this 'transactional' approach is worthy of a detailed comparative analysis (for its roots perhaps lie in an American sociology of formalism and pragmatism, recast as a 'Chicago anthropology' of India), we will confine ourselves to Marriott's general strategies for a study of India from within (and for a tacit commentary in this approach on India's otherness to the West).

Marriott's 'ethnosociological' approach incorporates some radical epistemological points of departure. After proposing an interactionist explanation of castes in India (see Marriott 1959), Marriott reveals, through a series of exercises (1968, 1976b, 1987, 1989), his preference for, and a dependence on, certain formal 'sociological' tools, techniques, and three-dimensional representations of transactions. Essentially, his research proceeds in two phases. Up to 1968, as he scored the 'field-collected' caste ranking transactions on matrices, he illustrated his case-specific, logico-empirical analysis of village life (whether changing or non-changing) in north India. During this early phase, Marriott approached India, the non-Western other, for a 'scientific' (field and comparative) study. Despite his keen observations of the local scene, his overall approach emphasised certain etic distinctions (Marriott 1955). Though his field visits remained rare, Marriott proved himself to be a keen and sensitive field worker (Marriott 1966). Reflecting cultural empathy and alienation, he keenly described the festival of Holi for its internal cultural content as well as its otherness. (Besides, to an Indian observer also appear in the same account the pragmatic sensibilities of a midwestern American coping with unfamiliar rituals of celebration.)

Such an allusion to an 'early Marriott' of the 1950s and the 1960s is necessary to recognise the shifts he makes during the 1970s and the 1980s to construct an 'ethnosociology' of India. His review of *Homo hierarchicus* (Marriott 1969) perhaps marked the transition, for within a few years he, with help from Inden, launched his 'monistic' approach to articulate 'flows' within the 'Hindu world' (and worldview) to reach 'analytical sociological models, comparable to the theoretical generalised social systems of Max Weber or Talcott Parsons . . .' (Marriott and Inden 1977: 229).

In Marriott's terms, such a departure argued that it 'would not be a bad objective for [Western social scientists] to make themselves—the knowers—somewhat like those South Asian objects that they would make known' (1976b: 195). His most recent statements (1989: 1–2) continue in the same direction: 'social science ideas . . . can be developed from the realities known to Indian people'. Indian ethnosociology offers social scientists 'a second lens', 'a conscious alternative' to see through Western presuppositions and blind spots. Marriott and his close followers do so by pursuing their own distinct 'substance-flow' based view of the Hindu universe. This way they seem to be pursuing their own foibles—a formal, substantivist approach for rendering the Hindu's world concrete and systematic (and therefore 'real') in respectable Western scientific terms.[14]

Crucial to his 'construction' of ethnosociology, and directly relevant to our discussion of the cultural other, Marriott (1989: 1–6) provides us with some of his general assumptions and viewpoints:

(*a*) 'All social sciences develop from thought about what is known to particular cultures and are thus "cultural" or "ethno-" social sciences in their origins' (p. 1).

(*b*) Western social science, though widely recognised, remains an example of 'ethnosocial sciences of only one limited . . . type', and we need 'to expand the world repertory of social sciences' by 'working with a culturally related, but non-European people's thought about their own realities' (p. 1).

(*c*) The social scientist should be fully aware about the implicit assumptions of 'the traditional categories of sociological questioning'. He should therefore not risk 'imposing an alien ontology and alien epistemology' on other people's thought and realities (p. 2).

(*d*) The 'precipitates of Western social, intellectual, and particularly academic history' rarely fit 'Indian definitions of reality' (p. 2).

(*e*) Yet Indian (or Hindu) notions and institutions are not 'impregnable' to Western style analysis. 'Indian joinings' of what the West would dichotomise 'often point to *alternative*, especially transactional concepts of integrative value' (p. 3, emphasis added).

(*f*) 'None yet appears to have attempted what is proposed here—following the Parsons and Shils method all the way to constructing an alternative general theoretical system for the social sciences of a non-Western civilisation, using that civilisation's own categories' (p. 5).

(*g*) Such an attempt requires 'metaconceptual categories and descriptive

[14] Physical bodies, material transfers, concrete spaces, food, blood, humours, alchemy, and mechanical 'flows' by heating and cooling (or other similar devices) constitute the main domains of analysis for Marriott's ethnosociology and its followers (e.g., Mines 1989; Moore 1989; Moreno and Marriott 1989).

For some reviews of Marriott's work, see Good (1982: 36) and Daniel (1984: 53). One of the central disputes concerns equating the Western social scientist with his object of study, and dealing with the necessity of according India an independent—and equal—epistemic voice vis-à-vis the West. Proposing such an epistemic goal from the West remains exemplary. Indian sociology can only approve such an initiative, but only after examining some of its underlying ambiguities and assumptions. See note 7.

terms' that 'remain congruent with the indigenously cognised features' on the one hand, and 'facilitate comparison' with Western social science, on the other. 'Some shifting of Indian meanings in a Western direction' is also undoubtedly involved but Marriott hopes such 'compromises' are 'equitable (p. 6).

(*h*) 'Together with the ethnosciences of other lands . . .', Marriott hopes, '[Indian ethnosocial sciences] may provide better bases for the future claim of an expanded, multicultural set of sciences to have that "*universal* significance and value", which Weber . . . prematurely reserved for rational social thought in the West' (p. 3).

(*i*) Thus developed Indian ethnosocial sciences may, in his view, eventually 'take their place beside the Western ethnosocial sciences' (p. 3).

Positioning himself distinctly apart from Dumont's firmly Europe-centred epistemology of the cultural other, Marriott views India's otherness in terms of Hindu culture's distinct principles and categories of transactions and the resulting knowledge of reality. Though India is more than the constructions of the Hindu world, we still need to know how (and how far) 'congruent' are his 'Indian ethnosocial sciences' to all that constitutes and moves the Hindu universe.[15] Is the Hindu universe limited only to transactions? The issue of congruency acquires added significance when his own analytic assumptions rest on Western science (i.e., his formal notions of consistency, simplicity and parsimony in 'transactions' and 'materiality'), while assuming compliance from Hindu conceptions of knowledge and reality. Does Marriott also, in the final analysis; work only with selected aspects of the Hindu universe? Does he also overlook the possibility that representational and interpretive devices of 'mathematical analogues' and 'three-dimensional graphing' could limit and distort Hindu conceptions (and expressions) of reality? As a test of Marriott's reading of the Hindu world, on the other hand, one might ask indigenous Hindu scholars (pundits and *śāstris*) to comment on Marriott's schemes. Some might find Marriott intriguing—even appealing, while others may dispute him.[16]

[15] Marriott's general conception of the cultural other is difficult to decipher because of his silence on the subject. Though he carefully selects and employs other appropriate works—descriptive and theoretical—to produce his 'verifying evidence', such a procedure does not give us the required larger picture. We do not know how he approaches India's positions on knowledge vis-à-vis those that are Western. His view of 'science' also demands that we know how he approaches debates between foundationalist and anti-foundationalist theories of knowledge within Western philosophies (e.g., Rorty 1989b; Taylor 1989).

[16] I discussed in Lucknow in 1988, though unsystematically, some of Marriott's (1976a) formulations with a few appropriate scholars of learned Hindu texts and daily practice (especially drawn from such fields as classical philosophy, Ayurveda and astrology). They discussed several contexts that interest Marriott. This way my informants tried to

How do we make sense of some crucial achievements and failures of Marriott's ethnosociology? Among its achievements, particularly from the point of view of post-colonial social science in India, are (*a*) its bold and uniquely culture-sensitive approach to the cultural otherness of India, and (*b*) its readiness to make Indic epistemology a congruent and potentially 'equal' partner in reaching reliable and rigorous systems of knowledge. He is unsparing of 'the imperial style of Western ethnosocial science', and of a host of inapplicable Western concepts and distinctions. He finds 'processual relativism' of the Hindu ethnosocial sciences 'the most ecumenical of urges' (p. 33). He also hints toward a universalistic social science that, as a climax of a 'multicultural' set of ethnosocial sciences, rises above the 'fears of parochialism and relativism'.

Marriott's ethnosociological approach encourages culturally 'accurate' ethnographies (see Marriott 1989, for his latest careful selection and interpretation of appropriate works).[17] Under Marriott's influence, if an ethnographer increasingly tends to become a textual and contextual exegete of aspects of learned Hindu thought, it is to discover flows (and 'fluidarity') of diverse substance-codes in diverse domains of transactions. So encumbered, a young ethnographer unfortunately might have far less time (or inclination) to learn from the field.[18]

Failures of ethnosociology, on the other hand, as already indicated, accumulate from one's predisposition toward one grand theory for explaining Hindu India or India as a whole (such inspiration usually originating in the West). Knowing India from up close makes such attempts increasingly less satisfactory. Ethnosociology is no exception in this regard. Though forging wide links, it still conveys to me that we can manage only aspects of the large picture. Confined to transactional domains, it lacks direct and sufficient capability to deal fully with the Hindu's sensual and suprasensual reality, especially when concerned with self, moral order, knowledge, experience, unconditional liberation, and the Absolute Reality (*jīvātmana, dharma, sat*

peek into a scholar's mind and his understanding of the Hindu world. One of my scholar-informants, in review, found Marriott's work to be similar to that of a university educated *śāstri*. However, when it came to reflecting the experience of the diverse Hindu universe and its *lived* sensibilities, several of my informants found Marriott remote (in Hindi, *vey basey kam par paṛhé adhika hain*).

[17] Given a careful citation of the studies of students, colleagues and 'others', Marriott has developed a way of doing a sort of 'field work' via other people's ethnographies. By representing and synthesising these, he produces a kind of 'metaethnography' of his own to support his theoretical formulations.

[18] This may be particularly true of young ethnographers from outside India coming to produce a study *of* or *on* India. Oriented to establishing or disputing a prevalent theoretical explanation, such scholars may rarely approach the field for unencumbered learning. While they theorise, local scholars often devote themselves to intricate details. Unfortunately, viewed over time, both tendencies, until coordinated, would yield unsatisfactory results for Indian sociology.

jñāna, anubava, mokṣa, and the Brahman).[19] Such considerations fundamentally determine the Hindu's conception of being, karma, birth (*yoni*), and body (*deha*), and determine the admissibility of otherness, including of what Marriott is after—a verification of the transactional nature of the Hindu's 'seen' material and social world. As an approach, Marriott's ethnosociology has already attracted some comments and criticisms from philosophers (see, for example, several contributions in O'Flaherty 1980, especially by Potter and Larson) and anthropologists, some of whom find Marriott's approach as ultimately 'an anachronism' for Western social science (e.g., Daniel 1984: 54; see also Good 1982; Trautman 1980).

For the Indian insider, ethnosociology remains silent on what Hindu culture and epistemology most vociferously assert—'The explanation of the seen is in the unseen' (see Satprakashananda 1965: 193, and his discussion of the place of 'suprasensual knowledge' within the Hindu notions of self, the worldly, and the otherworldly). As a philosopher has observed, Marriott continues to emphasise transactions over transcendence (or dharma over *mokṣa* or *pravritti* over *nivritti*; see Potter 1980), often producing the problem of indefensible distinctions and lop-sided emphases in descriptions as well as in theoretical formulations.

Overall, ethnosociology represents one of the major Western approaches of the 20th century for understanding India (or analogously other such non-Western cultures) from within. It renders India's cultural otherness negotiable. Marriott's scheme is perhaps the first bold attempt to explore an alternative to an exclusively Europe-centred epistemology and worldview.[20]

For the 20th century, therefore, Dumont and Marriott, separately and together, conclude another chapter in the Western approach to India and its cultural distance. Though both claim theoretical differences between them (in terms of Western sociological assumptions), both study the cultural uniqueness of India (especially the Hindu culture and its worldview) from within. Both concern themselves with the learned and popular cultures of India, and both seek a single, internally-consistent theoretical explanation of the diverse Indian social reality by applying the well-known

[19] Marriott is however not silent on the liberation issue any longer. His Table 2 (1989: 14–15) summarises the larger picture of a 'processual Hindu social science', and it includes reference to the Hindu's philosophic 'constants', where his transactional and mathematical analogues score 'empty set' and 'nonrelationality'. Are they conceived the same way by the ordinary and the learned (*jñāni*) Hindu? Though we lack appropriate ethnographies on dharma and *mokṣa*, such subjects as *jīva, ātman, parmātman, māyā, bhakti* and *sādhanā* directly constitute 'the realities known to Indian people'. These are *not* ethnographically empty; they only need to be studied as people account for them. They constitute the Hindu's active, indispensable voice.

[20] By implication, the same approach must mean new challenges to better understand Hindu epistemology and how its reasoning patterns work within society. It also means a fuller explication of what 'an Indian way of thinking' is (see Ramanujan 1989).

'scientific' criteria of simplicity, economy and parsimony. Although both scholars tried to view India beyond the coloniser's hegemonic 'other' (characterised by one-sided reportage on and epistemic domination of India), only Marriott could develop a much more rigorous and comprehensive conceptual apparatus to address some of India's distinct cultural insights. However, once we focus on India's ever-diversifying history, social situations and culture, Marriott's ethnosociological exercise tends to close in on itself by holding rigid views on 'substance', 'joinings', and 'fluidity'. Instead, to succeed ethnosociology needs to invite openness, criticism and ingenuity (*paṭutā*) of a whole range of scholars, whether Indianists or not.

Critical cultural studies and interpretations

Though Dumont and Marriott recognise the necessity of identifying Hindu India from within, both strangely shy away from the dominant Hindu way of dissolving the other—by the ideal of Universal Self (which dissolves all alterity; for the crucial upanishadic conception of the Brahman, see Hume 1985: 32–52). We miss learning from them that Hindu India has its own way of dealing with socio-cultural distance and alienation, and that its direct conception and expression are fundamental to Hindu India's predominant self-identity. Instead, Hindu India is *sociologised* by Dumont in terms of Durkheimian holism, and by Marriott (1989: 5) for constructing 'a general theoretical system' *a la* Parsons and Shils. Indian sociology gains most when it critically appraises both, and starts its own cultural critiques of ongoing researches.

Thus, in fact, it appears that the next phase is of careful cultural interpretation and criticism. It is increasingly characterised by (*a*) a general dispersal of the Dumont-style 'total' ideological contrast of India to the West; (*b*) a preference for alternative interpretive approaches to deal with India's regional and ethnic diversity; and (*c*) an effort to develop what may be called 'reciprocal sociology' between India and the West. This phase strengthens studies conducted from India of the West as much as those from the West. Similarly, instead of constructing a single grand theory (whether monistic or dualistic) to explain India or the West, now several investigators may prefer to conduct their analyses of substantive issues (e.g., foods, gifting, sacrifice, and principles of equilibrium and appropriateness) to develop a generally shared perspective on the larger picture. They may seldom feel the necessity to commit themselves for life to any single grand theoretical explanation.[21] Within such a picture, most Indian

[21] For theme-based conceptual analyses, see Daniel 1984; Dirks 1987; Khare 1976; Madan 1987; Parry 1985; Raheja 1988; Zimmerman 1987. Though most of these generally uphold the goal of viewing India from within, they rarely investigate the Eurocentred epistemology of social sciences.

anthropologists and sociologists concern themselves with Indian cultural diversity and particularity.[22]

Though less frequently represented, the semantic or semiotic anthropology of the 1980s also focuses on meanings and interpretations of crucial textual knowledge and everyday practice. For example, one may consider 'the semiotics of Indian identity' with the help of Peircean semiotics because 'it provides an unexpected access to the inwardness of Indians and of Americans alike' (Singer 1984: 160). If the Indian other thus becomes less distant to a Western anthropologist, it allows a South Asian colleague to explicate the 'inside' and 'outside' of the Tamil's world by explicating the semiotics of 'substance', 'experience' and 'equilibrium' (Daniel 1984). Even more rarely, we encounter a 'semiological' study of India's view of the West, and of the West's internal otherness (Uberoi 1968, 1984). As Uberoi juggles the West's frames of familiarity and distance vis à vis those of India, he provides us with a valuable and scarce comparative commentary on the West's management of its own identity crisis. Some innovative efforts from India's political scientists are equally noteworthy, especially when they do not remain prisoners of the West's conflict theories and their production of otherness.[23]

Accordingly, the anthropologist's recent interpretive and critical approaches have increased reflexivity in their discourses, rendering the axis between self and the other as full of multiple 'voices', changing vantage points, and competing epistemologies—people's own vis à vis those of anthropologists (e.g., Babb 1987; Carrithers 1983; Madan 1987). Similarly, as learned texts are interpreted as the locus of authoritative

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Papers published in *Contributions* (new series) since 1967 generally support similar wide-ranging interpretive tendencies. It may be hard to find, for example, a staunch Dumontian among Indian sociologists and anthropologists working in India. If T.N. Madan has stayed closely with Dumont's work on India for over three decades, he studied, in my view, what Dumont had emphasised as well as ignored (e.g., 'non-renunciation'. See Madan 1987). I adopted a more critical (but constructive) stance toward both major—Dumont's and Marriott's—approaches to comprehend India's learned and popular cultures (e.g., for the Brahman's and the Untouchable's ideals and practices on food, kinship, rituals, and alternative ideologies, see Khare 1976, 1983, 1984).

[22] With Independence, Indian social scientists increasingly study culturally and historically particular faces of India. Even those aware of the India-West ideological divide often examine India for its ground-level regional, historical and religious differences (Thapan 1988).

[23] Rajni Kothari provides us with a valuable perspective on the cultural otherness that India and other non-Western countries face from the West. Kothari's work depicts such otherness as crucial to evolving a non-Western 'alternative' to modern political philosophy as well as practice. He raises the issue of India's own version of modernity (see Khare 1988). He argues for a coherent evolution of Indian political culture, especially in the wake of accelerated social change and ethnic strife (Kothari 1970, 1976, 1986; for a review of Kothari's work, see Pantham 1988: 229–46). Localised anthropological accounts of dominant 'ideology' as a hegemonic discourse between the Hindus and 'antagonistic others' similarly need careful conceptual handling. Too much emphasis on local conflicts tends to obscure the larger picture (e.g., Contusi 1989).

knowledge against people's social experience and its communication, they generate an anthropological critique of hermeneutics. Ideological texts sometimes become 'live' discourses, where words constitute a protest for challenging the entrenched equations of social and epistemic dominance (Das 1982, 1986; Khare 1984).

Such an interpretive phase allows the anthropologist to address the contradictory, the chaotic, the emotional, and the mysterious within the informant's world. It is feasible because the investigator is not after the construction of a grand theory and is willing to test his own reasoning and conception against that of the people. He opens himself to the messages other people's epistemologies and ontologies provide (for a comparative discussion, see Ardener 1985; Evens 1979; Overing 1985, especially the Introduction; Parkin 1982; Rorty 1980; Salmond 1985). As a consequence of such initiatives a self-conscious investigator emerges, who watches the politics of epistemologies within his studies of other people's identity and otherness (Crapanzano 1980; Rabinow 1977). Ethnography becomes doubly reflexive. It concerns the anthropologist's intellectual world as much as it does the informant's, with a continuous construction of, and commentary on, the nature of the cultural other. A crisis of representation usually reflects a crisis in our assumptions about our own identity and difference (Fabian 1983; Whitten 1988).

Whether ethnosociological, symbolic or hermeneutical, an explanation of India's self-identity (and what it considers 'others') must ultimately reside in India's own cultural reasoning and historical experience. Equally important is the recognition of scholarship that proves that the two—classical Indian and Western—systems of logic and epistemology are in fact independently standing—and authentic—with comparable and congruent structures of significance (for a careful discussion of this issue, see Staal 1988: 1–56). As the Indian systems of logic and epistemology thus receive more attention by themselves (e.g., Matilal 1971, 1977, 1985) and in comparison to the European counterparts, we will not only have a genuine basis for congruency between the two systems, but we can also employ it toward the development of more rigorous universalistic formulations. At present, the otherness issues are usually defined, studied and decided by certain basic distinctions produced by Western epistemologies alone (whether it is called power, class, ethnicity, alienation, or nation). Most often, therefore, we still know India primarily through a West-manufactured lens, or by our sporadic reactions to it. We require a systematic study of how India has, over the centuries, formulated its approaches to the other.[24]

[24] India, like the West, has produced, over time, several schemes to deal with (*a*) the distinct other—the outsider and the stranger (*yavana*, *dasyu*, etc.), (*b*) the similar other (Buddhists and Jains), and (*c*) the illusory other, a product of delusion and worldly attachment.

Future prospects

The preceding four approaches of Indian sociology, pursued since the 1950s, reflect a definite—but still limited—progress in dealing with the problem of the cultural other and otherness within India, and between India and the West. Though some attempts now show a greater sensitivity toward India as a centre of reflexive cultural knowledge, the basic issue requiring our attention is a careful study of the West's (conscious and unconscious) alienation from, and processes of domination of, India (or other non-Western cultures). Indian sociology must examine the issue from *both* directions—to discover how contemporary India pursues its different values of otherness from within, and how it confronts as well as accepts Western modernity. A similar exercise is necessary when, within India, the Hindu world is compared with the non-Hindu other (Islamic, Buddhist, Jain and Sikh). The other here refers to all those psychological, social, religious, historical and philosophic differences which constantly mark the Indian's life. Indian sociology cannot afford to neglect such research.

Despite the sustained work toward its creation and rationale, a long-range survival of Indian sociology, especially as a prospering comparative discipline, depends on its research of the cultural other. Such a sociology has also to nurture deep intellectual roots and perspectives developed by Indian thinkers over time. As we have argued, at the heart of such a pursuit lies India's own dual cultural 'grammar' of otherness, one when it looks within, and the other when it faces the outside world, especially of the long-influencing West. India's recent social history provides us with ample clues about such a dual grammar of otherness. (For a historical analysis of some—internal and external—social forces in India, see Saberwal 1986.) However, this focus does not mean that Indian sociology can be self-absorbed or inward-looking. Instead, it must become 'reciprocal sociology', investigating Western ideas and explanations by non-Western (Indian and non-Indian) intellectual locations, commentaries and criticisms. Within India, issues of cultural unity *and* diversity must engage the same sociologist. He must attend to both as a part of the lived culture.

But in order to do this, Indian sociology requires a rigorous analysis of its own identity, including its otherness to India. We need a critical evaluation of the larger historical forces which produced such Western approaches to India as those of Dumont and Marriott, and of their generally limited receptivity within India. If Indian sociology itself has been a product of the post-War forces, it must be prepared to change with historical changes (including those now afoot in Europe and across East and West). Such a new Indian sociology may be increasingly independent to debate and evaluate various evolving positions of the West-based 'universal' (and thus

also of the West's rationality and relativism; see Hollis and Lukes 1982). Recent appropriate exercises from India (Uberoi 1968, 1984) may have to be evaluated in terms of the hegemonistic nature of knowledge and representation on the one hand, and in relation to changing internal constraints on Western social science research, on the other.[25]

Internally, Indian sociology needs to enlarge its domain of interest and inquiry, bringing into focus long-existing springs of local and regional scholarship and knowledge. It requires that there be no unexamined dependence on (or automatic acceptance of) Western viewpoints. This would allow for a more open comparison and evaluation of those authentic studies that widely diverged in assumptions and outlook from those favoured in the West.[26] To underscore the point of a larger intellectual landscape, I have purposely treated the works of Dumont and Marriott together, without dwelling on their West-located internal theoretical rivalry. We need to place Western works in the larger historical and intellectual picture of India. The general point for Indian sociologists is to translate also various forms of that otherness that Western and Indian scholars produce between them as they pursue their favourite intellectual predispositions.

Though started in the 1950s as a particular Western approach to view Hindu India and its cultural ideology, Indian sociology, as recorded in the pages of *Contributions* since 1967, has already been undergoing a slow but definite diversification (and even dispersal) in approach, content and perspective. It currently seems to entertain contributions exemplifying in some ways all the four approaches to the cultural other discussed in this paper. Such a diversity is bound to increase with time, raising the necessity of fostering periodic critical reviews of major analytic approaches and their intellectual assumptions. The continuing role of the 'colonial mentality' is perhaps one such issue which Indian sociology can neither easily dismiss nor fully disown. The 'colonial mind' itself was perhaps neither all-knowing nor internally homogeneous. Nor was it equally successful all over India. If recent Indian scholarship wants to study the entire colonial encounter to control its cultural role in contemporary India, it has to make a long-term study of transforming faces of hegemonic knowledge. The exercises of Dumont and Marriott are instructive in this context—as well-researched

[25] Simultaneously, the Western intellectual temper might also be changing toward a generally neoconservative, Eurocentred culture, accompanied by declining employment in the so-called 'area studies'. Such a change has already promoted the idea of doing 'anthropology at home'.

[26] For example, we should juxtapose the studies of A.K. Saran (1963) and Marriott to examine their comparative assumptions about the Hindu's cognitive categories and cultural sensibilities (see also Madan 1987: 161ff). In a similar step, both these attempts could be studied against the appropriate insights and observations of A.K. Coomaraswamy on Indic civilisation.

explanations of India's identity and otherness. Though Marriott gets much nearer to India than Dumont, they still share more than they differ.[27]

For Indian sociology, there is still much cultural otherness from near and afar that remains to be translated and understood in Indian terms. Some of the enduring issues that so arise for Indian sociology to investigate are: India's own changing discourses on universalism and parochialism; implicit forms of cultural reasoning within such discourses; 'live' interrelationships between learned thought, regional variations, and local life-experiences; and multiple 'fundamental' grounds of truth validation and the consequent moral relativism. India-rooted Indian sociology, in such a complex endeavour, may have to undertake a more critical evaluation of its own sources of identity, both indigenous (*deśī*) and foreign (*videśī*).

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[27] Apropos their claims of 'major' theoretical differences, Dumont and Marriott stand most of all for understanding Indian society from 'within' (i.e., as formulated from their respective Western—French and American—locations). Both evolve their distinct 'scientific' or 'formal' approaches to explain India in sociological 'universals'. Yet Marriott (1989) reflects a better awareness of the integral nature of the Hindu world.

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Social science inside out

L.A. Babb

I find it easiest to visualise this world by imagining myself floating somewhere near its centre—drifting in the middle of a metaphor.

The whole is without boundaries, an unpartitioned plenum. Above is heavy, salutary wetness and a realm of beings who, from their commanding altitude, shed flowing residues on those below. Underneath is submissive dryness; the denizens of this region are blotterish and easily stained by the moisture falling from above. Ahead is a hot, molten zone; here valences have a feverish energy—outer crusts soften, boundaries dissolve, entities seek each other, open into each other, and merge. Behind is a region of icy unmutuality—here are adamantine crusts, unyielding boundaries, black hole-like isolations. On the right is bright symmetry and stability—a region of immutable design. To the left is dark and windy chaos—hurrying clouds and fleeting shapes in jarring patterns.

The beings of this world are intelligent, and they must—as best they can—calculate ways of dealing with the surrounding cosmos and each other. Depending on the means at their disposal, and on the ever-changing eddies, backflows and deviations in the surrounding flux, each being must act, and, in acting, assume a position relative to the others: always (and at once) more or less up or down, in front of or behind, right or left. Those who rise shed stains on those below as they assert superiority by draining residues away. Those below, more drained upon than draining, humbly absorb the rains and drizzles from above. Those who drift toward the heat ahead are, more than others, open beings who mingle freely with other beings and entities, always creating themselves anew through intimate exchanges. They *are* the others whose substances they share. Others (not so many) back into frigid isolations; self-hoarders and self-definers, they give little of themselves and take little of the selves of others. Friends of consistency and order gravitate rightward; satisfied with themselves and the world, they seek to abide in inner and outer harmony. Others, to the left, are blown about in inner and outer confusion, at war with themselves

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or others. Some seek this condition, a hopeful throwing of the dice in pursuit of new combinations and unities.

This is surely a possible world. Whether it (or something like it) is an actual world, a world conceptually and perceptually dwelt in by Hindus, is one of the most interesting questions yet raised in the anthropology of India.

Social science the Hindu way

Unlike its practitioners, I have not lived with Hindu ethnosocial science on intimate terms, and I must frankly confess that I have had to struggle with its novelty. There are echoes here of ideas encountered before, but the overall formulation seems strange indeed. How does one begin to sort this out? One good way is to try to find points of connection between ethnosociology and some similar but more familiar theoretical vision. In my own ruminations Peter Winch came quickly to mind.

For Winch, as for ethnosociologists, the central issue is what our 'idea' (as he puts it) of a social science ought to be. Readers may recall that Winch insists that 'social relations are expressions of ideas about reality' (1958: 23), which carries the necessary implication that in order to understand social relationships one must understand the ideas about reality they embody. Put slightly differently, to understand conduct requires seeing it from the inside out—'getting the point' of things actors do by participating in their own shared understandings of the meaning of what they say and do. This cannot be done by means of 'models' imposed on a given social reality by outside observers, because conduct gets the sense that it has from models already possessed by actors themselves.

Of course many others have taken this general view—though not necessarily from Winch's Wittgensteinian starting point—but Winch has a special claim on the attention of anthropologists because he traces its implications for cross-cultural studies with exceptional clarity. He does so in his celebrated critique (1970) of Evans-Pritchard's study of witchcraft among the Azande.

Evans-Pritchard's crucial error, according to Winch, is that he defines his problem as that of explaining how the Azande can consider witches real despite the fact that witches do not exist. In putting matters this way, Winch says, Evans-Pritchard has imposed an egregious distortion on Zande reality: in effect he makes it seem as if the Azande *do* believe in the same witches that many Westerners do not believe in. The basic mistake is that of privileging ideas about reality shared within one community (the community of Western scientists, rationalists, and their fellow travellers) as representative of an 'independent reality' against which the beliefs and actions of members of other communities can be measured. The result is not only to make the Azande look a little silly, but also to 'miss the point'

about the meaning of Zande divination in its rich local context. To get the point about witchcraft-related behaviour among the Azande, one must understand its meaning against the background of Zande reality, not Western reality.

I take this to also be the view of Marriott and his colleagues, although the context is different. When conventional social-scientific ideas are applied to Indian materials they smuggle in visions of reality alien to India. So-called social science is a creation of the West, and as such represents a theoretical elaboration upon ideas about personhood and social relations embodied in 'Western' forms of social life. These theories might or might not shed light on behaviour in Western societies, but if they do, it is only because they carry with them ideas about reality shared in the West. Theories of this sort masquerade as 'scientific' under the mistaken assumption that they traffic in realities that are independent of the thoughts and perceptions of Western (or any) social actors, and this pretense may even be innocuous, so long as Western social theory is applied only to Western societies. If, however, such theories are imposed on South Asian societies (or other non-Western societies), serious distortions are sure to arise—distortions on the order of asserting that the Azande believe in the same witches that the modern West does not. This will throw a veil over Indian life, preventing us from ever truly 'getting the point'.

The aim of the ethnosociological project is in this sense Winchian: the object is to interpret conduct in South Asia from the actors' point of view, taking into account 'their realities'.

But there is a lot more, and it takes us very far indeed from Winch. At issue is theory. Were one to press forward with Winch's notion of how Zande witchcraft ought to be understood (Winch does so only incompletely in his critique of Evans-Pritchard), then presumably one would generate some 'theory' of why the Azande do the things they do. But as far as I know, Winch has never systematically examined the relationship between 'theorising' and people like the Azande, those who are theorised about. Would the result be a Zande theory? Not exactly, although it would incorporate Zande views of the world. Would it be Winch's theory? Probably, although much within it would be Zande. Marriott and his colleagues are suggesting that the Winchian programme should be pushed to an entirely new level, the indigenisation of theory itself. Hindu ethnosocial science is not just a theory *about* how South Asian social life is informed by South Asian realities; it is a theoretical system about such things done in the manner of South Asian theorising.

Now this is incontestably a good idea. It makes every kind of sense to insist that Indians speak for their own society and to grant a central role to Indian theories about things that Indians say and do. If one intends to be seriously Winchian, then it is in this direction that one must surely sooner or later go.

There is, however, one rather formidable impediment, and this gives rise to ethnosociology's central oddity. In India there has been theorising of many kinds, but as yet no theorising in this particular direction, which means that no Indian social theory actually exists—or not, in any case, at the level of abstraction seen in Marriott's essay. It will therefore have to be devised, which is precisely what has been done—not, we note with interest, in Madras, but in Chicago, Illinois. Hindu ethnosocial science is held to be (if I have understood matters rightly) the sort of theoretical system that Hindus *would* devise were they to theorise on a rough analogy with Parsonian theorising but on the basis of realities known to them. It exists in a strange limbo. Even though we have seen Marriott derive it before our very eyes, it is not exactly 'his'. It is held, rather, to be Hindu; but neither is it quite 'theirs'.

This rather startling feature of ethnosociology is related to a curious implication. One might have thought that the first and most decisive test of Hindu ethnosocial science would be the sceptic's obvious challenge, namely, 'But do Hindus *really* think like that?' But this question turns out not quite to apply to the case, or at least not in any simple sense. It is probable that until the recent advent of Hindu ethnosocial science no Hindu has ever thought like 'that'. Indeed, one could go even further and say that few Indians *will* ever think like that, simply because few ordinary men and women give much thought to social theory. The Parsonian system may be good sociology or bad, but to most Americans it will always be simply incomprehensible. The validity of the Hindu ethnotheoretical system cannot rest on whether or not it 'seems right' to Hindus or anyone else. Its real test has to be the insight that it yields and its consistency with the data, and this would be true even if it had been devised in Madras instead of Chicago.

But there is even more. It turns out that *this* theory is not, as one might put it, a 'mere' theory. This is because even in the absence of Hindu theorists (as yet), the theory itself is held to be a reality of Indian civilisation. Fragments of it are present in Hindu theories of other kinds. But most important of all, even if it has not (until recently) been distilled into the formal sentences and diagrams of the theorist, the theory has all along been implicit in the things that Hindus say and do, and this is because the vision of reality it carries is precisely the vision of reality that Hindu social relations express. Even if it is not fully or explicitly cognised, it is enacted, for it—the theory—simply expresses in a certain explicit and systematic way the principles that supply things said and done in Hindu India with their most comprehensive meaning.

One may therefore say that ethnosociology can exhibit either of two faces, depending on the frame of reference. It is a way of *knowing* conduct. It is also what most *needs to be known* about that same conduct. The indigenous theorist (or his or her foreign surrogate) ascends a ladder of

abstraction in a process that is like coming to know one's own mind. In the end the theorist's 'knowing' and what he or she seeks to know fuse together This is a style of sociology that might well be called advaita. It is also strikingly original.

Cubes

I think it is unfortunate (though probably unavoidable) that cubes are visually so prominent a feature of Hindu social science as outlined in the Special Issue of *Contributions* (23, 1, 1989). Strange geometries leap out from the page, creating—I fear—the impression that ethnosociology is arcane in ways that it is really not. The cubes, of course, are just a convenient way of representing the essential thing. The essential thing is a number. That number is 'at least three'.

It is claimed that Hindus tend to explain events or processes as the product of at least three independent variables acting in concert. This is said to be a tendency particularly visible in certain intellectually systematised domains—in certain Indian 'theories', although these are not to be confused with ethnosocial science itself. Classical Hindu biological theory, for example, posits the existence of three 'humours'. That they may be seen as independent variables is suggested by the fact that the body's functioning is believed to be governed by all three humours at once as they vary in relative quantity in the body. The cube is simply a convenient way of representing a domain in which three such variables operate by utilising the familiar (to readers of such publications as *Contributions*) Cartesian coordinate system. The simultaneous operation of three independent variables is graphed on three axes at right angles to each other, thus defining a three-dimensional property-space. Marriott graphs four domains in this fashion: the 'humours' (*doṣas*), 'elements' (*mahābhūtas*), 'strands' (*guṇas*) and 'human aims' *puruṣārthas*).

The cubes are said to be 'geometric metaphors and mnemonics' for the property-spaces in question (p. 9),[1] and presumably this means that we should allow some latitude in the way Indic concepts are related to the axes. This reader, however, was left somewhat confused. Some of the variables seem to be of the usual 'more or less' kind; in the body, for example, there can be more or less 'bile' (a humour), and this can be represented on an axis. But other variables are represented as axes joining qualities or processes with their contraries or opposites: for example, 'advantage' (*artha*) and 'disadvantage' (*anartha*). I am not sure this makes much sense unless we are prepared to say that 'disadvantage' really means 'less advantage' (or vice versa). This may be reasonable, and I shall

[1] Page references given without a date are to the Special Issue (Toward an ethnosociology of India) of *Contributions to Indian sociology* 23, 1 1989.

concede the point, but it needs more discussion than it receives. Furthermore, Marriott asks that we envision all six sides of such cubes as 'open to movements between their internal and external spaces' (p. 11). But a Cartesian coordinate system has to have an origin (the point at which the axes intersect) at which the value of each variable is zero. This means that unless the cube in question is offset from each of the axes of the coordinate system defining it, three of its faces (those intersecting the origin) have to be impenetrable, because it makes no sense to say that bile, or whatever, is present in a quantity less than zero. It would be better, perhaps, to say that the various qualities and processes in question may be graphed into a three-dimensional coordinate system, and that the immediate region of the planes defined by the axes tends not to be occupied, which means that each quality or process is always present in some quantity greater than zero.

Is anything actually gained by rearranging Indian concepts in this fashion? The answer seems to be that such a rearrangement is inescapable, and that it is not really a rearrangement in any case. What has to be accepted is that at least three independent variables operate in these domains.[2] To return momentarily to physiology, the three humours do not appear to lend themselves to systematic representation on either one or two dimensions. That is, it seems impossible to show that the humours represent some single 'quality' in greater or lesser intensity, nor does it appear plausible that any single humour is a compromise between, or amalgam of, any two others. If this much is granted, then representing the humours as independent variables is simply a way of describing, using a particular formal idiom, the way Hindus actually conceive the humours to function in the economy of the body. Whether this actually holds true for all of the domains in question is an investigable question and has to be pursued one domain at a time.

It might be objected, however, that the use of this idiom itself—the language of 'variables'—sneaks an alien (and thus culturally distorting) concept into the Indian materials. I think not, but the point is non-frivolous and raises an interesting issue.

Unless one takes the view that cross-cultural communication is impossible (which is plainly false and probably the first step on the road to complete solipsism), one must assume the existence of some kind of conceptual common ground between cultures. Winch sees this clearly, and in order to avoid a complete retreat into relativism (a difficulty inherent to his Wittgensteinianism) he proposes the idea of 'limiting notions'—his candidates are death, birth, and sexual relations—as transcultural constants (1970: 107–11).[3] It seems reasonable to me to suppose that the idea of

[2] Whether at least three variables operate in all or most domains is a separate and debatable question.

[3] I should note that these particular limiting notions are offered in the context of a discussion of ethical relativism, not conceptual relativism.

'variable'—which need not be numerically metrical—probably qualifies as a limiting notion in roughly this sense.

One of the most interesting proposals regarding cultural differences and similarities in the way the world is analysed comes from Robin Horton. Horton's candidate for cross-cultural common ground is what he calls 'primary theory' (1982: 228–38). Primary theory, he says, is much the same in every culture; it posits a world of middle-sized objects, push-pull causation, up and down, fore and aft, and so on. I recognise that ethnosociologists will probably find these ideas uncongenial and that there is plenty of room for argument about the specifics of Horton's proposal. Still, unless we are comfortable with complete conceptual relativism, Horton's basic idea is compelling. Contrasting with primary theory is what Horton calls 'secondary theory'. At this level thought moves beyond ordinary experience to a realm of hidden entities and processes posited in order to surpass the limits of primary theory in explaining why the world behaves as it does. It is in their secondary theories that cultures differ in the way they explain the world. In some cultures, for example, illness is attributed to microscopic forms of life, in others to disturbed social relationships, etc. Of great interest is the fact that the hidden processes and entities postulated by secondary theories are conceived on the basis of analogies drawn from the familiar world of primary theory. An example is understanding air pressure by visualising molecules of air as rushing billiard balls careening into each other and the walls of their container.

The simple, unadorned idea that the characteristics of a thing depend on the nature and quantity of its ingredients probably belongs to primary-theoretical discourse. Here, it would seem, is the root-notion of variable: the idea that how much of something—anything—makes a difference. But the idea that a mixture of three invisible 'strands' (the *guṇas*) in varying proportions accounts for the characteristics of (say) persons—an idea that draws from primary-theoretical insights—seems to belong to the realm of Horton's secondary theory. And so would theories utilising such notions as the 'elements', 'humours' and 'human aims'. It is here, presumably, that what is really Hindu about Hindu thoughtways comes to the fore: the nature of the variables postulated, the particular effects of their varying permutations, and the boundaries of the domains in which they operate.

The constituent cube

Were one to continue to use Horton's formulation as a frame of reference, then Hindu ethnosocial science—as outlined in the Special Issue—would seem to be a strange and very interesting sort of beast: a form of 'tertiary' theory, a theory of secondary theories. Though unmentioned by Horton, this category is an implicit possibility within his scheme. A tertiary theory's relationship with secondary theory would be roughly akin to the relationship between secondary theory and primary theory. It would postulate a higher orderliness hinted at, but inaccessible to, secondary theory. It would posit connections hidden at the secondary level but referable back to processes known to secondary theory. In a sense it would be even 'more Indian' than secondary theories, being twice removed from primary theory, which is the realm in which the thoughtways of different cultures rest on common ground.

Without attempting to reduplicate the derivation of the scheme in extenso, it may be useful to remind ourselves of the basic idea. Certain domains or spheres of experience are said to be understood by Hindus to be governed by the convergent influences of at least three independent variables, and the domains in which these triads operate can be represented as three-dimensional property-spaces. Now there is, Marriott suggests, a single deeper structure underlying the several structures seen in the distinct domains. This deeper structure can be discovered by abstracting general meanings common to the variables operating in the various domains, and, on the basis of these general meanings, the separate cubes can be merged into a single master cube that graphs a single, general, three-dimensional property-space.

The question of which variables from the various domains match with each other is settled on the basis of resemblances 'felt' (p. 12) in Hindu culture, and this is determined by tracing connections evident in texts and ethnography. For example, 'passion' (one of the *guṇas* or 'strands') is related to 'fire' (an element) through the shared notion of heat. The Bhagavad Gītā, in turn, links passion with 'attachment' (*kāma*, one of the 'aims of life'); and in Ayurvedic theory 'fire' (the element) is associated with 'bile' (a humour). Thus a series is derived—fire, bile, passion, attachment—uniting the spheres or domains of elements, humours, strands, and aims of life. The general shared meaning of 'intersecting, opening, expanding' is abstracted from these, and this meaning is given the shorthand tag, 'mixing'. Mixing has its natural contrary in 'closing, condensing'—that is, 'unmixing'—and mixing and unmixing (the latter abstracting the shared meaning of what might be called unfire, unbile, unpassion and unattachment) together define the axis of the general independent variable mixing/unmixing, which Marriott summarises as simply 'mixing'.

The same operation performed on the remaining two series completes the set of three general independent variables: mixing (mixing/unmixing), unmarking (unmarking/marking), and unmatching (unmatching/matching). The master cube thus produced, which Marriott calls the 'constituent cube', is said to represent a 'general semantic property-space in which Hindus conceptually and perceptually dwell' and in which 'other realities of the civilisation—processes, actions, institutions, issues, etc.—should readily find places to function meaningfully . . .' (pp. 22–23).[4]

[4] For simplicity I am leaving the dependent variable (grossening/subtilising) and the constant (consciousness) out of consideration.

Put somewhat differently, this is a three-dimensional landscape in which the possibilities of Hindu thought and life are distributed. It is also a landscape in which the actualities of Hindu life are distributed in a certain way. In contrast to the 'West', for example, mixing is not only regarded as a perfectly feasible way for persons to constitute themselves and get along in the world, but in fact is generally preferred to the isolationist self-reflexivity (unmixingness) of individualism. In its fullest meaning, unmarking—which involves not only the marking or 'staining' of inferiors, but the neutralisation (thus 'unmarking') of the stain-exuding marker's self—is an empty conceptual space in the West, either ignored or unrecognised altogether. In India, however, it is exploited to the full. 'As the only reliable, directional force', Marriott remarks, 'unmarking-marking is what seems to Hindus to give continuity and relative stability to social relations' (p. 19). And in contrast with the Western presumption that it is normal for persons to be consistent in their relations with each other, the Hindu world posits the possibility and necessity of inconsistent, non-transitive (unmatched) relations under certain circumstances.

At the heart of this remarkable vision of the Hindu world are the three general independent variables, and the convincingness of the scheme obviously depends, in part, on the convincingness of the operations by means of which these variables were derived. The congruencies postulated between the strands, humours and elements seem generally plausible to me; I am somewhat less certain about the extension of the scheme to the 'aims of life' (*puruṣārthas*).

In Marriott's analysis, *artha* (glossed as 'advantage'), *adharma* ('incoherence'; the contrary of *dharma*), and *kāma* ('attachment')—all belonging to the sphere of the *puruṣārthas*—are linked, respectively, with *sattva* ('goodness'), *tamas* ('darkness'), and *rajas* ('passion') from the sphere of the *guṇas* ('strands'). The *adhārma-tamas* and *kāma-rajas* linkages seem logical, although Manu's formulation (in which *artha* is linked with *rajas* and *kāma* with *tapas*) makes sense too, and 'Manu', after all, is an expert. Somewhat troubling is the conjunction *artha-sattva*, which is an important ingredient in the derivation of the variable unmarking/marking. While it is true that *artha* is *a* good in the sense that it is certainly pursued by many, and while it is also true that material prosperity is often seen as a sign of a life well led, the connection between *sattva* and *artha* still seems a bit loose in the materials presented. Indeed, in Moreno and Marriott (Special Issue, p. 154), *sattva* seems to be linked with matching rather than unmarking (which is the locus of *artha*). Marriott's criterion for demonstrating such associations is that of 'felt' resemblances, but in this instance he relies to a significant extent on *rajas-kāma* and *tamas-adharma* associations in the Bhagavad Gītā, leaving *sattva* to be linked with *artha* by a process of elimination. While this may be a reasonable procedure, I think it would have been helpful to buttress the argument with more discussion of supporting ethnography than we find in the somewhat compressed text on page 13.

The purpose of a theory is to enhance our understanding of things observed, and this should be as true of 'ethno' theories as any others. What kind of understanding, we may wish to ask, does Hindu ethnoscience promote? How does it go about the business of explaining things?

There seem to be two senses in which Hindu ethnoscience may be said to extend understanding of Indian life. To begin with, additional data may be 'mapped into' the scheme. Marriott suggests that if he has the general Hindu property-space right, then aspects of culture not utilised in the derivation of the scheme 'should readily find places to function meaningfully' within it (p. 23). In other words, property-spaces derived for domains other than the strands, elements, and so on, should turn out to be congruent with the general property-space. Showing this for any particular domain would extend our understanding of that domain by demonstrating how it exemplifies a deeper structure. It might also enable us to perceive distinctions and relationships inherent to that domain that would otherwise not be seen. In addition, the scheme also hypothesises that groups will be generally consistent in the way their activities in various spheres or domains map into the constituent cube (p. 28). A group, therefore, in whose makeup 'darkness' and 'passion' predominate in the relative absence of 'goodness', should be a group whose entire range of activities will emphasise unmatching, mixing and marking. Specific features of a group's way of life could then be 'explained' as correlative with the group's general location within the constituent cube.

The papers that follow Marriott's opening statement in the Special Issue are offered as illustrations of how Hindu ethnosociology enhances understanding of Hindu life in diverse spheres or domains. The type of explanatory value illustrated is mostly, but not entirely, of the 'mapping into' sort.

The contributions of Ramanujan, Dirks, Raheja, and Wadley and Derr are not actually attempts to apply Hindu ethnoscience. Although these fine studies are certainly attentive to Indian realities, they pursue their own separate agendas without explicit use of the constituent cube. In general, however, these studies do produce insights that can, as Marriott urges in his own essay, be brought into a relationship with the more general scheme. Dirks and Raheja, moreover, raise formidable objections to Dumont's unidimensional purity-impurity-based model of Hindu hierarchy, and there seems little doubt that a multidimensional model will better accommodate the multiplicity of transactional strategies actually employed by castes in their relations with each other.

The essays by Mines, Moore, and Moreno and Marriott are much more self-conscious than the above in their use of Hindu ethnosociological categories. Purity and pollution are concepts less perfectly understood than

one might guess from their prominence in anthropological accounts of Indian life. Mines' suggestion that death and birth pollution can be understood as unmatchings (an echo of van Gennep) is a helpful step away from the familiar and ethnographically simplistic organic model of impurity. Moreno and Marriott's analysis of Tamil ritual illustrates the ability of ethnosociological ideas (with the emphasis on ethnosociology) to reveal thematic ordering in a very diverse array of deity-worshipper transactions. Ethnography is squeezed to the last ethnosociological drop in Moore's contribution, a closely argued paper in which she traces associations between the spatial distribution of domestic activities in the Kerala house and properties of the constituent cube. This paper, one of the most interesting in the collection, is a testament to one of the principal virtues of a genuinely interesting theoretical proposal: its capacity to compel us to notice the apparently ordinary (and apparently unproblematic) in entirely fresh ways. The analysis strikes me as impressive in its intelligence and intensity, but also just a bit Procrustean (see, for instance, the reversal of the Foundation Man, pp. 181–82). With its clear emphasis on the search for correlations between the constituent cube and patterns of human activity, this study is exemplary of the kind of work needed to gauge the explanatory power of Hindu ethnosocial science.

Speaking generally, these highly diverse essays do not constitute 'proof' of the theory, nor is this the intention. They do demonstrate that the ethnosociological approach generates ideas that can propel researchers into novel and fruitful lines of inquiry. They also show that the general outlook embodied by this approach can support a common discourse that is both intellectually stimulating and accommodative of different and sometimes sharply divergent individual approaches. That is as it should be. Certainly they demonstrate, too, that research and thinking done within the ethnosociological ambit can enlarge our understanding of Hindu culture and institutions.

Does ethnosociology help us understand everything we ought to understand? Probably not altogether. This brings us back to the matter of 'doing it the Hindu way'; the issue is whether an 'ethno' sociology can generate a completely satisfactory account of a way of life.

In regard to the question of the kinds of choices actors might make in seeking to get along in the Hindu world, Marriott remarks that '. . . seeking "advantage" (*artha*) may be preferable when one has the means to obtain it, but submitting to disadvantage may be preferable when one does not . . .' (p. 13). But is there not a major issue here? It is, of course, necessary to understand how advantage and disadvantage are culturally defined and expressed in interaction, and this is where analysis in the style of ethnosociology is important. But the question of *how* differential access to the 'means' of gaining advantage comes about is a matter of great interest to social theory and crucial to understanding what actually goes on in a society. This may be an area in which ethnosociological approaches do not work as well.

I suspect that the question of how persons or groups achieve the means of seeking advantage (however it might be culturally defined) must inevitably draw us away from the world of cultural definitions to the world of soil, weather, topography, demography, technology, and historically actualised patterns of domination of some groups and persons by others. This is a world that is not necessarily well understood by those who live in it, and therefore a complete account of a way of life would seem to depend on the assumption that there are at least some relevant forces or factors that do not enter (or do not enter directly) the world in which actors 'conceptually and perceptually dwell.' (On this point, see also Dirks in the Special Issue, pp. 59–77.) Winch's cautions notwithstanding, the view that social analysis must posit the existence of some realities independent of realities known to social actors seems well-founded. Whether these are true 'independent realities'—that is, independent of *any* actors—is less easily decided. These points have been made many times before, and of course do not invalidate the position that in order to fully understand a form of social life one must know how actors perceive themselves and their world.

Is ethnosociology vulnerable to the charge of unfalsifiability? No, because it does in fact predict correlations that observation might or might not confirm. In practise, however, it may prove very difficult to operationalise ethnosociological concepts rigorously. Moore's shift to 'dialectics' when 'correspondences' between behaviour and the cube do not work out seems illustrative of the problem (pp. 192–99). Moreover, the possibility of alternative Indian ethnosociologies hinted at by Marriott (p. 32) could greatly complicate the issue of falsifiability. In any case, ethnosociology is not likely to stand or fall on anything resembling a single decisive experiment. In the end what will matter most is how well the theory, treated as a unifying view of Hindu culture, generates a tradition of fruitful scholarship that will necessarily interact with data in very complex ways. I think the auguries are good.

The question of falsifiability, however, does raise a related issue. I suspect that it will strike many readers that it is hard to imagine *any* process or event—whether occurring in the Hindu world or not—that is not mappable into the constituent cube. There is a strange kind of doubleness to the independent variables. On the one hand, they were derived by abstraction from clusters of Hindu ideas. In this sense they seem quintessentially Hindu. But then we are told that these selfsame independent variables correspond to 'fundamental relational properties of mathematics and symbolic logic' (p. 17). It is as if we have been transported by a leap of intellection from what is ostensibly the deepest inwardness of Hindu culture to a world of what look suspiciously like conceptual universals.

Seen from this standpoint, the general variables seem more like 'limiting notions' than ideas that are somehow unique insights of Hindu culture.

Could it be, one wonders, that the independent variables are ideas of physical process so elementary (not, of course, in their mathematical formalisations) as to be necessary if the world is to be present in thought at all? And if, after all, they are *implicit* in Hindu life and thought (needing the theorist, that is, to clothe them in explicit language), is it not possible that they are similarly implicit elsewhere? What then might be concluded is not that Hindus are distinctive in their possession of, say, the variable mixing/unmixing, but that they have—in contrast to other civilisations—seen in this universal possibility a plausible element in a conception of personhood. Other peoples—Americans or anybody—may have used it in different ways or in different departments of life and thought. The 'deep comparisons' alluded to by Marriott (p. 33) would then emerge as one of the most interesting of the possibilities opened up by this remarkable theoretical vision.

What should we ask of a theoretical proposal? Not, I think, that all of its theoretic or empirical implications be realised in one breath—not at the moment (which may be a fairly long moment) of birth. In the very nature of the case, a theoretical proposal is not a completed thing. It is, rather, a new way of looking at the familiar, and in this sense a bundle of possibilities. What will actually be seen looking at the world freshly is always a tale mostly told later on. The routines of scholarship will work cumulatively and self-adjustingly; mid-course corrections are a distinct possibility. But at the outset the issue seems to be this: Is the proposal not merely new but genuinely interesting? Does it jar us loose from unexamined habits of mind? Come what may, it is difficult to imagine an attentive reader of these essays ever looking at Indian social life in quite the same way again.

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Deconstructing McKim Marriott's ethnosociology: An outcaste's critique

Michael Moffatt

In 1977, anthropologist McKim Marriott and historian Ronald Inden, both at the University of Chicago, published an interesting short essay entitled 'Toward an ethnosociology of South Asian caste systems'. Together with four other publications, to which Chicago anthropologist Ralph Nicholas also contributed, it seemed to presage a new interpretation of South Asian culture, an equally global and comprehensive alternative to the structuralist theory of Louis Dumont which had emerged about a decade earlier (see Inden and Nicholas 1977; Marriott 1976a, 1976b; Marriott and Inden 1974, 1977). A book illustrating the application of this new approach to caste was promised. But Nicholas and Inden soon drifted away, and between 1977 and 1989 very little ethnosociology[1] appeared in print. Yet the perspective lived on under Marriott's aegis at the University of Chicago. By the mid-1980s, one insider could refer to it as 'Chicago *jati* anthropology' (Daniel 1984: 55).

In 1983, interested in how Marriott's approach was developing, and sharing with other colleagues outside Chicago the frustration of being unable to read and evaluate it in standard published sources—in books and journal articles—I decided to conduct an expedition to it: to talk to and listen to and argue with members of the ethnosociological caste, especially with its pundit, and to attempt to develop an edited published collection of some ethnosociology-in-progress.

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[1] Marriott and Inden defined 'ethnosociology' the first time they used it as 'cultural analysis . . . using concepts . . . understood and accepted by all sorts of South Asians in discussing their own social systems' (Marriott and Inden 1977: 227). That is, following conventions in American cognitive anthropology, it meant 'the natives' sociological notions and ideas', with the added implication that Indic sociological conceptions were at least as sophisticated as Western ones, and more appropriate for South Asia than imported European concepts. Marriott has, however, defined the term at greater length more recently (Marriott 1989: 3–6).

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As the project developed, however, Marriott eventually joined in, and later I dropped out due to disagreements about who exercised editorial control.[2] The present paper is what is left of my original summary and critique of ethnosociology, based on my own research into ethnosociology in the early and mid-1980s[3], and including more recent remarks on ethnosociology as it has finally been published in 1989 (see *Contributions 1989*).

Retrospective portions of this paper seem worth preserving on the grounds that Marriott's new paper, his first significant publication in 12 years, may be better comprehended and evaluated when one has a sense of the stages through which Marriott's thinking developed during the years in which he published virtually nothing. Despite his negative judgement of an earlier version of this critique, moreover, Marriott has made a number of rhetorical and explanatory changes between the last two drafts of his theory (between Marriott 1983 and Marriott 1989), some of which can be read as responses to this critique (see footnotes below). Earlier versions of the present half-silenced critique, in other words, are part of the history of the current version of Marriott's ethnosociological paradigm.

Anti-Dumontian roots

The four programmatic papers on ethnosociology which Marriott jointly or individually authored in the 1970s came as a surprise to those who knew his work. For in them, without abandoning certain aspects of his older behaviourism, formalism, and detailed, empirical methods, Marriott

[2] In 1983 and 1984, I initiated the project with panels on 'Ethnosociology and related approaches to Indian culture' at the meetings of the American Anthropological Association and at the Conference on South Asia in Madison, Wisconsin. Marriott was discussant in both panels—and at the latter, the Social Science Research Council also supported a day-long workshop on ethnosociology. Susan Wadley joined me as joint editor of the proposed volume in 1986. Marriott asked to be included as a third joint editor in 1988.

In December 1988, however, Marriott wrote me that my evolving summary and critique, which he had seen in earlier drafts, 'cannot be included' in the collection which has now been published in a Special Issue of *Contributions to Indian sociology* (23, 1, 1989). Though his judgement struck me as inappropriate, I withdrew my paper for fear I might further delay the already much-postponed preparation of papers by Marriott and his closest collaborators if I insisted on my right to my own critical voice. (More recently, Marriott has also unhappily succeeded in insisting that Susan Wadley withdraw from shared editorial credit in the second published version of this collection.)

[3] Retrospective parts of this critique are based on the published papers and unpublished working papers cited, on listening to several of Marriott's formal presentations and on two long conversations and a number of shorter chats with Marriott between 1983 and 1985, and on correspondence with Marriott from 1983 to 1988. All the working papers I am citing here were in wide circulation in the ethnosociological in-group for years, none of them has 'do not cite' or equivalent prohibitions written into it in received form, and one other such working paper by Marriott ('Terminating Indian impurity', 1982) has already been cited by Raheja in print (see Raheja 1988: 46, 275).

suddenly proposed the feasibility of constructing a global model of Hindu society which focused on Indian 'thinking' and on Indian 'cultural assumptions'.

The immediate source of Marriott's new mentalism and holism was David Schneider, via Ralph Nicholas and Ronald Inden. In his analysis of American kinship, Schneider had argued that a certain type of 'cultural account', aimed at uncovering deep, shared, implicit categories, could be done at a separate level of analysis from that involved in social or historical or political or economic explanations (Schneider 1968). Inden and Nicholas applied the same hermetic perspective to their analysis of Bengali kinship (published in 1977), and Marriott soon joined them in applying a similar deep-categorical treatment to broader aspects of South Asian culture.[4]

An equally important source of Marriott's shift between the late-1960s and early-1970s was, in my opinion, the challenge presented by Louis Dumont's equally grand theory of Indian civilisation, one which also centred, as ethnosociology would do, on Indian culture. Marriott has protested that there were many other influential culturalists in anthropology who influenced him besides Dumont—Boas, Malinowski, Benedict, Margaret Mead, Whorf, and Levi-Strauss; at Chicago, McQuown, Redfield, Geertz, Schneider, and Friedrich among others. Perhaps so. But why did these influences sink in so suddenly for Marriott between 1970 and 1972 in particular? And why has he continued to de-emphasise hierarchy and purity and pollution so vigorously if the Dumontian view was not crucial to him as a rival paradigm?

What, in any case, did Marriott think of Dumont in the late-1960s and early-1970s? His very negative review (1969) of Dumont's *Homo hierarchicus* is interesting to read in retrospect. Marriott politely praised Dumont's ambitious theory for asking new questions about South Asian culture which were 'immensely worth asking', but then criticised almost every particular of his structuralist interpretation—Dumont's sources, his 'intellectualism', his distinction between status and power, his privileging of purity and pollution, and, particularly devastatingly, his interpretation of the principles of rank in local Indian caste hierarchies.

Marriott was equally unhappy with what he saw as the theoretical indeterminacy of *Homo hierarchicus*, with its 'proposed dialectic of opposite, complementary, partial models at different levels', which 'leaves explanation of any actual data in an indeterminate state' (Marriott 1969:

[4] Steve Barnett had actually first applied Schneider's approach to Indian ethnography in a dissertation written under Marriott's supervision in 1969 and 1970 (Barnett 1970), but his work was not cited in the first drafts of the early ethnosociological working papers—on the grounds, Marriott later argued, that Barnett had not also included monism in his pioneering efforts. Two decades later, it is ironic to note that Barnett was originally thrown out of the ethnosociological garden for his non-monism, whereas in the 1980s Marriott himself decided that monism may not be the key ethnosociological proposition after all (see later).

1173). Marriott considered the cumbersome logic of Dumont's 'encompassment' particularly incomprehensible. What sense did it make to embed a relationship of contradiction within a relationship of inclusion? Marriott asked at the time, and has continued to ask.

More intriguingly, probably projecting the change he was about to make in his own thinking, Marriott detected a subtle interpretative shift in Dumont. The earlier Dumont of *Contributions to Indian sociology* had been 'on the fringe of South Asianist thought' due to 'certain bold assertions', Marriott claimed, while at the same time pursuing an 'all-encompassing, monistic intellectualism' by arguing for the unity of India, the inseparability of ideology and behaviour, etc. The newer Dumont of *Homo hierarchicus*, on the other hand, was advancing into 'a more complicated, confusing universe of discourse . . . more thickly populated by other scholars'—while concomitantly becoming disappointingly more 'dualistic'.

What was wrong with the new Dumont's dualisms? Behind them lay 'a Platonic metaphysics' and other forms of European social scientific ethnocentricity, Marriott asserted, which made nonsense of Dumont's claim to take a non-Western view of India. Dumont was really using India to illustrate his readings of Hegel, Marx and Durkheim. But despite his suggestion that the earlier Dumont had been less influential due to his monism, Marriott himself had jumped into such a position within a few years.

Monism, substance, fluidity and the cube

Rhetorically, monism was at the heart of ethnosociology in its first formulations. It was Marriott's initial characterisation of perhaps *the* fundamental property of Indic folk culture viewed in purely South Asian terms. It was his replacement for all those irrelevant Western ethnocentric dualisms in *Homo hierarchicus*—an all-purpose weapon against virtually all other Western scholarly approaches to the analysis of Indian society, in fact. If South Asian thought was fundamentally monistic, then materialistic or positivistic approaches misrepresented it, for South Asian thought presupposed the co-substantiality of the material and the ideational. Symbolic anthropology was also inappropriate, conversely, for Indian thought was fundamentally 'ethnometonymic' rather than 'ethnometaphoric' (my terms, not Marriott's), seeing sign and signified as inherently linked rather than as standing in representative, partially arbitrary relationship to one another. Likewise the irrelevancy of cognitive anthropology (there was no thought/action dichotomy). Etcetera.

Analytically, however, monism tells us what Indic thought is not; it does not tell us what it is. Accordingly, in his unpublished working papers in the late 1970s and early 1980s and in his oral presentations, Marriott tried to

rough out what some of this positive content might be. He did so in an often allusive, highly condensed language, however, with a minimum of exemplification or ethnographic detail.

As Marriott first stated the possible fundamental presuppositions of Indic thought as he understood them in 1976, all the elements of South Asian cultural reality were 'substances' or 'substance-codes'. Indic thought was highly particulate. And Indic thought was fundamentally transactional; its basic processes were flow and transformation, processes more amenable to fluid or gaseous metaphors than to mechanical, social structural models more commonly employed until recently by Western social scientists when looking at India.

The unity of 'substance' was in one sense a simple logical entailment of monism; philosophically, monism means that there is just (or ultimately just) one thing. Dumont's early structuralism, Marriott had suggested without elaboration in his 1969 review, had had the same implication. If, following Dumont, everything in the world of caste was relationally defined—ultimately by its relation to what Dumont simply called 'the whole'—then that relational whole was the only real 'thing' in the Indic world (my understanding of Marriott's point, not his own explanation).

To say, as Marriott did, that this Indic substance was also highly particulate was to say that it was minutely divisible down to subpersonal particles. In Dumont's theory, South Asian actors' identities were always defined by their structurally varying relations with other actors; the sannyasi aside, Indian thought never quite got down to the level of the individual for Dumont. For Marriott, on the other hand, the Western individual did not exist in traditional Indian culture because the stuff of Indic thought invaded and subdivided the actor—it reached down far beyond anything so simply and abstractly imagined as a whole bounded person. 'Actors are evidently seen as unique composites of flowing prior causes, understood as component substances and qualities' (Marriott 1975a: 1). Marriott's monistic South Asian substances included 'coded' bodily substances, food and territorial substances, and subtler 'things' conventionally seen by other analysts as immaterial or ideational—'perceived words, ideas, appearances, and so forth' (Marriott 1976a: 111).

Flow and other non-solid metaphors were and continue to be the images Marriott has employed to link up subpersonal coded-particles and other South Asian substances; process has been to Marriott's monism what structure was to Dumont's holism. And since 1969, Marriott has simultaneously criticised most of the specifics of Dumont's structuralist image of India and suggested a still-evolving set of processual replacements.

Dumont's status and power were not the necessary 'encompassed' contradictory dichotomy which explained workings of the middle levels of local caste hierarchies; four different transactional strategies operating within a homogeneously defined cultural universe would generate the same phenomena (Marriott 1976a).[5] Purity and pollution coexisted with many other indigenous interpersonal values (with power-as-*śakti*, for instance, or with auspiciousness), so it was ethnographically incorrect to privilege them; and Dumont's treatment of purity and pollution was oddly substantialist given his own structuralist assumptions, and inaccurate. (Later these particular criticisms were reduced, but purity-impurity was then discovered not to be conceptually primitive—see below).

In his working paper on the 'open person' written in 1979, Marriott suggested that there were three very general 'implicit Hindu cultural formulations' for the 'processes in flowing substance'. The first was 'transformation', which had to do with one-way flow, typically upward and downward; higher beings were 'more transformative', less 'marked' (in the linguistic sense), subtler, more discriminating, and lower beings were the opposite. If this sounded like Brahmans and Untouchables in Dumont's scheme, however—like purity and impurity—it was not, Marriott insisted, for theoretical reasons that will be reviewed below.

Marriott's second initial process, 'complementation', had to do with two-way reciprocal transactions, with being 'open' or 'closed' to interactions. This was the dimension of maximal-givers versus minimal-givers by which Marriott sorted out the alleged confusion in the middle of Indian caste hierarchies, *contra* Dumont. The Indian *guṇas* were first brought into Marriott's scheme at this point. Open beings were likely to have the *guṇa rajas* ('passion') and to be 'hot'; closed beings were apparently the opposite.

Marriott's third process-type, 'articulation', originally had to do with 'compatible' conjunctions with the substance of some other being, or with internal compatibilities or incompatibilities. The disarticulate was the impure, Marriott suggested in this first trial formulation, but also something perhaps in a state of positive change (Marriott 1979).

In a working paper written four years later, ' "Hindu social science" and its developing implications' (1983), Marriott renamed, expanded and reordered this same triad. He also formalised and mathematised it, and centred more and more of his analytic efforts on it from then on. He did this to an unfortunate degree, however, and—given the many weaknesses of the model, I will argue below—to the ultimate detriment of other

[5] Marriott's methodologically impressive analyses of local Indian caste hierarchies in the 1960s (see Marriott 1968) had failed to indicate the 'confusion in the middle' which more impressionistic ethnographic accounts had reported, and which Dumont had used as part of his evidence for encompassment—wealth and power overwhelmed caste principles in the middle of the system, Dumont believed. In this 1976 reanalysis, however, Marriott now demonstrated that castes in the middle of local hierarchies were nevertheless able to pursue more diverse transactional strategies than those at the extremes because, given the mechanics of the totalising scoring of caste transactions which he had also demonstrated, middle-ranking castes had more room to manoeuvre between 'maximizing' and 'minimizing' approaches to exchange.

potentially interesting or insightful aspects of his ethnosociological thinking.

Complementation became 'mixing' and was now mentioned first. It was still paired with the *guṇa rajas*; it was also now associated with other Indic essences—with fire, *kāma* and bile; and it was now said to be analogous to the mathematical property 'nonreflexivity' (roughly, not being in a relation to one's self). Transformation became 'marking'; it was paired with the *guṇa sattva* ('truth', 'goodness') and with water, *artha* and phlegm; and it was now said to be analogous to the mathematical property 'nonsymmetry' (because it was a one-way rather than a reciprocal relationship). And articulation, in a slightly confusing lexical inversion, became '*un*matching' (emphasis added), paired with the *guṇa tamas* (darkness), with air, wind, *adharma*, and said to be analogous to the mathematical property 'nontransitivity' (roughly, three or more elements not being in a mutually consistent relationship with one another; in a nontransitive system, if A, for example, is higher than B and B is higher than C, A is not necessarily higher than C).

Marriott then mapped these in a three-dimensional space, first drawing what he called 'the constituent cube' in order to make the point that they were logically independent. Left to right along the base of the cube ran 'unmixing-mixing', front to rear ran 'matching-unmatching', and top to bottom ran 'unmarking-marking'. Different three-dimensional shapes and diagrams, also coloured to represent similar mixes of the *guṇas*, were then drawn inside these cubes to demonstrate how the three fundamental 'presuppositions' or 'metaconcepts' of Indic thought might 'account for' a wide range of Indian 'embodiments' (different ethnographic or textual analyses as redrawn by Marriott) 'from the molecule to the cosmos'. Very little explanation of the exact relation between the diagrams and the data in the sources was provided.

Marriott's cubic drawing of Marvin Davis' account of a Bengali schoolteacher's analysis of the ranked life-forms (Davis 1983), for instance, showed 'Brahma' in the top right of the cube two-thirds back (mixed, unmarked, unmatched), 'deities' a little lower, further forward but far left (unmixed, more marked, more matched), 'humans' back under Brahma but further forward (mixed, medium-marked, matched), 'demons' directly under humans (mixed, more marked, equally matched); and, at the bottom, 'plants' (left rear: unmixed, marked, unmatched), 'objects' (left middle: unmixed, marked, more matched), and 'animals' (right rear: mixed, marked, unmatched).

If this drawing was difficult to interpret, Marriott's images of Francis Zimmerman on the *rasas* (Zimmerman 1980) and of 'family hydraulics' according to various sources were, among others, entirely inscrutable, to my eyes at any rate (Marriott 1983: diagrams 4, 13 and 16). Very few South Asia specialists at several sessions where I heard this paper presented seemed able to read or replicate these diagrams except Marriott. Though his new publication of 1989 contains the same basic cubes and further developments of the same mixing-marking-unmatching scheme, Marriott has, for the moment, dropped the complicated internal diagrams, though he hints they may return in the future with the help of 'matrices and computer programmes for multidimensional scaling and graphing' (Marriott 1989: 27).

Toward a critique

Despite the imponderables of the cube, Marriott's ethnosociology had been an often impressive creative and synthetic effort by the middle of the 1980s, a vision which could be more convincing when Marriott presented it himself than in second-hand renderings of its most general formulations, as here—a set of creative assertions which a diverse group of students and colleagues have found useful in various ways (see, for instance, Daniel 1984, Dirks 1987, Kemper 1977, Pugh 1983, Raheja 1989, Shweder and Bourne 1982, and Trawick 1988).

The knowledge of India on which Marriott's ethnosociology was built, the cultural insights which gave it much of its persuasive force, were often difficult to fault. The apparent systematicity of Marriott's thinking was remarkable, the way in which each new theoretical statement seemed to build on earlier statements at a higher level of formalisation or generality There were other strands in ethnosociology on which Marriott had written very little even in the form of working papers but where his influence had also been felt, in work on karma, the person and cross-cultural psychology And games had played a more important role in Marriott's thinking and communicating than this summary has indicated: the evolving simulation game *saṃsāra* (Marriott 1987), games as a pervasive theoretical model.

The new imagery accompanying Marriott's new ethnosociology could also be seductive. Marriott's India did not have a static, 'ascriptive' 'social structure' divided up by boxes and boundaries, nor was it an other worldly, life-denying culture. It was fluid, open, always essentially changing and life-affirming. Nor did Indian society prohibit social mobility. It was full of it, Marriott asserted: mobility without reference to caste, individual mobility between castes, and collective caste mobility (Marriott 1975b). The essence of India was not even caste any longer; caste was an artificially constricted Western notion of something actually much more general, 'natural history as understood by South Asians' (ibid.: 5). Nor was religion the essence of India—religion understood as 'the spiritual', at any rate. For given monism, what Westerners understood as the spiritual or the supernatural was always ineluctably materialistic and natural as well.

The question nevertheless had to be asked, were Marriott's ethnosociological characterisations of South Asian culture true? The following were

some problems with ethnosociological thinking and its relation to Indic cultural reality, as I was able to understand both of them from a certain distance after the first decade of the development of ethnosociology, as of the mid-1980s.

The first has been alluded to above: the abstractness of many of the terms employed, the difficulty anyone but Marriott had in understanding what some of the general propositions meant, and how they might or might not apply to any given ethnographic fact. The indeterminacy Marriott had complained about in Dumont's structural theory seemed to have come back to haunt him in his similarly high theoretical mode. Also, if Marriott found it easy to dismiss the vocabularies of other Western theorists on the grounds that the terms they used never exactly translated Indian cultural realities, he required, on the other hand, considerable suspension of disbelief when it came to his own theoretical language.[6]

Another difficulty seemed to be linked to the intrinsic problems involved in characterising any monism in any analytic language. Either you got it or you did not. As other critics noted, it was difficult to know where to go after monism had been accepted as the master principle. Why go anywhere, in fact, if ultimately 'all was one'? Perhaps similarly bothered himself, Marriott began to suggest in the early 1980s that he did not want to be taken too seriously on monism, that monism had only been a compromise in the interests of communication, a way of highlighting a difference in emphasis in Indian thought, a means of stressing its generally 'non-disjunctive' properties compared to Western thought—and from this time on, he referred his vision of the most authentic Indic thought increasingly to the putatively dualistic Indian philosophical school of Sāṃkhya.

So much earlier ethnosociology was logically predicted on monism, however, that this more recent reticence seems disingenuous. Marriott has also continued to make many of his older monistic arguments without identifying them as such.[7] And Sāṃkhya, his new touchstone, may be more monistic than it seems. For, according to Gerald Larson's recent expert commentary, its dualism is 'eccentric' by Western standards. Sāṃkhya is fundamentally monistic except for its high-level distinction between

[6] In his recent paper, for instance, Marriott says that ethnosociology is or will be a generalising 'science' based on 'Indian cultural realities', one which develops 'rigorous' methods of 'description, analysis, and explanation' and 'deductive strategies that can generate hypotheses for empirical tests'. But though he also suggests he is discarding the baggage of most previous Western anthropological approaches to South Asia for being 'imperialistic', ethnocentric or 'parochial' to Western thought, Marriott indicates no embarrassment about (or Indic roots for) his own obvious strong Western positivism—for the concepts of 'rigour', 'description', 'analysis', 'explanation', 'deduction', 'empirical testing', or '[social] science' itself (see Marriott 1989: 1–6).

[7] For example, 'since antiequivalence relations are understood by Hindus to inhere also in [illegible], they may appropriately be called "substances" as well as "processes" ' (Marriott 1989: 18; see also 2–3, 7–8).

'consciousness' and 'materiality', Larson suggests; it is so monistic at other levels that vedānta, which developed later, was able to incorporate most of its ontology (see Larson 1987: 74–83).

When he was still defending monism, Marriott often made rhetorical appeal to what he characterised as the prevalence of monism in contemporary Western natural science. The monism of Western science, however—at least in anything but its retired-scientist-as-amateur-philosopher versions—is in fact 'physicalism', historically produced by stripping away the mental properties of all phenomena, including mind itself, and regarding them as epiphenomenal. Western scientific monism, in other words, is Cartesian dualism minus one of its halves, not Indian monism, in which the mental and the physical are somehow fused or co-present. Western scientific monism is a highly reductionist monism.

The most sophisticated culture theory in anthropology, on the other hand, suggests that a basic feature of culture is its monism in something closer to the Indic sense of the term, in an unreduced sense. Culture properly understood combines conventionally thing-like properties (it is 'out there' between people; it constitutes even the most apparently material, infrastructural levels of reality) and other, perhaps uniquely mentalist, even literary, properties. Yet, in a kind of double irony, the models beneath Marriott's Indian culture often appeared to be very physical: pure mathematical logical relationships; interactions; transactions; flow; plumbing (Moreno and Marriott 1981); heat-exchange (Moreno and Marriott 1989). The effect is often more reductionist than the claim that ethnosociology is about culture seems to promise.*

The mathematics in Marriott's ethnosociology as of the mid-1980s was disconcerting for similar reasons. Most contemporary culture theorists work with a linguistically-derived tool-kit of symbol, sign, metaphor, metonym, etc., whose meanings have been fairly well established by long-standing dialogues in rhetoric, literary theory, linguistics and anthropology. But Marriott's 'cultural' models often made half-bridged jumps to a highly abstract language of mathematics or symbolic logic whose meaning and implications seemed opaque at best.

Thus, though one of Marriott's basic ethnosociological impulses seemed to be radically relativistic, in his continuing reliance on mathematical

* The coda to Marriott's recent paper can be read as a reply to these questions about mathematics and physical models in his theory (Marriott 1989: 33–34), as can the following non sequitur: 'Since vedic Hindus see their society as based directly upon understandings of nature (Dumont 1961: 36–37), the metaconcepts and terms applied here are largely drawn from the natural sciences' (Marriott 1989: 6). (In his classic paper on caste and race, Dumont argues that traditional South Asians consider hierarchy 'natural', or that they do not, more precisely, make the Western category distinction between the natural and the social. But why does this imply that Indic 'nature-cum-society' corresponds to the 'nature' of Western science or that models drawn from the very oppositionally-defined latter are therefore peculiarly appropriate to the very monistic former?)

models of some kind he also seemed to be moving in the opposite direction, toward the search for a universal, cultureless form of truth to which all these Indic particularities could finally be attached. In other words, Marriott had not changed as much in his most fundamental theoretical tendencies as he appeared to have done in other ways in the early 1970s. Just as Indian ideas about pollution and caste could ultimately be reduced to a mathematical analysis of dyadic transactional behaviours during Marriott's pre-ethnosociological days, for instance, so too Indian mentalities were now to be ultimately rooted in reflexivity, symmetry and transitivity.

Similarly, read against some recent proposals that ethnographic knowledge become more dialogic, that it share out more authority with the peoples-in-other-cultures whose point of view cross-cultural anthropology is supposed to represent or deliver (see Clifford 1983 and Geertz 1973 among many others), Marriott's ethnosociology as of the mid-1980s was very univocal. There was really only one voice in it, Marriott's own, saying what it was that South Asians thought or did not think. For someone who had once been a careful ethnographic empiricist, moreover, Marriott seemed to have grown more cavalier about whether the natives themselves explicitly recognised or articulated the Indic conceptions they were said to hold by Marriott.*

Though ethnosociology might have opened up new vistas on South Asian culture, it also seemed that it might blind ethnographers to matters of value in other theoretical frameworks. Does the fact that there is perhaps a great deal of processual imagery in Indian thought really mean that there is no important interest in thing-like 'structures', for instance? Why are physical, spatial boundaries so important in south Indian villages if flow is so valued? As for caste boundaries, would a naive Western reader, presented only with Marriott's fluid images of India in the mid-1980s, have been in a position to predict that most Indians do not in fact 'flow' out of the castes they are born into, nor do their children or their grandchildren?

* See, for instance, the way in which ethnosociological loyalist Melinda Moore disregards the significance of the absence of any real local meanings for what she analyses (discussed later), or consider such ethnographically unsupported pure assertions as 'Tamil equivalents of [the] variables [mapped by Marriott's cube] are known and currently used [in the region being analysed] . . . . In practice, attention may be directed to only one variable at a time or to one concept, leaving its metonymy with other concepts within that set implicit. Yet the classical triads and their equivalents remain universally available . . .' (Moreno and Marriott 1989: 150).

Also, related to this point, Marriott has relatively little to say about why it is that South Asians have never been able to come up with the triadic model contained in 'the cube' if it is really so fundamental to their thought. True, the most fundamental principles in many workaday cultures often remain tacit or unconscious to the natives (cf. Marriott 1989: 2, 7). But Marriott's India is not just any workaday culture. It is a sophisticated literate tradition with great philosophical and even 'scientific' power and capacity for abstract thought. So imagined, it seems particularly odd that South Asian thinkers should never have noticed the cube or something closely equivalent to it on their own.

Marriott also seemed to be attempting to read purity and pollution entirely out of Indic reality in his ongoing critique of these Dumontian master-principles. By the mid-1980s, his counter-Dumontian argument had become even more elaborate. If one accepted the principles behind the constituent cube, then purity and impurity—even if they might be important native categories and distinctions—were not conceptually primitive. Purity, he now argued, was most parsimoniously analysed as 'unmarked', 'unmixed' and 'matched'; pollution was the opposite (Marriott 1983).[10]

For mixing, marking and matching to be conceptually primitive, however, Marriott had to show that they *were* independent dimensions. But it was not at all clear that they were. Marriott had shown in his transactional analyses that what he now called marking and mixing could be somewhat independent of one another, that castes in the middle of hierarchies had some latitude about making many exchanges (maximising) or few exchanges (minimising) without altering their overall caste ranks. In Marriott's subsequent cube-building, a caste's asymmetric decision had to do with purity and impurity (or with unmarking or marking), but its symmetric decision about unmixing or mixing did not.

But why not? Middle-ranking castes can be either very mixey or not so mixey because—in terms of a simple image of the flow of negatively-valued substances (impurities, inauspicious things, etc.)—the sum of these castes' superiorising prestations and of their inferiorising acceptances comes out the same in either case. But 'marking' is still culturally fundamental in the calculation of this result; the result is worked out against the same set of background assumptions used to establish or affirm caste inequalities through asymmetric transactions. Mixey castes get and give lots of markings; unmixey castes do not.

When it came to 'unmatching' or 'disarticulation', a problem of definition was added to the problem of independence. This dimension is a particularly good example of the indeterminacy of many of Marriott's ethnosociological terms. Sometimes disarticulation seems to mean 'internally disarticulated' or heterogeneous; sometimes it seems to mean 'disarticulated with something else' or inconsistent; and sometimes it seems to mean 'typically in a disarticulated setting'.

And against what standard is 'disarticulation' judged? Is not the 'articulated' also likely to be the pure? When does 'purity/impurity' not scale with 'articulated'/'disarticulated'? Orally, Marriott would only answer in the language of the cube, giving 'violence' or *tamas* as an instance of the disarticulated but also of the *un*marked (rather than the marked). Whether or not violence was disarticulated, however, why was it unmarked? Because it was 'inclusive', Marriott answered; it could overwhelm anything else.

[10] Marriott is apparently making an even more deconstructive argument in 1989—that there are many different kinds of impurity, each mapped by different combinations of the cubic variables (see Marriott 1989: 30).

Well, may be. But would anyone but Marriott have seen violence as logically inclusive? Could Marriott have trained a disinterested evaluator to come up with the same judgments of unmarked/marked or articulated/disarticulated as he himself made?

Even more basically, Marriott never really justified the cultural correlations behind the cubic variables; he really only asserted them. Nor did he justify the details of his further mathematical mappings. Why were 'reflexivity', 'symmetry' and 'transitivity' the best or most adequate formal terms for what he seemed to be trying to model? Can something not be disarticulated or 'messy', for instance, without being intransitive in a strict logical sense?

Even if the mathematical terms *were* appropriate, why these particular links between them and the cubic variables? Why not link 'mixing' with 'symmetry' rather than with 'nonreflexivity', since mixing is by definition a symmetric operation? Why not, conversely, associate 'marking' or ranking with 'nonreflexivity', since any given element cannot take this particular relationship to itself (something cannot mark itself or rank above itself)?

One last problem with the cube as of the mid-1980s indicated as much as anything the degree to which Marriott's theory was set up *contra* Dumont—the cube and the theory continued to dehierarchise traditional Indian culture to what can only be called an unrecognisable degree. Unmarking and disarticulation, whether or not they scale together, both seem relevant to rank or to other cultural evaluations in South Asia. Likewise for mixing, as argued above. Furthermore, even if to be 'exchangey' is to be powerful and full of energy in traditional Indian culture, as Marriott argued, is not action, *śakti*, the female principle, ultimately subordinated to essence or being, *iśvaran*, the male principle? So my second-hand knowledge of higher Hinduism suggests (see also Wadley 1977).

At a higher level of the model, Marriott's 'Hindu social science' also suggested no hierarchical relation whatever between his three fundamental presuppositions. Yet modelled as they were in part on the *gunas*, a highly ordered set, why did the analysis not incorporate the ordering 'unmarked' over 'mixed' over 'unmatched' (the analytic terms paired with the ordered set *sattva, rajas* and *tamas* in the *guṇas*)? The same principles *were* listed in their appropriate Hindu order in Marriott's first version of the scheme (Marriott 1979).

Marriott replied that the three principles are in fact lined up with many other Indic terms besides the *guṇas* in his master scheme, with the elements (*mahābhūtas*), the aims (*puruṣārthas*), the humours (*doṣas*), and with time (*kāla*) among others. Since there was no invariant order to all these presumably correlated terms, no one South Asian set of values took precedence. Marriott then fell back for his order on the abstract mathematical property of the respective 'numerosity' of reflexivity, symmetry and transitivity, paired respectively with unmixing, unmarking and matching. Reflexivity is about one element being or not being in a relation with itself; symmetry requires two elements; transitivity requires three.

But by now, in a theory that was supposed to be about 'how Indians think', we seem to be a particularly long way from any Indic principles or values. And what is it we know when we know all these things may be lined up, depending on how the key presuppositions are interpreted, and may in turn correlate with something in Western mathematical theory?

Published at last!

In his recent 'Constructing an Indian ethnosociology' (Marriott 1989), Marriott makes only a few substantive changes in what has been outlined in the preceding pages, in the cube and other aspects of his general theory,[11] explaining and justifying ethnosociology with a wealth of detail he has never written down before, and attempting to defend it against possible objections, many of which have been raised above. Some aspects of his densely written new article might be valuable or illuminating without all of it being valid, of course. But the crux of his argument—the subject to which Marriott devotes the most pages, the contribution on which he evidently wants to be taken most seriously—remains the cube and the processes of mixing, unmarking and unmatching.

Marriott can be disarmingly frank about specifying the drawbacks of the cube as he describes it, though once he has stated them he tends to proceed as if he had laid them to rest. Hindu conventions would not draw such a model rectilinearily, Marriott admits. The right angles 'should be altered by evidence that the variables are not wholly independent'. The sides 'should be altered by evidence that the variable scales are not commensurate' (1989: 9). The orientation of things matched up by the cube does not always correspond to native intuitions of common meaning (ibid.: 16).

Marriott's claim that three dimensions are necessary because 'three appears to be the irreducible number of properties or components with which Hindus will comfortably think about human affairs' (1989: 8) is contradicted by a great deal of ethnographic and textual evidence, some of it cited in his paper as well as elsewhere in the same collection.[12] Conversely,

[11] Consistent with his increasing reliance on Sāṁkhya, for instance, Marriott invents two new variables, ' "grossening and subtilising" ', 'following the ancient ideas of *sāṁkhya* . . . of a devolving, increasingly marked . . . series of elements running from a relatively "subtle" (*sūkṣma*) or ethereal and inclusive source to a sink of "gross" (*sthūla*) materiality'; and 'one nonelement and antiaim . . . "consciousness" (*puruṣa*)' (1989: 21, 22). However, these new dimensions do not play the central analytic role played by the older cubic variables in the rest of the paper.

[12] Larson also suggests that while triadic categorisations are common in Sāṁkhya, so too are pentadic *and* binary ones, to about equal degrees (Larson 1987: 86–89).

if Hindu thought really is as multidimensional and multiperspectival as Marriott believes, three dimensions are actually a rather impoverished number of parameters for modelling it, not much of an improvement over two.

Marriott devotes many pages to what he refers to as the 'textual and ethnographic evidences' that the 'felt resemblances' between the different Indic categories associated together under any one of his cubic variables really exist in South Asian thought. He variously characterises these resemblances as 'compatibilities', 'homologies', 'mutualities', 'partial identities', 'partial congruences', 'metonymies' and 'family resemblances', however (ibid.: 7, 16). What could be the conditions for disproving correlations which are so loosely characterised?

The proof, in any case, should be in the pudding, and since Marriott is an anthropologist rather than a textual Indologist, the pudding should be the ethnographic validity or usefulness of his model. What more do we know about the anthropological grounding of ethnosociology, and of the cube in particular, on the basis of the recent published collection?

Though Marriott implies that all the remaining papers in the collection (*Contributions 1989*) are consistent with or support his ethnosociological paradigm, the five anthropologists, two historians and one linguist can in fact be divided, as T.N. Madan also notes in the Editorial, into two groups (ibid.: iv). Wadley and Derr (1989), Raheja (1989), Dirks (1989) and Ramanujan (1989) may take occasional ideas from Marriott's ethnosociology, and may in turn influence him in ways, but none of them uses all of his theory, least of all the cube. Marriott (1989), Moreno and Marriott (1989), Moore (1989) and Mines (1989), on the other hand, write papers which form a remarkably seamless whole.

There are no significant expressed differences of scholarly opinion among these four anthropologists. Their arguments are identical and mutually reinforcing at their respective levels. Even the writing styles, vocabularies, rhetorical methods and apparent habits of thought are surprisingly uniform across the four papers, which are generally more reminiscent of earlier articles by Marriott than of earlier drafts of single-authored articles by the students involved.[13] It is difficult not to read all four of these papers as the products of one single mastermind.

It would require another paper almost as long as this one to evaluate in detail the ethnographic evidence provided by those who accept the whole paradigm, as well as to look at one particular fellow traveller, Raheja, whose analysis Marriott cites as particular proof of the validity of mixing,

[13] Like Marriott 1966 and Marriott 1968 but unlike Moore 1984, for instance, Moore 1989 is written as a crisp little epic of discovery, in which the first-person-singular anthropologist apparently cracks a tough ethnographic puzzle by moving through vividly described stages of progressively more comprehensible data and theory.

marking and unmatching.[14] Instead, let me bring this critique to a close by commenting on the one ethnographically rooted article which announces its intention from the beginning as furnishing an almost concrete example of the cube—Melinda Moore's treatment of the cultural construction of a traditional Kerala house.

Traditional high caste houses, Moore noticed or was told in two years of field research in Kerala, are ideally four-sided, and centre on open atriums used for festivals, 'temporary worship' and the swearing of solemn personal contracts. Houses should face east, and they are best built on eastern slopes, the people agree, so the gods and humans in them can look out more easily to the east. Puja rooms for deities and important ancestors are located in western interior rooms so their sacred beings can look east through the houses, and so they can be approached from the east by their family worshippers.

Kitchens are in northeast corners, with family dining rooms just to the west. Outside to the northeast are tanks for daily bathing and 'trees of sacred basil'; to the north, the women typically grind grain and do other food preparations. Large reception rooms are in southeast corners, where formal meals and snacks are given to guests. Guests not invited into houses are entertained on verandas normally attached to eastern exteriors (for men) and northern exteriors (for women), with rules about how far they should remain from inner parts of houses according to the lowness of their castes.

Menstruating women stay in seclusion rooms in northwest corners, which also serve for childbirth. Outside to the northwest are family latrines and compounds for pounding and parboiling rice ('thought to kill small creatures') and for butchering animals. Newly married couples are given their own private sleeping rooms in southwest corners, upstairs. Outside to the southwest are cattlesheds. Older married women and men tend to sleep with members of their own sex, women in the inner family dining rooms to the north, men in the more open reception rooms to the south.

One diagonal is strongly marked, the northeast to southwest line, and it is sometimes left open by small holes through walls 'to facilitate flow',

[14] Briefly, however, Raheja gives us an admirably precise ethnographic account of the cultural dimension of auspiciousness and inauspiciousness in one Indian village—but not of a dimension, in my opinion, which is necessarily as independent of caste rank and purity/impurity as Raheja suggests. (Brahmins on the one hand and Barbers, Washermen and Untouchables on the other might accept inauspicious substances for different but still hierarchically linked reasons, and Brahmin sub-castes who accept such things are usually reported to rank below Brahmins who do not.)

Whatever she might think of my interpretation, however, Raheja clearly does not believe her material has demonstrated the conceptual independence of marking/unmarking (not to be confused with the epiphenomenal purity/impurity) and matching [= auspiciousness]/unmatching [= inauspiciousness]. For despite ample opportunities to adopt it, the cube appears nowhere in her article or in her book (Raheja 1988, 1989).

some people say. Cremation grounds are to the south, which is also the worst direction for houses to face, and the orientation everyone avoids when paying respect to daily household 'worship lamps' (Moore 1989: *passim*).

What do Moore's informants say about the reasons for these spatial orientations and dispositions? Not much. 'I could elicit from them only fragmentary explanations', Moore admits, '[such as] "the interior spaces are safer" ' (1989: 170).

Some anthropologists might have left it at this. Many routinised things that people do in many cultures have no discursive logic; the people just say they should do them, and do them, and anthropologists either report that fact or turn their attention to more rationalised aspects of particular cultures.

Other anthropologists might suspect that, with probing or careful listening to what people say while building houses or while orienting themselves to these spatial patterns, more general Indic cultural explanations and models might emerge. A perhaps insurmountable problem for Moore, however, is that these houses have not been built in 50 years, so local people might have forgotten just what interests the anthropologist most. If local meanings were more accessible, however, they would probably resemble some simple, but actually rather multidimensional, cultural notions recorded elsewhere in village India.

Such as: east and north are auspicious directions; south is very inauspicious; west has mixed values; different diagonals have no meanings or different meanings according to context. Pure or purifying things tend to go in auspicious places. Hence gods originate in the east and north, and typically look in those directions. 'Interior spaces are safer'—hence women and gods, whose purity needs protecting, stay inside households; men are intermediate; and impure, inauspicious, low or dangerous things are kept out (the latrine, strangers, the killing ground, the cremation ground).

Menstruating women, on the other hand, must be secluded somewhere. Northeast would be inappropriate; south would be dangerous; northwest is not a bad solution, especially since it is interior compared to the reception hall in the southeast; southwest might have worked almost as well. Married sex might also have been located elsewhere, as it is in Daniel's Tamil village (admitted by Moore, 1989: 177). And some dispositions might be matters of simple convenience. If kitchens are northeast, food preparation areas ought to be near them. Etcetera.

But Moore is interested in grander explanations, and she writes of her interpretative journey in passages whose convolutions leave ultimate cultural authority almost elegantly opaque:

Seeing my interest . . . [local people] referred me to Malayalam-language building manuals . . . . My readings of such manuals led me

> from the observed architectural and behavioural evidence to several layers of ritual, astral myth, and geometric formulations that in combination produce a structure much like the 'constituent cube' which Marriott constructs from the Hindu elements, humours, strands and aims . . . . [a model which] presents [household] residents with understandings about the possibilities of action in [the Indic universe] which may not have been available to them in other ways (1989: 170, 200).

Where does such a model reside? What South Asian person or persons are its knowers, either of the model or of the evidence from which it is constructed?

The logic of the analysis which follows is often similarly opaque. Malayalam building manuals show the houses' layouts in the form of 'Foundation Man', (*vāstu puruṣa*), a fallen antigod or *asura*, lying on his back, head northeast, 'pressed into a square oriented to the cardinal directions', with forty-five gods drawn in a grid on top of him and eight gods and demons around him. The people do not know much about these divine beings, Moore tells us, and correlations between their characteristics and where they are located are not impressive. Moore nevertheless believes that the mythic fact that Foundation Man fell from heaven and was subsequently overlaid by gods (as well as the fact that the central atrium of the house is open to the sky) implies that the Kerala house incorporates a notion of the vertical layering of the Indic cosmos (1989: 178, 179).

Moore further speculates that 'the Man's mouth in the northeastern corner could be said to demand food, and that is where the kitchen is actually located',[15] and 'the Man's genitals are a prominent feature of the southwest corner where the most favoured bedroom [is]' (1989: 181). But his left and right sides are inappropriately placed, she decides. For if the Man is lying face up, on his back, his left or impure side covers what Moore here regards as the relatively pure southeast, 'where guests are greeted'. His pure right side, on the other hand, corresponds to the impure northwest, 'where women are secluded during birth and menstruation' (ibid.: 182). So much for the possible cultural relevance of Foundation Man, then?

Not at all. For Moore now discovers that what have served as her indigenous cultural authorities so far, the Malayalam-language building manuals to which her people directed her, are actually all of them wrong about the way the Man is lying. The mythic research of Sanskritist Stella Kramrisch, on the other hand, 'leaves no doubt that [the Man] first fell . . . face-down from heaven' (1989: 182), which would make his left and right sides match up better, Moore decides. If this new orientation is correct, however, then earlier correlations no longer apply: the back of the Man's head now matches up with the kitchen, and his anus and buttocks with the room for married sex. Moore does not notice.

Moore proceeds to the 'up-down' dimension, declaring she was surprised the building manuals contained very little of it. But this does not faze her. Working from what the manuals say about the best relative orientations of temples and households, and from the local preference for eastern slopes, she decides that the apparently horizontal actually has a vertical aspect as well, positing a 'universal and underlying cline . . . running from southwest to northeast' (1989: 187).

Now that she has three dimensions, Moore segues to the cube and to how it maps the still mostly unexplained orientations of the Kerala house. Mixing and unmixing 'clearly' correspond to the east-west dimension, Moore declares, because 'the house opens its doors and expands largely to the east'. Unmarking and marking correspond to the vertical dimension 'as understood in Kerala'—i.e., to the literally vertical (the layering of Foundation Man and other gods), and to the vertical as she has just projected it along the horizontal southwest-northeast diagonal. And matching and unmatching correspond to the south-north dimension: the north is 'messy' with cooking, eating, menstruation, childbirth and other functions associated with women, while the south is characterised by 'the forces of unity': matchmaking, marriages, worship, formal meetings, etc. (1989: 192–94).

These mappings are at least as arbitrary as Marriott's applications of his older ethnosociological abstractions, however. On the mixing dimension, for instance (east-west), the reception room, where outsiders are most likely to be in the house, is as much in the south as it is in the east. Women also apparently mix with other women on the northern veranda. Moreover, why is sexual intercourse, which is found in the southwest or 'unmixing' part of the household, not an archetypal example of mixing?[16]

Regarding matching (south-north), the unifying actions said to correspond to the south are, on Moore's ethnographic evidence, also typical of the central atrium. Furthermore, why are the acts of eating together in the northern family dining room and worshipping together outside the western puja room not seen as unifying for the smaller family groups that carry out these activities in common? Conversely, why are the disarticulations of death to the south not the 'messiest' or most unmatching of all?

As for unmarking (up-down, southwest-northeast), the household gods are on the western side of the house according to the local people themselves—for this is one point on which they seem to have their own opinion—so that the gods can look to the east, the direction most appropriate

[15] A simpler possibility is that the head, the highest part of the body, is appropr[illegible] located in the northeast, the most auspicious direction.

[16] [illegible] mixing, Moore admits, but it fits 'dialectically' into the southwest corner (1989: 198); on 'dialectics' versus 'correspondences', see later.

to them. The fact that the gods are located in western rooms is thus epiphenomenal on the evidence of local exegesis; it does not suggest that the west is less 'marked' or more pure in local understandings than the east. What sense does it make, similarly, to identify the northeast, by virtually all other accounts of Indic culture a locus of auspiciousness and purity, as the most marked or impure corner of the household?

Moore deals with this last difficulty through a *deus ex machina* distinction she introduces toward the end of her article to explain how things fit into the cube. Some elements or activities 'correspond' to their locations, she suddenly proposes; they are 'congruent' or 'harmonious' with the cubic variables. Others, however, are in appropriately 'dialectic' relations with the cube, 'seen as opposing the constituents' (1989: 191–92).

Shades of 'dialectics' and 'unresolved paradoxes' worse than those which Marriott complained about in Dumont back in 1969! If Moore can justify any given item's location on the grounds that it either matches up with or dialectically opposes its cubic constituents, then by definition she can fit almost anything almost anywhere in the cube. And so she proceeds to do for the northeast corner, which receives what she refers to as a 'massive dialectic movement' from southwest to northeast, 'the triumph of purity', in which the 'most impure corner of the house' (the northeast) receives an 'infusion . . . of goodness from above [from the southwest]' (1989: 198).

Moore concludes, among other things, that her cubic analysis has 'greater precision' than Dumont's previous 'one-dimensional' theory based on purity and pollution (1989: 196). But a conclusion based on so dubious and culturally unsupported a line of reasoning is not worth investing any further intellectual effort in.

And if this is the best that an extremely close associate of Marriott's can do with the cube, then—whatever one thinks of other, more imagistic aspects of Marriott's ethnosociology, of monism, substance, flow, the person, etc.—the same has to be reluctantly concluded, on present ethnographic and theoretical evidence at any rate, in this critic's opinion at any rate, of much of the predominantly data-distant, abstract mathematical modelling of Marriott's recent ethnosociological years.

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*India through Hindu categories**: A Sāṁkhya response

Gerald James Larson

Let me begin my response by quoting two passages from McKim Marriott's lead essay entitled 'Constructing an Indian ethnosociology', one passage having to do with the methodology of his entire project and the other having to do with the basic sources utilised in building his theoretical framework. First, then, the passage on method:

> Constructing a theoretical social science for a culture requires somewhat more than providing a meaningful cultural account: it requires building from the culture's natural categories a general system of concepts that can be formally defined in relation to each other; it requires developing words and measures that can be used rigorously for description, analysis and explanation within that culture; and it especially requires developing deductive strategies that can generate hypotheses for empirical tests in order that the science may criticise itself and grow. It requires doing all this in terms that will be analytically powerful enough to define all the major parameters of living in that culture without violating the culture's ontology, its presuppositions, or its epistemology (Marriott 1989: 4).

Second, towards the conclusion of his essay he refers to the overall model that he has developed as follows:

> The model outlined below is undoubtedly biassed in the direction of its sources, which are mostly Hindu, more north Indian than southern, more learned than popular, more of *sāṁkhya-yoga* than of any other *darśana*, more *āyurvedic* than astrological, more orthodox than devotional, more high caste than low, and more male than female (Marriott 1989: 32).

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I highlight these two passages because they clearly distinguish the two levels on which Marriott is working. On one level, Marriott's effort is an ambitious attempt to construct an explanatory and testable ethno-'science', utilising categories from South Asian thought, an ethno-'science' that can stand as an alternative to another ethno-'science', usually referred to as Western social science. On this level the thrust of Marriott's work is to use Indian categories (and/or Indian cognitive structures) in order to illuminate problems within the social sciences in general. On another level, Marriott's effort is an attempt to rework and reinterpret South Asian categories of thought (cognitive structures) in a more systematic, or as Marriott himself puts it, a more 'analytically powerful' manner than has heretofore been achieved. On this second level, the thrust of Marriott's work is to use the generalisations of the social sciences to illuminate South Asian social reality.

Any assessment of Marriott's work has to keep both levels continuously in mind, for it is impossible to understand what Marriott is doing without an appreciation for this internal dialectic unfolding in his intellectual programme. Marriott's critique of the social sciences is unintelligible apart from his interpretation of the Indian cognitive framework. By the same token, Marriott's interpretation of the Indian cognitive framework is unintelligible apart from some fundamental logical and mathematical generalisations derived from Western theoretical traditions.

My own work is in Indian philosophy, and specifically with traditions of Sāṁkhya and Yoga—indeed, Marriott uses much of my work in his current theorising—and so I shall for the most part focus my comments on the Sāṁkhya (or, perhaps better, Indological) side of Marriott's analysis, but as I have just indicated, it is not possible to isolate the Sāṁkhya (or Indological) level from the other, general social scientific level, if one is to do justice to Marriott's overall undertaking. Thus, in what follows, I shall, first, comment on the general programmatic structure of Marriott's work, and then turn to the more specific Indological issues of Sāṁkhya and Yoga. Let me stress at this point that although I am critical of some of Marriott's formulations, I very much appreciate what he is attempting to do and consider it to be important. It is, in my view, a refreshing attempt to bring together Indology and social science, 'text' and 'context', and Western 'equivalence relations' with Indic 'antiequivalence relations', in a manner that is sophisticated, intellectually elegant and provocative.

The ethno-'science' construct

Oddly enough, to understand Marriott's basic theoretical or 'scientific' framework, the place to begin is not with the Western social sciences but, rather, with traditions of mathematics, Boolean algebra and set theory. The foundation of Marriott's theoretical framework is the general concept

of 'equivalence relation', available in any standard dictionary of mathematics. One can show at a glance what an 'equivalence relation' is by simply quoting one of these dictionary renderings. I shall use *Dictionary of Mathematics* by T.A. and W. Millington, as follows:

> Equivalence Relation. A *relation* between elements of a set which satisfies three conditions:
> (a) reflexive, aRa;
> (b) symmetric, aRb, then bRa;
> (c) transitive, aRb, bRc, then aRc,
> where a, b, c are members of the set and R means *in the given relation to* (Millington and Millington 1966: 84–85).

In terms of set theory, these three conditions can be related to 'intersection' (= reflexivity), 'inclusion' (=symmetry) and 'union' (=transitivity), wherein the terms 'intersection' and 'union' can be defined as follows: 'intersection (product) of A and B—the largest set containing elements in *both A and B*'; and 'union (sum) of A and B—the smallest set containing all elements in *either A or B*' (Ibid.: 213). Moreover, these three conditions are also isomorphic with the (Boolean) algebra of logic which treats mathematically the validity of propositions involving phrase connectives such as 'and', (conjunction), 'or' (disjunction), 'not', 'if, then', and 'if and only if, then', and so forth. Such 'equivalence relations' are at the core of Western thought and social reality. Says Marriott:

> Equivalence relations have tended to be assumed in recent Western popular thought and social science as essential to the organisation of human personality and society. Thus, persons and many other entities are postulated as being normally self-reflexive ('individuals', having identity with and being sufficient to themselves), and as symmetrical (equal) and transitive (consistent) in their relations with each other. 'Individuals' are indivisible, integrated, self-developing units, not normally subject to disjunction or reconstitution. Given such units, interpersonal influences, inequalities, and changes have to be brought in as external factors or pathologies. Other Western examples of equivalence thinking are a Euclidean plane and solid geometry, an Aristotelian syllogistic logic, and notions of legislation as fixed and uniformly applicable to all (Marriott 1989: 17).

What emerges, then, is something like the following set of correlations:

(I) Reflexive (identical)-intersective – *Individuality*
(II) Symmetric (interactive)-inclusive – *Equality*
(III) Transitive (distributive)-unitive – *Consistency*

These represent (as a set) '. . . the equivalence-based ideas of discrete, static and uniform entities' characteristic of 'conventional Western social science' (ibid: 34).

Hindu cognitive structures, in contrast, together with their correlative social implications, represent an interesting 'revision' or reordering of 'equivalence relations', according to Marriott, not in the sense of denying the very possibility of equivalence relations but, rather, in the sense of suggesting that equivalence relations are the exception rather than the rule in ordinary thought and social reality. Marriott's point here with respect to the social sciences is not unlike Prigogine's point with respect to the hard sciences when Prigogine argues that 'equilibrium' is a marginal notion in the most advanced theoretical science. The normal situation is one of 'open systems far from equilibrium', with 'equilibrium' only at the margins (Marriott 1989: 33). Marriott introduces the expression 'antiequivalence relations' as a way of characterising what is unique about the Hindu perspective, and he then defines 'antiequivalence relations' as follows:

> Antiequivalence relations are necessarily variable, since while they deny perfect reflexivity, symmetry and transitivity, they do not postulate the dichotomous opposites of these—absolute irreflexivity, asymmetry and intransitivity. Instead, they assert that various imperfect and inconstant intermediate states are to be expected, and thus that processes and intermediate states, rather than any fixed or polarised structures, are basic. Yet, since antiequivalence relations are understood by Hindus to inhere also in matter, they may appropriately be called 'substances' as well as 'processes' (ibid.: 18).

In other words, antiequivalence relations are neither the antitheses nor the contraries of equivalence relations. If they were, of course, they could not be 'anti'-equivalence relations. They could only be negations within the set of equivalence relations. To the contrary, antiequivalence relations and/or antiequivalence rules must represent a different dimension of relations or rules. Here it is useful to bring in a parallel with rules in linguistics, the difference between 'context-free' and 'context-sensitive' rules (Marriott 1989: 33; Ramanujan 1989: 47). Marriott's dichotomy of 'equivalence relations' versus 'antiequivalence relations' is roughly comparable to 'context-free' rules versus 'context-specific' rules in the study of languages. In any case, what emerges with respect to Hindu cognitive structures to the extent that they are 'antiequivalence relations' is something like the following set of correlations:

(I) Non-reflexive (not necessarily identical) –
composite – realm of the personal –
continuum of mixing-unmixing – *Dividuality*

(II) Non-symmetrical (not necessarily equal) –
irregular – realm of the interpersonal –
continuum of unmarking-marking – *Hierarchy*

(III) Non-transitive (not necessarily consistent) –
mixing (chaotic) – realm of society/cosmos –
continuum of unmatching-matching – *Disorder*

This, then, is the generalised framework in which Marriott proposes to pursue his ethnosociology of South Asia, although there is one additional formal component by way of visually exhibiting the manner in which antiequivalence relations interact with one another, namely, mathematical graphing of three independent variables in the form of interpretive 'cubes'. The width of the cube represents the unmixing-mixing continuum of Dividuality, with movement from left to right indicating more and more mixing. The height of the cube represents the unmarking-marking continuum of Hierarchy, with movement from top (higher, unmarked) to bottom (lower, marked) indicating decreasing interpersonal status. Finally, the depth of the cube represents the unmatching-matching continuum of Disorder, with movement from front (ordered, *dharma*) to back (disordered, *adharma*) as increasing social and/or cosmic decay as, for example, in the *yuga* theory. The left-top-front of the cube exhibits, therefore, the rare but nevertheless possible point of reflexive, symmetrical and transitive 'equivalence' that is essentially 'context-free'. The remainder of the positions within the cube represent departures therefrom, with the fullest manifestation of non-reflexive, non-symmetrical and non-transitive (or dividual, hierarchical and disordered) antiequivalence relations at the right-bottom-back of the cubic visualisation as the symbolic point of utter 'context-specificity'. Or, putting the matter another way, the left-top-front of the cube is the point of 'purity', whereas the right-bottom-back of the cube is the point of utter 'impurity' (Marriott 1989: 25).

Before moving on to the Sāṁkhya or Indological dimension of Marriott's work, let me offer just one query to this attempt by Marriott to formulate a non-Western theoretical framework of antiequivalence relations and/or rules. My query is simple: Is Marriott's theoretical framework really non-Western in any meaningful sense? Or is it simply a reworking of an essentially Western framework? One could take, for example, Marriott's three conditionals for equivalence, namely, reflexivity, symmetry and transitivity, and discuss them in terms of traditional Western logic. That is to say, one could frame the discussion in terms of the principle of identity, the principle of contradiction and the principle of the excluded middle. Moreover, one could take the 'purity' point in Marriott's three-dimensional cube as the position of what Kant called the analytic *a priori* (the realm of pure thought or pure tautology, depending on one's philosophical tastes) and the 'impurity' point in the three-dimensional cube as the position of what Kant called the 'synthetic *a posteriori*' (or, in other words, the empirical realm of immediacy, the realm of sheer data). The former would be the 'context-free' realm of pure thought, or, in other words, the goal of empiricism in all of its varieties. In other words, the goal of rationalism in all of its varieties. The latter would be the 'context-specific' realm of the messy data of everyday life, or, the goal of empiricism in all of its varieties. In other words, what appears to be a move away from the 'equivalence relations' of traditional Western thought and social practice in the direction of a set of 'antiequivalence relations' characteristic of Indic cognitive structures, is really, finally, only a move from an analytic *a priori* perspective of pure thought to a synthetic *a posteriori* perspective of empiricism.

I mention this query at this point for an important reason, for if it is the case that Marriott's theoretical framework is a simple reworking of the Western framework, then much further work needs to be done in working out the possibility for an Indic ethnosociology. Why? Since Indian philosophy does not accept (and does not know) the distinction between *a priori* and *a posteriori* or analytic and synthetic judgments. In other words, if Marriott's theorising about Indic categories entails the distinction between *a priori* and *a posteriori* and/or analytic and synthetic judgments (as, alas, it does, I suspect, in its present formulation), then it must be concluded that his attempt to construct an authentic Indic ethnosociology has thus far failed. This in no sense means that Marriott's effort is wrong-headed or misconstrued. It only means that the theoretical side of his general framework has not yet been sufficiently developed. But let me turn now to the Indological side itself for further clarification.

The Sāṁkhya and/or Indological construct

I should make it clear at the outset that my use of Indological materials will be quite different from Marriott's. I shall confine myself to the parameters of classical Sāṁkhya philosophy (Larson 1979, 1987), whereas Marriott makes use of bits of systematic thinking from a great variety of Indic contexts, both traditional and modern and/or textual and contextual (Marriott 1989: 6). In other words, my focus is much narrower than Marriott's. Nevertheless, since Marriott has extensively utilised Sāṁkhya philosophy and has acknowledged its relevance for understanding indigenous Indic conceptual structures, it is reasonable enough, I think, to appraise Marriott's construction of the Indic conceptual framework from the perspective of classical Sāṁkhya philosophy. Moreover, I want to do this for two basic reasons. First, I think that the classical Sāṁkhya perspective provides an interesting indigenous critique of Marriott's construction, a critique that suggests that Marriott has misconstrued some of the relations within his ethnosociological framework. Second, I want to suggest that the classical Sāṁkhya perspective might itself serve as a vehicle for putting together a classical Indic axiomatic, not in the sense of an ethnosociology

but, rather, in the sense of what one might call an 'ethnophilosophy', that is to say, a set of axioms with which Indian philosophising proceeds.

First, then, the Sāṁkhya critique of Marriott's Indic ethnosociological framework. Marriott begins by calling attention to the various 'layers' of systematic reflection in South Asia in terms of *dhātus* or *doṣas* (medical theorising), *puruṣārthas* (lawbooks, epics), *varṇas* and *āśramas* (lawbooks, epics, *purāṇas*), *mahābhūtas* (Sāṁkhya philosophy), *guṇas* (sāṁkhya) sense capacities, 'sheaths', 'tastes', 'sentiments', 'feeling-states', and so forth (Marriott 1989: 6). He finally simplifies these various schemes into four core sets, namely, the five gross elements (*mahābhūtas*) of philosophical substance, the three substantive elements in medical theorising, namely, *vāta, pitta* and *kapha* (or, 'wind', 'bile', and 'phlegm' respectively), the three constituent 'strands' or processes of Sāṁkhya philosophy (namely, *sattva, rajas* and *tamas*), and the four ends of life (*puruṣārthas*), namely, *dharma, artha, kāma* and *mokṣa* ('coherence', 'advantage', 'attachment' and 'release' in Marriott's idiom). It is important, says Marriott, to understand the common properties of these various lists if one is to build an ethnosociology. As Marriott puts the matter:

> An explicit analysis of the common properties, if any, of these layers is thus an urgent task, preliminary to constructing a general theoretical system for the Indian social sciences (ibid.: 7).

Among these lists that he has isolated (namely, *mahābhūtas, dhātus* and/or *doṣas, guṇas* and *puruṣārthas*), he notes at least three common properties: first, a tendency to mix up, or perhaps better, not to separate the natural from the moral; second, a tendency to think in terms of processes and relations; and third, that the number 'three appears to be the irreducible number of properties or components with which Hindus will comfortably think about human affairs' (ibid. 1990: 8). He reduces the five gross elements to three by suggesting that the first and the fifth, namely, ether and earth, provide largely directional indications (ether, upwards; earth, downwards), leaving three primary elements: wind, fire and water. He reduces the four ends of life (*puruṣārthas*) to three (*dharma, artha* and *kāma*) by pointing out that *mokṣa* is a special notion and that the four ends of life are really to be construed as 3 + 1. Because the notion of 'process' or 'relation' is fundamental in the Hindu scheme, the three strands or *guṇas* become fundamental in Marriott's tripartite construction of a Hindu ethnosociology, and he then proceeds to merge the various triads or sets of three, utilising the three *guṇas* as the fundamental relational notion or category. Moreover, the three *guṇas* also become the point of contact with the larger tripartite network of antiequivalence relations, and the full Indic ethnosociological framework then begins to emerge as follows:

(I) *Non-reflexive* (not necessarily identical): realm of personal *tejas* – *pitta* – *rajas* – *kāma* – mixing/unmixing – *Dividuality*

(II) *Non-symmetrical* (not necessarily equal): realm of interpersonal *ap* – *kapha* – *sattva* – *artha* – unmarking/marking – *Hierarchy*
(III) *Non-transitive* (not necessarily consistent): social/cosmic *vāyu* – *vāta* – *tamas* – *adharma* – unmatching/matching – *Disorder*

By way of framing a classical Sāṁkhya critique of this formulation, two sorts of observations appear to be pertinent. One would be along the lines of suggesting that Marriott has moved too quickly to a tripartite perspective. While it is true enough that the number three or a tripartite analysis is a crucial interpretive device in classical Sāṁkhya as well as in other intellectual and popular traditions, there are other important interpretive schemas as well. Another observation would be along the lines of suggesting that Marriott has not quite grasped the manner in which the three *guṇas* or constituent processes relate to the other items which he wishes to correlate, namely, *mahābhūtas, dhātus* and/or *doṣas*, and *puruṣārthas*.

Regarding the matter of moving too quickly to a tripartite perspective, I have in mind the predilection in classical Sāṁkhya not simply for triads (*triguṇa*, and so forth) but for dyads and pentads as well. I have collected (Larson 1987: 86–88) a great variety of these dyads, triads and pentads and have suggested that classical Sāṁkhya makes use of all three modes of analysis. By dyadic analysis I mean not only the obvious *puruṣa-prakṛti* but such garden-variety dyads as *kāraṇa-kārya* (cause-effect), *vyakta-avyakta* (manifest-unmanifest), *bhoga-apavarga* (experience-release), and perhaps most telling, *sūkṣma-sthūla* (subtle-gross). Marriott himself makes extensive use of dyadic analysis in his own construct of the South Asian ethnosociology with his mixing-unmixing, unmatching-matching, and unmarking-marking and his dyadically interpreted cubes. When Marriott comments, then,

> Thinking about constituted things in dualities is often condemned. At least three terms are always present, always combined (Marriott 1989: 8).

and when he comments, further, in a footnote:

> It appears to be Western dualistic structuralism, rather than indigenous thinking, that leads to reconceiving Hindu triads as dichotomies mediated by a third term (ibid.: 8),

he is wrong on both counts. There is clear evidence in classical Sāṁkhya for dyadic analysis. Moreover, there is clear evidence for a tendency in the Sāṁkhya texts on occasion for reconceiving triadic analyses in terms of dyads mediated by a third term, the most important being, of course, the tendency to construe the triadic *guṇas* as two mediated by a third, most often *sattva* and *tamas* mediated by *rajas* (as in the structure of egoity). By pentadic analysis I mean not only the five gross elements, upon which Marriott focuses, but the five subtle elements (*tanmātras*), the five sense

capacities (*buddhīndriyas*), the five action capacities (*karmendriyas*), the five 'breaths' (*prāṇas*), the five 'sources of action' (*karma-yonis*), and so forth. These cannot be reduced or reworked into triads and represent probably a fundamental numerical analytic framework, at least in classical Sāṁkhya and probably in many other intellectual and popular environments as well. The point in all of this is to suggest that in trying to formulate an interpretation of indigenous conceptual structures, it is probably simplistic to focus solely on triads or the number three. Dyads and pentads are equally important, and when one begins to combine dyadic, triadic and pentadic structures of analysis (as, for example, in classical Sāṁkhya philosophising), a much more sophisticated indigenous conceptual framework begins to emerge.

In addition to moving too quickly to a tripartite perspective, it must also be said from the perspective of classical Sāṁkhya that Marriott's application of *triguṇa* or *sattva, rajas* and *tamas* is not correct. Marriott wishes to correlate *sattva* with *ap* and *kapha, rajas* with *tejas* and *pitta*, and *tamas* with *vāyu* and *vāta*. From the perspective of classical Sāṁkhya, however, *all* of the correlated items, namely, *ap, kapha, tejas, pitta, vāyu* and *vāta* are all *tāmasa*, that is to say, one or another form of the *guṇa tamas*. They are gross (*sthūla*) constituents, and even if they are conceived or conceptualised in terms of their subtle presuppositions, they would still be *tāmasa*, for the *guṇa, tamas*, has reference to any determinate form, whether gross or subtle (that is, whether *mahābhūta or tanmātra*). A simple chart of the Sāṁkhya conceptual scheme will show at a glance how the three *guṇas* operate in the system:

(1) *puruṣa* (consciousness)

(2) *prakṛti* (nature: *sattva, rajas, tamas* in equilibrium)

↓

(3) *buddhi* (intellect or will)

↓

(4) *ahaṁkāra* (ego) (*rajas*-mode)

↙ ↘

ahaṁkāra (*sattva*-mode) — *ahaṁkara* (*tamas*-mode)

(5) mind

Sense Capacities	Action Capacities	Subtle Elements	Gross Elements
(6) hearing	(11) speaking	(16) sound	(21) space
(7) feeling	(12) grasping	(17) touch	(22) wind
(8) seeing	(13) walking	(18) form	(23) fire
(9) tasting	(14) generating	(19) taste	(24) water
(10) smelling	(15) excreting	(20) smell	(25) earth

The *guṇa, sattva*, rather than referring to such forms as *ap* (water) and *kapha* (phlegm), either subtle or gross, refers, rather, to such processes as the five sense capacities, the five action capacities, mind, egoity in some of its functions, and most importantly, the reflective discernment (*adhyavasāya*) characteristic of the *buddhi* or intellect. In other words, *sattva* has to do with the process of thinking, the subjective side of experience, or what might be called the intelligising process within *prakṛti*. The *guṇa, tamas*, on the other hand, refers to all phases of formulation, whether subtle or gross, and has to do with the process of reification or objectivation, the objective side of experience, or what might be called the objectivating process within *prakṛti*. Finally, the *guṇa, rajas*, as the chart indicates, refers to both the *sattva*-side (subjective) and the *tamas*-side (objective), serving, in other words, as a mediating function (see *Sāṁkhyakārikā* XXV in Larson 1979: 264). The *guṇa, rajas*, is the energy or action (*karman*) within *prakṛti* that is essential for all intellectual activity as well as for all objective formulation, whether subtle or gross. The three *guṇas*, in other words, must be construed together as constituting *prakṛti* (nature or materiality). It is not that the *guṇas* are qualities or attributes of *prakṛti*. According to Sāṁkhya philosophy, they *are prakṛti*. Moreover, according to the above chart, they manifest themselves in experience primarily on the level of egoity (*ahaṁkāra*), with ordinary 'subjectivity' showing itself when *ahaṁkāra* is in its *sattva*-mode and ordinary 'objectivity' showing itself when *ahaṁkāra* is in its *tamas*-mode, and both modes depending on *ahaṁkāra* in its *rajas*-mode. Furthermore, it is crucial to note that these *guṇa*-transactions have nothing whatever to do with *puruṣa* or consciousness. That is to say, the *guṇa*-transactions are all only modalities of *prakṛti* (nature or materiality). In other words, according to Sāṁkhya philosophy, ordinary 'subjectivity' and ordinary 'objectivity' are both encompassed within *prakṛti* or materiality.

A useful parallel to the Sāṁkhya formulation of *sattva, rajas, tamas* may be found in *Yogasūtra* I. 41 (H. Āraṇya 1983: 88), in which a distinction is made between the *grahītṛ* (the 'subject' of experience), the *grāhya* (the 'object' of experience) and the *grahaṇa* (the 'grasping' or process of experience itself). The *grahītṛ* or 'subject' is the *sattva*-mode; the *grāhya*, whether subtle or gross, is the *tamas*-mode; and the *grahaṇa* or apprehending is the mediating *rajas*-mode. All three, however, are modes of *citta* or mind-*stuff*, that is to say, all three are manifestations of the *material* functioning of *prakṛti* and have nothing whatever to do with pure consciousness (*puruṣa*).

Finally, in terms of what might be called the 'cosmology' of the three *guṇas*, according to Sāṁkhya philosophy, the *Sāṁkhyakārikā* (verses 53 and 54, Larson 1979: 271–72) informs us that the divine (*ūrdhva*) realm is predominantly *sattva*; the middle (*madhya*) or human realm is predominantly *rajas*; and the lower (*mūla* or root level) or animal and plant realm is

predominantly *tamas*. The expression 'predominantly' (*viśāla*) is important, for it is not the case that there are clear separations between the *sattva*-realm, the *rajas*-realm and the *tamas*-realm. The three *guṇas* must always be construed together, recognising that in some instances thinking and intelligising will be dominant (namely, in the divine realm), in other instances willing and/or determinate formulation will be dominant (namely, in the human and animal realms, respectively). All sentient creatures, however, can transact and/or move (*saṁsāra*) through the various levels. There is no hard and fast distinction between the divine and human or between the human and non-human. There is, rather, a hierarchy of life in which all sentient creatures participate 'from Brahmā down to a blade of grass' (. . . *brahmādistambaparyantaḥ, Sāṁkhyakārikā* 54, Larson 1979: 272).

If one were to ask about the possible 'ethnosociological' implications of this classical Sāṁkhya perspective, perhaps the best place to look is the *Mānavadharmaśāstra*, XXI. 35–50. In the passage Manu links up *sāttvika* with the realm of divinity (*devatva*), *rājasa* with the realm of humanity (*manuṣyatva*), and *tāmasa* with the realm of 'animality' (*tiryaktva* (XII. 38). Perhaps more to the point, Manu then correlates the realm of *dharma* with *sattva*; the realm of *artha* with *rajas*; and the realm of *kāma* with *tamas* (XII. 38), presumably because *dharma* is the realm of intelligible order (*sattva*), *artha* is the arena of activity in the *saṁsāric* world for maintaining order (*rajas*), and *kāma* is the sphere of physical interaction in which determinate formulation and embodiment occur (*tamas*). Manu then goes on (XII. 41–50) to describe the tripartite hierarchy of life (or what he calls the *gauṇikī gati* or '*guṇa*-destinies', XII. 41) of sentient creatures. He lists three levels (*sattva, rajas, tamas*) with three sub-levels within each (high *sattva*, middle *sattva*, low *sattva*, and so forth), or, in other words, nine levels of sentient life, with Brahmā at the highest and 'plants' or a blade of grass at the lowest. The passage illustrates beautifully Marriott's point about the 'fluidarity' of the Hindu world (Marriott 1989: 3).

Oddly enough, however, Marriott, while calling attention to this passage from Manu, nevertheless rejects it (1989: 13), and here is where I think Marriott goes seriously off the track in his attempt to develop an ethnosociology for South Asia. Marriott gives the impression that Manu is somewhat aberrant in his interpretation of the *guṇas* and in his linkage of the *guṇas* with the *puruṣārthas*. But, as I have suggested, quite to the contrary, Manu appears to be very close to a precise classical Sāṁkhya interpretation of a cosmology-cum-ethnosociology. If anyone, it is Marriott who is aberrant in his linkages. In any case, with respect to the Manu passage, this is what Marriott says:

> Manu (at 12.38) differs from these alignments and seems to be unique among the classical authors of *dharmaśāstra* in linking (*i*) "coherence"

(*dharma*) with "goodness" (*sattva*), (*ii*) "advantage" (*artha*) with "passion" (*rajas*) and (*iii*) "attachment" (*kâma*) with "darkness" (*tamas*). Manu's linkages would result from simply placing the lists of aims and strands side by side, each in its conventional order, and reciting them together.

. . . . Manu's alignments in this verse are no doubt congenial to some others who are situated as he is, but they conflict with more widespread understandings. They have been influential . . . , but would confound development of a more generalisable and realistic sociology (Marriott 1989: 13 fn. 10).

The crucial passage in the above, in my view, is the very last line: '. . . would confound development of a more generalisable and realistic sociology.' In terms of what? In terms, I am inclined to think, that would 'confound' a definition of 'antiequivalence relations' *à la* Non-reflexivity, Non-symmetricality and Non-transitivity. In other words, my critique of Marriott at this point from the perspective of Sāṁkhya philosophy is that at a crucial point he forces the South Asian evidence to fit a generalised construct derived from a non-South Asian environment, whereas in fact he needs to do just the reverse. The task, in my view, is to take the indigenous theoretical constructs in South Asian environments in order to *re*-construct the generalisations of Western sociology. Instead, what Marriott does is to take a set of generalisations from Western thought (namely, the set known as 'equivalence relations' reworked as 'antiequivalence relations') and to massage the South Asian data to fit the essentially Western tripartite scheme. It is hardly surprising, therefore, that his correlations look odd and, in fact, confound the South Asian analytic schemes.

But my response to Marriott is already overly long. Let me quickly move on to make an alternative suggestion by way of suggesting how to highlight some of the unique features of South Asian cognitive structures. Instead of 'ethnosociology' I find myself wondering if it might not be helpful to back up and ask about what might be called an 'ethnophilosophy'. In other words, instead of remaining on the level of social theory and/or social-anthropological theory, there could be merit in looking at the philosophical axioms that operate in our theorising. In terms of Western social science, for example, it is clear enough that the great early theoreticians were Marx, Durkheim and Weber. Marx, of course, is unintelligible without Feuerbach and Hegel. Durkheim is clearly neo-Kantian in his orientation, and Weber's work is clearly dependent on *verstehen*-theory (Dilthey, *et al.*) and traditions of 19th-century historicism. All three, in other words, are heavily dependent on the axioms of modern philosophy since Descartes.

One interesting way to approach pre-modern cognitive structures in South Asia and/or traditional Indian philosophy is to think about the tradition in terms of the *absence* of the axioms of modern philosophy. Putting the matter somewhat differently, certain kinds of *separation* are

absent in traditional Indian philosophy, and these absent separations tend to highlight some of the unique features of traditional Indian philosophising. I would quickly mention five such absent separations in order to illustrate what I mean:

> In traditional Indian philosophy there tends to be an 'absence' of a 'separation'
> (1) between 'mind' and 'body';
> (2) between 'reason' and 'experience';
> (3) between 'formal logic' and 'material logic';
> (4) between the 'divine' and 'human'; and
> (5) between 'birth' and 'rebirth'.

I think that a good case can be made for each of these absences in traditional Indian philosophy. This is not to say that Indian philosophy does not distinguish or cannot distinguish between these various notions. It is only to say that traditional Indian thought does not find such separations useful in the task of identifying important philosophical issues.

Now, it is not my purpose to expound in this context precisely what I mean by these absences. It is only to suggest that if such 'absences' are even partly the case, then, issues in ontology, epistemology, logic, theology and social anthropology are framed in radically different ways in traditional South Asian thought, and one of the key intellectual tasks of the future is to develop a sophisticated comparative philosophy able to deal with issues like this. We need, in other words, not only further reflection about 'ethnosociology' but further work in 'ethnophilosophy' as well.

Marriott is to be commended for making all of us aware of the limitations of our respective methodologies and our specialised subject-areas. He invites us to a much broader dialogue in which we look critically at the presuppositions of our various approaches and open ourselves to traditions and perspectives other than our own. In this sense his invitation to develop 'at least one non-Western social science' is welcome indeed!

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Western sociology with Indian icing

K.N. Sharma

Marriott's 'Constructing an Indian ethnosociology' (1989: 1–39) is a challenging and provocative paper. It is a welcome addition to the Indian sociology being developed by Western social scientists from Louis Dumont onwards. Indian social scientists are always at the receiving end, whether it is Parsonian general theory, Marxian dialectical materialism, or Dumontian structuralism.

In this short paper I would like to express my appreciation of Marriott's enterprise and at the same time indicate what I consider to be its very serious shortcomings. Let me first present very briefly what I think are the key ideas of Marriott's thesis. At the very outset he challenges rightly the claims to 'universal significance and value' made on behalf of Western social sciences. Marriott maintains that all social sciences are cultural or 'ethno' sciences because they 'develop from thought about what is known to particular cultures', and so far they are 'of only one limited, Western type'. Western social sciences cannot therefore deal with the questions to which Indian social institutions are the answers. Besides, terms such as 'individual' and 'class' rarely fit Indian definitions of reality. Marriott hopes that 'by working with a culturally related, but non-European people's thought about their own realities', the ethno sciences of other lands 'may provide better bases for the future claim of an expanded multicultural set of sciences' to universality (pp. 1–3).[1]

Marriott outlines his methodology for the development of an ethnosociology on the basis of the ideas of Talcott Parsons and Louis Dumont. He however cautions, again rightly, that while one's analysis should be analytically powerful enough to define all the major parameters of the culture under study, this should be done without violating the culture's ontology and epistemology (p. 4).

Without going through all the steps that Marriott takes towards developing an Indian ethnosociology, let me pinpoint the most crucial ones. He

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[1] Page references without a date refer to Marriott 1989.

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adopts a truncated Sāmkhya Yoga perspective (p. 32) to provide himself with the theory of *triguṇa* or the three strands of *sattwa*, *rajah* and *tamah*. He uses this theory for an explicit analysis of the 'common properties', if any, of cognitive 'layers' as a preliminary step for 'constructing a general theoretical system for the Indian social sciences' (p. 7). These 'layers' include the three strands (*guṇas*) of Sāmkhya Yoga, the three humours (*doṣas*) of Ayurveda, the three plus one ends of the *puruṣārtha* scheme, and the four classes of the varṇa order. Marriott presents them in the form of cubes as geometric metaphors and mnemonics for Indian spaces within which everything must be rated along at least three dimensions (p. 9). This leads him to identify three 'fundamental relational properties of mathematics and symbolic logic—reflexivity, symmetry and transitivity' and to establish partial links with 'the operations of set-theory—intersection, inclusion and union' (p. 17).

Marriott then returns to his old concern with the identification of relations, 'dividuals', and transactions, which has its roots in Western culture and the Western academic orientation to sociology or social anthropology as a social science. For example, he contrasts the 'equivalence relations' in Western thought with what he calls the Hindu postulation of 'antiequivalence relations' (p. 17). In doing so, he leans on the Western concepts of 'person' and 'individual'.

II

I appreciate Marriott's continuing search for a paradigm which may illuminate the distinctiveness of Indian social phenomena. For some time he leaned on vedānta *darśana* (see Marriott 1976) but, probably finding it unsuitable, he has now turned to Sāmkhya Yoga. After the publication of a volume of essays on karma (see O'Flaherty 1980), I was expecting such a major turn in Marriott's thinking. I also appreciate his bold stand on the nature of Western social sciences and their limitations in comprehending non-Western social phenomena and his mild criticism of the Western social sciences to be universal. I can also understand his unhappiness with Dumont's hierarchical perspective to uncover social relations in India in structural terms. However, his efforts at the construction of an Indian ethnosociology remain Western despite the Indian icing of Sāmkhya Yoga.

I concede that academics are (and should be) free to construct theoretical frameworks according to individual preferences. I also know that Marriott will find enough evidence to support his contentions just as Dumont found enough material to support his hierarchical perspective. However, there is a limitation on such freedom and Marriott himself acknowledges the same: the paradigm must not violate the culture's ontology, its presuppositions, or its epistemology (p. 4).

My critique of Marriott's paper is divided into two parts. In the first I

argue that Marriott's ethnosociology is rooted in Western categories of thought and the incorporation of a few peripheral ideas of Sāmkhya Yoga cannot make it Indian. In the second part I have highlighted the serious problem of the misinterpretation of Indian concepts and texts found not only in Marriott's work but in the writings of other scholars as well.

It is unfortunate that Marriott has overlooked totally the Indian theory of 'man' in his concern with the study of relations. Any concern with the study of relations among 'men' in a particular society must be preceded by an understanding of the concept or theory of 'man' in the relevant tradition. The Indian theory of 'man' is rooted in the notion of '*puruṣa*'. Marriott is aware of D.P. Mukerji's seminal address on the subject (see Mukerji 1958), for he refers to it on page 4 of his article. He however misses the significance of Mukerji's emphasis on the significance of the concept of '*puruṣa*' in Indian thought in contrast to the Western concept of 'individual'. Marriott's own notion of 'dividual' does not rectify the situation. It is rather strange that he brushes the Sāmkhya emphasis on the plurality of '*puruṣa*' (*puruṣa bāhulya*) under the carpet in its English rendering as 'consciousness' (p. 22), and tears it away from the *triguṇa* theory.

Marriott borrows only a part of the *triguṇa* theory from the Sāmkhya *darśana*, which aims at '*ātyantika dukhanivṛtti*' (final and ultimate warding off of sorrow) of man (*puruṣa*) through the replacement of the goal of *bhoga* (experience) by that of *apvarga* (discarding of *bhoga*). (Incidentally, it is only later in the development of the Indian tradition that the notion of *apvarga* was replaced by the idea of *mokṣa*). It is in this context that the theory of *triguṇa* is presented in the Sāmkhya *darśana*, for explaining '*prakṛti*' (materiality, according to Gerald Larson) and its *vikṛti* or changing manifestations. But if you take away the *puruṣa*, it loses all meaning. For lack of space, I cannot go further into the Indian theory of 'man' here; I have attempted to explain it elsewhere (see Sharma 1986a, 1986b, 1988a, 1988b, 1989).

On the basis of my personal efforts to understand the relevant Sanskrit texts and their underlying meaning, I am convinced that one has to abandon thinking in terms of Western categories of thought in order to arrive at the threshold of an understanding of the Indian tradition. For behind every theory there is a philosophy, a perspective, a vision or *dṛṣti*. If, in place of the total perspective, we take up bits and pieces of a particular theory (say, Sāmkhya) and graft them on Western categories, or their derivatives or negations (e.g., 'individual' or 'dividual', 'structure' or 'fluidarity'), I am afraid we may end up with an impotent theoretical perspective, losing the vitality and vision of both.

Sāmkhya *darśana* is the oldest of India's philosophical traditions. It has influenced, by itself or in conjunction with Yoga, other *darśanas*. It has inspired not only Hindu theology but also the Indian sciences such as Ayurveda and Jyotiṣa. Therefore, if we have to develop an Indian social science theory, it should indeed flow from the cosmology of Sāmkhya—no less. If one sacrifices the cosmic context for 'man' postulated by it at the altar of Western scientism, one cannot claim that such playing around with the original and holistic perspectives does not violate the culture's ontology, presuppositions, and epistemology—the excellent criterion laid down by Marriott.

III

This leads me to the second problem I have with Marriott's paper. This is the problem arising from the construction of the meaning of Sanskrit terms devoid of context. From the many such wrong meanings that I have recently encountered I will mention here only a few to illustrate my point. Marriott translates *sattwa* (which he misspells *sattva*), *rajah* and *tamah* as goodness, passion and darkness (p. 7), and *dharma*, *artha* and *kāma* as coherence, advantage and attachment (p. 11). All these meanings are misleading and inappropriate. Larson (whom Marriott recognises as an authority) and Bhattacharya (1987: 154) have translated *sattwa*, *rajah* and *tamah* as intelligibility, activity and inertia, respectively, and this is certainly more accurate.

None of the definitions of *dharma* in Sanskrit texts can be made to yield the connotation of coherence as suggested by Marriott. Besides, *dharma* and *adharma* are not static categories which may be fitted into Marriott's cubes. *Dharma* can be *sāttwika*, *rājas* or *tāmas*, and *dharma* for one person, at a particular moment of time, may be *adharma* for another, or for the same person at another moment. In the context of *puruṣārtha*, *artha* may at best be translated as a worldly or material goal, the core of which is *dhana* (money). *Kāma* may lead to attachment. One may be attached to *dharma* even, but *kāma* itself means desire, epitomised by the desire for sexual gratification.

The problem of misleading translation becomes dangerously contagious when scholars like Marriott feel the need to quote authorities. Let me give a few examples. On page 3 he quotes Karl Potter to say that 'actors are products of actions'. It should be the other way around, i.e., action (*kriyā*) must be seen as a product of the actor (*kartā*), for the actor is independent of action. This is what the notion of 'independent actor' (*swatantrah kartā*) denotes. In case Potter insists on actors being the products of their actions, then such products can only be called *bhoktā*, i.e., a person experiencing the consequences of previous actions (*kārma*).

On page 8, Marriott quotes Ronald Inden to say that *dhātu* and *dharma* have been formed from the root '*dhṛ*'. I would like to ask a simple question: If the word *dhātu* has been formed from the root '*dhṛ*', where has the letter *ṛ*, or its derivative *ra*, gone? Actually, as any one who knows Sanskrit would know, *dhātu* and *dhātā* are derived from the root '*dudhān*',

which also means *dhāraṇa* [upholding][2] and *poṣaṇa* (nourishing). The root '*dhṛ*' also means *dhāraṇa*.

On page 19, Marriott translates *tīrtha* as spatial crossings. Etymologically, the word means going across any mass of water—river, pool, or ocean—by swimming and not spatial crossing. Figuratively, it means any action that helps one to go across 'the ocean of sins'. There are three kinds of such *tīrthas*: mobile (Brāhmaṇas), immobile (places of *puṇya* or merit), and mental (acquisition of virtues such as truthfulness, control of the senses, giving of *dāna* or gifts, etc).

Marriott quotes Veena Das on page 18 to elaborate the nature of the three *guṇas* as three kinds of movement. Let me quote Larson again: his translation of Sāmkhya Kārikā numbers 12 and 13 which define the nature and mutual relationships of the three *guṇas* (see Larson and Bhattacharya 1987: 154):

> [*Kārikā* 12]: The constituents or constituent processes (*guṇa*) are experienced as agreeable (*prīti*), disagreeable (*asprīti*), and oppressive (*viṣāda*). Moreover, these constituents have as their purpose illumination (*prakāśa*), activity (*pravṛtti*) and restriction (*niyama*). Finally, with respect to the operation of the constituents, they mutually and successively dominate, support, activate and interact with one another.
>
> [*Kārikā* 13]: The intelligibility constituent (*sattwa*) is light-weight (*laghu*) and illuminating (*prakāśaka*) . . . . The activity constituent (*rajas*) is stimulating (*upastambhaka*) and moving (*cala*) . . . the inertia constituent (*tamas*) is heavy (*guru*) and enveloping (*varaṇaka*) . . . . These three, though different in operation and make-up, nevertheless function together for the purpose of illumination.

Veena Das, it appears, is nowadays accepted as an authority on Sanskrit and lately on *triguṇa* theory also. In one of her papers which I happen to have seen (Das 1985), she uses the words *sāttvika* (*sic*), *rājasika* and *tāmasika*. Marriott also uses the word *rājasika* (p. 30). While *sāttwika* is the correct adjective formed from *sattwa*, *rājasika* and *tāmasika* cannot be formed from *rajah* or *rājas* and *tamah* or *tāmas*, respectively, by any known rule of Sanskrit grammar. The adjectives are *rājas* and *tāmas*. It is high time that ethnosociologists realise that a familiarity with words without a knowledge of grammar is perilous for anyone who hopes to use Sanskrit properly. Or does a bow to ethnosociology absolve one from adherence to the rules of grammar? I may here add in passing that Das, in the paper I have cited, mishandles other Sanskrit words too, producing bizarre translations, and besides, applies uncritically Dumont's notion of hierarchy

[2] The English equivalent of *dhāraṇa* has been provided by us because the author is of the opinion that the word is untranslatable.—Ed.

(encompassing and encompassed) to the *triguṇa* theory (ibid.: 188). The translation of Sāmkhya Kārikā numbers 12 and 13, quoted above, should dispel all such misconceptions.

Diane Mines, another contributor to the ethnosociology number of *Contributions*, cites Veena Das for elaborating the meaning of the word *sūtaka* (Mines 1989: 103). She asserts that it implies 'connection through the female genitals'. The etymology of the word *sūtaka* is *sūyate prasūyate asmin*, i.e., in which birth takes place. Birth does not take place in the vagina but from it, and these are precisely the kind of fine distinctions that Sanskrit requires us to make. Besides, in the above etymology the word used for 'in which' is *asmin* which is in masculine gender, while the word for vagina, *yoni*, is in feminine gender. Further, the etymology alone leaves the statement incomplete as it does not explain 'in which' precisely as to whether time or a group (*parivāra, kuṭumba*, etc.) is the referent. There are many other mistranslations of Sanskrit words in the papers in the ethnosociology number of *Contributions* but I trust I do not have to mention them all. I do however hope that the main point I wanted to make is clear and that *Contributions* will not become a forum for the mutilation of Sanskrit and for the obfuscation of the concepts of Indian metaphysics and a vehicle for the spread of misinformation. In this connection, I cannot help recalling the Sanskrit adage that a blind man attached to another blind man falls at every step (*andhasyeva andhalagnasya vinipātah pade pade*). I am afraid that if the present tendency is not checked the sociology of India itself will fall.

Finally, a word about the study of Sanskrit texts. The reference to such texts to elucidate particular usages has been a well-established practice and nobody would object to it. The trouble started when Dumont insisted in the mid-1950s that the sociology of India would lie at the confluence of sociology (fieldwork data) and Indology (textual knowledge). Sociologists and social anthropologists are, by and large, neither equipped linguistically nor have enough time to study and understand the Indian discourse in which are embedded the values that Dumont wants to make the basis of our studies. Individual scholars pick up ideas selectively from various discourses and texts. Such a *śiloncha vṛtti* ('livelihood based on only picking fallen grain on the road') does not serve the cause of genuine scholarship. I assume that ethnosociology is not exempt from such simple but important requirements.

Let me give two examples of how texts may not readily mix. While the Bhagavad Gītā (5: 18) propounds the thesis of equal treatment of all (*sama bhāva*), the Manusmṛti and all the dharmaśāstra texts emphasise differential treatment according to varṇa. Similarly, the Bhagavad Gītā advises that *kāma* be discarded but the *puruṣārtha* scheme includes it as a legitimate end of life. Basing themselves on different texts, protagonists of opposite views could carry on a fruitless argument. And it will not do to

defend eclecticism by avoiding situations of disagreement, brushing them under the carpet as it were.

For my second example I turn to the *puruṣārthas*. While many texts have classified and interpreted *dharma, artha* and *kāma* according to the three *guṇas* (*sattwa, rajah* and *tamah*, respectively) though they are all conceived as the products of *rajah*, the Mahābhārata goes a step further and classifies even *mokṣa* as *sāttwika, rājas* and *tāmas*. The mutual relationship among the three *puruṣārthas* of *dharma, artha* and *kāma* can neither be explained in terms of Dumont's notion of linear hierarchy nor that of Charles Malamoud's concept of revolving hierarchy. If one wants to understand it in terms of Indian tradition one has to fall back on the *triguṇa* theory. It explains their 'mutuality, successive domination, support, activation, and interaction' like the relationship among the three *guṇas*.

My humble suggestion is that social anthropologists and sociologists interested in developing a social science theory which is sensitive to if not rooted in Indian tradition should devote more time to the study of the Indian discourse(s). The proper level for comparison is the discourse and not individual texts. Marriott's 'inevitable compromises' (p. 6) between Indian cosmological discourses and the Western discourse limited to society and the individual will also not illumine because the proper approach to discourse analysis and comparison is holistic. It is rather unfortunate that Marriott begins on a promising note, seeking to develop an 'Indian ethno-social science', which would offer 'a second lens', but ends up with 'some [in my view, considerable] shifting of Indian meanings in a Western direction' (p. 6). He thus ends up doing the very same thing for which he criticises Dumont.

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R IX (3)

Since World War II, a new situation has come about. There are today some 2,352,000,000 people in underdeveloped nations.[3] About 773,000,000, or ⅓, of them have already, through revolution, passed out of the sphere of Western imperialism into the new socialist states of China, Mongolia, North Korea, North Vietnam, and Cuba. However arduous and conflictful their conditions, they are now beyond the domination of the capitalist powers and are off on tracks of their own. Because of the Cold War (and, in the case of Vietnam, the hot war), American anthropologists are unable to study these societies directly and have made few comparisons of their political economies or community structures with those of underdeveloped nations with capitalist or with "mixed" economies. When American studies of socialist societies are made, the built-in assumption that "communism," especially revolutionary communism, is bad and unviable commonly produces distortions of both theory and fact.[4] Granting the difficulties of obtaining reliable information, I believe that more objective studies could be made if greater attention were paid to the work of the few Western social scientists who have lived in these countries, for example, Lattimore (1962), Robinson and Adler (1958), Robinson (1964), Myrdal (1965), and Crook and Crook (1959, 1966). In addition to primary sources from the socialist nations there are also, of course, the writings of Western journalists and other specialists who have lived or travelled in the new socialist countries since their revolutions. Examples are Dumont (1965, 1967), Gelder and Gelder (1964), Greene (1961, 1964, 1966), Snow (1962), Hinton (1966), Han Suyin (1965, 1966, 1967), Strong (1962, 1964), Burchett (1963, 1965, 1966), Taylor (1966), and many others. Most of these writers are favorable to the newer socialisms, and most tend to be neglected or scoffed at in the United States. Yet American social scientists think nothing of using travellers' reports to eke out their knowledge of non-Western societies of the 15th to 18th centuries, biased or mission-oriented though some of them may have been. Certainly such studies are not discarded on the grounds that their authors happened to like the societies they visited. There is no reason why anthropologists cannot apply similar criteria of objectivity to modern writers who admire China or other socialist countries today.

There remain about 1,579,000,000 people, or 67% of the total, in non-Western nations with capitalist or with "mixed" economies. Of those, 49,000,000, or 2% of the total, are still in more or less classical colonial societies such as South Africa, Mozambique, or Angola, ruled by small white elites drawn from the "mother country" or else now severed from it as separate settler populations. About another 511,000,000, or 22% of the total, live in what may be regarded as satellite or client states, states which have indigenous governments, but are so constrained by Western military or economic aid and by private investments that they have little autonomy. Most of their governments are opposed to social reforms and would probably collapse if Western aid were withdrawn. The largest of these states, with populations of over 5,000,000, are Columbia, Argentina, Peru, Brazil, Ecuador, Chile, Venezuela, the Philippines, South Vietnam, South Korea, Thailand, Taiwan, Malaysia, the Congo, Nigeria, Iran, Southern Arabia, Cameroon, and Turkey. The list is very tentative, for modern neo-imperialism varies in intensity. Some might include Mexico and Pakistan, bringing the total to 657,000,000, or 28% of the underdeveloped world. About 318,000,000, of these people or 14% of the total, live in nations beholden to the United States, either in Latin America, the traditional preserve of U.S. capital, or else in a fringe around China, where the United States has established satellite regimes in an effort to stave off the spread of revolutionary socialism. If we include Pakistan and Mexico, U.S. client states amount to about 20% of the total.

The remaining 873,000,000, or 37% of the total, live in nations that are usually considered in the West to be relatively independent, under governments containing popular nationalist leaders. Most of these leaders conducted nationalist struggles against European colonialism a decade or two ago, and some fought wars of liberation. (By contrast, the governments of most of the client states were either installed by, or arose after, military coups at least partly inspired from the West.) Most of the independent "Third World" nations regard themselves as politically neutral and as in some sense socialist or aspiring to become socialist. Because the appeal of their governments is of a multi-class character, Peter Worsley (1964) calls them "populist." The economies of these nations have both a public sector, with an emphasis on national planning, and a large private sector dominated by foreign capital. The largest of these states, with populations over 5,000,000, are India, Burma, Cambodia, Ceylon, Indonesia, Afghanistan, Nepal, Syria, Iraq, Yemen, the United Arab Republic, Algeria, Morocco, Kenya, Tanzania, Sudan, Ethiopia, Uganda, and Ghana.

During the 1950's, many liberal social scientists and others hoped that these neutral nations would form a strong Third World that could act independently of either the Western industrial or the Communist powers. I suggest that in the 1960's this hope has dimmed, and is now almost extinguished, chiefly because of the expansion of American capital and military power, the refusal of European nations to relinquish their own economic strongholds, and the failure of many new governments to improve the living conditions of their people. In the past 15 years, at least 227,000,000 people in 16 nations, or 10% of the underdeveloped world, have, after a longer or shorter period of relative independence, moved into, or moved back into, a client relationship, usually with the United States. These nations are Guatemala, Honduras, the Dominican Republic, Guyana, Venezuela, Brazil, Argentina, Bolivia, Ecuador, Trinidad and Tobago, South Vietnam, Thailand, Laos, the Congo, Togo, and, Gabon. In most of these countries the shift in orientation followed a military coup. A further 674,000,000 in India, Indonesia, Afghanistan, Ceylon, Kenya, and Ghana, which I have classified as "independent," have recently

[3] I use the term "underdeveloped" to refer to societies which have, or have recently had, particular features of economic structure produced as a result of several decades or centuries of overt or covert domination by Western industrial capitalist nations. I have included in this category all the nations and the remaining colonies of Latin America, Africa, and Asia, with the exception of Japan. These and later figures are derived from United Nations totals of 1961, as provided in the *World Almanac* of 1967. For some of the more general characteristics of underdeveloped economies see Myrdal (1956), especially Chapters 11–13, Baran (1957), and Frank (1966, 1967*a*).

[4] There are, of course, notable exceptions to this statement, among them, for example, Schurman (1966).

moved into much closer dependence on the United States, so that their future as independent nations is now uncertain. Together with the U.S. client states and colonial dependencies, this brings to 1,140,000,000, or 48% of the total, the number of people whose governments' policies are very heavily influenced by the United States. We must also remember that U.S. capital and military power now exert a strong influence on the colonies and client states of European powers (11% of the total), as well as on most of the remaining 8% of "neutral" states. In these circumstances, U.S. power can truly be said to be entrenched with more or less firmness throughout the underdeveloped world outside of the socialist states.

Countering this re-imposition of Western power, armed revolutionary movements now exist in at least 20 countries with a total population of 266,000,000. These countries are Guatemala, Peru, Venezuela, Ecuador, Paraguay, Brazil, Honduras, Bolivia, Columbia, Angola, Mozambique, the Congo, Cameroon, Portuguese Guinea, Yemen, Southern Arabia, the Philippines, Thailand, Laos, and South Vietnam. About 501,000,000 people live in seven other countries where unarmed revolutionary movements or parties have considerable support, namely India, Rhodesia, Southwest Africa, South Africa, Nicaragua, the Dominican Republic, and Panama. In more than ⅓ of the underdeveloped world, therefore, socialist revolution against both native elites and Western dominance is a considered possibility, while in another ⅓ it has already been accomplished. Even in the remaining relatively stable colonial, client, or neutral states, a majority of the people are getting poorer, and a small minority of rich are getting richer. Populations are increasing, discontent is widespread, and revolutionary struggles are quite possible within a decade or two. Whereas in the 1950's it looked to some of us as though much of the non-Western world might gain genuine political and economic independence of the West by peaceful means, this is no longer the case. Western dominance is continuing under new guises, even expanding and hardening. At the same time, revolution now begins to appear as *the* route by which underdeveloped societies may hope to gain freedom from Western controls.

In this revolutionary and proto-revolutionary world, anthropologists are beginning to be in difficulties. From the beginning, we have inhabited a triple environment, involving obligations first to the peoples we studied, second to our colleagues and our science, and third to the powers who employed us in universities or who funded our research. In many cases we seem now to be in danger of being torn apart by the conflicts between the first and third set of obligations, while the second set of loyalties, to our subject as an objective and humane endeavour, are being severely tested and jeopardized. On the one hand, part of the non-Western world is in revolt, especially against the United States government as the strongest and most counter-revolutionary of the Western powers. The war in Vietnam has, of course, exacerbated the non-Western sense of outrage, although the actual governments of most of these nations are so dependent on the United States that they soften their criticisms. On the other hand, anthropologists are becoming increasingly subject to restrictions, unethical temptations, and political controls from the United States government and its subordinate agencies, as Beals's (1967) report on problems of anthropological research and ethics amply shows. The question tends to become: what

does an anthropologist do who is dependent on a counter-revolutionary government in an increasingly revolutionary world? To complicate matters, into the arena has stepped a fourth and most vociferous public, namely students, who once imbibed knowledge peaceably, but who are now, because of their own crises, asking awkward questions about ethics, commitments, and goals.

There is little wonder that with all these demands many anthropologists bury themselves in their specialties or, if they *must* go abroad, seek out the remotest, least unstable tribe or village they can find. As Peter Worsley (1966) has recently pointed out, however, in a paper called "The End of Anthropology?" we shall eventually have to choose either to remain, or become, specialists who confine themselves to the cultures of small-scale pre-industrial societies, or else, bringing to bear all our knowledge of cultural evolution and of primitive social institutions, embark fully on the study of modern societies, including modern revolutions. If we take the former path, as our subject matter disappears, we shall become historians and retreat from the substantial work we have already done in contemporary societies. If we take the latter path—which is the one some of us must inevitably follow—we shall have to admit that our subject matter is increasingly the same as that of political scientists, economists, and sociologists. The only way that we can *not* admit this is by confining ourselves to studies of small segments of modern society; but as the scale of these societies widens, such studies are less and less justifiable theoretically or methodologically except within a framework of understanding of what is happening to the larger system. Anthropologists have, moreover, some right to demand of themselves that they do study the larger system as a totality, for they have 50 years of experience of analysing the interconnectedness of political, economic, and religious institutions within smaller-scale systems. While they must necessarily depend for much of their data on the other social sciences, anthropologists do have some historical claim to play a synthesizing role.

Unfortunately, we have I think a serious drawback in our own history which makes it very difficult for us to approach modern society as a single, interdependent world social system. This is that although we have worked for over 100 years in conquered societies, and although for at least 50 of them we have emphasized the interconnectedness of parts of social systems, we have virtually failed to study Western imperialism as a social system, or even adequately to explore the effects of imperialism on the societies we studied. Of late a few pioneer works have appeared which attempt this task, notably Worsley's (1964) book, *The Third World*. Wallerstein's (1966) collection, *Social Change: The Colonial Situation*, draws together useful extracts by social scientists and nationalist leaders over the past 20 years. Wolf's study of Mexico (1959), Steward's and others' of Puerto Rico (1956), Epstein's of politics in the Zambian copper-belt (1958), and a number of others also move in this general direction; but it is remarkable how few anthropologists have studied imperialism, especially its economic system.

It is true, of course, that anthropologists have made numerous studies of modern social change in pre-industrial societies, especially in local communities. They have, however, usually handled them through very general concepts:

"culture contact," "acculturation," "social change," "modernization," "urbanization," "Westernization," or "the folk-urban continuum." Force, suffering, and exploitation tend to disappear in these accounts of structural processes, and the units of study are usually so small that it is hard to see the forest for the trees. These approaches, in the main, have produced factual accounts and limited hypotheses about the impact of industrial cultures on pre-industrial ones in local communities, but have done little to aid understanding of the world distribution of power under imperialism or of its total system of economic relationships. Until recently there also has been, of course, a bias in the types of non-Western social units chosen for study, with primitive communities least touched by modern changes being preferred over the mines, cash-crop plantations, white settlements, bureaucracies, urban concentrations, and nationalist movements that have played such prominent roles in colonial societies.

Why have anthropologists not studied world imperialism as a unitary phenomenon? To begin to answer this question would take another article. I will merely suggest some possible lines of enquiry, namely: (1) the very process of specialization within anthropology and between anthropology and related disciplines, especially political science, sociology, and economics; (2) the tradition of individual fieldwork in small-scale societies, which at first produced a rich harvest of ethnography, but later placed constraints on our methods and theories; (3) our unwillingness to offend, by choosing controversial subjects, the governments that funded us; and (4) the bureaucratic, counter-revolutionary setting in which anthropologists have increasingly worked in their universities, which may have contributed to a sense of impotence and to the development of machine-like models.

It may be objected that I have ignored the large volume of post-war American writing in applied anthropology and in economic and political anthropology concerned with development. This work certainly exists, and some of it is fruitful. I would argue, however, that much of it springs from erroneous or doubtful assumptions and theories that are being increasingly challenged by social scientists in the new nations themselves. Among these assumptions are (1) that economic backwardness can be explained in terms of values and psychological characteristics of the native population; (2) that it is desirable to avoid rapid, disruptive changes; (3) that the anthropologist cannot take value-positions that oppose official policies; (4) that causation is always multiple; (5) that the local community is a suitable unit for development programs; (6) that the main process by which development occurs is diffusion from an industrial centre; and (7) that revolution is never the only practicable means toward economic advance.[5] In general, applied and economic anthropology stemming from North America has assumed an international capitalist economy in its framework. The harsh fact seems to be, however, that in most countries of the underdeveloped world where private enterprise predominates, the living conditions of the majority are deteriorating and "take-off" is not occurring. If this is true, it will not be surprising if the intellectuals of these countries reject the metropolitan nations' applied social science and seek remedies elsewhere.

There are of course already a large number of studies, indeed a whole literature, on Western imperialism, most although not all by writers influenced by Marx. In addition to the classic treatments by Hobson (1954), Lenin (1939), and Luxemburg (1951), Moon (1925), Townsend (1940), Williams (1944), Steinberg (1951), Baran (1957), and the anthropologist Mukherjee (1958) have provided outstanding examples of such work. More recent studies include, of course, Baran and Sweezy (1966), Nkrumah (1966), Dumont (1965, 1967), Fanon (1963, 1965), and Frank (1967*a*). Such books tend in America to be either ignored or reviewed cursorily and then dismissed. They rarely appear in standard anthropological bibliographies. I can only say that this American rejection of Marxist and other "rebel" literature, especially since the McCarthy period, strikes me as tragic. The refusal to take seriously and to defend as intellectually respectable the theories and challenges of these writers has to a considerable extent deadened controversy in our subject, as well as ruining the careers of particular individuals. It is heartening that in recent years the publications of Monthly Review Press, International Publishers, *Studies on the Left*, and other left-wing journals have become a kind of underground literature for many graduate students and younger faculty in the social sciences. Both orthodox social science and these Marxist-influenced studies suffer, however, from the lack of open confrontation and argument between their proponents. There are of course political reasons for this state of affairs, stemming from our dependence on the powers, but it is unfortunate that we have allowed ourselves to become so subservient, to the detriment of our right of free enquiry and free speculation.

I should like to suggest that some anthropologists who are interested in these matters could begin a work of synthesis focusing on some of the contradictions between the assertions and theories of these non-American or Un-American writers and those of orthodox American social scientists, and choosing research problems that would throw light on these contradictions. For example:

1) We might examine Frank's (1967*c*) argument, from United Nations figures, that per capita food production in non-Communist Asia, Africa, and Latin America has declined in many cases to below pre-war levels since 1960, whereas it has risen above pre-war levels in China and Cuba, in contrast to the common assumption in the United States that capitalist agricultural production in underdeveloped countries is poor, but socialist production is even poorer.

2) We might develop a set of research problems around comparisons of the structure and efficiency of socialist and capitalist foreign aid. One might, for example, compare the scope and results of American economic and military aid to the Dominican Republic with those of Russian aid to Cuba. Although Americans cannot go freely to Cuba, it is conceivable that a European and an American, co-ordinating their research problems, might do such comparative work. In countries such as India, the UAR, or Algeria, comparable socialist and capitalist aid projects might be studied within the same locality.

3) We might undertake comparative studies of types of modern inter-societal political and economic dominance which would help us to define and refine such concepts as

[5] For these and other criticisms, see Bonfil Batalla (1966), Onwuachi and Wolfe (1966), Stavenhagen (1966–67), and Frank (1967*b*).

imperialism, neo-colonialism, etc. How, for example, does Russian power over one or another of the East European countries compare with that of the United States over certain Latin American or Southeast Asian countries with respect to such variables as military coercion, the disposal of the subordinate society's economic surplus, and the relations between political elites? How does Chinese control over Tibet compare, historically, structurally, and functionally, with Indian control over Kashmir, Hyderabad, or the Naga Hills, and what have been the effects of these controls on the class structures, economic productivity, and local political institutions of these regions?

4) We might compare revolutionary and proto-revolutionary movements for what they can teach us about social change. In spite of obvious difficulties, it is possible to study some revolutions after they have occurred, or to study revolts in their early stages or after they have been suppressed (for a rare example of such a study, see Barnett and Njama 1966). There *are*, moreover, Westerners who live and travel with revolutionary movements; why are anthropologists seldom or never among them? We need to know, for example, whether there is a common set of circumstances under which left-wing and nationalist revolutions have occurred or have been attempted in recent years in Cuba, Algeria, Indo-China, Malaysia, the Philippines, Indonesia, Kenya, and Zanzibar. Are there any recognizable shifts in ideology or organization between these earlier revolts and the guerrilla movements now taking shape in Guatemala, Venezuela, Columbia, Angola, Mozambique, Laos, Thailand, Cameroon, Yemen, or Southern Arabia? What are the types of peasantry and urban workers most likely to be involved in these revolutions? Are there typologies of leadership and organization? Why have some revolutions failed and others succeeded? How did it happen, for example, that some 1,000,000 Communists and their families and supporters were killed in 1966 in Indonesia with almost no indigenous resistance, and how does this affect the self-assessment and prospects of, say, the Left Communist Party in India?

I may be accused of asking for Project Camelot, but I am not. I am asking that we should do these studies in *our* way, as we would study a cargo cult or kula ring, without the built-in biases of tainted financing, without the assumption that counter-revolution, and not revolution, is the best answer, and with the ultimate economic and spiritual welfare of our informants and of the international community, rather than the short run military or industrial profits of the Western nations, before us. I would also ask that these studies be attempted by individuals or self-selected teams, rather than as part of the grand artifice of some externally stimulated master plan. Perhaps what I am asking is not possible any more in America. I am concerned that it may not be, that Americans are already too compromised, too constrained by their own imperial government. If that is so, the question really is how anthropologists can get back their freedom of enquiry and of action, and I suggest that, individually and collectively, we should place this first on the list.

Comments

by Olga Akhmanova ☆

Moscow, U.S.S.R. 15 v 68

The articles by Kathleen Gough, Gerald D. Berreman, and Gutorm Gjessing are all that could be desired: profound scholarship, clarity, brilliance, force, conviction! They cannot fail to bring it home to all anthropologists that the modern, changing world and its destinies are their immediate concern. One wonders if CA's voice is loud enough to be heard by those responsible for the present state of affairs.

by Ralph Beals ☆

Los Angeles, Calif., U.S.A. 27 v 68

Berreman, Gjessing, and Gough have belatedly joined the Women's Anthropology Society of Washington, who, in 1885, called for study of their contemporary society—asking "What state, what town, what household is destitute of the choicest materials for our work?"—and urged attention to social problems in order to "chant in noble unison, 'I count *nothing* that affects humanity foreign to myself.' " That this was not empty rhetoric was demonstrated by the Society's co-operating in 1886 in perhaps the first housing survey in the United States and then organizing and partially financing a project which built 808 low-cost housing units in Washington (Helm 1966).

Concern with contemporary society later was submerged as the very few anthropologists concentrated on "urgent anthropological research," the discovery and recording of the enormous variety of vanishing human cultures. After 1900, social action was largely confined to combating racist doctrines in the United States. A related movement was the re-examination and rejection of 19th-century cultural evolutionism, both as a justification of colonialism and as the foundation for "scientific history." The modern micro-evolutionism of Steward and others in the United States is dedicated to seeking recurrent uni-directional cultural processes within relatively short time spans and has little if any relation to 19th-century evolutionism. To attribute Leslie White's neo-evolutionary "culturology" to the influence of United States post-World War II global strategy is absurd. Not only do White's basic ideas antedate the war, but Opler (1961, 1962) has traced the remarkable parallelism in ideas and language between the writings of White and those of Marx, Engels, Plekhanov, Bukharin, Lenin, Stalin, and other Marxist theorists on the foundations of historical materialism. Far from belonging to history, 19th-century evolutionism lives on in "scientific history" and markedly influences the work of anthropologists in the Communist countries. (For a humanist's analysis of the inhumane consequences of "scientific history," see Camus 1956: 188–252.)

Perhaps one reason more specific social action programs were less popular among U.S. anthropologists after 1900 was the catastrophic effects of anthropological intervention in behalf of the American Indian. Alice Fletcher, acting in behalf of what the Omaha tribe believed was the best way of protecting their homes and lands and using the best anthropological arguments of the day, played an important role in the adoption by the United States Congress of the Dawes Severalty Act of 1887 (Helm 1966). It quickly became apparent that this probably was the single most damaging act of legislation for the American Indian ever adopted. With reason, many United States anthropologists came to doubt whether their knowledge of culture and society was adequate to proposing major anthropological solutions to social issues, although a great many were individually concerned with problems of social justice. It is true that social scientists have vastly improved their ability to make meaningful assertions about social issues and the consequences of policy decisions. Many have been doing so for a long time, but unfortunately policy-makers have not always listened—partly, perhaps, because there are still some pretty important gaps in our basic knowledge of human behavior and the cultural and social contexts in which it occurs, and our reputation for predictive reliability still is poor.

All three of the authors are concerned with the not wholly original discovery that man, including the social scientist, is a creature of his culture and its value system and hence science is never wholly objective. Rather than accepting this as a limitation against which we must constantly struggle, they would end the dilemma by surrendering wholly to an ideology which implicitly —and pretty explicitly in Gough's case, with her simplistic analysis of the world situation—is the systematic dialectic of one side of the Cold War, with all its weary clichés. I prefer to struggle with an imperfect objectivity rather than to surrender what little I have in a total commitment to an unanalyzed set of ideological premises.

Similar questions arise concerning the call for relevance in our research. Insofar as I understand the authors, they consider relevance in terms either of the needs of the emerging nations or of an ideological system. These certainly should have weight, but I am not sure the definitions of relevance by the bureaucracy or politicians of a developing nation are much more satisfactory than those of their counterparts in the United States. Further, in a discipline such as anthropology, still far from developing an adequate theoretical structure, one aspect of relevance is what research will contribute to the maturing of the discipline. As the Committee on Science and the Promotion of Human Welfare of the American Association for the Advancement of Science (1965) has pointed out, when science succumbs to social pressures or comes to be regarded only as a means of satisfying immediate social demands, there is a loss of knowledge which is against the long-range interests of society itself. Gjessing quotes Tolstoy's criticism of historians who answer questions no one has asked. He might have added Tolstoy's remark (quoted in Helm 1966):

> But there are laws directing events.... The discovery of these laws is only possible when we have quite abandoned the attempt to find the cause in the will of one man.

I still think that among our duties is to seek out and refine such "laws" and their application.

The articles contain numerous errors or misleading statements. I mention only a few. According to the Editor of the *New York Times*, to whom I wrote protesting the Braestrud article, the quotation with which Berreman heads his article was intended to be a joke. In a way I am sorry this is the case, for the idea might be worth trying. Berreman also implies that the resolution passed at the American Anthropological Association meeting in Pittsburgh in November, 1966 condemned the U.S. role in the war in Vietnam. This is simply not true. The amended motion passed condemned genocide and certain kinds of warfare "by anyone anywhere" and asked "that all governments put an end to their use at once and proceed as rapidly as possible to a peaceful settlement of the war in Vietnam." The United States was not named specifically, and, in the opinion of many who voted for it, the resolution applied equally to North Vietnam as well as to many other countries.

Gough states that she left the United States because the "proper goals of intellectual work have been undermined" through the use of students' academic grades by draft boards under the Selective Service System. In fact, this effort was resisted by universities and was abandoned by Selective Service authorities in 1967. No responsible university in the United States releases information about student grades except at the request of the student.

The three authors all are in favor of anthropologists figuratively mounting the barricades in behalf of causes in which they believe. I agree that people should be active in support of what they believe. As one who has suffered a number of unpleasant events as a result of supporting unpopular positions, I could hardly believe otherwise. I am not, however, under the illusion that being an anthropologist has always been relevant. More specifically, I insist on the right to chose my own barricades, and I reject the totalitarian effort to commit all anthropologists to political positions through their professional organizations. Despite Gough's criticisms, anthropologists in the United States still have a great deal of freedom of choice about their research. In a significant part of the world they have little.

by P. M. Butler ☆

Surrey, England. 23 v 68

Not being a social anthropologist and only very marginally an anthropologist at all, any comments I make must be merely those of an interested observer. The problems raised by the articles are in principle similar to those faced by scientists in many other fields: What is the function of the anthropologist in his own society, and what can he do if he disapproves of this function? Although social anthropologists mostly study cultures other than their own, they cannot help being members of their own society, predominantly Western and capitalist, and this is bound to colour their ideas, as Gjessing emphasizes. Nevertheless, the social anthropologist is in a better position than most members of his society to reach an understanding of other cultures, and the increasing intercultural contact of a shrinking world creates an increasing need for such understanding. That modern industrialized societies (whether capitalist or communist) should use anthropological knowledge to increase their domination of other societies is probably inevitable, and it is very doubtful whether the anthropologist as such can do anything to prevent this process. Even a study of revolution, as proposed by Gough, could be used for counter-revolutionary purposes. This does not mean that studies of such an important element of human social behaviour should not be made, but how knowledge is used must depend upon the political and economic power groupings to which the knowledge is available. When it comes to understanding his own society, including the determination of its external policies and his own role in relation to these, the anthropologist is not very well equipped; he tends to leave the field to the sociologist and economist. Perhaps more co-operation between social anthropology and these disciplines is called for; perhaps encouragement should be given to members of other cultures to carry out anthropological studies in North America and Europe. Until a more objective view of "Western" culture is available, it will be impossible for anthropologists reared in that culture to correct their own bias when studying other cultures.

by Daniel Cazès ☆

México, D.F., Mexico. 20 v 68

CURRENT ANTHROPOLOGY has at last satisfied the need for an open and extended discussion of a theme that seems to many of us, expecially those of us whose major interest is in "underdeveloped"—super-exploited—countries, to have been floating around since the beginnings of anthropology as a discipline. These papers point up our great need for a permanent tribunal by which "irresponsibility" may be condemned, suspicious research and applications unmasked, and the principles of our scientific and humane responsibility established. Very little can be added to the papers by way of comment, for they set forth admirably the essentials of this most important theme. Perhaps for this reason, what follows will be (after the example of Berreman) passionate, trying to be at the same time, reasonable.

Problems of responsibility have faced the Mexican anthropologist from the very beginning of his education. The principles which guide the work among Indians of the Mexican government (Instituto Nacional Indigenista 1965:11–13)—an accepted prototype for such work in all of Latin America—are based on the goals that Gjessing lists for efficient colonial administration and the introduction of middle-class values (Mexican in this case, but Western in any case). This situation is contradictory to the fundamental truth, now forgotten (Swadesh 1940: 251–53), that:

There are interests which seek to foster discord and hostility among human groups in order to prevent them from uniting against common dangers. These forces have taken advantage of certain erroneous concepts and certain vague ideas about race and culture . . . To counteract these ideas, the schools . . . must teach the truth, the scientific facts, in order that no well-intentioned man be susceptible to the confusion fostered with pseudo-science by the enemies of humanity . . . The Whites must learn to appreciate the Indians, and the latter must come to have confidence in the former. This condition of mutual respect can be achieved by making both groups understand their common problems and by counteracting the prejudices of the past.

This contradiction will not surprise any Latin American or anyone who is familiar with this part of the world. If anthropology is the daughter of Western imperialism—a conclusion accepted by the three authors and certainly by any anthropologist who knows anything at all about the history of his science—the national anthropology of countries like Mexico and Peru is an expression of the fact that in reality the mentality (and the interests) of the ruling bourgeoisies in relation to the Indian groups is the same as that of imperialists in relation to their subject peoples (not to mention that the national anthropologies end up being allied with, and overlapping with, the imperialist anthropologies). The term "internal colonialism" used by some of our social scientists (e.g., Gonzalez Casanova 1963) to refer to this situation, is adequate up to that point, although it is unfortunate for the understanding of the total system of which our countries are a part in that, among other things, it implies a dualism and even a pluralism which do not exist (Frank 1966, 1967*a*, *b*).

The anthropologist in our countries sees himself as affected, also, when he notices that, as the doors to research are closing to the few anthropologists that these countries produce, they are being opened indiscriminately to foreign institutions whose research results, if they are not frankly questionable, at least leave much to be desired. One wonders to what extent their presence serves the interests of the U.S. government—its need for information on little-known regions and peoples, especially those in which future uprisings might occur, and its desire to propagate Western values of consumption. I shall not discuss this problem further here (but see Cazès 1966*a*, *b*, 1968).

Swadesh (1940:21) has said:

Science is not for the satisfaction of the individual scientist nor for the diversion of intellectuals. If society [*society*, not *governments*] supports us, it is for the benefit of society. As long as there are social problems in the world, those scientists who are capable of solving them cannot but face this responsibility . . . Nor is science for achieving personal glory. We have no right to devise new doctrines or dress up old theories in new verbiage in order to lend luster to our names . . .

Further, he has said—with reference to linguistics, but surely applicable to the rest of science (1966:9)—

. . . it is an urgent necessity to present these concepts to the general public . . . Specialized terms, technical symbols, and the other forms that professionals value are not of great importance and at times even impede communication . . .

These statements, along with those of Frank (1967*b*) on the need for an integrated and coherent view of all problems within the total social systems of which they are part, synthesize what the committed social scientist ("committed" in the sense of Berreman, citing Wolf; for it goes without saying that there is no such thing as an uncommitted social scientist) can demand of himself.

Some committed social scientists, both theoretical and practical, have (as Gough mentions) pointed out paths little traveled by those of us who have been educated under the North American school (whose maxim has been "look at the trees; the woods don't matter much, or looking at them may be dangerous"). I am talking about theories and courses of action for the holistic solution of broad social problems, theories and courses of action that have undeniably produced results even though the discussion of them is proscribed in our Western democratic publications. It is absurd, to say the least, to deny that the analysis of Russian society by Lenin, that of Chinese society by Mao Tse-tung (the results of which have recently been examined objectively and critically by Snow [1962] and Karol [1967]), and that of the problem of nationalities in the U.S.S.R. by Stalin, and the action based on these theories, have validity and have brought well-being and development to the societies to which they were applied. In these three cases, the social commitment of the authors was translated immediately into direct, revolutionary political action, the only thing that seems to offer the possibility of change in a society exploited by the democratic West. (Finland, for example, separated itself from the U.S.S.R. partly on the basis of theoretical principles which proved, in the end, not very different from those which have failed to bring liberation in the West.) The work of these thinkers and activists, whether we like it or not increasingly influential in the world, may be open to criticism and discussion; but the *action* of the committed social analyst is manifest in indisputable facts and also, though at times not clearly expressed, in the theories of Ernesto (Che) Guevara (1967) and Regis Debrey (1968). These latter theories, like those of Fanon (1963, 1965*a*, *b*), have yet to withstand the ultimate test of their validity; but in the last analysis, the social scientist who carefully examines the present situation of the societies he has traditionally studied can only end by arguing that a theory of development like that of W. W. Rostow (cited by Frank 1967*b*), supported by guns, defoliation bombs, bacteriological weapons and other genocide, can only be combated with an anti-imperialist theory that the armed benefactors can understand, one based on the right that Fray Bartolomé de las Casas (1966) ascribed to the peoples conquered by Spain in the 16th century:

. . . they have earned the right to make war on us and to wipe us off the face of the earth, and this right will remain theirs until the Day of Judgment.

Development, under Rostowian philanthropy, is in fact (as Frank [1967*b*] has masterfully shown) the development of the philanthropists themselves at the great expense of those whom it pretends to benefit. To fight it—and it is being fought, though precariously—is to make anthropological theory cease to be conservative and to provide a place for the anthropologist anywhere in the world; it is to bring about the scientific revolution that Gjessing demands; and, finally, it is to place in the hands of the "non-White" peoples among whom the anthropologist has worked the instruments of the self-determination that the message we have hitherto brought them ("middle-class Western or Westernized values are the highest achievement of mankind") has never permitted. As a result, the world will no longer be made up of, as Satre has put it (Fanon 1963), a few men and a multitude of natives.

Certain aspects of our mestizo Latin American culture make us suspicious in the extreme. To many of us, the angry protest of the A.A.A. (Beals *et al*. 1967) seems a bit naïve in that the situation with which it deals, no secret to anyone who has been facing the social problems of underdeveloped countries, seems to come as a surprise to our North American colleagues. The most astonishing thing about it is that this same Beals, together with Hoijer (1953), in listing some of the fields of anthropological activity in the U.S., expounded point by point that which was later to scandalize him. We can be thankful, nevertheless, that reason and passion have caught up with him, even though so late.

Gough's proposals for the future are unquestionably magnificent. It is obvious, and she herself implies it, that in order to realize them it would be necessary to have an anthropological "Switzerland", entirely neutral—more neutral than the actual Switzerland—whose citizens had free access to the whole world and could freely investigate all the phenomena that exist in it, constrained only by a methodology that made them integrate each social fact into the total system and by a code of ethics that kept them committed always. It is also clear, however, that this type of body of anthropologists would be very dangerous and that its commitment would soon require that it be subject to such philanthropic action as has been carried out in Guatemala, in the Bay of Pigs, and in Santo Domingo, and is today being carried out, with the help of some social scientists, in Vietnam.

The argument that science is not committed but the scientist is can be dealt with in other terms: Science *has* a commitment—that which Gough, like Swadesh, has pointed out, namely participation in the solution of social problems, amounting to a quest for well-being and for the possibilities of individual development for all of the members of human society—and it is the task of the honest scientist to seek this commitment in his own field and to act upon it.

Sol Tax (cited in INI 1965) wrote some time ago that what anthropologists want to do is awaken hope. Nothing is more difficult in the world as these three authors have described it; for it is undeniable that the hope of those groups which our action as committed scientists is designed to benefit is based on the possibility of freeing themselves from those chains that are all they have to lose. Besides, not all of us are capable of doing what Debray, a social scientist, has done; the opportunity does not always present itself, nor would it always be appropriate. For those of us who, for one reason or another, must remain idle in our studies or in the field—the idleness ever more insupportable as it becomes ever more apparent that we *ought* to be committed—there is only one way out. Ideally, we would pursue it within universites that are autonomous enough, that are not involved in military or repressive projects, and that offer enough freedom for research and teaching, rather than in private or governmental institutions, national or foreign—even though this means the reduction of our income. This is, however, not always possible, and so the responsibility of the social scientist must be to continue confronting the problem individually and to seek to make his professional societies more effective in this regard. The way out consists in investigating those problems which permit us to define most clearly the hopes and the needs of the modern world (and for 2,352,000,000 of its members these needs, as Gough has indicated, continue to be the most basic ones) and that allow us to construct a profound critique, rational and passionate, of those institutions and dominant social forms that impede the immediate satisfaction of these hopes and needs.

We will not always be able to make our critique public, especially since many of us are already potentially definable as "undesirables"; but in this also must our fight consist. CURRENT ANTHROPOLOGY, which has brought the matter into international discussion (thanks to the initiative of Frank), may be—if the political climate remains favorable to it—the forum for further discussion on the scientific level. Popular books and even the news media constitute other means, in many cases more effective, for reaching the general public, who in general have come to the same conclusions as we have but would like to hear them from "authorities" abandoning their sterile Ivory Towers. Only this, in these times, can make anthropology relevant again, can bring it back to life, can allow it to fulfil its scientific and humane commitments.

by ERIK COHEN ☆

Jerusalem, Israel. 20 v 68

Looking from a vantage point which does not lack problems of its own, namely Israel, and being well-acquainted with the intricacies facing applied sociology and anthropology, I am baffled by the black-white approach of these papers. Are the waters really so muddied that so much reiteration (as in Gjessing's paper) of basic moral postulates is necessary? Are the issues really so crystal-clear that mere commitment to mankind on the part of the anthropologist, or anybody else, can provide a way out of the immensely complex moral and political dilemmas facing the contemporary world? Somewhat unwillingly I must confess that I was not very much enlightened by these papers, though I agree with their ethical standpoint of humanism and feel a strong sympathy toward the moral integrity which they express.

To begin with, there is some terminological confusion in Gjessing's and Berreman's papers. "Value-freedom" (*Wertfreiheit*) is a term coined by Weber to cleanse *statements of social fact* from evaluative admixtures; it was never meant to imply that the statement of the problem, or the findings of research, are irrelevant to values, much less that the scientist, as a human being, should be morally uncommitted. Indeed, for Weber, value-freedom goes with value-relevance (*Wertbezogenheit*), and Weber himself was a deeply committed man. This standpoint was later distorted in the ideology of the American sociologists, as Gouldner (1964) has pointed out, and the very instrument which should have served the realization of deeply felt values—namely, effective social analysis—was thereby blunted. There is nothing wrong with "value-freedom" in research. The important point is that our research should be value-relevant.

This does not in itself mean, however, that social scientists are, by the very nature of their subject matter, namely man, committed to an ethics of humanism and to such values as "service of mankind." Humanism in itself is not a prerequisite for the scientific study of humanity and does not follow as an ethical conclusion from such study. The very strength of every moral position derives from its unconnectedness with any state of things in nature. If humanism were an implication of our scientific findings on man, it would lose its moral relevance. A finding, for example, that men are unequal in their natural gifts does not deny the moral value of the equality of men, though it might influence the ways in which this value is realized. Therefore no amount of documentation that races are in general equally endowed by nature should in any way strengthen our belief in the equality of men. If it were to do this it would actually debase the very values of humanism.

Though I myself accept, on purely ethical grounds, a commitment to humanism, I fail to see any connection between anthropology and, indeed, science as such and any type of moral commitment. Morally, the scientists are not better off than any other kind of people, and their mastery of facts does *not* necessarily lead them to deeper moral insight. Therefore, most of the statements made in the papers by Berreman and Gjessing have very little to do with the fact that we are scientists but rather derive from the moral commitment of man as an ethical being.

Thus, there is nothing exceptional about the moral position of the social scientist. He does differ from other people, however, in his ability to see the implications and costs of ethical decisions. Whereas many ethical precepts are often stated in highly abstract and vague terms, the social scientist, by following out the way in which these precepts impinge upon society, can discover how they work: what the *real* (apart from the imagined) consequences of an ethical or political decision are; what values have to be foregone in order that another may be fully realized; what is the least costly way to put some highly cherished value into practice. These, to my mind, are highly relevant problems of "applied ethics," and the study of them is one of the chief professional responsibilities of the social scientist. These three papers do not really attempt to tackle them.

Since the social scientist is not by his training endowed with the right to decide which social goals should, and which should not, be served, he cannot reasonably restrict himself to goals which agree absolutely with his own moral position. This does not necessarily mean, however, that he has to take the ideological stance of "value-freedom" and serve the powers-that-be, but rather that he must define the "range" of acceptable goals which, though none may exactly represent his personal moral commitment, still excludes those oals which seem to him clearly wrong. uch a position cannot be defined a riori; it is a matter of moral decision in ach concrete situation.

Another complication stems from the enerality of science. Its findings can be ut to different kinds of immediate and ore indirect use. The scientist who estroyed his meteorological findings ecause specialists on gas warfare became iterested in them might have prevented ie discovery at a later stage of means of efense against gas attacks made by an nemy whose scientists were less scrupu- ous. The only moral maxim for research cceptable to me is: research which nplies immoral ways or means of investi- ation is in itself immoral. The social nplications of the *findings* of research are o far-reaching that I cannot really see ow a sound judgment as to their moral gnificance can be made in the abstract; ich issues can only be decided concretely, y weighing the pros and cons of short- nd long-range implications.

To conclude, let me illustrate from my wn experience doing applied sociology nd anthropology in Israel. Gough, in her aper, touches upon the complex problem f sponsor-researcher relationship under onditions in which the sponsor and the eople studied belong to different societies. . similar problem arises in any study of nmigrants or minorities who at least iitially belong to cultures different from ie majority culture of veteran settlers. his is the situation of the Oriental nmigrants and the Arab minority in rael.

The Israeli government and other public stitutions sponsor a variety of social udies. Though I do not consider myself a ionist, I agreed to study problems con- ected with the absorption of immigrants; ich problems are within my "range of cceptable goals." In the conclusions and commendations of my studies, however, felt free to point out the "social costs" f a policy of absorption, costs which the onsors had not been aware of, or con- erned about, before. Through this kind f suggestion, the work of sociologists and ther professionals can be a subtle political iechanism which serves in the long run to roaden the approach of the sponsoring istitutions and modify their goals—in iis case, for example, perhaps causing iem to become better acquainted with ie folklore and original traditions of the arious groups of immigrants and thus be iore careful not to destroy them by an idiscriminate policy of mass absorption.

In Israel, as in other countries, use is ometimes made of social research for the irtherance of vested political interests. or example, by quoting selectively critical search reports, sponsoring institutions e prone to assert that their policy has een shown successful by scientific in- estigations. So as not to be used for party-political purposes, I decided of late to withdraw from directly sponsored studies and secure funds which would enable me to conduct studies independent of policy-making institutions. Such a decision has its drawbacks, however; for if all social scientists were to do this it would deprive the policy-maker of professional advice and thus actually increase the "social costs" of his policy.

Because most of the funds in Israel are dominated by institutions interested in Jewish problems, there has been until recently very little interest in the problems of the Israeli Arabs. I would have refused to do studies on Arabs which were intended to further Jewish national goals at the expense of the Arab minority; this would be outside the "range of acceptable goals." I did, however, agree after the recent war, to carry out a government-sponsored study in the occupied territory, my reason being that such a study might help to facilitate the administration of these territories and, particularly, to supply information which would enable Jews to better understand Arab society and thus serve mutual rapprochement in the long run.

I have cited these examples at some length because I think they represent common ethical dilemmas in applied research; the difficulty in these cases is how to decide when a committed social scientist is really "serving humanity."

by ROBERT CRESSWELL ☆

Paris, France. 22 v 68

To open this brief comment on the three articles concerning social responsibility of anthropologists by Kathleen Gough, Gerald Berreman, and Gutorm Gjessing, I quite agree with the specific theme of the articles that anthropology must become relevant to the changing world and anthropologists involved in this world, but I feel that one aspect of the problem has not been sufficiently stressed: the *value*, for society, of the conclusions of the committed social scientist.

I would agree with Gough that anthropology should become problem-oriented—one might almost add, as part of the process of becoming a full-fledged science. I might, perhaps, hesitate about placing students in a separate category; I write this from Paris in the midst of the crisis provoked by students assuming in full their social responsibilities. The equally arbitrary division between pure and applied research should also be abolished, for it is not only scientifically untenable (even problem-oriented research can have theoretical implications), but also throws up a smoke-screen around the problem of the social responsibility of social scientists. It is easy to eschew commitment by arguing that "pure" research is the focal point of any science; and, since many anthropologists unconsciously seek foreign lands and simple peoples because they are unable or unwilling to adapt to their own societies and cultures, relegating "applied" research to the status of some kind of minor technical spin-off is a convenient stairway to the Ivory Tower.

Gjessing's article seems to me particularly timely as to the relevance of anthropology and the relationship between the degree of relevance and social responsibility. I would like to point out, however, that even if the subject matter of anthropological research should become largely irrelevant to the modern world, the basic method of this research—inductive fieldwork—would remain relevant. This characteristic of anthropology makes sociology and anthropology complementary and indispensable to each other, as I have suggested elsewhere (Cresswell 1967). Gjessing raises another aspect of this complementarity in speaking of the oscillation between the hypothetical-deductive and the empirical-inductive.

In an idealized and very simplified summary of scientific process, one moves from the collection of facts to their analysis, and then to the interpretation of the analyses. At this point two possibilities are offered: The scientist moves on to the formulation of hypotheses and then back to the field or the laboratory experiment to verify the predictions implicit in his hypotheses. The planner, or the politician (or the business man), chooses a course of action. The choice is, of course, governed by the set of values operative in the society in which the choice is made.

Now, as far as an action such as the Vietnamese war is concerned, there would seem to be no problem. The anthropologist who is also American (such as myself) has a double duty with reference to American involvement in this war: first, as a citizen, to protest against the atrocious obliteration of another country by his own government, and secondly, as an anthropologist, to point out that the fundamental values and goals of his culture are being subverted by the means used to attain them, and this on the basis of a false analysis of the nature of present-day revolutionary or socialistic movements.

When, however, the commitment precedes research, or when it is a question of research clarifying the factors involved in a decision to be made affecting a segment of society, the social scientist finds himself in the uncomfortable position of being able to influence the object of his research by the goals he has set up. A physicist can fulfil his sense of social responsibility by speaking out, for instance, against the use that politicians and the military make of the atom bomb he has made possible, without affecting the laws describing the behavior of atoms and molecules. A social scientist, by taking part, as he must, in the decisions of the society of which he is a member, has probably only a negligible effect on facts, but can have a considerable influence on the analysis of those facts; and it can be argued that were he not committed, he might construct a more complete structural analysis. It could also be said that such a social scientist is setting himself up as prosecutor and judge, and of using his value judgment of a course of action—which is his prerogative as a citizen—to determine the scientific arguments he will use to persuade his fellow citizens—which is not.

There does not seem to be a ready answer to this problem. Perhaps the social responsibility of the social scientist is to point out, as clearly as he is able, and after having made as explicit as possible his own psychological and philosophical profile, the different choices of action open to the society in which he lives, the probable effects of the possible decisions, and the relative integration of each course of action with the set of fundamental sociocultural values.

by ANDRE GUNDER FRANK ☆

Montreal, Canada. 24 v 68

Che Guevara, upon being asked what, as a writer, one could do for the Revolution, answered that he used to be a doctor. The issue is not whether medicine, or anthropology, is less useful or relevant than other fields of human endeavor. The issue is the responsibility of the anthropolog*ist*. His responsibility is to use anthropology only as far as it is sufficient, while doing whatever is necessary to replace the nearly world-wide violent, exploitative, racist, alienative capitalist class system which embraces most anthropologists and the people they study. Appeals to truth (Berreman) and for a humanistic approach (Gjessing) are insufficient liberal critiques of the liberal support which most anthropologists give and the benefits they derive from the system they serve. Anthropologists, more than anybody, can be expected to know that values, mythology, science, and other facets of culture are intimately related to the structure of the society—even if many anthropologists like to observe this fact only among other peoples. Berreman and Gjessing, who devote most of their essays to negating the possibility of doing value-free anthropology, would therefore seem to be beating an anthropologically dead horse.

Suggestions that anthropologists abandon the integrity of their discipline (Gjessing) to overcome the limitations of specialization and individual fieldwork (Gough), though perhaps necessary, are also far from sufficient. Gjessing goes on to claim that economists, political scientists, and sociologists have largely replaced anthropologists and that American anthropology is now closer to reality than its European counterpart. Insofar as this is true, however, it does not mean that Gjessing's proposal to do interdisciplinary work in their footsteps offers any solution; for these liberal social "scientists" and their techniques now simply serve American imperialism better and more efficiently than the perhaps more outmoded children of an earlier imperialism (Gough and Gjessing). Thus, in his introduction to the Social Science Research Council-sponsored *Social Science Research on Latin America*, the anthropologist Charles Wagley (1964:3) observes that in the United States for the last three decades

> Latin America has also been neglected by our scholars who in the end must provide the basic data for academic and public consumption. As much as Africa, Latin America has been in many ways a "dark continent." This situation is now changing. There is a new public interest in Latin America stimulated by a realization of its importance to our own national interests. The National Defense Education Act supports the study of Spanish and Portuguese and of Latin American society. The Alliance for Progress has . . . dramatized the importance of the region to us. Private foundations have supported research on the study of Latin America. . . .

The same can surely be observed in the sudden spurt of African studies in the United States, which must be traced less to the Africans' increasing "independence" from Europe than to their growing dependence, sponsored by American imperialism, on the United States. The participation of anthropology, not to speak of applied anthropology, in this transfer of the "white man's burden" across the Atlantic is evident, and its scientific and political results are predictable.

Project Camelot was not an isolated event, and the widespread hue and cry against direct Defense Department employment of social scientists rather misses the point that virtually the whole of the "free" world's social science is in effect one huge imperialist Camelot project, whoever pays for it. Politically naïve, though not so innocent, liberal social scientists may not be aware of why their research is financed and how their results are used; but, as William Domhoff (1967) shows in his *Who Rules America?*, the uses and abuses of social science and scientists are well known to the bourgeois upper-class trustees of the suitably named Carnegie, Rockefeller, and Ford foundations, *et al.*, and of the major American universities (who liberally exchange their presidents [Rusk], deans [Bundy], and finances with each other and the U.S. Department of State). No less uninterested in a "free"-worldwide Camelot project is the Panel of the Defense Science Board of the National Academy of Sciences of the United States, which cogently observes (Defense 1967:33, 38, 40–43, 52):

> In recent years the Department of Defense (DoD) has been confronted with many problems which require support from the behavioral and social sciences. . . . The Armed Forces are no longer engaged solely in warfare. Their missions now include pacification, assistance, "the battle of ideas," etc. All of these missions require an understanding of the urban and rural populations with which our military personnel come in contact—in the new "peacefare" activities or in combat. For many countries throughout the world, we need more knowledge about their beliefs, values, and motivations; their political, religious, and economic organizations; and the impact of various changes or innovations upon their socio-cultural patterns . . . [Innovating in] conventional social science methodology . . . is one of the happy cases in which there is substantial overlap in the interests of both DoD and the academic community producing the research. . . . [We] believe that DoD has been singularly successful in enlisting the interest and services of an eminent group of behavioral scientists in most of the areas relevant to it. . . . On the other hand, the DoD could probably make improvements by assuming more responsibility for stating its needs in terms which are meaningful to the investigator rather than the military. To ask people to do research in "counter-insurgency," "guerrilla warfare," etc., not only produces a less than enthusiastic reaction but also provides no basis for insight into the ways in which they might contribute. . . . The behavioral science community must be made to accept responsibility for recruiting of DoD research managers. . . . The following items are elements that merit consideration as factors in research strategy for military agencies. *Priority Ordered Research Undertakings* 1. . . . methods, theories and training in the social and behavioral sciences in foreign countries. . . . 2. . . . programs that train foreign social scientists . . . 3. . . . social science research to be conducted by independent indigenous scientists . . . 4. . . . Social science tasks to be conducted by major U.S. graduate studies centers in foreign areas . . . 7. . . . studies based in the U.S. that exploit data collected by overseas investigators supported by non-defense agencies. The development of data, resources and analytical methods should be pressed so that data collected for special purposes can be utilized for many additional purposes . . . 8. . . . Collaborate with other programs in the U.S. and abroad that will provide continuing access of Department of Defense personnel to the academic and intellectual resources of the free world . . .

Similarly aware are the nearly 500 intellectuals from 70 countries who unanimously proclaimed in the "Appeal of Havana" (*Proceedings of The Cultural Congress* 1968) to all intellectuals of the world:

> . . . we recognize that this enterprise of domination assumes the most diverse forms, from the most brutal to the most insidious, and that it operates at all levels: political, military, economic, racial, ideological and cultural; and we also recognize that this undertaking is carried on with enormous

inancial resources and with the help of ›ropaganda agencies disguised as cultural nstitutions.

Imperialism seeks, by the most varied .echniques of indoctrination, to ensure social :onformity and political passivity. At the same time, a systematic effort is made to mobilize technicians, men of science and intellectuals generally in the service of capitalistic and neocolonialist interests and purposes. Thus, talents and skills which could and hould contribute to the task of progress and iberation become, instead, instruments for he commercialization of values, the degradaion of culture and the maintenance of the :apitalist economic and social order.

It is the fundamental interest and the mperative duty of intellectuals to resist this iggression and to take up, without delay, the hallenge thus posed to them. What is required of them is support for the struggles for national liberation, social emancipation and :ultural decolonisation for all the people of Asia, Africa and Latin America, and for the struggle against imperialism waged in its very :entre by an ever greater number of black and white citizens of the United States; and to enter the political struggle against conservative, retrograde and racist forces, to demystify the latter's ideologies and to attack the structure upon which these rest and the nterests they serve. . . .

This commitment must begin with an unqualified rejection of the policy of cultural subjection of the United States, and this implies the refusal of all invitations, scholarships, employment, and participation in programmes of cultural work and research, where their acceptance could entail collaboration with this policy.

West European and North American anthropologists can best fulfil this responsibility by working in their own societies. First, their work abroad serves the interests of imperialism rather than the interests of the colonialized peoples. According to Gjessing (quoting Myrdal), not autonomous reorientation, but mighty political changes redirect scientific work; and contemporary political changes are not redirecting metropolitan anthropologists or other scientists to work in the interest of the colonized people, except insofar as they bring these scientists to work toward the destruction of imperialism in the metropolis and thereby toward the liberation of the people everywhere. A second reason for working at home is that, on the other hand, the imperialist metropolis, is witnessing mighty political changes which can direct some anthropologists into responsible work at home—as participants in the liberation movement there.

For those North American anthropologists and others who would take this responsibility seriously, Barbara and Alan Haber (1967:95–96) have summarized its implications:

1. The movement must be seen as a utility—which helps us define what we do and without which our work loses political relevance . . . If our personal aspirations or professional work precludes our doing things that are safe or respectable, then we are kidding ourselves about our politics. 2. High status and respect and rewards in the professional establishment are foreclosed. We must expect job instability, the likelihood of getting fired periodically, the danger of increasing difficulty in finding jobs. 3. A radical cannot see his loyalty as being to the profession, or institution in which he works. Our loyalty is to our political comrades and to the political aims for which we are organizing . . . Obviously this presents a moral difficulty because others will assume we have traditional loyalties . . . We are not intellectuals above it all who say the truth to whomever will listen or asks: we are *partisans*. . . . 4. Radicals cannot accept without reservation the code of ethics and responsibility of their professions. Ethics are not abstract ideals. They are sanctifications of certain types of social relations, purposes and loyalties (which is no news to anthropologists so long as the reference is to other peoples' ethics rather than their own). Conventional ethics entrap us into support of things which we do not support politically and into loyalties which conflict with our own values and politics. . . .

North American and West European anthropologists who recognize these facts about their society and who are prepared to accept the concomitant responsibility can and must draw on their special skills to serve the movement in two ways:

1) By analyzing the shoddiness of the emperor's social scientific clothes, these anthropologists should, like Gough, expose imperialism in its ideological nakedness and denounce those of their colleagues who continue to enjoy the physical comforts that their pseudo-scientific suit affords them. Among other thing this involves showing, contrary to Gjessing, that the theoretical and political limitation of Firth and his followers is not that their theory of social organization only deals with changes generated inside rather than outside the social structure; it means showing, like Gough, that the real limitation of imperialist-fathered anthropological theory and theorists is that they arbitrarily define villages or tribes as social systems and invent theoretical categories like "folk" to hide the naked truth of economic exploitation and cultural alienation of "my" people by the real determinant social system, which is imperialism; and, further, that their theory is, naïvely or intentionally, restricted to analyzing social change *in*, but not *of*, the imperialist and capitalist system. If, as some anthropologists would have it, the social—including economic and political—structure really determines culture and ideology, then the socially responsible anthropologist can go on to analyze why most of his colleagues prefer studying change in their own society to promoting change of it.

2) They should pursue the research and develop the theory required and requested by the liberation movement at home. If their fieldwork techniques are any good, then let them use them in community studies to analyze the social structure of their own society for a political movement that promotes the necessary social change of that society. This task offers a host of research problems not only in "the other America" but also in the bourgeois anthropologist's own America and Europe. The politically committed and active anthropologist, like the guerrilla doctor who treats his wounded comrades, can also draw on his expertise to help the liberation movement at home by doing specific social research jobs for his comrades, rather than for publication. The anthropologist becomes a real partisan—an intellectual revolutionary rather than a revolutionary intellectual. Many metropolitan anthropologists, disregarding mighty political changes, will of course not redirect their work, but will persist with their fieldwork abroad. They might suitably take a cue from de Tocqueville, who 130 years ago observed that the true nature of the metropolis is best seen from the perspective of its colonies.

Anthropologists from the economically, politically, and culturally colonized—and therefore underdeveloped—countries must also work at home for the same reasons. They may be assured that as long as imperialism persists, metropolitan social science, including anthropology, will never analyze their societies or the imperialist system for them. Still less will it develop the (underdeveloped countries') problem-oriented anthropology Gjessing asks for. If world view is at all derived from social structure, then only the formerly or still colonialized peoples of the underdeveloped world and the internally colonialized Afro-Americans are likely to find the necessary perspective. For those who would truly seek that perspective, the way has been pointed out by the apostle and practicing anthropologist of the damned of the earth, Frantz Fanon (1966:122–23):

Now, precisely, it would seem that the historical vocation of an authentic national middle-class [mistranslation of "bourgeoisie" from the French original] in an under-developed country is to repudiate its own nature in so far as it is bourgeois, that is to say that in so far as it is the tool of capitalism, and to make itself the willing slave of that revolutionary capital which is the people. In an under-developed country an authentic national middle-class [bourgeoisie] ought to consider as its bounden duty to betray the calling fate has marked out for it, and to put itself to school with the people: in other words to put at the people's disposal the intellectual and technical capital that it has snatched when going through the colonial universities. But unhappily we shall see that very often the national middle-class [bourgeoisie] does not follow this heroic, positive, fruitful and just path; rather, it disappears with its soul set at peace into the shocking ways—shocking because anti-national—of a traditional bourgeoisie, of a bourgeoisie which is stupidly, contemptibly, cynically bourgeois.

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In Asia, Africa, and Latin America responsible anthropologists must be moved by the mighty political changes in these continents; they must become conscious of the responsibility of the intellectual as defined in the Appeal of Havana; and they must fortify themselves by the moral commitments demanded by Haber and Fanon. Then, rather than metropolitan anthropologists, however responsible or committed they may be, it is the anthropologists and others from the underdeveloped countries who will be most likely to build the theoretical framework asked for by Gjessing, in which change and stability are complementary factors. Among other research problems, this involves analyzing how class structure and indeed culture and personality in Asia, Africa, Latin America, and also North America are formed and deformed by the world capitalist colonial, neo-colonial, and internal-colonial structure. These same anthropologists from the underdeveloped rather than from the developed countries must also become partisans and activists in the liberation movements of their own countries and begin to work on the many facets of a "research project devoted to the problem of how poorly armed guerrillas might more effectively resist a brutal and devastating military technology" (mentioned by Berreman, citing Chomsky). This in turn involves, among other things, studying how the colonial and class structure and their contemporary transformation generates not only counter-insurgency but insurgency as well, and thanks to what grievances which sectors of the population can at particular times and places be politically and militarily mobilized in the long war to destroy the violent, exploitative, racist, and alienating capitalist system and in the liberated areas to build a truly free and humane society.

This endeavour requires more than the simple study of anthropological medicine. It calls for the practice of that medicine, following the example of Che Guevara and thousands like him, also including some anthropologists, in Vietnam and elsewhere. Then the counter-insurgency formula of ten anthropologists for each guerrilla (cited by Berreman) will surely have to yield to a victorious popular insurgency formula of 10,000 guerrillas with each anthropologist worthy of the name.

by John Gulick☆

Chapel Hill, N.C., U.S.A. 20 v 68

I agree with Gjessing and Gough that anthropology is in danger of becoming irrelevant to mankind's struggles to understand and cope with its major, largely self-imposed, problems, such as war, overpopulation, social oppression, and environmental ruination.

Gjessing says that the way to counteract this danger is for anthropology to be more responsible to society. This sounds good in the abstract, but what does it really mean? Consider those anthropologists, mentioned by Berreman, who are working professionally for the United States government for the primary purpose of facilitating the militaristic policies of the United States in Southeast Asia. They could quite logically claim that they are being professionally responsible to society; yet to Berreman, and to a large number of American colleagues including myself, they are "Establishment" anthropologists, and the use of this label is intensely pejorative. I doubt, however, that an equally large number of anthropologists would want to apply the label also to Benedict, Kluckhohn, and the many others whose professional work facilitated American militarism in World War II (it was this type of work to which Kluckhohn was referring when he used to protest—perhaps too much—that anthropology was *not* "aloof and preoccupied"); though the situations are structurally the same. Such involvements must come to terms, professionally, with political goals and the values surrounding them. So troublesome can this coming to terms become that many, of course, want to keep anthropology purely academic and therefore "irrelevant." On the other hand, I question whether the type of involvement that Berreman advocates can be considered "professional." There is nothing "anthropological" about the American Anthropological Association's resolution against genocide; and Chomsky's eloquent condemnation of American actions in Vietnam is an expression of his feelings of outrage, not the result of any application of linguistics to the problem. In both these cases, the *profession* remains "irrelevant," even though the anthropologist as a *person* is involved.

Gjessing says that anthropology can become responsible to society by teaching everyone the basic precepts of culture. Good idea, but we cannot teach these precepts until we understand what they are. Ironically, despite all the theorizing on the subject, the lack of a clear definition of the precepts of culture is itself a barrier to the discipline's increasing its "relevance." For example, the "people of poverty" in the United States are variously said to (1) need and want to be helped to participate fully in "middle-class culture," not having any distinct culture of their own (Liebow 1967:223); (2) have a distinct subculture with its own positive values combined with "substandard" traits (Lewis 1966:xlv–lii); and (3) have a culture of their own, which is antithetical to that of the middle class and should neither be denigrated nor interfered with. There is here a direct contradiction between items 1 and 3 on the very structure of culture itself, and therefore there would be direct contradiction in the policies which a "relevant" anthropology would recommend. The hatred of middle-class culture often made explicit by those who hold the third view is, in turn, in direct contradiction to the precept of cultural relativity, which, in effect, grants to every culture the right to be accepted dispassionately as somebody's way of life rather than being dismissed in stereotypic terms. In this case, intense emotional bias violates the precept among anthropologists themselves. Another culture which appears to be hated by many American intellectuals, including anthropologists, is that of the Arabs; many of the liberals who denounce the American dropping of napalm on Vietnamese villagers are not in the least concerned about its being dropped on Arab villagers by Israel. In this case, political bias violates the precept. The amnesty of cultural relativism cannot be made "relevant" to others by professional anthropologists as long as it is so inconsistently conceived by anthropologists themselves.

The current fad of ethology, with its suggestions that cultural choices may be relatively less important than mechanistic imprints, is further evidence of anthropology's still-inadequate conceptualization of human nature and of the prematurity of any serious efforts to make the precepts of culture relevant to public policy. Particularly serious, as far as the interests of Berreman and Gough are concerned, is the prevailing tendency for many anthropologists and other behavioral scientists to make dogmatic assertions about innate human aggressiveness and innate male and female traits, with apparently no regard for the fact that their evidence is culture-bound and inconclusive.

I think that Berreman, Gjessing, and Gough would all encourage, as I would, the involvement of anthropologists *as individuals* in the non-academic problems of our time. I also think, however, that none of them is clear on the important distinction which exists between that sort of involvement and the making relevant of the principles of anthropology as a discipline; and it is the latter, only, which can be thought of as "responsible anthropology."

by T. Kawabata☆

Tokyo, Japan. 22 v 68

These articles seem to me to reflect a conscientious approach to the anthropologist's work. The views the authors express with reference to their own countries are equally relevant to Japan. Japanese newspapers and magazines take up the problem of the war in Vietnam almost every day; but the writers are almost all journalists and politicians, not anthropologists. Of the problem of Okinawa, a matter of primary concern for us, the same is true; research on Okinawa is being carried out by Japanese folklorists and ethnologists, but for some unknown reason they do not often join in the plea for the return of Okinawa. Some of our young folklorists complain that their research is of very little service to the people of the impoverished agricultural villages they study. I believe that this complaint, like the views presented above, is also a voice of conscience.

by LEO S. KLEJN☆

Leningrad, U.S.S.R. 15 v 68

I have read the articles of Berreman and Gjessing, brilliant and noble in spirit, with great sympathy and interest. (Gough's article did not reach me in time for comment.) The problem of the social responsibility of the social scientist is here raised straightforwardly, courageously, and at a good time. Many of the propositions would, I think, be supported by every Soviet scientist and by every Marxist. Both articles expound the thesis that a neutral science is an illusion, cultivated because of its advantages for the ruling circles of capitalist society. This thesis arises from the very essence of Marx's teachings, was repeatedly stated by Marx and Engels in general terms, and was applied specifically to science by Lenin in his discussion of the party affiliation (party spirit) of science. It would not have been so delayed a discovery in America if scientists there had understood Marxism earlier and more profoundly. Of course, better late than never; but I doubt that it is better to learn from the bitter and dangerous errors of one's own society than from the generalized experience of the whole of mankind. Both Berreman and Gjessing insist on the necessity for the social scientists to take a clear and active social position. Berreman quotes Winetrout: "It is not enough to understand the world; one must seek to change it." He might also have quoted Marx (1948: 385) first-hand: "Philosophers have merely explained the world in different ways, but the main point is to change it."[1]

Both Berreman and Gjessing believe that the obligation of the social scientist is to contribute to changing the world for the benefit of mankind: to fight for the victory of humanity, to defend the oppressed, to condemn and deny the oppressor. I share with them this belief, but I feel that the means by which they would achieve these ends are disputable. Berreman thinks the main way is through the constant effort of social scientists to discover and disseminate the truth. In this rule he follows Mills (1964*b*:611), who said of the social scientist: "his politics, in the first instance, are the politics of truth." To say this, however, is to oversimplify the question—to return to the old idea of the neutral, disinterested, above-class position of the scientist. Truth is one only as long as one deals merely with the simple statement of the facts. As soon as one turns to values and to the utilization of the facts, the single truth disappears; each social class has its own truth. The social scientist must look at the evidence from the point of view of a certain social group (class), that is, he must take a certain social position, choose certain politics. The crucial question is: which social group will he place his science in the service of—that which fights for the benefit of the whole of mankind, or that which pursues only its own ends? In this sense (but only in this one), one may conclude, not that truth is the social scientist's politics, but rather that politics are the social scientist's truth.

Berreman himself comes close to this notion with his suggestion that the leaders of the society must lend an attentive ear to the voice of scientific truth and scientists must have the right to control the utilization of their results. It is obvious that the nature of the scientists' control would be determined by the scientists' political convictions. Teller and Oppenheimer would be very different controllers, as would the anthropologist in Thailand cited by Berreman in the epigraph and Berreman himself.

Another side of the problem is overlooked by Berreman: the scientific truth is not always easy to discover, and since the views of scientists are in many respects formed by their social position, they must often quite differently perceive the same evidence, not to speak of values. Meanwhile, to insist on and disseminate the truth, one must be convinced that it and it alone is the truth; but what is the proof of this? Precisely this aspect of the problem is minutely discussed by Gjessing. He presents the idea of self-searching with the intention of neutralizing one's unconscious preconceived ideas. He assumes that when one knows what social forces have formed his attitude, he knows *eo ipso* what part of the attitude constitutes preconceived ideas and so can be watchful for them in examining the evidence. Here, however, one risks throwing out an idea that, although conformable to social interests, has indeed arisen from facts and is their adequate reflection. Furthermore, the methodological elements of this general attitude must certainly hamper and prevent proper perception. To follow Gjessing's recipe would foster melancholic indecision, diffidence, and a disastrous skepticism. To continue Russell's animal behavior metaphor, in addition to the fussy animal studied by the Americans and the profound animal studied by the Germans, we might observe an animal shifting from one foot to the other, unable to decide which way to move—would this be the animal studied by the Norwegians?

It seems to me that one must expose the social bases, not only of one's own scientific position, but of all the concepts and theories in this branch of social science. The main goal of this should be not to know one's own weak points (and why merely the weak ones and not both the weak and the strong?), but rather to make our choice easier by pointing out the most progressive scientific position, the most fruitful methodological conception, and the most correct theory. The Marxists believe that the most progressive scientific position (that of the class which is most interested in a realistic view), that the most fruitful methodological conception is dialectical materialism; and that the most correct theory is that of the sequence of socio-economic forms, socialism, and the social revolution. Lenin (1958:419) wrote of the superiority of Marxist materialism to bourgeois objectivism as follows:

> . . . the materialist, on the one hand, is more consistent than the objectivist and conducts his own objectivism more deeply, more perfectly. He does not confine himself to the statement of the necessity of a process, but clarifies exactly which socioeconomic form it is that gives content to the process, exactly which class determines the necessity . . . On the other hand, materialism includes, so to speak, a party spirit, and it places upon us the obligation to stand up frankly and openly for the point of view of a certain social group in every appraisal of an event.[2]

What, then, is the social basis of the objectivity of science? It is the position of a social group that is not afraid of a realistic estimation of the state of affairs and can therefore support objectively in science. Its truth is *the* truth. The concern for strict adherence to the rules of research is a consequence and an indicator of such a position. Elaborating a system of such rules and sticking to it is a guarantee of the successful overcoming of preconceived ideas, one's own and those of others, conscious and unconscious. Proof based on facts and demonstrated in action is the criterion of truth.

Although narrowness in theme is natural in the passionate defense of

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[1] «Filosofy lish' razlichnym obrazom ob"yasnyali-mir, no delo zaklyuchaetsya v tom, chtoby izmenit' yego.»

[2] «Matyerialist, s odnoy storony, posledovatyeln'eye ob"yektivista i glubzhe, polnyeye provodit svoy ob"yektivizm. On nye ogranichivayetsya ukazaniyem na nyeobkhodimost' processa, a vyyasnyayet, kakaya imyenno obshyestuyenno-ekonomicheskaya formaciya dayot sodyerzhaniye etomu processu, kakoy imyenno klass opryedyelyayet etu nyeobkhodimost' . . . S drugoy storony, matyerializm vklyuchayet v syebya, tak skazat', partiynost', obyazyvaya pri vsyakoy ocenkye sobytiya pryamo i otkryto stanovitsya na tochku zryeniya opryedye-lyonnoy obshyestuyennoy gruppy.»

an idea, it has led in these articles to a certain one-sidedness of treatment. One gets the impression that social factors alone have determined the history of social science; this is, of course, not the case. Mortillet's and Montelius' evolutionist theories and the rest of 19th-century evolutionism did have a social base in the optimistic perception of the world of the bourgeois class, which had recently come to power. (I cannot accept Gjessing's linking of evolutionism with colonialism.) These ideas also had a general gnosiologic base in the feeling that the whole world was changing, a feeling related to the great acceleration of the tempo of development in the period of the Industrial Revolution. The evolutionist theories in archaeology were also based, however, on the accumulation of scientific evidence—and the theories built on this evidence—the study of sites and their stratigraphy, the discovery of the Palaeolithic, the creation of the "three ages" system, etc. Thus they were grounded in the whole previous and on-going development of the science.

The anti-evolutionism of Breuil, Obermeier, *et al.* arose with the general fear among the bourgeoisie of the major capitalist countries of the proletarian revolutions and their consequent adoption of a conservative position. Since such questions affect religious dogma, one would expect a particularly active counter-offensive from the camp of the Catholic ideology—and, indeed, studies taking the anti-evolutionist viewpoint flourished precisely in the main Catholic countries: France, Spain, and Austria. Here again, this flourishing required previous preparation: the detailed classification of flint implements by Mortillet; the discovery of the cave art; and the intensive field research which yielded assemblages that did not fit into the evolutionist scheme. Italy, although a Catholic country, lacked such a factual base and has remained marginal to the anti-evolutionist camp.

Diffusionism is treated by Daniel (1962: 82) as an "extraordinary intellectual error"; but the social basis for it can readily be found in the analogies usually made by the diffusionists in illustrating their conception of the dissemination of culture: Britons in Fiji (Rivers 1913:478–79), Britons in Egypt (Smith 1913:540–41). Indeed, it is probably not accidental that such a conception should arise and reach its highest development precisely in Britain, the greatest colonial empire. We must remember also, however, the great excavations conducted by the British archaeologists in many parts of the world, their participation in the discoveries of the most ancient civilizations—the factual base without which it would have been impossible to build diffusionist schemes. On the other hand, while the Swedes and the Americans have also conducted excavations in remote lands, and the Germans and French have participated in the discovery of the most ancient civilizations, none of them was led to an intensive elaboration of diffusionism, because among them the particular social stimuli were absent or relatively weak.

The whole of the rationalist migrationism of Kossinna *et al.* was directed toward the discovery of prehistoric ethnic boundaries which might justify the Indo-Germans' recent territorial claims. Its relation to *Geopolitik* is evident. It is highly significant that such a conception should arise precisely in the intermediate period between old-fashioned capitalism and imperialism and precisely in Germany, a large imperialist country which had arrived too late on the scene to get its share of territory and had therefore been especially aggressive. Once again, *die Siedlungsarchäologie* required a background: the idea that culture is an emanation of a *Volksgeist*; research on the human races; and Montelius' typological method (for discovering genetic ties). *Die Siedlungsarchäologie* in turn produced the cartographic method, which was later to be of use to the ecologist school of Crawford, Fox, and Clark in Britain and to the "geographic-archaeological" school in West Germany.

The skepticism now spreading more and more widely in archaeology (Daniel, Piggott, Wahle, de Laet, Leroi-Gourhan, the American taxonomists, *et al.*) is associated with the denial by the bourgeois ideologists of our times that there are laws of history which may be exposed and studied (this being an epoch in which these laws work against the bourgeoisie). In considering the general unconscious preconceived ideas of some Western intellectuals (including social scientists)—i.e., the ideas of Gjessing's first level of human motivations—we must understand first of all the following: they cannot reconcile themselves to the fact that such odious laws are the truth! It is striking that this conception should be developed especially intensively in those countries (U.S.A., Britain, West Germany, Belgium, and, to some extent, France) whose ruling circles most strongly resist the powers of socialism. Archaeological skepticism, like cultural relativism in ethnography (ethnology, cultural anthropology), is only a part and a reflection of the tendency of all Western social science toward agnosticism and indeterminism. It has found a serious philosophical basis in positivism and the teachings of Rickert. We must also accept, however, the gnosiologic grounds of this new attitude—the enormous complexity of the subject that has become apparent in the past decades and the enormous acceleration in the rates of increase of our knowledge, making the results already achieved soon out of date and shaking our faith in new results. This trend, in its turn, has engendered among social scientists special hopes for the methods of natural science and stimulated the development of these methods (at the expense of humanitarian ones); it has raised the question of the criteria of proof, and this has become a stimulus to the application of statistics. The elaboration of these methods is important not only for those who argue that historical laws cannot be demonstrated, but also for those who believe the opposite. It is one more step up the long stairway of cognition, and it will serve as a base for the next step. Of course, this is no excuse for hyper-skepticism (which, in my opinion, generally hampers the development of science). My point is that there are regularities, logic, and reason in the history of social science, that this history is not *simply* a reflection of the social and political changes in society.

I have not mentioned in this sequence the Marxist theory of archaeology, largely because I have not intended to give here a full history of archaeology. Furthermore, the Marxist archaeologists do not deny their views to be socially determined. This is, so to speak, too clear a case. The statements of the classics of Marxism on the philosophical and scientific forerunners and sources of Marxism are well known, as also are the genetic ties of Marxist archaeology and 19th-century evolutionism and the intention of Marxist archaeologists to assimilate and to take into account in their conception all the achievements of modern archaeology.

Of course, I agree completely with Gjessing when he holds that social anthropologists should approach the problems of the "rising nations of the non-Western world" with a view to satisfying the requirements and interests of these nations themselves. I am worried, however, about the implication that we should reduce the aims of social anthropology to such narrowly pragmatic ones. Does this not turn out to be the same as Malinowski's functionalism, but inside out—the same pragmatism, merely diverted from the British colonial administration to the local national interests? Of course, it would be much better than to have the "ten anthropologists" continue the job of the "ten troops"; but I think that to limit thus the aims of our science would benefit neither the science nor the rising nations. General theoretical research is important; we need to know the laws of social life, the lessons of social history. This need was aptly expressed by an African, Dr. Hastings Banda, who in 1963 asked that Stonehenge be moved to Nyasaland so that the Africans could see that the Britons also had once been savages. As to the pragmatic formula used as an epigraph by Berreman, I think that the course of history will soon bring us to another solution of the problem: one leader will be forced to withdraw from the other's land both the troops and the anthropologists. This will be to the benefit of all—the leader, the guerrillas, both countries, all the other countries, the troops, and the anthropologists—and to the normal development of social science as well.

by DAVID LEVINE☆

Lincoln, Nebraska, U.S.A. 9 v 68

These three papers demonstrate that the relationship between the social sciences and society is beset by a host of complex ethical, practical, and scientific dilemmas. The sincerity and passion of the papers is obvious, however, and much of what they contain is unarguable: I endorse Kathleen Gough's recommendation for international studies of revolutionary movements done "... without built-in biases ..."; Gjessing's emphasis on a "conscious commitment to ... serve humanity ... with the increased sensitivity to unconscious sociocultural bias that this commitment may produce ..."; and Berreman's view that "scientists are people ... they cannot escape values in the choices they make nor in the effects of their acts." Nevertheless, the papers raise issues which require further consideration; the authors seem to have reached certain conclusions which remain open to debate.

One issue, for example, is raised by Nadel's suggestion (quoted favorably by Gjessing) that "... some form of psychological testing seems imperative ... for all prospective fieldworkers ...". It is not clear whether Gjessing *fully* endorses this recommendation, but he does write that "it would be a significant step forward simply to state clearly one's own sociopolitical position—the extent to which one's general attitude toward the problems of life conforms to, or deviates from, the values of one's own civilization." I have discussed elsewhere (Levine 1968) the vital importance of specifying in advance the purpose of a psychological examination. What purpose does Nadel (or Gjessing) think would be served by these psychological tests? There is the implication that, done voluntarily, they would assist the anthropologist in learning what his own "preconceived ideas" about a scientific problem might be. Current research suggests that psychological tests are still a long way from being able to make a significant contribution to such a goal, despite claims to the contrary. Such a use of psychological tests, though probably a waste of time, presents no ethical problem.

Is it possible, though, that Gjessing is suggesting that psychological tests be used to determine whether a prospective fieldworker be permitted to carry out his research? Such a recommendation runs directly counter to any notion of scientific freedom and borders on dictatorship under the guise of benevolent despotism. Gjessing might be suggesting that psychological test results, or a clear statement of "one's own sociopolitical position," should precede or accompany an anthropologist's publication of his research findings so that other scientists could take the researcher's bias into account. Fortunately, adherence to principles of scientific method make such a distasteful, and potentially dangerous, invasion of privacy unnecessary.

This specific issue of psychological testing is only one of several difficult ones raised by these papers, but it reflects, I believe, an essential fallacy in the extreme position implicit in these papers, namely that scientists are somehow "special." Even if one agrees that the anthropologist shares with society the responsibility for the manner in which his research findings are understood and implemented by society, one might hesitate to take the further step and conclude that scientists necessarily have a greater responsibility to serve society than any other group of citizens. If an experienced businessman, for example, has a demonstrated talent for dealing with complex labor-management disputes, does he not have the responsibility to use this ability when society needs it?

It seems important to remember that scholars tend to become specialists and that a specialist in one area has no claim to expertise in another area. Further, science is a never-ending process; data and explanations which seem correct today may prove deficient tomorrow. Even when it is absolutely clear that a scientist knows more than anyone else about a specific problem, considerable care should be taken not to allow him to assume too large a share of the responsibility for social planning and decision-making; for when a person, or group of people, accepts an inordinate share of social responsibility, he, or they, may lay claim to special powers or privileges and, by scarcely perceptible stages, alter a democratic form of government. B. F. Skinner in *Walden Two* and George Orwell in *1984* present contrasting fictional illustrations of, and attitudes toward, such a development.

One need not go to fiction, however, for examples of potential danger. Psychiatrists are one group of professionals who have been finding themselves increasingly in difficult ethical situations because of their presumed expertise. Courtroom battles in which two psychiatrists give contradictory testimony have long been widely publicized; potentially even more dangerous is the psychiatrist's role in national and international affairs. The case of Ezra Pound, for example, has been well described by T. Szasz (1963). More recently, according to a document signed by 99 Soviet mathematicians, Alexander Yesenin-Vulpin, a Soviet scientist who was active in the protests against the Sinyavsky-Daniel trials, has been taken "forcibly, without preliminary examination and without the consent of his relatives, and confined in psychiatric Hospital No. 5 ..." (*New York Times*, March 12, 1968). At about the same time, in the U.S.A., former Defense Secretary McNamara "confirmed that a psychiatric examination was given to a Navy Officer who discussed the 1964 Gulf of Tonkin incidents with Sen. J. William Fulbright." Although Mr. McNamara said the examination "can in no sense be viewed as an act of intimidation or reprisal" against the officer, he also said that disclosure of the report's details "would undoubtedly be harmful to the officer" (*International Herald Tribune*, Feb. 28, 1968). These incidents illustrate the complicated pattern of pressures at work on a scientist who tries to fulfil his social responsibility.

Space permits only one final illustration of the need for caution when scientists become involved in social planning. During the late 1950's the New York City Board of Education was ready to start using in all kindergarten classrooms the Glueck prediction tables for identifying potential juvenile delinquents. With the best of intentions, a research team had recommended the use of these tables without realizing that their apparent utility was a function of extreme base rates and that they would not only have been unsuccessful in identifying juvenile delinquents with greater than chance accuracy, but would probably have had unfortunate consequences for the large number of children prematurely labelled as "potential delinquents" (*SPSSI Newsletter* 1960).

If one believes that a representative democracy, based on the principle of "one man, one vote," is the least dangerous form of government, then one must consider the scientist to be no different from any citizen in his ethical commitment to truth rather than lies, to knowledge rather than ignorance, to concern rather than apathy. His responsibilities are no less and no greater; his privileges are no greater and no less.[1]

[1] These comments were written while the author was Visiting Scientist at the Clinical Psychiatry Research Unit, Graylingwell Hospital, Chichester, Sussex, England under the auspices of MH grant NIMH 10993-01.

by I. M. LEWIS☆

London, England. 10 v 68

I disagree with Gjessing's diagnosis that social anthropology is "today in grave danger of becoming irrelevant." This, indeed, is the exact opposite of what Kathleen Gough's gloomy assessment of the strategic importance ascribed to anthropological research in the United States would suggest. Whatever may be thought of the uses to which our findings may be put by interested parties, it seems to me self-evident that, in the present context of Third World politics, they could scarcely be more relevant. Just as the contemporary situation of social change in the newly independent states provides an unparalleled opportunity for testing the validity of our analyses (cf. Lewis 1968: xx–xi), so these countries, if they are to be understood in depth, require the kind of elucidation which so far our subject seems most apt to provide. It is natural that in this particular context social anthropology should often have acquired a conservative reputation, both because of its historical connexion with colonialism and —more significantly, I believe—because of its traditional concern with particularistic communities. This is not to say, however, that it has lost its importance. The problems facing the leaders of the new states in building up a viable cultural nationalism can only be appreciated in the light of the special character of the particularistic divisions which they have to overcome. Moreover, if the syndrome of tribalism is to be effectively eradicated it is important to diagnose it properly and understand its implications. Not only political, but also economic, development throughout the Third World requires that detailed understanding of its local social implications which social anthropologists are particularly qualified to supply. The record of failures, precisely because such knowledge has been lacking, is staggering (cf. Lawrence 1966).

Fortunately, however, the need for anthropological support is increasingly recognized by the economists, political scientists, and historians with whom we are now sharing the field. Here the point is, as Forde (1967:403) has recently put it, that

> the anthropologist is viewing social change in contemporary Africa . . . as a movement in which the "present" still encapsulates deeply held values generated in the "past" and is infused with partial and often discrepant aspirations for the future.

As Gough remarks, *pace* Peter Worsley, our distinctive contribution is precisely our concern for a holistic analysis which sees social phenomena as interconnected within a total field of action. We must, of course, jettison the old status-quo-maintenance assumptions of Radcliffe-Brown; but these have worn pretty thin by now in any case, and such of them as linger on have not prevented the development of a growing body of studies of the kind which Gjessing says are not being done. Many examples could be quoted here; obvious ones for me are Ardener 1968; Bailey 1960; Bradbury 1968; Bujra 1968; Cohen 1965; Fallers 1964; Lewis 1961, 1965, 1967; Lloyd 1966, 1967; and Van den Berghe 1964.

Moreover, it is far from true to suggest that the discoveries which stem directly from the Radcliffe-Brown tradition are now all obsolete and without interest or application. It is from this tradition that we derive such important insights as our understanding of the sociology of mystical affliction; of politics in uncentralised societies; of lineage structure; of marriage stability; of "rebellions" and "revolutions"; of "cross-cutting ties"; and many others. By the same token, while much current kinship controversy, or symbolic analysis in the manner of Lévi-Strauss, may at the moment seem a highly esoteric, not to say cabalistic, game, the possibility that new ideas and insights of wide relevance may in the course of time be discovered here cannot be rejected out of hand. Social anthropology has never been *merely* an aid to enlightened colonial administration, nor should be viewed now as *merely* an aid to development in new states. When anthropology ceases to have anything of wider interest to say, it deserves to die.

On the question of the moral commitment of the anthropologist to the communities in which he works, it seems to me that the three contributors tend to oversimplify the issues involved. Ideally, of course, an anthropologist owes a strong loyalty to those amongst whom he has worked and upon whom he depends for a living. In practice, however, the extent to which he feels and acts upon such obligations varies greatly with the kind of personal relations he enjoyed in the field. The (changing) political circumstances of the community concerned are also highly significant. Here Gjessing's reference to the anthropologist's duty to side with the "oppressed" rather than the "oppressor" may have more poignancy than he realises for many who work in the contemporary Third World; for commitment to the interests and aspirations of particular communities may be at complete odds with such other wider issues as economic development, national integration, interstate relations, or, for that matter, Pan-African unification. On the other side of the board, Gough's ready classifications of the regimes of new states do not seem particularly helpful either. The world is not simply divided into "good" revolutionary and "bad" counter-revolutionary regimes; there are revolutions and revolutions, and who is to decide, and by what means, when the "real," "true," "good" revolution has at last occurred? What is posed here is a moral question, which anthropologists are not necessarily better placed to judge than others, and which different anthropologists will in any case answer differently according not only to the facts of the situation but also to their own ideological convictions.

by Thomas McCorkle☆

Long Beach, Calif., U.S.A. 29 v 68

It should never have been necessary to write these papers; but it *was* necessary, and their publication here is a good thing. From my experience in teaching and visiting in various North American social science departments, large and small, over the past dozen years, the following pattern emerges: For many years, anthropologists, like other academics, walked, rode bicycles, or drove 10-year-old automobiles. They wore shirts with turned cuffs and collars. They scraped for a few dollars to support their fieldwork. At some point, there began to flow into the coffers of some institutions and the hands of some individuals what appeared to social and behavioral scientists (although it would not impress other social groups so) as "big" money, and what appeared to university administrators as "easy" money. Some anthropologists joined the "gold rush" and thereby became involved, both in the sacred "academic" situations and the secular "applied" ones, in activities which, if only rarely intentionally dishonorable, were at least very different from those for which graduate school had prepared them. Other anthropologists either fled from the new money or refused to recognize its existence. These latter anthropologists, roughly those whom our authors assign to the ivory tower, include both "conservators," who feel that "some of us have got to maintain the values and the integrity of our profession," and "dichotomizers," who argue that "those who work in applied situations are no longer like us; they have stopped being trained observers and recorders of human behavior; they are trying to change people, and they are not scientists." I feel that both these views can be reconciled with the need for application of anthropological knowledge in the process of teaching.

Ideally, teaching presents combinations of facts and ideas that can enlarge the student's view of the universe—change his mind. If you teach anthropology, you are an applied anthropologist. Published anthropological materials may be misapplied by others. One social scientist working in an "applied" situation recently said in private that those he attempts to work with cannot even absorb census data, let alone his, more sophisticated, offsprings, and so he now invests most of his imagination in work that may serve the administrators of the next century. It seems unlikely, in any case, that even the most successfully applied social scientist actually brings about more change than does the ordinary teacher.

There are some frontiers and some open questions. Can we produce better thinkers and actors by providing, say, elementary genetics in kindergarten and the ABC's of cultural difference in the first grade? Such things are being done: certain primary teachers in the United States, at least, have for years been informing Gentile children about the festival of Hanukkah. Would it, on the other hand, be better to allow traditional prejudices to be absorbed and internalized and *then* provide facts and ideas that allow the individual to re-think, and retain or discard, elements of his own sub-sub-cultural tradition? This, too, has already been done with multitudes of readers and students.

In addition to teaching, strictly defined, the teacher of anthropology in the year 1968 can regularly address himself to some vital fieldwork situations without even stepping off his own campus by, for example, asking himself such questions as, "Who are all these students? What is it that they are doing here? What ought I to observe and record as significant? And (following the Swiss social psychologist, Bavelas) what might a boy about my age and size do now that would be a good thing?"

by BRUCE MACLACHLAN☆

Carbondale, Ill., U.S.A. 23 v 68

A theme common to the articles by Berreman, Gjessing, and Gough is rejection of *Wertfreiheit*. Most of the objections which they state, or to which they allude, appear aimed at perversions or misuses of the concept, rather than at the concept itself. In the space allotted I cannot answer specific objections or persuade anyone who is not already persuaded. I shall simply offer a timely reaffirmation of, and personal rationale for, value-free scholarship.

Two of my heroes, Max Weber (1949) and Robert Redfield (1963), exemplify the ideal. Neither absented himself from the forum or violated the academy.

The essential point of *Wertfreiheit* is that in principle there cannot be an immediate relationship between inquiry in the academy and advocacy in the forum. There must be a mediating agency—in the context of individual action, the total personality of the individual, which transforms activities of one role into activities of the other in a manner both idiosyncratic and not unrelated to other roles which the individual also happens to play. The bundles of ideals, expectations, and sanctions remain distinct, because any particular role is one pole of a relationship with an alter with his rightful expectations. What distinguishes scholarship from other approaches to knowledge or understanding as well as from partisanship, is the nature of the rhetoric appropriate to each and, more particularly, the ways in which one prepares to enter into each kind of rhetoric. In an academic discipline, the rules as to how one attempts to persuade others *and oneself* (in a formal sense) of the validity of one's findings run counter, at crucial points, to the rules of forensic advocacy. A good scholar continually questions his assumptions and premises, being especially skeptical of those propositions which his own commitments make self-evident. This kind of questioning could make an ineffectual advocate. There is a tension between the contingent, skeptical, self-critical attitude of value-free scholarship and the gnostic certitude and manipulations conducive to successful passionate advocacy. A partisan deals in answers, not bona fide questions. Surely a "science" full of answers and solutions, empty of doubts, is dead. As Redfield (1963:196) has said,

> To every partisan the social scientist appears an enemy.... To partisans on both sides he appears unsympathetic and dangerous.

Obviously, there must be advocates. Men with convictions and men with relevant special knowledge should—*qua* citizen, not *qua* scholar—appear in the forum in its terms. Academics in the forum should not expect special deference or influence simply because they are "experts"—this is quackery. At the same time, if our solutions to our great problems are to be "cultured" (in almost any sense of the word) rather than brutish, there must also be scholars. One individual *can* play both roles; but the transition between them is accompanied by tension.

I see three possible ways of dealing with the tension: (1) One can deny it by acting the scholar role in inappropriate contexts. At worst, this is the Ivory-Tower evasion of the individual scholar's responsibilities *qua* citizen—an evasion which is often misrepresented as *Wertfreiheit*. (2) One can deny it by acting the partisan role in inappropriate contexts. At worst, this leads to polarization of groups and attitudes, rigidly bounded ideologies, and perhaps ultimately the destruction of scholarship in the name of humanity. Some advocate this destruction; I am culture-bound enough to find it paradoxical. (3) One can explicitly accept the tension; use *Wertfreiheit* as ideal, standard, and method; strive to be sensitive to contexts and, where necessary, to translate the demands created by one's participation in one sphere of action into behavior appropriate to the other. At worst, this results in frustration and ineffectuality in both spheres; but it is by academic discipline that one can be "free" of the values of one sphere while operating in the other. In this sense the other resolutions are "unfree," the first implicitly, the second explicitly. In my judgment the value-free course is preferable, when one compares the optimum and the worst consequences of each for the profession and the larger society with the consequences of the others. The value-free course may be harder on the individual than either indifferentism or partisanship.

This is not a hasty judgment arising out of a new-found concern with professional ethics. A decade ago as a graduate student I publicly referred to the likely consequences of direct and indirect involvement by anthropologists in intelligence work (MacLachlan 1957). External events forced the American Anthropological Association to act on this problem in the form of the Beals report (1967). In the same paper I observed the tendency of politicians in academic robes to present in the forum their personal convictions, or the value judgments of a professional guild, as the validated conclusions of a scholarly discipline. I expressed the fear that by such patent misrepresentations the advocates would ultimately both weaken the very worthy causes they sought to advance and also discredit their profession and their discipline. Regrettably, I appear to have been right on both issues.

by F. C. MADIGAN☆

Cagayan de Oro, Philippines. 13 v 68

I sympathize with Berreman's view that anthropologists ought to contribute to the solution of the great problems of our times. To withdraw into the security of picayune projects while the world crashes down about our ears is, if not cowardly, at least irresponsible. Further, I agree wholeheartedly that an anthropologist or other social scientist who with good reason believes that the organization that has funded his research intends, or is likely, to put his findings to immoral use must refuse to work further upon this project. I further fully concur that teachers must strive to inculcate into their students a sense of honesty and of moral responsibility for their actions. In addition, no doubt a person's values must influence his choice of a field of science as well as his choice of research problems within that field.

It seems to me, however, that Berreman throws out the baby with the bath. What kind of discipline would result if his thesis were to be widely accepted in social science? Max Weber once told his Roman Catholic students that they must either train themselves to counterbalance their religious values in analyzing and interpreting research data, so as to neutralize their biasing effect, or leave the field of science to others.

Can a scientist trust his ability to accept or reject a hypothesis in accordance with objective truth if he does not endeavor to lean heavily against his own admitted or suspected biases? In statistical analysis, it is common to "load the scales" against what we suspect our values favor, by setting a low or high level of rejection in opposition to our known or suspected bias. If personal values are to be allowed to bias our interpretation of data, would it not be cheaper and more honest to skip the research pretence altogether and apply our values directly to the problem under discussion?

G. K. Chesterton once said that while an open mind is a virtue in that it prevents prejudgment of the facts before enough of them are in, the whole point of it is eventually to close it upon the truth. It is not enough, however, to teach our students a sense of moral responsibility and of commitment to humane values. We must equip them to distinguish between what they *know* is true (or false) and right (or wrong), and what they *believe* (or *would like to be able to prove*) is true (or false) and right (or wrong).

Berreman apparently does not distinguish between a scientist's role as *a citizen* and a scientist's role as *a scientist*. As a citizen, he certainly has a right to attempt to convince his students and others of the properness and betterness of his views and values; but it seems to me that he does not confine himself to this. The thrust of his appeal is that anthropologists *as anthropologists* (as scientists) should attempt to present their values and beliefs to students and others as *facts* which they know to be true on the basis of scientific evidence. Not to distinguish beliefs and values from the facts or observations of science is to prostitute science, as it results in clothing a personal conviction in the aura of scientific discovery.

Berreman presents (it appears, as a scientist) the proposition that American participation in the Vietnamese war is immoral and genocidal; but the proof he offers is rhetorical (using loaded words, stereotypes, and strongly worded assertions) rather than scientific. Nor does he mention the Vietcong onslaughts upon the Vietnamese civilian population. Apparently he expects his scientific confrères to accept his viewpoint upon the war on the basis of his own and others' authority rather than upon a reasoned and unbiased presentation of evidence.

Gjessing feels deeply the conservative bias in modern anthropology and the danger of the preconceived ideas and hypotheses at the level of the implicit and unconscious. As one outside the circle of professional anthropologists (I am a sociologist), I would not have thought of anthropologists as especially sinners among social scientists upon this score. Although I do not question Gjessing's professional judgment on the point, my own impression is that anthropologists, compared with other social scientists do fairly well.

I heartily concur with the suggestion that social scientists (not just anthropologists) need to be schooled to be alert for signs of, to reflect upon, and to attempt to counter their unconscious biases and motivations. Anthropologists, with their concern for cultural conditioning, might well lead other social scientists to an increased sensitivity in this area. I particularly applaud the emphasis on our need for a much more adequate theory of change, including both self-generated and extraneously generated change (perhaps after the fashion of Gerhard Lenski's *Power and Privilege* [1966]). Indeed, a treatment of the political crises of the Congo without mention of Tshombe's alliance with the Union Minière or of the international financial interests involved does point up the inadequacy of traditional social science approaches in our times. I feel too that a rapprochement with the humanities will pay great dividends to those who make the investment.

Gough's article reads more like a piece of political propaganda than a serious attempt at a scholarly contribution. It is thus hard to take her thesis very seriously.

It is certainly controversial to maintain that anthropology was sired *by* Western imperialism (with its connotation of imperialistic instrumentality and imperialistic stratagems) rather than *in* Western countries which at that time were imperialistic (without any necessary connection with foreseen imperialistic utility). Statistical science and many public health technologies were conceived in imperialistic nations, but no one (to the best of my knowledge) has yet hinted that they were created to further imperialistic purposes.

It is certainly true, as Gough maintains, that a large segment of the developing world has passed from the sphere of "Western imperialism" into the sphere of the new socialist states. It is also true that Western social scientists should make all endeavors to comprehend as fully as possible the ethos and way of life of these newly formed national communities. It is hard to take seriously, however, her suggestion that we accept uncritically as sources of such knowledge the works of writers like Owen Lattimore and Edgar Snow.

by Thomas Maloney☆

Ripon, Wis., U.S.A. 27 v 68

These papers bring up a whole range of problems: the purposes of anthropology and of scientific endeavor in general; a definition of objectivity; the place of morality and values in research; and the role of students of man in the present world. To one such as myself, who has been both a chemical engineer and a Unitarian minister before becoming an anthropologist, it is especially encouraging to see colleagues facing problems engineers and physical scientists have avoided and espousing values of universal humanism that have always seemed to me implicit in anthropology.

The fiction of ethical neutrality is being exposed. This is uncomfortable to some social scientists, who perhaps have salved their consciences about their inaction toward, or even complicity in human exploitation by invoking such neutrality. There has always been a minority of anthropologists who do not try to fracture their lives, who are open in combining their ethics and beliefs with their professional work; and there have always been those who, without deceiving self or others, have been handmaidens of exploitive, even destructive forces. Perhaps we can at least say we have fewer scholars deliberately and proudly working in humanly destructive fields than my former colleagues in engineering. We find it much more difficult to compartmentalize our lives, to dehumanize our work, to be well-paid "working stiffs." These three articles and a great deal of ferment within the profession of anthropology in the United States demonstrates the impossibility of keeping the issue of involvement submerged beneath a professional posture of objectivity and neutrality.

In our fieldwork we find ourselves identifying with our subjects of research. Our graduate training in part gives us this ability along with an objectivity that does not deny our own and our subjects' humanity. For many of us it has been impossible even to avoid identification and sympathy with one faction or cause or viewpoint within the community we study. We take this into account in our ethnographic results. My own work in the American Southwest among both Indians and Hispano-Americans has taught me that, try as I may, I cannot be an anthropologist without "getting involved." Our work is the better, the richer, the truer, even, for such truly participant observation. The "hit-and-run" ethnographer who treats his subjects as objects to be squeezed for information, who cares little for the fate of those informants except as they advance his fame, is no model for anthropology. "Our" people are not equivalents of white rats in a laboratory, to be used for "the advancement of science" and then forgotten.

Anthropologists have much to say to people. It is not just in the classroom or the museum that our knowledge, our perspective on humanity, should be presented. Our discipline is unique in its capacity to show the people of the world that "Westernization," moral, economic, or politico-military imperialism are things to be questioned, criticized, modified, and even resisted; that man can control his fate. At least in the United States, much of the important work of rapid and often humanly destructive change has been taken up by political scientists and economists instead of by us. This seems particularly true of African problems. With a few exceptions, such as Fred Burke in Tanzania, these other scholars are clearly on the side of some modern version of imperialism, "big power" domination.

We can be effective helpers to people who want the physical, material advantages of modern industrial life but who want to control their own political and social affairs. We can't remain neutral, cultural middlemen, and still retain either our full humanity or our full professional standing. As men of science, fully both human and scientific, we have a choice. We are involved, as men and as anthropologists, and will not be let alone in a neutral corner, an ivory tower. There is nothing incompatible in being partisans, making value judgments, and remaining good objective students of man. If we choose to remain silent on moral and political issues, then others will make the value judgments, using us and our science for their own purposes.

by OTTO VON MERING☆

Pittsburgh, Pa., U.S.A. 27 v 68

In their argument for "revitalizing" social anthropology, Berreman and Gjessing cogently present ways of "exorcising the Minotaur of value-free social science" and "scientific unaccountability," and they reiterate the need for anthropology as a humanistic science to become more "problem-oriented" and attuned to the pressing issues of the condition of man in the developing *and* established nations of the world. Gjessing urges greater use of a "holistic approach," drawing on results of other disciplines. Berreman asks that we rededicate ourselves to being "entirely free" to follow our intellect in posing relevant questions. Both enjoin the anthropologist to speak the truth and expose common falsehoods about present-day reality. Neither is explicit enough, however, about the necessary prior commitment to embrace the personal and economic sacrifices demanded of the anthropologist who insists on remaining an independent participant-observer of "Establishment intellectualism." All of the social sciences are now suffering from an insufficiency of individual scholars willing to make these sacrifices. Still, it is heartening to know that there are a number of scientists who, having accepted the consequences of such existential daring, have never ceased doing small-scale studies to answer questions that nobody has asked them.

What none of the authors makes sufficiently clear is that the humanistic involvement in the scientific study of pressing sociocultural problems of man makes it urgent not to compromise one's judgment and effectiveness as a scientist by choosing too easily the road of direct activism or change-agentism. The essential scientific obligation is to illuminate for public inspection and redress the functioning (or "malfunctioning") of a particular social order. The most abiding scientific support of change and betterment of the human condition can only come from calm and unsparingly critical teaching of every new generation of students and elders.

Gough's essay illustrates the human difficulty of effecting a workable distinction between activism and doing socially relevant research. She speaks as an ideologue, not as a scientist, in her one-sided exegesis on the "Third World," "U.S. imperialism," and "client states." When she presents her "new proposals" on relevant problem areas for anthropological research, she can again be taken seriously as an involved and potentially objective student of man and his ways. There is reason to expect that if such needed studies were to be undertaken she would count herself among those who would insist that Marxism is no more "value-free" than capitalism. Considering Gough's views regarding revolution as sometimes "the only practicable means toward economic advance," it is only regrettable that she chose to leave the U.S.A. at a time when the "Black Revolution" and "White counter-insurgency" are locked in fateful struggle.

"Time present and time past are both perhaps present in time future, and time future contained in time past" (Eliot 1949). The ability to perceive time past can on occasion be man's most useful gift. Human events can be taken out of their immediate context of years and compared side by side, and antecedents, if they are relevant, can provide impetus to the future. Working within this frame of reference, the anthropologist aims to present the reality of man and his works at several depths; his work is an unfolding of *how* the individual goes about shaping himself within and apart from the matrix of his culture *and* of *why* the man-made social order and the remolded natural order are changing the quality of his condition as man and organism. In the end, we as anthropologists will, in J. R. Oppenheimer's words (1964), "be guided not by what it would be practically helpful to learn, but by what it is possible to learn."

by R. MUKHERJEE☆

Calcutta, India. 15 v 68

The publication of these three excellent articles in CURRENT ANTHROPOLOGY marks a new stage of maturity in our discipline. Previously, those who voiced such thoughts were ostracized by their colleagues for propagating "non-scientific" or "unscientific" views. It is gratifying, therefore, that the articles have found a world-wide forum for discussion. I shall comment on three points implicit in these articles.

1) While anthropology should not contribute to any manœuvre to obstruct the development and well-being of all people in the world, as Berreman has stressed particularly, this attitude should not persuade us (*a*) to renounce the knowledge gained from information collected with that purpose in view, or (*b*) to deprecate the theories and schools of thought which may support, directly or indirectly, the non-progressive trends in world society. To do so is neither to promote the development of social sciences nor to fulfil the social responsibility of social scientists. For example, under colonial rule, facts about the "primitive" way of life of the Indian villagers (collated by Mayo [1927] and evaluated by Churchill [Rai 1928:255]) were collected in order to justify the policy that "any quickening of general political judgement . . . is bound to come very slowly indeed" (Indian Statutory Commission 1930:15). Most of the social scientists of the 1930's and 1940's rejected these facts and brought out the counter-facts, no less relevant, of colonial exploitation and the consequent poverty and distress of the rural folk. The unqualified rejection of the former set of facts, however, did not help us to appreciate the dynamics of rural society and to plan measures, on that basis, to remove the social evils; and the unilateral stress on so-called social facts of the 1950's was equally inadequate to these purposes (Mukherjee 1957:ix–x; 1965:167–84). Again, while we may find it necessary to castigate Malinowski for his deductions on the colonial problem of Africa, an analysis of colonial Africa may usefully employ the structure-function approach (Mukherjee 1956:39–40, 267–70).

2) Gjessing and Gough, particularly, imply that (*a*) anthropological studies should take the form of problem-oriented diagnostic investigations on an empirical base; (*b*) these investigations must take into consideration the entire social organism and, thus, form a component of interdisciplinary research in the social sciences; and (*c*) in this field of knowledge, anthropological research should be conducted in its own way. These premises are surely necessary to develop fruitful anthropological research, but, in order to be effective, they require clarification as to the concepts, principles, and methodology of empirical *social* research and as to exactly how anthropological research differs, in this context, from, say, sociological research. For this purpose, we may begin with Gjessing's proposal to "build up a theoretical framework in which change and stability are complementary factors"; for social research must be concerned with these two phenomena.

We may conceive of a *social space* which is *infinite but enumerable* in its properties of change and stability; for knowledge can form, ultimately, an asymptote with reality. The space, then, will be progressively (but never finally) enumerated with reference to its three principal dimensions of variation in the place, time, and object of change and stability. For the enumeration of this space, we should proceed with the *null hypothesis* that the space is in a state of static equilibrium. This hypothesis will, obviously, be refuted; but the *alternate hypotheses* which will be formulated in light of the *nature of the refutation* of the null hypothesis will bring the contour and content of the social space into focus, without a subjective or any other manner of false emphasis on its highlights and shadows. In the course of this research, the role of anthropology *vis-à-vis* the other disciplines in the social sciences will be automatically determined in reference to the problems encountered, the requirement for a particular approach to execute a specific task, and so on.

3) Gough has stressed the "synthesizing role" of anthropology. This may be exemplified by the study of "tradition" in a society. In reference to variations in change and stability, the flowering of facts should be brought to account comprehensively: economic, sociological, political, and psychological analyses deal, essentially, with the petals; historical analysis deals with the stem; and the root can be accounted for, in most instances, only by the study of tradition (Mukerji 1961:26). Tradition may play a decisive role in change and stability; in contemporary India, it lies submerged in the social organism, holding the people in status quo, acting as a shock-absorber and as a brake against any attempt at social transformation, and requiring of any course of social mobilization compromise, adjustment, and co-existence with the other behaviour patterns (Mukerji 1961:20–31; Mukherjee 1965:185–213; 1968:39–40).

To be sure, as Gjessing has mentioned, "it is difficult to understand it [tradition] empirically." Nevertheless, it is not an impossible task; and it is a task which may require, particularly, the anthropological approach. With reference to Indian society, for example, it can be formulated as follows: To date, when tradition figures in social research on India, it is mostly limited to "the organized and unorganized superstructure operating through rituals, *achar* [manners], *kriya* [customs] which consolidate collective behaviour and give it style" (Mukerji 1961:29). Tradition, however, goes much deeper in society. It is expressed through the principle of *anubhava*, or personal experience of the people, which takes a collective character and eventually forms "social symbols" (Mukerji 1961:28–30). The complementary principles of *sruti* and *smriti*, generally beyond the comprehension of the people, provide the dialectical exegesis to traditional behaviour. Thus Indian tradition can be examined empirically, for diagnostic purposes, in terms of the relationship of the "high" traditions (*sruti* and *smriti*) and the "low" tradition (*anubhava*) to those social characteristics which have been ascertained, empirically, to promote or retard social change or to remain neutral in that context.

by ETHEL NURGE☆

Hamilton, Canada. 23 v 68

At one time, I was certain that anthropology would replace history, which is nationalistic or ethnocentric; that it would attract and influence sociologists and psychologists, who have interests similar to the anthropologists but who are culture-bound; and that anthropologists in general, with bicultural or tricultural perspective, would light the path for benighted fellow practitioners. It has not been so. Some of the reasons are spelled out in these three essays. Each author has stated points relevant to the place of anthropologists in the modern world. From Gjessing's essay I restate and emphasize the following:

The need for anthropologists and other social scientists to become aware of cultural or idiosyncratic assumptions is crucial. In these days of introspection and increased self-awareness, it is perhaps a commonplace that unconscious motivations exist and greatly influence all "scientific" and professional work. Nonetheless, despite an increased awareness, we are far from making the most of this knowledge and its implications. On the positive side, individual or group differences in unconscious motivation may be explored systematically as a means to enrich research. One reason for the excellence of the investigation into primate social behavior by the Japan Monkey Center is that Japanese colleagues do not perceive the universe as so arbitrarily divided into man and animal as do Western colleagues. The assumption of the similarity between men and animals has as a correlate an emphasis on culture-like characteristics among the non-human primates. This, in turn, has led to the documentation of surprising kinds and variability of learned behavior among the *Macaca fuscata* (Frisch 1963). Capitalizing on the variability of unconscious motivation is one approach.

Another part of the task is to develop techniques for routine recognition and listing of unconscious motivations, a check-list of propensities and tendencies toward destructive behavior, ego-deflating aggression, constructive suggestion, or emotional support. Perhaps what we need is to establish a board of psychiatric review. We could delegate this responsibility, or we could train anthropologists in psychiatry so that we may do the policing in our own field. Whatever the division of authority to examine and judge, the need for examination and judgment is clear.

I suggest that we make a psychiatric examination as mandatory as a medical examination before appointment to a faculty or an institute is granted. It will deplete our ranks, but it will make way for the healthy—literally and legally, for the sane. Nadel said in 1951 that some procedure for filtering students who were to be allowed to undertake fieldwork was imperative; it is now 1968, and we are no nearer achieving the imperative for fieldwork, or for anything else. Hasten the day! The need for selection and clearance is clear, especially in view of the unfortunate fact that, all too often, it is the unreasonable, neurotic, destructive, and self-protective individuals who are in positions of trust and responsibility; these very qualities aid aspirants, and impel them, to attain power.

Gjessing suggests that self-searching is a useful discipline for scientists and that the study of enculturation processes may lead to insight into harbored cultural assumptions. Further, he says that simply to state clearly one's sociopolitical position —"the extent to which one's general attitude toward the problems of life conforms to, or deviates from, the values of one's own civilization" would be a significant step forward. I differ with him, not as to the goal, but rather as to what is possible and practical. Can a "sociopolitical position" or the "values" of a civilization be stated clearly? One can pledge allegiance to a candidate, a side, an issue, an ideal, but it seems to me that in the larger sense one's whole life is a sociopolitical position (and an accepting and rejecting of values) and as such is a detailed, enormously complex, and ever subtly changing circumstance. It may be that our furtherance lies not in macroscopic analysis, as Gjessing suggests, but in more personal and familiar kinds of self-knowledge, and in the investigation of smaller units, preferably dyads. Let us start with statements about feelings toward infants, children, adolescents, adults, mature and old people. Especially important, for all of the stages of life we need to know an individual's reaction to both sexes. Such self-examination and clear statement can be the catalyst for enormous change in self and professional work. In the last analysis, our problem is not one of opposing sociopolitical systems but of the frailty of man—in our own and in the opposing sociopolitical systems. What we are depends on our personal relations, not on abstruse doctrines. "People are the walls of our room, not philosophies" (Golding 1959:226).

Gjessing ends with the clear statement that the social sciences should serve humanity—no more, no less. The statement is simple, but the implications are not. Margaret Mead (1964:323–24) has pointed to a danger more devastating than the irrelevance or disappearance of anthropology:

We have come full circle. Our human situation no longer permits us to make armed dichotomies between those who are good and those who are evil, those who are right and those who are wrong. The first blow dealt to the enemy's children will sign the death warrant of our own. The processes of evolution have kept us one species, and now the technical advances of cultural evolution, having power to destroy us, have made it necessary for us, at last, to make the invention that will protect every member of the human species. . . .

by SOLLIE H. POSINSKY☆

Jamaica, N.Y., U.S.A. 17 v 68

It is difficult to discuss these articles in a brief compass. Each is welcome, important, and refreshing. Their publication represents the authors' willingness—shared, fortunately, by a growing minority of American scholars and scientists—to run the risk of official disfavor; for one may wonder whether the powers-that-be in American universities, professional societies, private foundations, and government agencies will care to bestow their smiles (and funds) on scholars and scientists who are overtly hostile toward the several manifestations of "corporate liberalism" and aggressive imperialism.

It is especially difficult, and yet painfully necessary, to discuss these problems in an *international* journal. Citizens of the modern nation-state, whether scientists or animists, are so heavily manipulated and indoctrinated—despite the myth of "value-free orientation"—that any criticism of the political apparatus (and the class structure which the State represents and reflects) is taken as a kind of treason (or, at best, ingratitude) toward the national mother. Be that as it may, intellectual and moral judgments need not be kept within the "family circle" when the national mother abandons herself to sadistic or meretricious behavior which constitutes a threat to herself and others. History honors Socrates, not Meletus. Alcibiades was passionate, courageous, and ambitious; but the damage he inflicted on Athens is incalculable. Marcus Aurelius tried to be both a good Roman and a human being, with a signal lack of success. Our contemporary Pharisees pay token allegiance to dissent but beat down the dissenters; they pray for "peace" but piously dispense death at home and abroad.

To the best of my knowledge, no public figure has attempted to explain the *why* and *how* of the U.S. involvement in Vietnam. It is indeed rather ironic that some of the architects of the U.S. intervention in Southeast Asia are now troubled by the *failures* of imperialism, and not by its propriety or morality. What I have elsewhere described as a "tutelary war" (Posinsky 1966), or a war of example, will be repeated in Europe, Africa, or Latin America in the near future. As in the disastrous Vietnamese adventure, reasons and explanations will be available on demand. The time is long past when our leaders can say, with Admiral Halsey (in his New Year's message to the fleet, January 1, 1945), "Keep the bastards dying!!" Instead, we kill in the name of peace, freedom, or the American way of life. Patriotism, unlike chauvinism or jingoism, is not to be despised. Indeed, as Montesquieu noted, we must speak the truth always, even of *la matrie* or *patrie*—every citizen may be duty-bound to die for his country; *no one is obliged to lie for it.*

I have read these articles with great interest and gratitude. I can only hope that those of us who are on the wrong side of fifty will prove worthy of our students and younger colleagues. For them, the risks are inordinately heavy; but so are the potential gains. We must not, I pray, abandon them to the tender mercies of Lyndon B. Johnson, Lewis B. Hershey, or Grayson (L. B.) Kirk.

by CARA E. RICHARDS☆

Lexington, Ky., U.S.A. 20 v 68

Trying to comment on three such provocative articles in 500 words is like trying to convince a Protestant fundamentalist in five minutes that evolution is true. My remarks will therefore be confined chiefly to Berreman's article, with a shorter comment on Gough's.

It was both pleasant and depressing to read Berreman's plea that anthropologists do something most applied anthropologists have been doing for years. Those of us who, like Berreman, trained at Cornell while Allan Holmberg was alive generally accept the responsibility of taking stands as a matter of course. I can only applaud the encouragement Berreman offers to others in the profession to do the same. Apparently, however, Berreman thinks that the argument for individual involvement applies with equal strength to professional organizations, and with this I disagree.

Regardless of the internal organization of a professional society, to outsiders it represents *all* of its accredited members, not just some of them. Unless an organization was specifically organized to promote a political candidate, platform, position, or party, in taking a stand on a controversial political issue it betrays those members who either do not agree with the stand, or who regard such action as inappropriate for a professional organization regardless of how they feel about the particular position taken. Berreman must realize that although there may be general agreement among anthropologists regarding certain goals, there is considerable dissension regarding the means for attaining those goals. Unless he would argue that the A.A.A. should accept only members with the "correct" viewpoint (i.e., his), Berreman must also realize that if the organization takes a stand on a controversial issue without complete consensus (not just a majority vote) on *both* political position *and* the appropriateness of the action, it is disregarding its responsibilities to the total membership.

Furthermore, a scientific association engaging in political action risks losing public confidence in its scientific objectivity. If such an organization takes a political position, the rest of the U.S. citizenry will necessarily begin to regard it as a political group rather than as a professional society and will tend to judge its utterances from the viewpoint of their own political biases. A biased man may accept or at least consider ideas from a source he regards as objective (or trying to be objective) that he would reject from a source with a known bias other than his own. If the A.A.A. becomes categorized in the mind of the general public as a political action group, even statements of fact made with full consensus of all members will be regarded with suspicion. This would do tremendous harm to the profession and would actually impede our usefulness in the very area that Berreman and others are concerned about.

There is no space to comment adequately on the Gough article. To correct all the factual distortions alone would take more than 500 words. Unless one notes such qualifiers as "until recently" and "this work [of applied anthropologists] certainly exists," one gets the impression that North American anthropologists have completely refused to study modern problems such as "mine town, cash-crop plantations, urban concentrations," etc.—to which anthropologists such as Powdermaker (1939, 1950, 1962), Warner (1941, 1942, 1945, 1947), Mintz (1960), and Lewis (1959, 1961, 1964, 1966), among many others, should certainly take exception—and that interest in primitive societies has been due to North American reluctance to leave the Ivory Tower, which reveals a pathetic ignorance of the stated purposes of earlier anthropologists in the U.S., to say nothing of current developments. Gough conveys her impression by failing to mention any work which does not meet with her approval, and by dismissing any that a reader might know or hear of with her statement that such work "springs from erroneous or doubtful assumptions and theories that are being increasingly challenged by social scientists in the new nations themselves." Gough then lists assumptions she regards as erroneous or doubtful. Some of these (her numbers 3 and 6) are certainly not bases for the work of applied anthropologists I know, and others (numbers 7 and 2) are so loaded with absolutes such as "never" and "only" or double-barreled concepts (equating rapid with disruptive change, for example) that they are unacceptable as stated to most anthropologists. This type of slanted, semi-true–semi-false, inaccurate writing is more appropriate to an advertising agency or a propaganda publication than to a professional journal.

by Wolfgang Rudolph☆

Berlin (West), Germany. 26 iv 68

Since space is limited, I shall restrict my remarks to just one point which, to my mind, is fundamental. This point is relevant more to Berreman's and Gjessing's articles than to Gough's, and so I will be saying little about the latter beyond adding one more proposal to her list of new topics for anthropological study: an analysis of the present "student revolution" as a millenarian movement. As to the articles of Berreman and Gjessing, I would like to say at the outset that they were a disappointment to me; I am unable to find the kind of *analytical* treatment which is appropriate to the issues dealt with. This deficiency cannot, in my opinion, be compensated for by the pathos which these authors sometimes indulge in. The opinions they express, moreover, are embellished by a host of quotations and citations virtually all of them taken from one side of the bar and simply ignoring the most distinguished proponents of possibly significant counter-arguments. If this be the method proposed for a new kind of anthropology, I cannot but view the future of our science with considerable misgiving.

The fundamental issue alluded to above is: What are the possible, desirable or feasible forms of social responsibility for (social) science? The question is by no means a new one. It has been dealt with thoroughly ever since the '40's (at the latest): cf. the "Statement on Human Rights" (Executive Board 1947) and the subsequent controversy (Steward 1948; Barnett 1948; Bennett 1949); Embree's (1950) article and the comments on it by Fischer (1951), Henry (1951), and Haring (1951); Williams' (1947) article and Heyer's (1948) reply to it; Gregg and Williams' (1948) article and Kroeber's (1949) response. Nevertheless, I shall here try to contribute some additional remarks.

I quite agree with the contention that a so-called value-free science is not possible; nor do I know of any colleague who has ever made an explicit statement to the contrary. Where science *qua* science is concerned, however, there is only *one* unconditional and all-pervading value: truth, limited only by the capabilities of human nature. In this sense, science is equivalent to, as Benjamin (1965:15) puts it, "the pursuit of truth." In none of the foregoing articles is this point given due emphasis. Instead, the authors stress the role colonialism, imperialism, national characteristics, for example, have played in the rise of anthropological science. I do not deny that this is in some measure valid, but it is grossly, and misleadingly, overstated by the authors. Even if there are, or have been, situations in which anthropologists—knowingly or unknowingly—prostituted themselves to, say, unscrupulous colonial administrators, this has *in principle* nothing to do with the character of anthropology *as a science*. And are there not countless anthropological works in which no traces of a "colonialistic attitude" or the like can be detected—among the work of Boas or Kroeber, for example, or among that of Bogoraz or Jochelson, themselves exiles of their colonialistic government? Are there any national idiosyncracies to be found in the work of the latter pair and Boas, working together on the Jessup Expedition? Do the Chukchee display any Russian, or the Kwakiutl American (or German), traits as a result of the nationality of their observers? If not, then I can see no purpose in quoting (for instance) Bertrand Russell about animals showing the national character of their observers. Does Gjessing really mean that, say, Koehler's experiments with chimpanzees are without genuine scientific value, or that there has been no genuine scientific progress in animal psychology?

The authors confuse two analytically separable aspects of (concrete) science which I have called the genetic-historical (i.e., the extent to which it is the product of historical "accidents") and the generic-functional (i.e., its intrinsic characteristics as to purpose and method (Rudolph 1968:117–32). The first, important as it may be at times, is *in principle* ephemeral (as is "colonialism", etc.) for anthropology. Only the second is significant. Science being "the pursuit of truth," the generic-functional aspect of science is "common sense thinking, after it has been subjected to certain refinements and controls" (Benjamin 1965:6). Needless to say, the "common sense" referred to is not relative to culture, nationality, social class, and the like, as are the traditional systems of values, attitudes, and precepts which, within the context of their respective cultures, *are called* "common sense." The "common sense" meant by Benjamin is that universal system of thought necessary for human existence. As Malinowski (1955:17–18) puts it:

> No art or craft however primitive could have been invented or maintained, no organized form of hunting, fishing, tilling, or search for food could be carried out without the careful observation of natural process and a firm belief in its regularity, without the power of reasoning and without confidence in the power of reason; that is, without the rudiments of science.

Science, then, is nothing more than a development based upon these rudiments, retaining as the *only* criterion of truth (i.e., reliability) empirical (direct or indirect) verifiability.

This being so, it seems that the best way we can fulfil our social responsibility *as scientists* is to reinforce the generic-functional nature of our science by defending it against (scientifically detrimental) genetic-historical influences. To the extent that this is the intention of the authors in question, I believe that every anthropologist should back them without reservation. This means, however, that the answer to Berreman's question, "How can I be involved responsibly [in the affairs of men]?" is: By "the pursuit of truth" in the strict sense just mentioned, making no distinction between "higher," or "holistic," and "lower," or "particular," truths and the like. I have the impression that the authors believe in the validity of such distinctions. Such a position can only result in a compromised science, leading to compromised truths which, in the long run, would be a greater threat to science itself and the affairs of men than the (allegedly) "value-free scientist" in his ominous Ivory Tower.

by Henning Siverts☆

Bergen, Norway. 24 v 68

The Vietnam war, conflicts and revolutions in countries where anthropologists habitually work, and finally, the growth of economic colonialism produce a state of affairs which compels social scientists to ask questions about intellectual responsibility and scientific integrity.

Gough, Berreman, and Gjessing expose some important problems. Their statements about the present situation and the proposals concerning the future are sometimes "obvious and argumentative" (paraphrasing Gough), but that is not disturbing. I am dissatisfied, however, with their general characterization of present-day social anthropology. If we take Gough's list of erroneous assumptions as representative for her treatment I fail to see how these points in any way characterize theoretical anthropology and therefore question their explanatory value in applied anthropology. Furthermore, the formulations of these so-called assumptions are so simplified that it is easy to reject them all, as Gough does, except for point 4, "that causation is multiple." This point may in spite of its primitive appearance, cove or represent a more complete statement o a generative theory of anthropology. If tha is the case, I am not willing to label it a "erroneous assumption."

Gjessing's description of anthropolog 1968 is spectacular in its insistence upo themes and problems which at best can b characterized as marginal. Indeed, it is m impression that the portion of his essa dealing with what he calls the "delusion of the Ivory Tower" is a kind of Don Quixote performance. Instead of fearing that "anthropology is today in grave danger of becoming irrelevant," I suspect that Gjessing's view of anthropology is approaching this point.

While Gjessing is contemplating the fate of anthropology, Berreman concentrates on the fate of anthropologists if they ignore contemporary world events, that is, if they insist that "public issues are beyond the interest or competence of those who study and teach about man." In principle I agree, and I am inclined to think that Berreman's presentation of the dilemmas and pressures likely to face anthropologists is adequate and informative. Given such a situation I would, like Berreman, choose to act as a human being and as a social scientist, making sure that any contribution derived from my professional work would not be misused. In order to achieve such control over exclusive information, political (i.e., corporate) action on the part of anthropologists seems inevitable: Anthropologists as a professional group may find it appropriate to establish a board of supervisors or council whose primary function would be to examine pilot projects to which anthropologists are, or ought to be, attached. The main point is that anthropologists be permitted to take part in the decision-making.

Responsibility is the headword and the main theme for these three articles, and the authors do bring facts and present arguments which illuminate the issue. They do not, however, offer any detailed discussion of the applicability and shortcomings of the theoretical social anthropology as a universe of discourse in present world affairs. To speak of responsibility is somewhat pretentious if we do not state exactly *why* anthropology is relevant; this, and not Gjessing's assumption that anthropology is in danger of becoming irrelevant (i.e., why bother at all?), must be the point of departure. We may criticize anthropologists as persons and professionals, and we may discuss particular anthropological analyses in their own terms, but it is futile to revolutionize anthropology in order to make it fit transcendental ends. The profession for which we should feel responsibility is present-day anthropology, not any historical phase of the discipline or any future development.

by PETER SKALNÍK☆

Prague, Czechoslovakia. 24 v 68

This step taken by CURRENT ANTHROPOLOGY in the direction of a true emancipation of the social sciences ought to receive a great deal of attention and may perhaps even lead to resolute international action. The three articles show clearly that the social sciences are in real danger of being relegated to a position of servant to anti-scientific elements. Not only is anthropological data being used to further the war and espionage purposes of the various governments (in particular that of the United States), but it is being neglected in the pursuit of practical and social programs by these governments. Up until this year, the scientific data accepted by the leading group of the state in Czechoslovakia for presentation to the general public was for the most part biased and oversimplified. A similar situation has long existed in a number of other countries as well. Many social scientists still uncritically uphold state policies and force their scientific achievements into the framework of ready-made theories like dogmatic Marxism and Stalinism. Thus anthropology and the other social sciences are not answering the questions that society—whether this be the common people or the political system—is asking them.

This appeal to social scientists, and anthropologists in particular, to make our science more influential in the affairs of the world is the more welcome in that it comes from the United States. American anthropologists, perhaps the most directly involved in the dilemma of the uses of anthropology, are by this token the best able to express the dangers we face. Though the intensity with which we feel this dilemma may vary from one country to another, the difference between the scope of anthropology in the past and what is demanded of it today is gradually coming to be recognized everywhere. Anthropologists must focus upon present-day developments in the countries of Asia, Africa, and the rest of the "underdeveloped" world, employing anthropological methodology (and developing theory in the process) and drawing upon their knowledge of these societies' pre-industrial past. No less important is the necessity to disseminate social science results among the people, both in the "developed" and in the "underdeveloped" countries. Popularization of social science concepts is, in my view, just as important as research and publication in scientific journals and monographs. Unless social scientists make some effort to teach their findings to the lay public, they cannot be said to be truly interested in man. The great gap between the discovery of new facts and their penetration into the consciousness of the common people is due primarily to the passivity of social scientists. Anthropology has shown that racism, anti-Semitism, and a belief in the mental superiority of Western civilization are wrong; but it is well known how many people in Europe and America still consciously or unconsciously support such ideologies. It is the task of anthropology to make it clear that these views are false. It is not enough to discuss them in anthropological journals; we must discuss them also on TV and on the radio, in newspapers, in textbooks for students of all ages. We can free anthropology from the danger of non-engagement with the problems of the world and from the influence of official "customers" by concerted effort. The initiative of CA will surely find a response in action.

Replies

by GERALD D. BERREMAN☆

I agree with Mukherjee that publication of these articles by a professional journal is significant in these days of world crisis—crisis which social scientists have too often greeted with scientism rather than with sociological imagination. Such publication was not easily accomplished; it required persistent effort on the part of the authors. I am somewhat surprised and, from the point of view of responding in print, gratified, that the comments are as mild as they are. Responses to the oral presentations of the papers by Gough and myself were considerably sharper and of two types: vehemently critical and enthusiastically supportive, with the latter predominating (at least as reported to us).

My reply will focus on the comments of Klejn and Rudolph, but first I want to agree with Levine that social scientists should not be regarded as a special kind of people with unique responsibilities to serve society beyond those inherent in their roles as scientists and teachers. Elsewhere, with reference to ethics and responsibility in research, I have made the point that the social scientist, "like any other person, is a creature of the world. The responsibilities he bears are human responsibilities." And, "it is pointless to expect of an ethnographer superhuman insights or behavior" (Berreman 1968:372). My concern in the present article has been precisely with social scientists' tendency to regard themselves as special in such a way that their scientific credentials prohibit them from publicly stating their informed opinion, and from publicly acting thereupon. They too often assume that social science somehow divorces its practitioners from their values and therefore from their humanity. If I may repeat, "I do not advocate special powers (beyond those which come to reasoned statement) for the well-informed, but I decry special restrictions on them, whether externally imposed or self-imposed."

As scientists we do have, however, a peculiar commitment to truth as we see it. This is central to the definition of science. Those of us who are also teachers have that same special commitment compounded.

Klejn thinks that in emphasizing the social scientists' responsibility to speak the truth—to provide an adequate definition of reality—I have failed to adequately recognize that truth is relative to the social position of the one who perceives and communicates it. He notes that for Marxists, "the most progressive scientific position is that of the working class," and "the most correct theory is that of the sequence of the socioeconomic forms, socialism, and the social revolution." He makes the point that while truth is relative to social position, it is nevertheless absolute from the point of view of those in a particular social position. Therefore, the social scientist derives *the* truth by adhering to his group's rules (i.e., social scientists' rules) of procedure which preclude the interference of individual differences in the discovery of truth.

Rudolph, arguing in quite a different direction, holds that social scientists can discover absolute truth "limited only by the capabilities of human nature." He believes that this truth lies in "intrinsic characteristics" comprising the "generic-functional" aspect of science. It is discoverable through "common sense" which is "not relative to culture, nationality, social class, and the like." At this point, my impulse is to step aside and simply say, "Professor Klejn, meet Professor Rudolph," for the class-defined truth of the former is the absolute truth of the latter; but I feel constrained to say a bit more.

It seems to me naïve in today's world to adhere, as Rudolph does, to a belief in absolute truth based solely on "empirical verifiability." Such a belief is sustained by a diminishing number of natural scientists and even fewer social scientists. There are simply too many obstacles to agreement on a unitary standard of verifiability to reach agreement on generic-functional truth. We may try, in social science, to make explicit the "genetic-historical" aspects of our work (i.e., the effects of "historical accidents"), but we cannot eliminate them. The literature on the sociology of knowledge is illuminating on this subject. Further, to the generic-functionalist I would recommend Thomas Kuhn's book, *The Structure of Scientific Revolutions* (1962), and especially Chapter 10, "[Scientific] Revolutions as Changes of World View." Kuhn says (1962:4),

> . . . observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of such belief. An apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time.

"Normal science," says Kuhn (p. 5), ". . . is predicated on the assumption that the scientific community knows what the world is like." It is the repeatedly successful challenges to that assumption which Kuhn terms "scientific revolutions." It is in this regard that the notion of an absolute, empirically verifiable truth—a common sense which is independent of culture, nationality and social class, a "universal system of thought necessary for human existence"—is highly suspect.

Alfred Schutz has discussed the matter in detail and with insight. He says (1962:3–5), paraphrasing Alfred North Whitehead,

> . . . the so-called concrete facts of common-sense perceptions are not so concrete as it seems. They already involve abstractions of a highly complicated nature, and we have to take account of this situation lest we commit the fallacy of misplaced concreteness. . . . All our knowledge of the world, in common-sense as well as in scientific thinking, involves constructs. . . . Strictly speaking, there are no such things as facts, pure and simple. All facts are from the outset facts selected from a universal context by the activities of our mind. They are, therefore, always interpreted facts. . . .

W. I. Thomas said 40 years ago that if people define situations as real, they are real in their consequences. This is as true for scientists as for others.

Anthropological research abounds in manifestations of these problems. Ralph Piddington (1957:546) notes wryly: "a critic once remarked that the Trobriand Islanders are very like Malinowski, and the Tikopia very like Professor Raymond Firth." The differential facts and interpretations derived from Tepoztlán by Robert Redfield and Oscar Lewis are well known (Redfield 1930; 1955:132–48; Lewis 1951). John Bennett (1946) has brilliantly contrasted two major views of Pueblo culture (exemplified by Ruth Benedict and Esther Goldfrank) in which the same evidence was available to the anthropological observers but the interpretations were divergent. Li An-Che (1937), with a different cultural heritage, got still different impressions of Pueblo life. I suspect that accounts of the Chukchee and the Kwakiutl do indeed reflect the nationality of their observers—in fact, I am sure of it. As Schutz (1962:5) has pointed out, however,

> This does not mean that, in daily life as in science, we are unable to grasp the reality of the world. It just means that we grasp merely certain aspects of it . . . .

I am skeptical of absolutes in social science. They are usually social fictions, often with a purpose; they are usually ephemeral, and they are never independent of the life situations and experiences of the ones who discover or espouse them. Value-freedom is an absolute of this nature. I think each scientist has to define truth as he sees it, test it, and argue for it just as he defines, tests, and defends his values. At the same time, if he is to be believed, he must state as explicitly as possible the assumptions underlying his research, the methods used, the conditions of research—in short, he must tell how he has come to know what he knows of the truth as he sees it. I have previously advocated this in calling for a "sociology of ethnographic knowledge; an ethnography of ethnography" to lend credibility and verifiability to our research efforts (Berreman 1966:350).

Only history can determine whose truth and whose values will prevail and with what consequences. Those who prove to have held the lasting truths and to have espoused the lasting values will bear the credit or blame for their effects. Quite simply, I argue for human responsibility in our science—human responsibility as distinguished from superhuman pretensions with their too-often inhuman consequences.

Addendum

The above reply was written in response to comments by Akhmanova, Klejn, Levine, Mukherjee, and Rudolph. I have now received copies of comments by Beals, Butler, Cohen, Cresswell, Frank, Gulick, MacLachlan, Maloney, McCorkle, Posinsky, Richards, Siverts, Skalnik, and von Mering. These represent a broader spectrum of critical opinion than the first group, vitiating to some extent my earlier characterization of the comments as mild.

Although many issues are raised by these additional comments, I cannot now undertake an extended reply. Part of the urgency of such a reply is overcome by the fact that the several commentators address many of the same issues from various and even diametrically opposed perspectives. What I could say in response would essentially be to side with those who have expressed the views I share. The views are now before our readers, and I think on most issues they speak for themselves. The comments of Richards, MacLachlan, Gulick, and, in quite another direction, Frank exemplify positions which my article was aimed at counteracting, and I again affirm my original statements. David Aberle made clear in his remarks at the Fellows' meeting in November 1966 that the Association has repeatedly taken stands on political issues in the past. To assert that our professional association must take only unanimous positions and that anything less is a betrayal of the minority is nothing short of frivolous.

We are a large organization, and the world is complex. Either we say and do nothing, thereby totally abdicating from our social responsibilities as an association of students of man, or we act as other such bodies act, on the basis of votes. No one assumes such action represents concurrence by each individual member. It is an act of political significance to oppose taking any stand as surely as it is to endorse or oppose a particular stand. It appears that most of those who disagree with the stand taken on the war at the Pittsburgh meeting oppose the content of that stand rather than the principle of such stands, pious statements to the contrary notwithstanding. Harner's query still echoes: Is genocide not a matter of professional interest to anthropologists? Politics and science are simply inextricable.

I applaud the "Beals Report" and its author, but I must respond to Beals' comments printed herein. Instead of writing the editor of the *New York Times* to find out the meaning of the words quoted in his newspaper, I would advise reading the article. Let me quote in context:

> ... 157 anthropologists, engineers, ordnance specialists and other researchers ... are part of Project Agile, the Pentagon's worldwide counterinsurgency research program.... A third of the research is concerned with Thailand's people and their environment. Here as elsewhere in Southeast Asia, social scientists have bloomed under Project Agile.
>
> "The old formula for successful counterinsurgency used to be 10 troops for every guerrilla," one American specialist remarked. "Now the formula is ten anthropologists for each guerrilla."
>
> One group is studying water traffic and village habits along the Mekong River, which forms Thailand's border with Laos and a prime infiltration route.
>
> In northern Thailand, three anthropologists are living with the opium-growing Yao Akha and Miao Hill tribes. In this part of the country maps do not agree on the names and situation of thousands of villages. A gazetteer is being prepared by the center.
>
> In the Gulf of Siam, another team is preparing an illustrated recognition manual, nicknamed "Jane's Fighting Junks," an allusion to "Jane's Fighting Ships," a reference work on the navies of the world.
>
> "Jane's Fighting Junks" is designed to enable government patrol boats and aircraft to identify junk types characteristic of each area of the gulf.

Obviously the sentence quoted was an intentional overstatement with a touch of irony—but it was no joke. I hope Beals was joking when he said the idea might be worth trying.

Gough will doubtless defend herself, but I will point out that her decision to leave the United States was taken before Selective Service authorities abandoned use of academic grades in selecting draftees, and at a time when the university at which Beals and I both teach was following precisely the policies Gough found abhorrent at the university at which she taught. The effort of the Selective Service System was resisted not at all by those particular universities.

Beals is correct that the "Resolution Against Warfare" passed in Pittsburgh had been amended to eliminate specific reference to the United States, and I indicated that it has been amended. We condemned "the use of napalm, chemical defoliants, harmful gases, bombing . . ." and asked "that all governments put an end to their use at once and proceed as rapidly as possible to a peaceful settlement of the war in Vietnam" (Fellow Newsletter 1966). It has not come to my attention that any government other than that of the United States has been responsible for these atrocities in Vietnam. We also condemned other things, notably torture and killing of prisoners of war. These are probably indulged in by all participants, and they are certainly equally condemnable. I think it is clear that in the eyes of most who voted, the U.S. was the primary target of the resolution, if only because it is the nation for whose actions we have some responsibility and some possibility of influencing policy.

Opposition to totalitarianism is unexceptionable, but reference to totalitarianism in the Association is a red herring. If it is totalitarian to vote a resolution of the sense of the Association, then every body run by parliamentary rules is totalitarian.

Finally, I am proud to join, if somewhat belatedly, the Women's Anthropology Society of Washington in "counting *nothing* that affects humanity foreign to myself." As is so often the case in matters of humanity, and in our profession, women have led the way. My article will have been successful beyond my hopes if it leads in some small way to others joining in the anthropological and human commitment expressed and acted upon by those ladies.

by GUTORM GJESSING

I am certainly grateful for the many and thoughtful comments on my article. I did not, of course, expect, nor even wish, that all commentators would share my point of view. Anthropologists are such a tremendously heterogeneous group that complete agreement would in a sense refute my main thesis. The article was consciously written in a somewhat provocative form (although in Frank's opinion by far not sufficiently provocative) in the hope of evoking serious discussion on a subject of vital concern to every anthropologist. In this respect the three of us—Gough, Berreman, and myself—seem to have succeeded. It is to be hoped, however, that the discussion will not end here.

Rudolph is right, therefore, in pointing to the onesidedness of my quotations. On the other hand, I would have liked Rudolph to have defined his concept of "truth." In my feeling, the word "truth" is so ambiguous that it should possibly be barred from scientific usage (Klein would probably agree with me in this), and so I gladly admit not having given "the pursuit of truth" due emphasis. (I was a little surprised, in this connection, to learn that von Mering finds that I enjoin the anthropologist to speak the "truth.")

I do not see that Rudolph's "genetic-historical" aspect is ephemeral to anthropology. Every socioculture has emerged from the past and is developing in terms of the whole matrix of changing conditions, past and present (as von Mering so clearly states in connection with his quotation from Eliot). The "genetic-historical" and the "generic-functional" aspects are complementary, and both are essential to the understanding of a given sociocultural environment. This, I thought, would be commonplace to anthropologists today.

I cannot have expressed clearly enough my opinion in regard to the necessity of a study of the process of enculturation, for Levine's comment is mainly a discussion of Nadel's suggestion of psychological tests as a precondition for fieldwork. Although I cannot accept that Nadel's suggestion "borders on dictatorship in the guise of benevolent despotism," I did want to suggest that a study of the process of enculturation as outlined by Herskovits (1939:39–41) would probably be more profitable in the absence of culture-free psychological testing. That something in this line is crucial is explicitly stated by Nurge, although I do not find her suggestion of psychiatric training to be the best solution. In any case, to prepare a student theoretically for his fieldwork (and I see this as an important part of an anthropologist's theoretical training) certainly does not border on dictatorship in any disguise whatever.

I understand perfectly Levine's scruples concerning anthropologists' share in social planning, not least after the unveiling of the Camelot project and other misuses of anthropology in the politics of today. Frank's interesting and very pertinent remarks on this show clearly how necessary a reorientation of our field is. Beals is obviously right in assuming that Gough, Berreman, and I are all influenced by certain ideologies, as is also very explicitly, Frank. It is, however, an illusion that one can avoid being ideologically influenced when dealing with the study of man. Ours (in any case, Frank's and mine) is a socialist ideology; Beals' is an American liberal one. Gulick refers to what I have said about social responsibility and goes on to take issue with Berreman's views on this subject (which I have not yet read). From my own article, Gulick ought to have discovered that social responsibility, as I conceive it, is exactly the opposite of the "social responsibility" of those anthropologists who put their expertise at the disposal of their own government's power politics. Moreover, it was not I, but one of Gulick's compatriots and colleagues, who made the statement that anthropology can become responsible to society "by teaching everyone the basic precepts of culture"; I myself am painfully aware of "the lack of a clear definition of the precepts of culture."

"Gjessing ends with a clear statement that the social sciences should serve humanity—no more, no less. The statement is simple, but the implications are not" (Nurge). Indeed, they are not; and it is precisely for this reason that they should be a challenge to all of us. Charles Morris (in a letter quoted by Hirschfeld 1957:107, italics mine) wrote:

> If we think of mankind in this way then what is good for mankind is determined by the requirement of the human system. *Here is where careful research is needed, and here the social scientist can make important contributions.* As we get insight into these requirements we can begin to act intelligently for the good of mankind.

I have not yet read Gough's article, but I suspect that there is no disagreement between us on the strategic importance of anthropology, and I feel convinced that Lewis understands that I consider such misuses of anthropology unethical regardless of the pressures anthropologists may be exposed to. Prostitution is not moral, whatever the pay! This has also some bearing on Cohen's work in Israeli-occupied Arab territories. What Cohen is aiming at is exactly what Malinowski was aiming at in Africa—to make the occupation more tolerable—and is thus aiding Israel in making the occupation permanent.

Of course, nobody will try to deny Beals the right to choose his "own barricades." It is, however, precisely in this choice that his social responsibility will inevitably be involved. I may certainly have been wrong in pointing to a possible interdependence between White's "culturology" and American post-World War II global politics. That his view was influenced to some extent by Marxist writers has also been obvious to me. I did not know, however, that his basic ideas antedated the war. In any case, I would assume that their subsequent development has been influenced by, among other things, Emery Reve's best-seller, *The Anatomy of Peace.*

Lewis is certainly right in arguing that holistic analysis is of tremendous importance. The main postulate of holism, however, is that "the whole is more than the sum of its parts." This means that any holistic analysis (Frank 1966:137) must

> begin with a particular existing society and go on theoretically to analyse it and its transformation in its entirety. Even the best functionalists, on the other hand, almost always eschew the study of a whole society. In the few instances in which they do analyse the whole, they either leave reality aside altogether or depart from functionalist theory.

The holistic approach, in other words, requires that one explain the whole and thereby its parts.

I differ entirely from Siverts in his opinion that

> we may criticize anthropologists as persons and professionals, and we may discuss particular anthropological analyses in their own terms, but it is futile to revolutionize anthropology in order to make it fit transcendental ends

—whatever he may mean by "transcendental." I would, in fact, prefer the Gandhian principle of attacking institutions, not persons. Further it is a commonly accepted view that one must follow the rules of the game in science; but if one disagrees with the rules of the game—if, along with Haldane and Lévi-Strauss, one considers the rules of anthropology as an outcome of colonialism—then one must surely try to find rules relevant to the present world situation, whether some consider this "futile" or not. All three of us, I think, are concerned with present-day anthropology as well as with its future development. I wish that Siverts had elaborated upon his suspicion that my view of anthropology is approaching the point of irrelevance so that we might have discussed it.

At this point I must return once more to Beals' comment. Beals is not sure that "the definitions of relevance by the bureaucracy or politicians of a developing nation are much more satisfactory than those of their counterparts in the United States." It is very possible that the élites in power in most of the new states are not very representative of the opinions of their people. I am very much convinced, however, that, even though these leaders may be more or less isolated from the masses, they know vastly more of the needs of their people than "their counterparts in the United States." American foreign policy does not contradict this conviction.

Naturally I am very glad of the support I have received from several commentators, not least from Mukherjee, who has provided new material and new points of view, but also from Akhmanova, Butler, Cresswell, Nurge, and Posinsky. Nurge, moreover, points to one of the serious weaknesses in Western anthropology with her statement "that Japanese colleagues do not perceive the universe as so arbitrarily divided into man and animal as do Western colleagues."

by Kathleen Gough

Let me first dispose of some criticisms of statements I neither made nor intended. I. M. Lewis writes: "Social anthropology has never been *merely* an aid to enlightened colonial administration, nor should it be viewed as *merely* an aid to development." I agree with him and did not say otherwise. Father Madigan thinks I hinted that anthropology was created to further imperialistic purposes. I did not say or think so, but rather that its development in imperialist countries, and the fact that its subjects were the objects of imperialism, affected (although it did not by any means entirely determine) anthropological theories; and that, perhaps because they were themselves subjects of imperialist countries, anthropologists on the whole failed to carry out interconnected analyses of imperialism as a system.

Wolfgang Rudolph, again, writes:

> Even if there are, or have been, situations in which anthropologists—knowingly or unknowingly—prostituted themselves to, say, unscrupulous administrators, this has *in principle* nothing to do with the character of anthropology *as a science.*

and he cites the work of Boas, Kroeber, and others as containing no trace of "colonialist attitudes." In fact, it was not primarily the prostitution of anthropologists to imperialists that concerned me in this paper, although insofar as it occurs, it is likely to affect their theories. What concerned me more was anthropologists' much more widespread blindness to the implications of imperialism, which affects their theories too. Thus I would argue that while Boas and his students did invaluable work on race differences and race prejudice, they did not systematically explore the relationship of race prejudice to the world-wide historical and structural development of White nations' imperialism. Had they done so they might have concluded that a complete shift in the power relations between White and colored races would be necessary to undermine White racism, rather than concluding—as I think they tended to do—that the solution lay mainly in the liberal education of White people.

On a different point of fact, Ralph Beals questions the validity of my statement that "the proper goals of intellectual work have been undermined" through the use of students' academic grades by draft boards under the Selective Service System. He notes that this effort was resisted by universities and abandoned by Selective Service in 1967, and that no responsible university in the United States releases information about student grades except at the request of the student. The university with which I was connected, like many others, did not resist the Selective Service provision, and I wrote my paper and re-igned from that university several months before the practice was abandoned. It is rue that information about grades was not released except at the request of tudents. Nevertheless, I thought and still hink it a form of complicity with geno-ide in Vietnam, as well as of intellectual rostitution, to accede to the requests of *ther* students *or* the military to help rocess potential draftees by issuing rades. Beals may reasonably dispute the riousness of this kind of complicity compared with other kinds such as ying taxes, but I do not think what I rote was inaccurate or misleading.

Now for some criticisms of arguments tually made in my paper. Von Mering, als, and Lewis take issue with my classi-ation of "Third World" nations. There grounds for criticism: the data on ich the classification is based are not sented. It was, however, impossible to sent them in a 20-minute paper or in thing short of a book or several books. sted some of the works on which the sification was based. (Pierre Jalée's *Pillage of the Third World* [translated by ry Kopper, Monthly Review Press, 8] and Michael Barratt-Brown's *After rialism* [Heinemann 1963] are useful itions.) Finally, I said that my list of it states was "extremely tentative."

'hat needs to be pointed out is that debate need not be primarily ideologi- as these critics have left it, but could me at least partly empirical. It is isely because we have failed, as a pline, to define and analyse types of inance and to research the kinds of ions posed at the end of my paper individuals who do try to research fairly seriously are accused of logical surrender" (Beals). Instead, I st that anthropologists should famili- themselves with arguments and facts these largely empirical questions. Is e, as Frank and Jalée would argue, here is a net export of capital from some, if not most, of the non-socialist Third World nations to the industrial nations? If so, does this occur among socialist nations as well? Do the governments or the intelligence agencies of industrial nations overtly or covertly undermine or remove those of poor nations when they see these as threatening their interests? When, where, and why has this happened? In which Third World countries do the industrial nations have military bases, and what demonstrable effects do these have on the economics and policies of these nations? These questions can be answered more or less completely and objectively depending on our intellectual capacities, diligence, and integrity. I do not claim any premium on them; but if we do not probe them at all we cannot hope to move beyond prejudice or wilful ignorance towards an enlightened overview of the contemporary world.

I must hasten to add that I do not think such research problems should stay at the level of "problem-oriented diagnostic investigations on an empirical base," although the list of questions posed in my paper justifiably gave Mukherjee that impression. Certainly the findings to which they lead should be connected to form theories. Whether or not such theories will contribute to "the maturing of the discipline" (Beals) depends on what we want the discipline to mature into. I want it to mature into an interconnected body of empirical knowledge and theory, continually being revised, about the total process and main directions of the evolution of human societies and cultures, geared ultimately, although not at every point directly and immediately, to a search for the enhancement of human happiness and dignity.

Such a view of the ultimate goals of anthropology does lead to criteria of relevance or, as I would prefer to put it, of significance. These are not necessarily those defined for us by "the bureaucracy or politicians of a developing nation" (Beals), although, when we address ourselves to contemporary societies, they surely will have some bearing on "the needs of the emerging nations." I do not say that all of us should study only contemporary societies, or that all our research should bear directly on current ideological controversies within and between nations. If, however, while studying contemporary micro-systems, we refuse to relate them to any holistic analysis of world society—in its historical perspective and in relation to earlier forms of society—and if, in addition, we refuse ever to refer our researches back to the fundamental humanist question, "Knowledge for what?", we shall indeed be likely to lapse into disconnected trivialities, insignificant or even harmful make-work, and alienating mental exercises. In Paul Baran's (1965) phrase, we shall be intellect-workers rather than intellectuals, and thus, all too easily, the dupes of political élites whose goals are quite opposed to the humanism from which we have proudly dissociated our scientific endeavors.

In discussing my attempt to classify underdeveloped nations, Beals accuses me of surrendering to "the systematic dialectic of one side in the Cold War." I would find this intellectually offensive if it were not ridiculous. Fifteen years ago it would have been both offensive and dangerous, for it was then possible to persuade large numbers of intelligent Americans that any concepts derived, however indirectly, from Marx or Lenin were evidence of intellectual and political enslavement to a monolithic international conspiracy directed from Moscow. Today things are less simple. Which "side" does Beals mean? Cuba, China, the Soviet Union, or possibly Students for a Democratic Society? Surely Beals knows that their "dialectics" differ and at some points are deeply opposed? Of course, if it is suggested that I belong to a *movement*, that is true. In America, thanks to Arlo Guthrie, it is perhaps best known this year as the Alice's Restaurant Anti-Massacre Movement. Some members have read Marx and Lenin, Che Guevara and Chairman Mao. Most feel warmly about the Chinese cultural revolution, the Isle of Youth, the French, Columbia University, and Eastern European student rebellions, and the ideologically motley guerrilla movements of Asia, Africa, and Latin America. Personally, together with some of my colleagues such as A. G. Frank, I *am* somewhat more systematic. I am a revolutionary socialist. On the basis of reading, observation, and intuition, I think that historically, both the capitalist system and the industrial nation-state are in decline and that there are likely to be revolutionary developments throughout the Americas, Western Europe, and a large part of the non-socialist Third World. It seems quite possible to me that revolutionary developments may also occur in Eastern Europe, but I am poorly informed on that region. Having reached these conclusions as a tentative assessment of where we are, I think, with Frank, that it is my duty to aid revolution as best I can. This does not mean, however, that I am unwilling to think or incapable of thinking for myself, that I have "surrendered" to a ready-made Cold War "dialectic." There is no widely acceptable, unified dialectic for socialists of the Western world, although Baran, Marcuse, and numerous others have made advances. Unmodified, the theories of Marx and Lenin are outmoded. Stalinism produced its own sombre lessons, and the socialism of povertystricken new nations struggling against present imperial powers cannot offer more than flashes of understanding for those who would build post-imperial, post-national society. A theoretical system has to be formed out of the old ones. That is the task of socialist intellectuals; I think it is also consonant with my role as an anthropologist interested in contemporary society. As theories do develop, the pace of events makes it unlikely that they will remain closed or rigid. Beals may not like my belief in and commitment to revolution, but he cannot justifiably accuse me of intellectual surrender. Perhaps what Beals really objects to is any systematized theory of contemporary society or any attempt to make up one's mind where the world is heading. If so, I suppose he will have enough opponents outside the slim ranks of revolutionary socialist anthropologists to make it needless to engage him.

Similarly bizarre is Father Madigan's statement that "we ought not to accept uncritically as sources of such knowledge [about new nations] the works of writers like Owen Lattimore and Edgar Snow." I did not suggest that we should read anything uncritically, but I am puzzled to know why Father Madigan would not find worthy of careful consideration the recent field reports on Mongolia of the Head of the Department of Far Eastern Studies at Leeds University. After all, Lattimore has four decades of Far Eastern scholarship behind him, and there are not many such first-hand reports by Western scholars around. Edgar Snow, a quite different kind of writer, is a journalist of immense knowledge, compassion, and insight, Chinese-speaking, with twelve years of experience in that country, partly as a university teacher. The works of both authors speak for themselves. Madigan's rejection of them strikes me as a classic example of the kind of parochial prejudice against which my paper was directed.

Turning to a second set of criticisms, Richards and Siverts take exception to my criticism of what I think have been some common (although not invariant) assumptions of applied anthropology in America. Here, too, there is room for criticism. For lack of time, my statement was a brief précis of a number of concerns that I may have phrased too simply. I did, however, footnote authors who have expanded these criticisms in detail (Batalla, Onwuachi and Wolfe, Stavenhagen, Frank, and Worsley). It is true, as Richards says, that I did not quote all of the anthropological studies which *have* focused on mines, cash-crop plantations, urban concentrations, etc. Her attack is, however, unjust, for I did introduce modifiers into my generalizations (which, as she admits, she chooses *not* to note), and I spoke of trends, not absolutes. Of the works cited by Richards, moreover, Powdermaker (1950) and Warner (1941, 1942, 1945, 1947) do not deal with pre-industrial non-Western societies, to which my remarks in the relevant passage were specifically related. Richards says my criticisms 2 and 7 of applied anthropology are "so loaded with absolutes such as 'never' and 'only' that they are unacceptable as stated to most anthropologists." Now, I do not find the word "never" in this passage of my paper at all, while "only" is used in, I should think, a precise and justifiable statement, namely that much applied anthropology assumes "the refusal to contemplate the possibility that for some societies revolution may be the only practicable means towards economic advance." Am I wrong? How many American applied anthropologists do in fact contemplate or openly discuss the possibility that revolution may be the only practicable means toward economic advance? I do not think there are many. If Richards is so angry that she cannot read and report what I have written, I do not think she should accuse me of "slanted, semi-true, semi-false, inaccurate writing," etc.

A. G. Frank's comment is of a quite different character from the rest and is to me the most challenging and pertinent. Frank's statement will reach a different, and probably younger, audience from that for which mine was intended. My paper addressed what I took to be the large majority of "liberal" anthropologists in the United States. It asked them to recognize that revolutionary developments are occurring over wide areas, to consider the significant controversies that stem from these, and, in the light of empirical research, to examine both their own roles and the assumptions handed to them by the propaganda organs of their society. Frank implicitly address the already committed or half-committed radical anthropologist, urging him to clarify his priorities and loyalties, to divorce himself unconditionally from the corruptions, potential or actual, in his conventional role as social scientist, and to commit his life and work to revolutionary change. Frank's statement is also written 15 months after mine, when the pace of events has quickened in the ghettoes and universities of America, in Vietnam, and most recently in Europe. It is therefore timely and welcome. It is also pertinent to notice that this statement comes from a social scientist whose own trenchant analyses of capitalism in Latin America have led to his exclusion from the United States, where he has spent most of his life, and more recently also, from two of Canada's more "liberal" universities (including my own). Beals argues that "anthropologists in the United States still have a great deal of freedom of choice about their research," and that "in a significant part of the world they have little." While the latter statement may be true, I think that the former is true only insofar as anthropologists choose to work within the framework of capitalist imperialism. If they try to transcend it, their path is, understandably, thorny.

I agree with most of Frank's statement and admire his dedication. I would query two points. It is hard to accept, without further explanation, the statement that "virtually the whole of the 'free' world's social science is in effect one huge imperialist Camelot project, whoever pays for it." It is clear from the quotations in Frank's statement that the U.S. Department of Defense would like to gear most if not all American social science research, as well as that of foreign scholars, to the United States' counter-revolutionary policies, and that this aim is facilitated and often actively promoted by the major foundations, the Department of State, American-sponsored cultural institutions abroad, and an unknown number of American anthropologists. Nevertheless I think we must distinguish between the goals of the U.S. government and the motives of individual anthropologists, taking into consideration the degree of relevance for counter-revolution of the anthropologists' research subjects and findings. Project Camelot, like, no doubt, other similar projects, aimed specifically to research the roots of revolutionary insurgency and find ways to prevent it. However muddled their motives, the people in this project knew what they were working on, and for whom. Although the work of other anthropologists may be misused by their government, I do not think the culpability is as great in the case of an archaeologist working on Neolithic pottery or an anthropologist studying kinship change or female initiation rites. Rather than condemning all American anthropologists equally, I think it is necessary to distinguish degrees of culpability and gullibility. It is also necessary to point out, however, as Frank does, that the anthropologist's attempts to do independent research on contemporary societies may be used by his government to harm his informants. Further, even if anthropologists choose the most trivial, recondite, or historically remote problems, the fact that they take money from branches of the United States government, yet do not strongly and publicly protest against its policies through their professional organizations, allows their work to be used as a smoke-screen and gives an air of respectability to nefarious imperialist actions. Frank also does a service by pointing out that cultural imperialism is inherent in the spread of American research institutions, quite apart from their selective effects on indigenous research or their potential use as spy-stations.

A second issue is that of where anthropologists who are committed to revolution can work most fruitfully. Frank argues that Western social scientists should research their own societies and that social scientists of the Third World should aid revolutionary struggles there. He gives three reasons: Western-sponsored research in the Third World is used by Western governments for counter-revolutionary purposes; Western anthropologists, coming from the imperial nations, cannot, or cannot be expected to, further revolutions in the colonies; and potentially revolutionary situations now exist in the metropolitan countries themselves. I do not find these reasons entirely compelling. Provided their allegiance is clear, Westerners' studies of the Third World can be of value in exploring the goals and methods of colonial revolutionary movements, and indeed the whole imperial situation, for potential radicals at home. It is possible to be a "déclassé intellectual" internationally as well as within one's own nation, although admittedly few achieve it. I do not think it can be argued that the revolutionary movements in Cuba, China, Kenya, or Vietnam would have been better served if Edgar Snow, William Hinton, Jan Myrdal, K. S. Karol, Paul Sweezy, C. Wright Mills, Robert Taber, William Appleman Williams, Donald Barnett, Wilfred Burchett, Felix Greene, and others like them had stayed at home. The skills and regional training of social scientists and journalists take years to acquire; the unique experiences and abilities of individual anthropologists should not be wasted. If, moreover, as Frank believes, the Revolution is ultimately one and international, there are multifarious roles to be filled within and between the heartland and the hinterlands of imperialism.

It is true, however, that many, perhaps most, radical anthropologists in the West will no longer be able to finance their research abroad when their ethical stands and political allegiances are made public. If their purpose is primarily to aid revolution, most may find it necessary to work at home, and some, for various reasons, will prefer to do so. By the same token, many radical anthropologists, like other professionals, may, as Staughton Lynd (1968) points out, either be cast out of the universities or find their setting too constricting to carry out the research or the active organization that will best aid revolution. It seems to me, however, too early to lay down rules for the settings and creative activities in which revolutionary anthropologists can best engage. Presumably several roles will emerge, some more contemplative, theoretical, and global, others more local or more activist. Frank's statement draws attention, however, to the fact that the relationship between social science and values is not abstract or absolute. It has been defined in a particular way by bourgeois society during its period of growth. We must not be hampered by that. As long as we strive to be truthful and to clarify our values, the relationship between values and science can be whatever we choose to make it, within the limits imposed by our external situation. As crises sharpen, both counter-revolutionary repression and our own sense of urgency will press us more strongly toward unusual activities and may, for many anthropologists, mean abandoning conventional channels and canons of research, teaching, and publication. Our "subject" will not thereby be necessarily impoverished. Schoolcraft, Morgan, Darwin, Marx, Engels, Lenin, Trotsky, Rosa Luxemburg, Gramsci, Paul Sweezy, Herbert Aptheker, and Isaac Deutscher, to cite a few luminaries, worked outside universities, were thrown out of them, or not permitted to enter them (Lynd 1968). Social science and modern society would have been much poorer without them.

Frank's statement, in contrast to MacLachlan's and Levine's comments, brings out the obvious but often ignored fact that social scientists have a social role that is distinguishable both from the content of their research and from their duties as "mere" citizens. Even if their work and experience have not led them to opt for revolution, I should think that a large number, if not most, anthropologists would agree that they have an obligation, individually and collectively, to ask what effects their government's policies have, not only on their work, but on the welfare of the world's peoples, especially of the non-Western societies in which so much of their research has been done. Certainly we should examine the effects of other governments' actions too, but it is especially incumbent on us to make known our judgments of the governments that hire us and for whose acts we are, as citizens, most directly responsible. Thus it is not enough, for example, for anthropologists to condemn the employment of their colleagues by the Central Intelligence Agency on the grounds that such employment may lead to dissimulation or prevent the free publication of research. It is surely necessary to condemn it on the grounds that the Central Intelligence Agency has committed crimes against international law, undermined foreign governments, and helped to rob other nations of their independence, dignity, and physical safety. I do not say that only the United States government commits these crimes, although it seems obvious to me that it has been the most flagrant offender for the past two decades. Anthropologists in other countries, capitalist and socialist, must as Peter Skalník points out, examine their own institutions, social roles, and consciences. Members of the American Anthropological Association have a special duty to address themselves to the acts of governments within the Americas.

After referring to the "Vietnam" resolution, Beals ends his comment by rejecting "the totalitarian effort to commit all anthropologists to political positions through their professional organizations." I do not see totalitarianism in asking a professional organization to take a vote on a world-wide ethical and humanitarian issue. Minorities can present independent statements. I wonder whether Beals found similarly "totalitarian" earlier resolutions of the Association on nuclear weapons, atomic testing in the Pacific, the welfare of American Indians, or racial views and policies in relation to American Black people. Regarding totalitarianism, it *is* the American government that for years has been wantonly slaughtering an Asian peasant people, using internationally forbidden weapons, and preventing Vietnam, North and South, from forming its own united government in accordance with international agreement. To protest against such external fascism, individually or collectively, is not totalitarian. Personally, I found totalitarian the suffocating silence preserved in the American Anthropological Association for over two years and the strenuous and hectoring efforts of some senior anthropologists to prevent open debate on this subject. Had debate occurred and facts been carefully considered throughout this period, Beals and all of us would have been better qualified to assess the roles of the United States and North Vietnam in this conflict, and our own responsibilities as anthropologists in relation to it.

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R IX | (4)

INTRODUCTION

By M. FORTES AND E. E. EVANS-PRITCHARD

I. *Aims of this Book*

ONE object we had in initiating this study was to provide a convenient reference book for anthropologists. We also hope that it will be a contribution to the discipline of comparative politics. We feel sure that the first object has been attained, for the societies described are representative of common types of African political systems and, taken together, they enable a student to appreciate the great variety of such types. As the sketch-map on p. 2 shows, the eight systems described are widely distributed in the continent. Most of the forms described are variants of a pattern of political organization found among contiguous or neighbouring societies, so that this book covers, by implication, a very large part of Africa. We are aware that not every type of political system found in Africa is represented, but we believe that all the major principles of African political organization are brought out in these essays.

Several contributors have described the changes in the political systems they investigated which have taken place as a result of European conquest and rule. If we do not emphasize this side of the subject it is because all contributors are more interested in anthropological than in administrative problems. We do not wish to imply, however, that anthropology is indifferent to practical affairs. The policy of Indirect Rule is now generally accepted in British Africa. We would suggest that it can only prove advantageous in the long run if the principles of African political systems, such as this book deals with, are understood.

II. *A Representative Sample of African Societies*

Each essay is a condensation of a detailed study of the political system of a single people undertaken in recent years by the most advanced methods of field-work by students trained in anthropological theory. A degree of brevity that hardly does justice to some important topics has been necessary for reasons of space.

THE DISTRIBUTION OF THE PEOPLES DEALT WITH IN THIS BOOK

1. Zulu
2. Ngwato
3. Bemba
4. Banyankole
5. Kede
6. Bantu Kavirondo
7. Tallensi
8. Nuer

Each essay furnishes, nevertheless, a useful standard by which the political systems of other peoples in the same area may be classified. No such classification is attempted in this book, but we recognize that a satisfactory comparative study of African political institutions can only be undertaken after a classification of the kind has been made. It would then be possible to study a whole range of adjacent societies in the light of the Ngwato system, the Tale system, the Ankole system, the Bemba system, and so on, and, by analysis, to state the chief characters of series of political systems found in large areas. An analysis of the results obtained by these comparative studies in fields where a whole range of societies display many similar characteristics in their political systems would be more likely to lead to valid scientific generalizations than comparison between particular societies belonging to different areas and political types.

We do not mean to suggest that the political systems of societies which have a high degree of general cultural resemblance are necessarily of the same type, though on the whole they tend to be. However, it is well to bear in mind that within a single linguistic or cultural area we often find political systems which are very unlike one another in many important features. Conversely, the same kind of political structures are found in societies of totally different culture. This can be seen even in the eight societies in this book. Also, there may be a totally different cultural content in social processes with identical functions. The function of ritual ideology in political organization in Africa clearly illustrates this. Mystical values are attached to political office among the Bemba, the Banyankole, the Kede, and the Tallensi, but the symbols and institutions in which these values are expressed are very different in all four societies. A comparative study of political systems has to be on an abstract plane where social processes are stripped of their cultural idiom and are reduced to functional terms. The structural similarities which disparity of culture conceals are then laid bare and structural dissimilarities are revealed behind a screen of cultural uniformity. There is evidently an intrinsic connexion between a people's culture and their social organization, but the nature of this connexion is a major problem in sociology and we cannot emphasize too much that these two components of social life must not be confused.

rights of subjects and the obligations of rulers, and the checks on authority. Those who studied societies of Group B had no such matters to discuss and were therefore forced to consider what, in the absence of explicit forms of government, could be held to constitute the political structure of a people. This problem was simplest among the Nuer, who have very distinct territorial divisions. The difficulty was greater for the Logoli and Tallensi, who have no clear spatially-defined political units.

V. *Kinship in Political Organization*

One of the outstanding differences between the two groups is the part played by the lineage system in political structure. We must here distinguish between the set of relationships linking the individual to other persons and to particular social units through the transient, bilateral family, which we shall call the kinship system, and the segmentary system of permanent, unilateral descent groups, which we call the lineage system. Only the latter establishes corporate units with political functions. In both groups of societies kinship and domestic ties have an important role in the lives of individuals, but their relation to the political system is of a secondary order. In the societies of Group A it is the administrative organization, in societies of Group B the segmentary lineage system, which primarily regulates political relations between territorial segments.

This is clearest among the Ngwato, whose political system resembles the pattern with which we are familiar in the modern nation-state. The political unit is essentially a territorial grouping wherein the plexus of kinship ties serves merely to cement those already established by membership of the ward, district, and nation. In societies of this type the state is never the kinship system writ large, but is organized on totally different principles. In societies of Group B kinship ties appear to play a more prominent role in political organization, owing to the close association of territorial grouping with lineage grouping, but it is still only a secondary role.

It seems probable to us that three types of political system can be distinguished. Firstly, there are those very small societies, none of which are described in this book, in which even the largest political unit embraces a group of people all of whom are united to one another by ties of kinship, so that political relations are coterminous with kinship relations and the political structure and kinship organization are completely fused. Secondly, there are societies in which a lineage structure is the framework of the political system, there being a precise co-ordination between the two, so that they are consistent with each other, though each remains distinct and autonomous in its own sphere. Thirdly, there are societies in which an administrative organization is the framework of the political structure.

The numerical and territorial range of a political system would vary according to the type to which it belongs. A kinship system would seem to be incapable of uniting such large numbers of persons into a single organization for defence and the settlement of disputes by arbitration as a lineage system and a lineage system incapable of uniting such numbers as an administrative system.

VI. *The Influence of Demography*

It is noteworthy that the political unit in the societies with a state organization is numerically larger than in those without a state organization. The largest political groups among the Tallensi, Logoli, and Nuer cannot compete in numbers with the quarter to half million of the Zulu state (in about 1870), the 101,000 of the Ngwato state, and the 140,000 of the Bemba state. It is true that the Kede and their subject population are not so populous, but it must be remembered that they form part of the vast Nupe state. It is not suggested that a stateless political unit need be very small —Nuer political units comprise as many as 45,000 souls—nor that a political unit with state organization need be very large, but it is probably true that there is a limit to the size of a population that can hold together without some kind of centralized government.

Size of population should not be confused with density of population. There may be some relation between the degree of political development and the size of population, but it would be incorrect to suppose that governmental institutions are found in those societies with greatest density. The opposite seems to be equally likely, judging by our material. The density of the Zulu is 3·5, of the Ngwato 2·5, of the Bemba 3·75 per square mile, while that of the Nuer is higher and of the Tallensi and Logoli very much higher. It might be supposed that the dense permanent settlements of the Tallensi would necessarily lead to the development of a centralized form of government, whereas the wide dispersion of shifting villages among the Bemba would be incompatible with centralized rule. The reverse is actually the case. In addition to the material collected in this book, evidence from other African societies could be cited to prove that a large population in a political unit and a high degree of political centralization do not necessarily go together with great density.

VII. The Influence of Mode of Livelihood

The density and distribution of population in an African society are clearly related to ecological conditions which also affect the whole mode of livelihood. It is obvious, however, that mere differences in modes of livelihood do not determine differences in political structure. The Tallensi and the Bemba are both agriculturalists, the Tallensi having fixed and the Bemba shifting cultivation, but they have very different political systems. The Nuer and Logoli of Group B and the Zulu and Ngwato of Group A alike practise mixed agriculture and cattle husbandry. In a general sense, modes of livelihood, together with environmental conditions, which always impose effective limits on modes of livelihood, determine the dominant values of the peoples and strongly influence their social organizations, including their political systems. This is evident in the political divisions of the Nuer, in the distribution of Kede settlements and the administrative organization embracing them, and in the class system of the Banyankole.

Most African societies belong to an economic order very different from ours. Theirs is mainly a subsistence economy with a rudimentary differentiation of productive labour and with no machinery for the accumulation of wealth in the form of commercial or industrial capital. If wealth is accumulated it takes the form of consumption goods and amenities or is used for the support of additional dependants. Hence it tends to be rapidly dissipated again and does not give rise to permanent class divisions. Distinctions of rank, status, or occupation operate independently of differences of wealth.

Economic privileges, such as rights to tax, tribute, and labour, are both the main reward of political power and an essential means of maintaining it in the political systems of Group A. But there are counterbalancing economic obligations no less strongly backed by institutionalized sanctions. It must not be forgotten, also, that

those who derive maximum economic benefit from political office also have the maximum administrative, judicial, and religious responsibilities.

Compared with the societies of Group A, distinctions of rank and status are of minor significance in societies of Group B. Political office carries no economic privileges, though the possession of greater than average wealth may be a criterion of the qualities or status required for political leadership; for in these economically homogeneous, equalitarian, and segmentary societies the attainment of wealth depends either on exceptional personal qualities or accomplishments, or on superior status in the lineage system.

VIII. Composite Political Systems and the Conquest Theory

It might be held that societies like the Logoli, Tallensi, and Nuer, without central government or administrative machinery, develop into states like the Ngwato, Zulu, and Banyankole as a result of conquest. Such a development is suggested for the Zulu and Banyankole. But the history of all the peoples treated in this book is not well enough known to enable us to declare with any degree of certainty what course their political development has taken. The problem must therefore be stated in a different way. All the societies of Group A appear to be an amalgam of different peoples, each aware of its unique origin and history, and all except the Zulu and Bemba are still to-day culturally heterogeneous. Cultural diversity is most marked among the Banyankole and Kede, but it is also clear among the Ngwato. We may, therefore, ask to what extent cultural heterogeneity in a society is correlated with an administrative system and central authority. The evidence at our disposal in this book suggests that cultural and economic heterogeneity is associated with a state-like political structure. Centralized authority and an administrative organization seem to be necessary to accommodate culturally diverse groups within a single political system, especially if they have different modes of livelihood. A class or caste system may result if there are great cultural and, especially, great economic divergencies. But centralized forms of government are found also with peoples of homogeneous culture and little economic differentiation like the Zulu. It is possible that groups of diverse culture are the more easily welded into a unitary political system without the emergence of classes the closer they are to one another in culture. A centralized form of government is not necessary to enable different groups of closely related culture and pursuing the same mode of livelihood to amalgamate, nor does it necessarily arise out of the amalgamation. The Nuer have absorbed large numbers of conquered Dinka, who are a pastoral people like themselves with a very similar culture. They have incorporated them by adoption and other ways into their lineage system; but this has not resulted in a class or caste structure or in a centralized form of government. Marked divergencies in culture and economic pursuits are probably incompatible with a segmentary political system such as that of the Nuer or the Tallensi. We have not the data to check this. It is clear, however, that a conquest theory of the primitive state—assuming that the necessary historical evidence is available—must take into account not only the mode of conquest and the conditions of contact, but also the similarities or divergencies in culture and mode of livelihood of conquerors and conquered and the political institutions they bring with them into the new combination.

IX. The Territorial Aspect

The territorial aspect of early forms of political organization was justly emphasized by Maine in *Ancient Law* and other scholars have given much attention to it. In all the societies described in this book the political system has a territorial framework, but it has a different function in the two types of political organization. The difference is due to the dominance of an administrative and judicial apparatus in one type of system and its absence in the other. In the societies of Group A the administrative unit is a territorial unit; political rights and obligations are territorially delimited. A chief is the administrative and judicial head of a given territorial division, vested often with final economic and legal control over all the land within his boundaries. Everybody living within these boundaries is his subject, and the right to live in this area can be acquired only by accepting the obligations of a subject. The head of the state is a territorial ruler.

In the other group of societies there are no territorial units defined by an administrative system, but the territorial units are local communities the extent of which corresponds to the range of a particular set of lineage ties and the bonds of direct co-operation.

Political office does not carry with it juridical rights over a particular, defined stretch of territory and its inhabitants. Membership of the local community, and the rights and duties that go with it, are acquired as a rule through genealogical ties, real or fictional. The lineage principle takes the place of political allegiance, and the interrelations of territorial segments are directly co-ordinated with the interrelations of lineage segments.

Political relations are not simply a reflexion of territorial relations. The political system, in its own right, incorporates territorial relations and invests them with the particular kind of political significance they have.

X. The Balance of Forces in the Political System

A relatively stable political system in Africa presents a balance between conflicting tendencies and between divergent interests. In Group A it is a balance between different parts of the administrative organization. The forces that maintain the supremacy of the paramount ruler are opposed by the forces that act as a check on his powers. Institutions such as the regimental organization of the Zulu, the genealogical restriction of succession to kingship or chiefship, the appointment by the king of his kinsmen to regional chiefships, and the mystical sanctions of his office all reinforce the power of the central authority. But they are counterbalanced by other institutions, like the king's council, sacerdotal officials who have a decisive voice in the king's investiture, queen mothers' courts, and so forth, which work for the protection of law and custom and the control of centralized power. The regional devolution of powers and privileges, necessary on account of difficulties of communication and transport and of other cultural deficiencies, imposes severe restrictions on a king's authority. The balance between central authority and regional autonomy is a very important element in the political structure. If a king abuses his power, subordinate chiefs are liable to secede or to lead a revolt against him. If a subordinate chief seems to be getting too powerful and independent, the central authority will be supported by other subordinate chiefs in suppressing him. A king may try to buttress his authority by playing off rival subordinate chiefs against one another.

It would be a mistake to regard the scheme of constitutional checks and balances and the delegation of power and authority to regional chiefs as nothing more than an administrative device. A general principle of great importance is contained in these arrangements, which has the effect of giving every section and every major interest of the society direct or indirect representation in the conduct of government. Local chiefs represent the central authority in relation to their districts, but they also represent the people under them in relation to the central authority. Councillors and ritual functionaries represent the community's interest in the preservation of law and custom and in the observance of the ritual measures deemed necessary for its well-being. The voice of such functionaries and delegates is effective in the conduct of government on account of the general principle that power and authority are distributed. The king's power and authority are composite. Their various components are lodged in different offices. Without the co-operation of those who hold these offices it is extremely difficult, if not impossible, for the king to obtain his revenue, assert his judicial and legislative supremacy, or retain his secular and ritual prestige. Functionaries vested with essential subsidiary powers and privileges can often sabotage a ruler's acts if they disapprove them.

Looked at from another angle, the government of an African state consists in a balance between power and authority on the one side and obligation and responsibility on the other. Every one who holds political office has responsibilities for the public weal corresponding to his rights and privileges. The distribution of political authority provides a machinery by which the various agents of government can be held to their responsibilities. A chief or a king has the right to exact tax, tribute, and labour service from his subjects; he has the corresponding obligation to dispense justice to them, to ensure their protection from enemies and to safeguard their general welfare by ritual acts and observances. The structure of an African state implies that kings and chiefs rule by consent. A ruler's subjects are as fully aware of the duties he owes to them as they are of the duties they owe to him, and are able to exert pressure to make him discharge these duties.

We should emphasize here, that we are talking of constitutional arrangements, not of how they work in practice. Africans recognize as clearly as we do that power corrupts and that men are liable to abuse it. In many ways the kind of constitution we find

in societies of Group A is cumbrous and too loosely jointed to prevent abuse entirely. The native theory of government is often contradicted by their practice. Both rulers and subjects, actuated by their private interests, infringe the rules of the constitution. Though it usually has a form calculated to hold in check any tendency towards absolute despotism, no African constitution can prevent a ruler from sometimes becoming a tyrant. The history of Shaka is an extreme case, but in this and other instances where the contradiction between theory and practice is too glaring and the infringement of constitutional rules becomes too grave, popular disapproval is sure to follow and may even result in a movement of secession or revolt led by members of the royal family or subordinate chiefs. This is what happened to Shaka.

It should be remembered that in these states there is only one theory of government. In the event of rebellion, the aim, and result, is only to change the personnel of office and never to abolish it or to substitute for it some new form of government. When subordinate chiefs, who are often kinsmen of the king, rebel against him they do so in defence of the values violated by his malpractices. They have an interest greater than any other section of the people in maintaining the kingship. The ideal constitutional pattern remains the valid norm, in spite of breaches of its rules.

A different kind of balance is found in societies of Group B. It is an equilibrium between a number of segments, spatially juxtaposed and structurally equivalent, which are defined in local and lineage, and not in administrative terms. Every segment has the same interests as other segments of a like order. The set of intersegmentary relations that constitutes the political structure is a balance of opposed local loyalties and of divergent lineage and ritual ties. Conflict between the interests of administrative divisions is common in societies like those of Group A. Subordinate chiefs and other political functionaries, whose rivalries are often personal, or due to their relationship to the king or the ruling aristocracy, often exploit these divergent local loyalties for their own ends. But the administrative organization canalizes and provides checks on such inter-regional dissensions. In the societies without an administrative organization, divergence of interests between the component segments is intrinsic to the political structure. Conflicts between local segments necessarily mean conflicts between lineage segments, since the two are closely interlocked; and the stabilizing factor is not a superordinate juridical or military organization, but is simply the sum total of inter-segment relations.

XI. The Incidence and Function of Organized Force

In our judgement, the most significant characteristic distinguishing the centralized, pyramidal, state-like forms of government of the Ngwato, Bemba, &c., from the segmentary political systems of the Logoli, the Tallensi, and the Nuer is the incidence and function of organized force in the system. In the former group of societies, the principal sanction of a ruler's rights and prerogatives, and of the authority exercised by his subordinate chiefs, is the command of organized force. This may enable an African king to rule oppressively for a time, if he is inclined to do so, but a good ruler uses the armed forces under his control in the public interest, as an accepted instrument of government—that is, for the defence of the society as a whole or of any section of it, for offence against a common enemy, and as a coercive sanction to enforce the law or respect for the constitution. The king and his delegates and advisers use organized force with the consent of their subjects to keep going a political system which the latter take for granted as the foundation of their social order.

In societies of Group B there is no association, class, or segment which has a dominant place in the political structure through the command of greater organized force than is at the disposal of any of its congeners. If force is resorted to in a dispute between segments it will be met with equal force. If one segment defeats another it does not attempt to establish political dominance over it; in the absence of an administrative machinery there is, in fact, no means by which it could do so. In the language of political philosophy, there is no individual or group in which sovereignty can be said to rest. In such a system, stability is maintained by an equilibrium at every line of cleavage and every point of divergent interests in the social structure. This balance is sustained by a distribution of the command of force corresponding to the distribution of like, but competitive, interests amongst the homologous segments of the society. Whereas a constituted judicial machinery is possible and is always found in societies of Group A, since it has

the backing of organized force, the jural institutions of the Logoli, the Tallensi and the Nuer rest on the right of self-help.

XII. Differences in Response to European Rule

The distinctions we have noted between the two categories into which these eight societies fall, especially in the kind of balance characteristic of each, are very marked in their adjustment to the rule of colonial governments. Most of these societies have been conquered or have submitted to European rule from fear of invasion. They would not acquiesce in it if the threat of force were withdrawn; and this fact determines the part now played in their political life by European administrations.

In the societies of Group A, the paramount ruler is prohibited, by the constraint of the colonial government, from using the organized force at his command on his own responsibility. This has everywhere resulted in diminishing his authority and generally in increasing the power and independence of his subordinates. He no longer rules in his own right, but as the agent of the colonial government. The pyramidal structure of the state is now maintained by the latter's taking his place as paramount. If he capitulates entirely, he may become a mere puppet of the colonial government. He loses the support of his people because the pattern of reciprocal rights and duties which bound him to them is destroyed. Alternatively, he may be able to safeguard his former status, to some extent, by openly or covertly leading the opposition which his people inevitably feel towards alien rule. Very often he is in the equivocal position of having to reconcile his contradictory roles as representative of his people against the colonial government and of the latter against his people. He becomes the pivot on which the new system swings precariously. Indirect Rule may be regarded as a policy designed to stabilize the new political order, with the native paramount ruler in this dual role, but eliminating the friction it is liable to give rise to.

In the societies of Group B, European rule has had the opposite effect. The colonial government cannot administer through aggregates of individuals composing political segments, but has to employ administrative agents. For this purpose it makes use of any persons who can be assimilated to the stereotyped notion of an African chief. These agents for the first time have the backing of force behind their authority, now, moreover, extending into spheres for which there is no precedent. Direct resort to force in the form of self-help in defence of the rights of individuals or of groups is no longer permitted; for there is now, for the first time, a paramount authority exacting obedience in virtue of superior force which enables it to establish courts of justice to replace self-help. This tends to lead to the whole system of mutually balancing segments collapsing and a bureaucratic European system taking its place. An organization more like that of a centralized state comes into being.

XIII. The Mystical Values Associated with Political Office

The sanction of force is not an innovation in African forms of government. We have stressed the fact that it is one of the main pillars of the indigenous type of state. But the sanction of force on which a European administration depends lies outside the native political system. It is not used to maintain the values inherent in that system. In both societies of Group A and those of Group B European governments can impose their authority; in neither are they able to establish moral ties with the subject people. For, as we have seen, in the original native system force is used by a ruler with the consent of his subjects in the interest of the social order.

An African ruler is not to his people merely a person who can enforce his will on them. He is the axis of their political relations, the symbol of their unity and exclusiveness, and the embodiment of their essential values. He is more than a secular ruler; in *that* capacity the European government can to a great extent replace him. His credentials are mystical and are derived from antiquity. Where there are no chiefs, the balanced segments which compose the political structure are vouched for by tradition and myth and their interrelations are guided by values expressed in mystical symbols. Into these sacred precincts the European rulers can never enter. They have no mythical or ritual warranty for their authority.

What is the meaning of this aspect of African political organization? African societies are not models of continuous internal harmony. Acts of violence, oppression, revolt, civil war, and so forth, chequer the history of every African state. In societies like the Logoli, Tallensi, and Nuer the segmentary nature of the social structure is often most strikingly brought to light by armed conflict between the segments. But if the social system has reached a

sufficient degree of stability, these internal convulsions do not necessarily wreck it. In fact, they may be the means of reinforcing it, as we have seen, against the abuses and infringements of rulers actuated by their private interests. In the segmentary societies, war is not a matter of one segment enforcing its will on another, but is the way in which segments protect their particular interests within a field of common interests and values.

There are, in every African society, innumerable ties which counteract the tendencies towards political fission arising out of the tensions and cleavages in the social structure. An administrative organization backed by coercive sanctions, clanship, lineage and age-set ties, the fine-spun web of kinship—all these unite people who have different or even opposed sectional and private interests. Often also there are common material interests such as the need to share pastures or to trade in a common market-place, or complementary economic pursuits binding different sections to one another. Always there are common ritual values, the ideological superstructure of political organization.

Members of an African society feel their unity and perceive their common interests in symbols, and it is their attachment to these symbols which more than anything else gives their society cohesion and persistence. In the form of myths, fictions, dogmas, ritual, sacred places and persons, these symbols represent the unity and exclusiveness of the groups which respect them. They are regarded, however, not as mere symbols, but as final values in themselves.

To explain these symbols sociologically, they have to be translated into terms of social function and of the social structure which they serve to maintain. Africans have no objective knowledge of the forces determining their social organization and actuating their social behaviour. Yet they would be unable to carry on their collective life if they could not think and feel about the interests which actuate them, the institutions by means of which they organize collective action, and the structure of the groups into which they are organized. Myths, dogmas, ritual beliefs and activities make his social system intellectually tangible and coherent to an African and enable him to think and feel about it. Furthermore, these sacred symbols, which reflect the social system, endow it with mystical values which evoke acceptance of the social order that goes far beyond the obedience exacted by the secular sanction of force. The social system is, as it were, removed to a mystical plane, where it figures as a system of sacred values beyond criticism or revision. Hence people will overthrow a bad king, but the kingship is never questioned; hence the wars or feuds between segments of a society like the Nuer or the Tallensi are kept within bounds by mystical sanctions. These values are common to the whole society, to rulers and ruled alike and to all the segments and sections of a society.

The African does not see beyond the symbols; it might well be held that if he understood their objective meaning, they would lose the power they have over him. This power lies in their symbolic content, and in their association with the nodal institutions of the social structure, such as the kingship. Not every kind of ritual or any sort of mystical ideas can express the values that hold a society together and focus the loyalty and devotion of its members on their rulers. If we study the mystical values bound up with the kingship in any of the societies of Group A, we find that they refer to fertility, health, prosperity, peace, justice—to everything, in short, which gives life and happiness to a people. The African sees these ritual observances as the supreme safeguard of the basic needs of his existence and of the basic relations that make up his social order—land, cattle, rain, bodily health, the family, the clan, the state. The mystical values reflect the general import of the basic elements of existence: the land as the source of the whole people's livelihood, physical health as something universally desired, the family as the fundamental procreative unit, and so forth. These are the common interests of the whole society, as the native sees them. These are the themes of taboos, observances and ceremonies in which, in societies of Group A, the whole people has a share through its representatives, and in societies of Group B all the segments participate, since they are matters of equal moment to all.

We have stressed the fact that the universal aspect of things like land or fertility are the subjects of common interest in an African society; for these matters also have another side to them, as the private interests of individuals and segments of a society. The productivity of his own land, the welfare and security of his own family or his own clan, such matters are of daily, practical concern to every member of an African society; and over such matters arise the conflicts between sections and factions of the

society. Thus the basic needs of existence and the basic social relations are, in their pragmatic and utilitarian aspects, as sources of immediate satisfactions and strivings, the subjects of private interests; as common interests, they are non-utilitarian and non-pragmatic, matters of moral value and ideological significance. The common interests spring from those very private interests to which they stand in opposition.

To explain the ritual aspect of African political organization in terms of magical mentality is not enough; and it does not take us far to say that land, rain, fertility, &c., are 'sacralized' because they are the most vital needs of the community. Such arguments do not explain why the great ceremonies in which ritual for the common good is performed are usually on a public scale. They do not explain why the ritual functions we have been describing should be bound up, always, with pivotal political offices and should be part of the political theory of an organized society.

Again, it is not enough to dismiss these ritual functions of chiefship, kingship, &c., by calling them sanctions of political authority. Why, then, are they regarded as among the most stringent responsibilities of office? Why are they so often distributed amongst a number of independent functionaries who are thus enabled to exercise a balancing constraint on one another? It is clear that they serve, also, as a sanction against the abuse of political power and as a means of constraining political functionaries to perform their administrative obligations as well as their religious duties, lest the common good suffer injury.

When, finally, it is stated as an observable descriptive fact that we are dealing here with institutions that serve to affirm and promote political solidarity we must ask why they do so. Why is an all-embracing administrative machinery or a wide-flung lineage system insufficient by itself to achieve this?

We cannot attempt to deal at length with all these questions. We have already given overmuch space to them because we consider them to be of the utmost importance, both from the theoretical and the practical point of view. The 'supernatural' aspects of African government are always puzzling and often exasperating to the European administrator. But a great deal more of research is needed before we shall be able to understand them fully. The hypothesis we are making use of is, we feel, a stimulating starting-point for further research into these matters.

That part of it which has already been stated is, perhaps, least controversial. But it is incomplete.

Any item of social behaviour, and therefore any political relation, has a utilitarian or pragmatic content. It means that material goods change hands, are disbursed or acquired, and that the direct purposes of individuals are achieved. Items of social behaviour and therefore political relations have also a moral aspect; that is, they express rights and duties, privileges and obligations, political sentiments, social ties and cleavages. We see these two aspects clearly in such acts as paying tribute to a ruler or handing over blood-cattle in compensation for murder. In political relations, consequently, we find two types of interests working conjointly, material interests and moral interests, though they are not separated in this abstract way in native thought. Natives stress the material components of a political relation and generally state it in terms of its utilitarian and pragmatic functions.

A particular right or duty or political sentiment occurs as an item of behaviour of an individual or a small section of an African society and is enforceable by secular sanctions brought to bear on these individuals or small sections. But in a politically organized community a particular right, duty, or sentiment exists only as an element in a whole body of common, reciprocal, and mutually balancing rights, duties, and sentiments, the body of moral and legal norms. Upon the regularity and order with which this whole body of interwoven norms is maintained depends the stability and continuity of the structure of an African society. On the average, rights must be respected, duties performed, the sentiments binding the members together upheld or else the social order would be so insecure that the material needs of existence could no longer be satisfied. Productive labour would come to a standstill and the society disintegrate. This is the greatest common interest in any African society, and it is this interest which the political system, viewed in its entirety, subserves. This, too, is the ultimate and, we might say, axiomatic set of premisses of the social order. If they were continually and arbitrarily violated, the social system would cease to work.

We can sum up this analysis by saying that the material interests that actuate individuals or groups in an African society operate in the frame of a body of interconnected moral and legal norms the order and stability of which is maintained by the political

organization. Africans, as we have pointed out, do not analyse their social system; they live it. They think and feel about it in terms of values which reflect, in doctrine and symbol, but do not explain, the forces that really control their social behaviour. Outstanding among these values are the mystical values dramatized in the great public ceremonies and bound up with their key political institutions. These, we believe, stand for the greatest common interest of the widest political community to which a member of a particular African society belongs—that is, for the whole body of interconnected rights, duties, and sentiments; for this is what makes the society a single political community. That is why these mystical values are always associated with pivotal political offices and are expressed in both the privileges and the obligations of political office.

Their mystical form is due to the ultimate and axiomatic character of the body of moral and legal norms which could not be kept in being, as a body, by secular sanctions. Periodical ceremonies are necessary to affirm and consolidate these values because, in the ordinary course of events, people are preoccupied with sectional and private interests and are apt to lose sight of the common interest and of their political interdependence. Lastly, their symbolic content reflects the basic needs of existence and the basic social relations because these are the most concrete and tangible elements of all social and political relations. The visible test of how well a given body of rights, duties, and sentiments is being maintained and is working is to be found in the level of security and success with which the basic needs of existence are satisfied and the basic social relations sustained.

It is an interesting fact that under European rule African kings retain their 'ritual functions' long after most of the secular authority which these are said to sanction is lost. Nor are the mystical values of political office entirely obliterated by a change of religion to Christianity or Islam. As long as the kingship endures as the axis of a body of moral and legal norms holding a people together in a political community, it will, most probably, continue to be the focus of mystical values.

It is easy to see a connexion between kingship and the interests and solidarity of the whole community in a state with highly centralized authority. In societies lacking centralized government, social values cannot be symbolized by a single person, but are distributed at cardinal points of the social structure. Here we find myths, dogmas, ritual ceremonies, mystical powers, &c., associated with segments and defining and serving to maintain the relationship between them. Periodic ceremonies emphasizing the solidarity of segments, and between segments, as against sectional interests within these groups, are the rule among the Tallensi and Logoli no less than among the Bemba and Kede. Among the Nuer, the leopard-skin chief, a sacred personage associated with the fertility of the earth, is the medium through whom feuds are settled and, hence, inter-segment relations regulated. The difference between these societies of Group B and those of Group A lies in the fact that there is no person who represents the political unity of the people, such unity being lacking, and there may be no person who represents the unity of segments of the people. Ritual powers and responsibility are distributed in conformity with the highly segmentary structure of the society.

XIV. *The Problem of the Limits of the Political Group*

We conclude by emphasizing two points of very great importance which are often overlooked. However one may define political units or groups, they cannot be treated in isolation, for they always form part of a larger social system. Thus, to take an extreme example, the localized lineages of the Tallensi overlap one another like a series of intersecting circles, so that it is impossible to state clearly where the lines of political cleavage run. These overlapping fields of political relations stretch almost indefinitely, so that there is a kind of interlocking even of neighbouring peoples, and while we can see that this people is distinct from that, it is not easy to say at what point, culturally or politically, one is justified in regarding them as distinct units. Among the Nuer, political demarcation is simpler, but even here there is, between segments of a political unit, the same kind of structural relationship as there is between this unit and another unit of the same order. Hence the designation of autonomous political groups is always to some extent an arbitrary matter. This is more noticeable among the societies of Group B, but among those of Group A also there is an interdependence between the political group described and neighbouring political groups and a certain overlapping between them. The Ngwato have a segmentary relationship to other Tswana

tribes which in many respects is of the same order as that between divisions of the Ngwato themselves. The same is true of the other societies with centralized governments.

This overlapping and interlocking of societies is largely due to the fact that the point at which political relations, narrowly defined in terms of military action and legal sanctions, end is not the point at which all social relations cease. The social structure of a people stretches beyond their political system, so defined, for there are always social relations of one kind or another between peoples of different autonomous political groups. Clans, age-sets, ritual associations, relations of affinity and of trade, and social relations of other kinds unite people of different political units. Common language or closely related languages, similar customs and beliefs, and so on, also unite them. Hence a strong feeling of community may exist between groups which do not acknowledge a single ruler or unite for specific political purposes. Community of language and culture, as we have indicated, does not necessarily give rise to political unity, any more than linguistic and cultural dissimilarity prevents political unity.

Herein lies a problem of world importance: what is the relation of political structure to the whole social structure? Everywhere in Africa social ties of one kind or another tend to draw together peoples who are politically separated and political ties appear to be dominant whenever there is conflict between them and other social ties. The solution of this problem would seem to lie in a more detailed investigation of the nature of political values and of the symbols in which they are expressed. Bonds of utilitarian interest between individuals and between groups are not as strong as the bonds implied in common attachment to mystical symbols. It is precisely the greater solidarity, based on these bonds, which generally gives political groups their dominance over social groups of other kinds.

Scope of Anthropology
Claude Levi-Strauss

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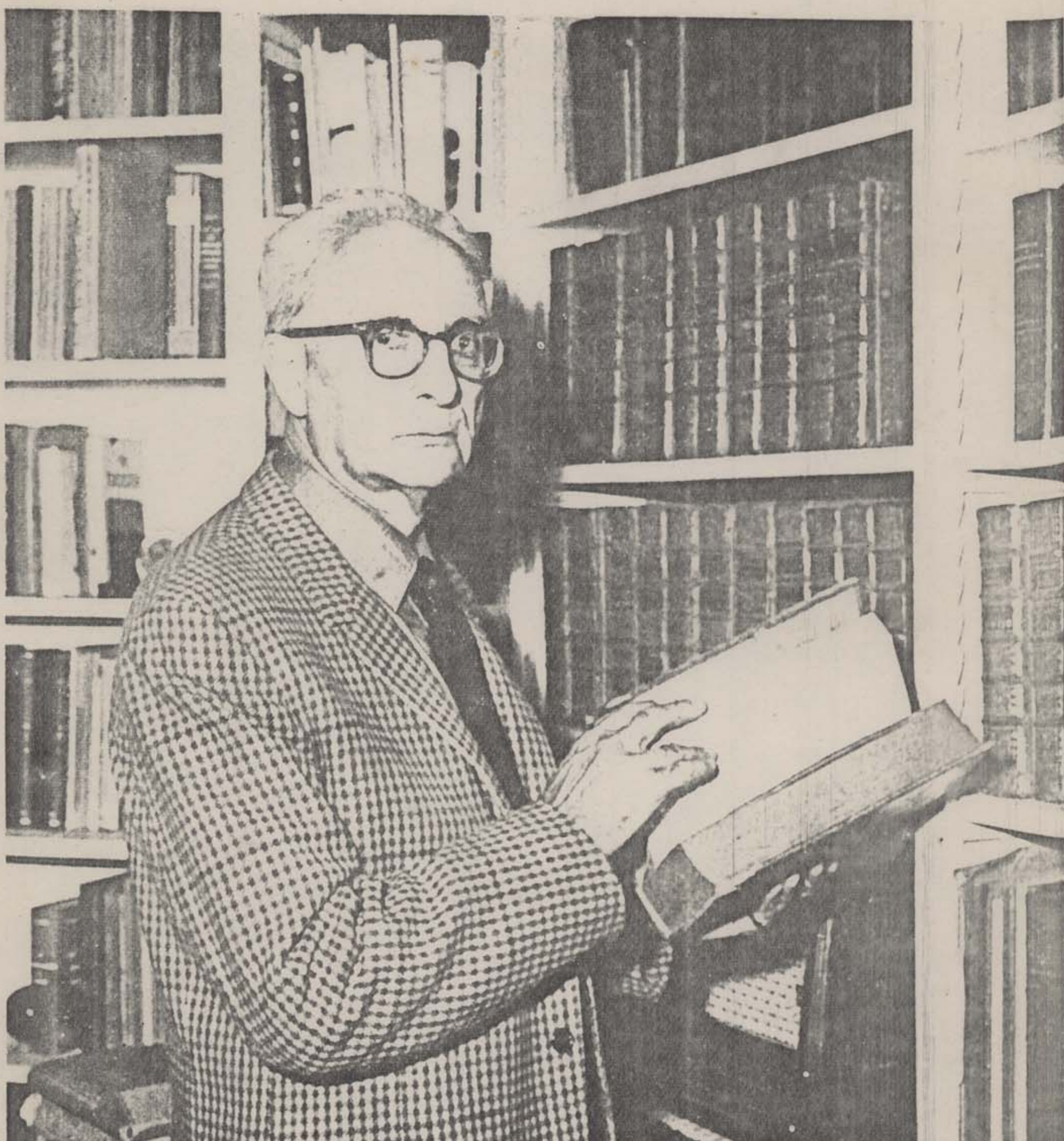

CLAUDE LÉVI-STRAUSS, Professeur titulaire at the Collège de France, Paris, was born in 1908 in Belgium and educated at l'Université de Paris (Licencié de Philosophie, 1928; Licencié de Droit, 1929; Agrégé de l'Université, 1932; Docteur ès Lettres, 1949). He is Honorary Fellow, Royal Anthropological Institute of Great Britain and Ireland; Foreign Fellow, American Philosophical Society, American Academy of Arts and Sciences, Royal Academy of the Netherlands, Norwegian Academy of Science and Letters; Doctor *honoris causa*, Université Libre de Bruxelles, Yale University, Oxford University; Officier de la Légion d'Honneur.

From 1935 to 1939 he was a professor at the University of São Paulo, and the leader of several ethnological expeditions into central Brazil. He returned to France in 1939, where he served in the French army. After the armistice in 1941, he was invited to the United States as Visiting Professor at the New School for Social Research in New York. From 1942 to 1945 he taught at the New School and also at l'Ecole Libre des Hautes Etudes de New York. From 1946 to 1947 he served as Cultural Attaché to the French Embassy to the United States. In 1947 he returned to France to become Associate Director of the Musée de l'Homme. Since 1950 he has been Directeur d'études at l'Ecole Pratique des Hautes Etudes, Paris. In addition to the Chair of Social Anthropology at the Collège de France, he occupies the Chair of Comparative Religion of Nonliterate Peoples. He is also the Director of the Laboratoire d'anthropologie sociale of the Collège de France and l'Ecole Pratique. He has been an editor of *L'Homme, revue française d'anthropologie* since its inception in January, 1961.

His chief interests are social anthropology, kinship, and social organization; religion and mythology; art; and the comparative anthropology of North and South America.

Viking Fund Medal and Award

The Wenner-Gren Foundation for Anthropological Research has asked that the following announcement be printed in reference to the selection of the Viking Fund Medalist (CA 6:71):

It is a pleasure to announce that on November 16, 1965, the Board of Directors voted, in accordance with the expressed wishes of the Associates of CURRENT ANTHROPOLOGY, to award the medal to Claude Lévi-Strauss. The medal carries with it an award of $10,000.00.

In recognition of his contributions to the field CURRENT ANTHROPOLOGY publishes a translation of Prof. Lévi-Strauss' address on the occasion of his assuming the Chair of Social Anthropology of the Collège de France on January 5, 1960, usually called his Inaugural Address; his Smithsonian address, "Anthropology: Its Achievements and Future," delivered at The Bicentennial Celebration Commemorating the Birth of James Smithson and held in Washington on September 17, 1965; and a bibliography of Prof. Lévi-Strauss' published works.

RECIPIENTS OF VIKING FUND MEDALS AND AWARDS

1946	Alfred V. Kidder, Alfred L. Kroeber, Franz Weidenreich
1947	John O. Brew, Earnest A. Hooton, Robert H. Lowie
1948	Alex D. Krieger, Adolph H. Schultz, John R. Swanton
1949	W. King Gregory, Hallam L. Movius, Jr., George P. Murdock
1950	Emil W. Haury, Clyde Kluckhohn, Wilton M. Krogman
1951	Carleton S. Coon, Ralph Linton, Frank H. H. Roberts, Jr.
1952	Alfonso Caso, Julian H. Steward, William L. Straus, Jr.
1953	Melville J. Herskovits, T. Dale Stewart, Gordon R. Willey
1954	William W. Howells, Robert Redfield, W. Duncan Strong
1955	A. I. Hallowell, W. E. Le Gros Clark, J. Eric S. Thompson
1956	Junius B. Bird, Fred Eggan, Mildred Trotter
1957	Raymond A. Dart, James B. Griffin, Margaret Mead
1958	Raymond W. Firth, Jesse D. Jennings, Henri V. Vallois
1959	William W. Greulich, Irving Rouse, Leslie A. White
1960	Samuel K. Lothrop, Leslie Spier, Sherwood L. Washburn
1961	Edward E. Evans-Pritchard, Robert Heine-Geldern,
1965	Louis S. B. Leakey, Sol Tax
1966	Claude Lévi-Strauss

The Scope of Anthropology[1]

by Claude Lévi-Strauss

IT WAS A LITTLE MORE THAN A YEAR AGO, in 1958, that the College of France decided to create in its midst a chair of social anthropology. This science is too attentive to those forms of thought which, when we encounter them among ourselves, we call superstition, for me not to be allowed to render to superstition a preliminary homage; is it not the characteristic of myths, which have such an important place in our research, to evoke a suppressed past and to apply it, like a grid, upon the present in the hope of discovering a sense in which the 2 faces in which man is confronted with his own reality—the historic and the structural—coincide? It would seem to me also permissible on this occasion, on which all the patterns or features of myth are for me reunited, to proceed on their example, seeking to discern in past events the meaning and the lesson of the honor which has been done me, to which, my dear colleagues, the very date of your deliberation bears witness: by the strange recurrence of the number 8, already well-known from the arithmetic of Pythagoras, the periodic table of chemical elements, and the law of symmetry of the medusa-jellyfish, the proposal in 1958 to create a chair of social anthropology revives a tradition which even if I had wished to I would not have been able to escape.

Fifty years prior to your initial decision, Sir James George Frazer delivered the inaugural lecture of the 1st chair of social anthropology in the world, at the University of Liverpool. Fifty years earlier, in 1858, 2 men were born—Franz Boas and Emile Durkheim—whom posterity will regard as, if not the founders, at least the chief engineers, 1 in America and the other in France, of anthropology as we know it today.

It is appropriate that these 3 anniversaries, these 3 names, have been evoked here. Those of Frazer and Boas give me occasion to express my gratitude, if only briefly, for all that social anthropology owes to Anglo-American thought, and for what I owe it personally, since it was in close conjunction with it that my 1st works were conceived and developed. But it will not surprise you that Durkheim occupies a larger place in this lecture. He incarnates the essence of France's contribution to social anthropology, even though his centennial, celebrated with enthusiasm in many foreign countries, passed almost unnoticed here and has not yet been marked by any official ceremony.[2]

How are we to explain this injustice to him, and to ourselves, if not as a minor consequence of that desperate eagerness which drives us to forget our own history, to hold it "in horror," in the words of Charles de Remusat? This sentiment today opens social anthropology to the possibility of losing Durkheim as it has already lost Gobineau and Demeunier.

And yet, my dear colleagues, those among you who share these distant memories will not contradict me if I recall that, around 1935, when our Brazilian friends wanted to explain to us the reasons which led them to choose French missions to organize their 1st universities, they always cited 2 names: 1st, of course, Pasteur, and after that Durkheim.

But in reserving these thoughts for Durkheim, I am carrying out another duty. No one would have appreciated more than Marcel Mauss an homage addressed to him at the same time as to the master of whom he was pupil and then successor. From 1931 to 1942, Marcel Mauss held the chair at the College of France consecrated to the study of society, and so brief was the passage in these halls of the unfortunate Maurice Halbwachs that it seems that one can, without being untruthful, consider that in creating a chair of social anthropology, it is Mauss's chair which you wanted to restore. In any case, I owe too much to Mauss's thought not to take pleasure in this notion.

To be sure, his chair was called "Sociology," for Mauss, who did so much (together with Paul Rivet) to make ethnology a science in its own right, had not completely succeeded by the 1930's. But to attest to the bond between our fields, it will suffice to recall that in Mauss's field ethnology took an ever growing place; that beginning in 1924, he proclaimed that the "place of sociology" was "in anthropology" (Mauss 1950c: 285); and that, if I am not mistaken Mauss was the 1st (in 1938) to introduce the term "social anthropology" into French terminology (Mauss 1950*d*:362). He would not disavow the term today.

* * *

1 "Inaugural Lecture" delivered on 5 I 60 at the Collège de France. The article was translated by Sherry Artner Paul and Robert Paul.

2 A commemoration took place at the Sorbonne on 30 I 60.

Even in his boldest advances, Mauss never felt that he departed from the Durkheimian line. Better than he, perhaps, we perceive today how, without betraying the fidelity so often affirmed, he knew how to simplify and soften the doctrine of his great precursor. This doctrine has never ceased to astonish us by its imposing proportions and its powerful logical framework, and by the perspectives which it opened onto horizons where so much remains to be explored. Mauss's mission was to finish and furnish the prodigious edifice sprung from the earth at the passage of the demiurge. He had to exorcise some metaphysical phantoms that were still trailing their chains in it, and shelter it once and for all from the icy winds of dialectic, the thunder of syllogisms, and the lightning flashes of antinomies. But Mauss secured the Durkheimian school against yet other dangers.

Durkheim was probably the 1st to introduce the requirement of specificity into the sciences of man, thereby making possible a renovation from which most of these sciences, and especially linguistics, benefited at the beginning of the 20th century. In all forms of human thought and activity, one cannot ask questions of nature or of origin before having identified and analyzed the phenomena and having discovered to what extent the relations which unite them suffice to explain them. It is impossible to discuss an object, to reconstruct the history which gave it its being, without knowing 1st *what it is;* in other words, without having exhausted the inventory of its internal determinations.

Yet when one rereads *The Rules of Sociological Method* today, one cannot help thinking that Durkheim has applied these principles with a certain partiality: he appeals to them in order to constitute the social as an independent category, but without recognizing that this new category, in its turn, entails all sorts of specificities corresponding to the different aspects in which we apprehend it. Before affirming that logic, language, law, art, and religion are projections of the social, would it not have been reasonable to wait until the particular sciences had thoroughly explored the mode of organization and the differential function of each of these codes, thus permitting the understanding of the nature of the relations among them?

At the risk of being accused of paradox, it seems to me that in the theory of the "total social fact" (so often praised and so poorly understood), the notion of totality is less important than the very special way in which Mauss conceived of it: foliated, one might say, and made up of a multitude of distinct yet connected planes. Instead of appearing as a postulate, the totality of the social is manifested in experience; these privileged instances which one can apprehend on the level of observation, in well-defined situations, when "the totality of society and its institutions . . . is set in motion." Now, this totality does not suppress the specific character of phenomena, which remain "at once juridical, economic, religious, and even aesthetic, morphological"; and so it consists finally in the network of functional interrelations among all these planes (Mauss 1950*b*:274).

This empirical attitude of Mauss's accounts for his so quickly overcoming the repugnance which Durkheim had felt from the beginning with respect to ethnographic investigation. "What counts," said Mauss, "is the Melanesian of such-and-such island . . ." (Mauss, 1950*b*:276). Against the theoretician, the observer should always have last word; and against the observer, the native. Finally, behind the rationalized interpretations of the native—who often makes himself observer and even theoretician of his own society—one will look for the "unconscious categories" which, Mauss wrote in 1 of his 1st works, are determinants "in magic, as in religion, as in linguistics" (Mauss 1950*a*:111). Now, this analysis in depth was to permit Mauss, without contradicting Durkheim (since it was to be on a new plane), to re-establish bridges—which at times had been imprudently destroyed—to the other sciences of man: to history, since the ethnographer deals in the particular; and also to biology and psychology, since he recognized that social phenomena are "first social, but also, and simultaneously, physiological and psychological." (Mauss 1950*c*:299). It will suffice to take the analysis far enough to attain a level where, again as Mauss said, "body, soul, society—everything merges" (Mauss 1950*c*:302).

This healthy sociology considers men as they are depicted by travelers and ethnographers who have partaken of their existence in a fleeting or in a lasting way. It shows them engaged in their own historical development, settled in a concrete, geographic space. It has, says Mauss, "as principle and as end . . . to perceive the entire group and the entire range of its behavior" (Mauss 1950*b*:276).

If disembodiment was 1 of the perils which lay in wait for Durkheimian sociology, Mauss protected it with equal success against another danger: "automatic explanation." Too often since Durkheim—and even among some of those who believe themselves to be liberated from his doctrinal grip—sociology has seemed like the product of a raid hastily made at the expense of history, psychology, linguistics, economics, law, and ethnography. To the booty of this pillage, sociology was content to add its label: whatever problem was posed to it, one could be assured of receiving a prefabricated "sociological" solution.

If we have not yet arrived at that state, we owe it in large part to Mauss and to Malinowski. At the same time, and no doubt aided by one another, they showed—Mauss as theoretician, Malinowski as experimenter—what could constitute proof in the ethnological sciences. They were the 1st to understand clearly that it was not enough to break down and dissect. Social facts do not reduce to scattered fragments. They are lived by men, and that subjective consciousness is as much a form of their reality as their objective characteristics.

While Malinowski was instituting uncompromising participation of the ethnographer in the life and thought of the natives, Mauss was affirming that what is essential "is the movement of all, the living aspect, the fleeting instant in which society becomes, or in which men become, sentimentally conscious of themselves and of their situation vis-à-vis others" (Mauss 1950*b*:275). This empirical and subjective synthesis offers the only guarantee that the preliminary analysis, carried as far as the unconscious categories, has let nothing escape.

Without a doubt, the proof will remain largely illusory: we will never know if the other, with whom we cannot, after all, identify, makes from the elements of his social existence a synthesis exactly superposable on that which we have worked out. But it is not necessary to go so far; all that is needed—and for this, inner feeling is sufficient—is that the synthesis, however approximate, arises from human experience. We must be sure of this, since we study men; and as we are ourselves men, we have that possibility. The way in which Mauss (1950:285) poses and resolves the problem in the *Essay on the Gift* brings to view, in the intersection of 2 subjectivities, the nearest order of truth to which the sciences of man can aspire when they confront the wholeness of their object.

Let us make no mistake: all this which seemed so new was implicit in Durkheim. He has often been reproached for having formulated, in the 2nd part of *The Elementary Forms of the Religious Life*, a theory of religion so vast and so general that it seemed to render superfluous the minute analysis of Australian religions which preceded it and—one hoped—paved the way for it.

The problem is to know if Durkheim the man could have arrived at this theory without being forced, at the outset, to superimpose upon the religious representations received from his own society those of men who historical and geographical evidence guarantees were entirely "others," not accomplices or unsuspected acolytes. Such is certainly the approach of the ethnographer when he goes into the field, because—however scrupulous and objective he may want to be—it is never either himself or the other whom he encounters at the end of his investigation. At most he can claim to extricate, by the superposition of himself on the other, what Mauss called the facts of general functioning, which he showed were more universal and had even more reality.

In thus completing the intention of Durkheim, Mauss liberated anthropology from the false opposition (introduced by thinkers such as Dilthey and Spengler) between explanation in the physical sciences and explanation in the human sciences. The search for causes ends with the assimilation of an experience, but this is at once external and internal. The famous rule to "consider social facts as things" corresponds to the 1st step, the search for causes, which are left to the 2nd to validate. We already discern the originality of social anthropology: it consists not in opposing causal explanation and understanding, but in bringing to light an object which may be at the same time objectively very remote and subjectively very concrete, and whose causal explanation may be based on that understanding which is, for us, but a supplementary form of proof. A notion like that of empathy inspires great mistrust in me, because it connotes irrationalism and mysticism. In his demand for additional proof, I prefer to imagine the anthropologist modeled after the engineer, who conceives and constructs a machine by a series of rational operations: it has to work; logical certainty is not enough. The possibility of trying the intimate experiences of another upon oneself is but 1 of the means at one's disposal for obtaining that final empirical satisfaction for which the physical sciences and the human sciences equally feel the necessity: it is less a proof, perhaps, than a guarantee.

* * *

What, then, is social anthropology? No one, it seems to me, was closer to defining it—if only by omission—than Ferdinand de Saussure, when, presenting linguistics as 1 part of a science yet to be born, he reserved for this science the name *semiology* and attributed to it as its object of study the life of signs at the heart of social life. Did he not, furthermore, anticipate our adherence when he compared language to "writing, to the alphabet of deaf-mutes, to symbolic rites, to forms of politeness, to military signals, etc."? (de Saussure 1960:33). No one would deny that anthropology counts within its own field at least some of these systems of signs, along with many others: mythic language, the oral and gestural signs of which ritual is composed, marriage rules, kinship systems, customary laws, and certain terms and conditions of economic exchange.

I conceive, then, of anthropology as the occupant in good faith of that domain of semiology which linguistics has not already claimed for its own, pending the time when for at least certain sections of this domain, special sciences are set up within anthropology.

It is necessary, however, to make this definition more precise in 2 ways.

First of all, I hasten to recognize that certain items which have just been cited are already within the scope of particular sciences: economics, law, political science. However, these disciplines examine the very facts which are closest to us and thus of particular interest. Let us say that social anthropology apprehends these facts, either in their most distant manifestations, or from the angle of their most general expression. From this latter point of view, anthropology can do nothing useful without collaborating closely with the particular social sciences; but these, for their part, would not know how to aspire to generality were it not for the cooperation of anthropology, which alone is capable of bringing them the accounts and the inventories which it seeks to render complete.

The 2nd difficulty is more serious, because one can ask oneself whether all the phenomena in which social anthropology is interested indeed manifest the character of symbols. This is sufficiently clear for the problems we study most frequently. When we consider some system of belief (let us say totemism), some form of social organization (unilineal clans, bilateral cross-cousin marriage), the question which we ask ourselves is, "What does all this mean?", and to answer it, we force ourselves to *translate* into our language rules originally stated in a different language.

But is this true of other aspects of social reality, such as tool-making, techniques, and modes of production and of consumption? It would seem that we are concerned here with objects, not with signs—the sign being, according to Peirce's celebrated definition, "that which replaces something for someone." What, then, does a stone axe replace, and for whom?

The objection is valid up to a certain point, and it explains the repugnance which some people feel toward admitting phenomena which come from other sciences, such as geography and technology, into the field of social anthropology. The term "cultural anthropology" will be appropriate, then, to distinguish this part of our studies.

However, it is well known—and it is 1 of Mauss's claims to fame that he established this, along with Malinowski—that in the societies with which we are concerned above all, but also in others, these domains are pregnant with meaning. From this point of view, they still concern us.

Finally, the intention of being exhaustive which inspires our researches broadly transforms their object. Techniques taken in isolation may appear as raw fact, historical heritage, or the result a of compromise between the needs of man and the constraints of environment. But when one puts them into that general inventory of societies which anthropology is trying to construct, they re-emerge in a new light, for we imagine them as the equivalents of choices which each society seems to make (I here use convenient language, which must be stripped of its anthropomorphism) among the possible ones which will constitute the complete list. In this sense, a certain type of stone axe can be a sign: in a given context, for the observer capable of understanding is use, it takes the place of the different implement which another society employs for the same purpose.

Consequently, then, even the simplest techniques of any primitive society have hidden in them the character of a system, analyzable in terms of a more general system. The manner in which some elements of this system have been retained and others excluded permits of conceiving of the local system as a totality of significant choices, compatible or incompatible with other choices, which each society, or each period within its development, has been led to make.

* * *

In admitting the symbolic nature of its object, social anthropology does not thus intend to cut itself off from *realia*. How could it do this, when art, in which all is sign, utilizes material media? One cannot study the gods without knowing their images; rites, without analyzing the objects and the substances which the officiant makes or manipulates; social rules independently of the things which correspond to them. Social anthropology does not confine itself to a part of the domain of ethnology; it does not separate material and spiritual culture. In the perspective which is its own, and in which we must find a place, it brings to each of them the same interest. Men communicate by means of symbols and signs; for anthropology, which is a conversation of man with man, everything is symbol and sign, when it acts as intermediary between 2 subjects.

By this deference toward objects and techniques, as well as by the conviction that we must work on meanings, social anthropology becomes appreciably different from Radcliffe-Brown's conception who—right up to his untimely death in 1955—did so much to give autonomy to our science.

According to the always marvelously clear views of the English master, social anthropology is to be an inductive science which, like other sciences of this type, observes facts, formulates hypnotheses, and submits these to the control of the experiment, in order to discover general laws of nature and society. It thus sets itself apart from ethnology, which tries to reconstruct the past of primitive societies, but with means and methods so precarious that it can teach social anthropology nothing.

When it was formulated, around 1920, this conception—inspired by the Durkheimian distinction between *circumfusa* and *praetrita*—marked a salutary reaction to the abuses of the diffusionist school. But, since then, "conjectural history," as Radcliffe-Brown called it, not without contempt, has perfected and refined its methods, thanks especially to stratigraphic excavations, the introduction of statistics into archaeology, the analysis of pollens, and the use of carbon-14, and above all the closer and closer collaboration between ethnologists and sociologists, on the 1 hand, and archaeologists and prehistorians, on the other. One may well ask oneself, then, if Radcliffe-Brown's mistrust of historical reconstructions did not correspond to a stage of scientific development which will soon have passed.

On the other hand, several of us hold more modest views on the future of social anthropology than those encouraged by the great ambitions of Radcliffe-Brown. These views picture social anthropology not on the model of the inductive sciences as they were conceived in the 19th century, but rather as a taxonomy, whose purpose is to identify and to classify types, to analyze their constituent parts, and to establish correlations between them. Without this preliminary work—and let us not deceive ourselves; it has barely been begun—the comparative method recommended by Radcliffe-Brown in fact risks being kept at a standstill: either the facts which one proposes to compare are so close to each other geographically or historically that one is never certain one is dealing with distinct phenomena, or they are too heterogeneous, and the comparison is illegitimate because it brings together things which one cannot compare.

Up until few years ago, we assumed that the aristocratic institutions of Polynesia were phenomena of recent introduction, the result of the arrival from elsewhere of small groups of conquerors scarcely a few centuries ago. But now the measurement of the residual radioactivity of organic remains from Melanesia and Polynesia reveals that the difference between the dates of occupation of the 2 regions is less than was supposed. All at once, the conceptions about the nature and unity of the feudal system must be modified; for at least in this part of the world, it can no longer be denied, after the fine work of Guiart, that some kind of feudalism existed prior to the arrival of the conquerors, and that certain forms of feudalism can arise in humble gardening societies (Guiart, 1963*a*, *b*).

The discovery in Africa of the art of Ifé, as refined and masterful as that of the European Renaissance, but perhaps earlier by 3 or 4 centuries, and much preceded in Africa itself by the art of the so-called Nok civilization, influences our conceptions of the recent arts of black Africa and the corresponding cultures. We are now tempted to see them as impoverished, rustic replicas of high art forms and high civilizations.

The shortening of the prehistory of the Old World and the lengthening of that of the New which carbon-14 has permitted us to recognize will perhaps lead us to judge that the civilizations which developed on the 2 sides of the Pacific were even more akin than it appears and to understand them differently, each in its own terms.

We must lean toward facts of this order before tackling any classification or comparison. For if we hasten to postulate the homogeneity of the social field, and if we cherish the illusion that it is immediately comparable in all its aspects and on all its levels, we will let the essential fact escape. We will fail to appreciate that the coordinates required for defining 2 apparently very similar phenomena are not always the same, or of the same number; and we will believe we are formulating laws of social nature when in fact we will only be describing superficial properties or enunciating tautologies.

To scorn the historical dimension on the pretext that the means are insufficient to evaluate it except approximately will result in our being satisfied with an impoverished sociology, in which the phenomena are disengaged, as it were, from their foundations. Rules and institutions, states and processes seem to float in a void in which one strains to spread a tenuous net of functional relations. One becomes wholly absorbed in this task, and one forgets the men in whose thought these relationships are established, one neglects their concrete culture, one no longer knows whence they came and what they are.

It is not sufficient, indeed, that phenomena can be called social in order for anthropology to be eager to claim them as its own. Espinas, another of the masters we allow ourselves the luxury of forgetting, was certainly right from the point of view of social anthropology when he refused to accept the notion that institutions shorn of their biological roots have the same coefficient of reality as other things: "The administration of a great railroad company," he wrote in 1901, "is not at all a social reality . . . nor is an army" (Espinas, 1901:I:470).

The statement is excessive, since administrations are the object of thorough studies in sociology, in social psychology, and in other particular sciences; but it helps us to specify the difference between anthropology and the preceding disciplines: the social facts which we study are manifested in societies each of which is *a total entity, concrete and cohesive*. We never lose sight of the fact that existing societies are the result of great transformations occurring in mankind at certain moments in prehistory and at certain places on the earth, and that an uninterrupted chain of real events relates these facts to those which we can observe.

The chronological and spatial continuity between the natural order and the cultural order upon which Espinas insisted strongly (in a language which is not our own and which, for that reason, we have sometimes poorly understood), is also the basis of Boas' historicism. It explains why anthropology, even social anthropology, proclaims itself to be joined in a common cause with physical anthropology, whose discoveries it watches for eagerly. For, even if social phenomena ought to be provisionally isolated from the rest and treated as if they arose from a specific level, we know well that in fact, and even rightfully so, the emergence of culture will remain a mystery to man: he will not succeed in determining, on the biological level, the modifications of the structure and functioning of the brain of which culture was both the natural result and the social mode of apprehension, and which at the same time created the intersubjective milieu indispensable for further transformations. These transformations, although certainly anatomical and physiological, can be neither defined nor studied with reference only to the individual.

* * *

This historian's profession of faith will come as a surprise, because I have at times been reproached for being closed to history and for giving it a negligible place in my works. I do not practice it much, but I am determined to reserve it its rights. I simply believe that in this formative period of social anthropology, nothing would be more dangerous than an unmethodical eclecticism which seeks to give the illusion of a finished science by confusing its tasks and mixing its programs.

Now it happens that in anthropology, experimentation precedes both observation and hypothesis. One of the peculiarities of the small societies which we study is that each constitutes a complete experiment, because of its relative simplicity and the limited number of variables required to explain its functioning. But on the other hand, these societies are alive, and we have neither the time nor the means to manipulate them. By comparison with the natural sciences, we benefit from an advantage and suffer an inconvenience: we find our experiments already prepared, but they are ungovernable. We thus are forced to substitute for them models, systems of symbols which preserve the characteristic properties of the experiment, but which we can manipulate.

The boldness of such an approach is, however, compensated for by the humility—one might almost say the servility—of observation as it is practiced by the anthropologist. Leaving his country, his hearth, for long periods; exposing himself to hunger, sickness, and sometimes danger; surrendering his habits, his beliefs, his convictions to a profanation to which he becomes an accomplice when, without mental reservation or ulterior motive, he assumes the forms of life of a strange society, the anthropologist practices total observation, beyond which there is nothing except—and it is a risk—the complete absorption of the observer by the object of his observations.

This rhythmic alternation between 2 methods—the deductive and the empirical—and the strictness with we which practice each in its extreme and purified form give social anthropology its distinctive character: of all the sciences, it is without a doubt unique in making the most intimate subjectivity into a means of objective demonstration. The same mind which has abandoned itself to the experience and allowed itself to be modeled by it becomes the theater of mental operations which, without suppressing the experience, nevertheless transform it into a model which makes possible further mental operations. In the last analysis, the logical coherence of these mental operations is based on the sincerity and honesty of him who can say, like the explorer bird of the fable, "I was there; such-and-such happened to me; you will believe it to be there yourself," and who in fact succeeds in communicating that conviction.

But this constant oscillation between theory and observation requires that the 2 planes always be distinguished. To return to history, it seems to me that the same holds true, whether one devotes oneself to the static or to the dynamic, to the order of structure or to the order of the event. The history of the historians does not need to be defended, but it is not attacking it to say (as Braudel admits) that next to a short time there exists a long time; that certain facts arise from a statistical and irreversible time, others from a mechanical and reversible time; and that the idea of a structural history cotains nothing which could shock historians (Braudel 1954). The 2 come together, and it is not contradictory that a history of symbols and signs engenders unforeseeable developments, even though it brings into play a limited number of structural combinations. In a kaleidoscope, each recombination of identical elements yields new results; but it is because the history of the historians is present—in the succession of flicks of the finger, as it were, which bring about the reorganization of the structure—and because the chances are practically nil that the same arrangement will appear twice.

I do not intend by this to take up again, in its original form, the distinction introduced in the *Course in General Linguistics* between the synchronic and the diachronic orders. From this aspect of the Saussurian doctrine, modern structuralism, along with Troubetzkoy and Jakobson, has most resolutely diverged; and recent documents show that the editors of the *Course* ... may at times have forced and schematized the master's thought (Godel 1957).

For the editors of the *Course in General Linguistics*, there exists an absolute opposition between 2 categories of fact: on the 1 hand, that of grammar, the synchronic, the conscious; on the other hand, that of the phonetic, the diachronic, the unconscious. Only the conscious system is coherent; the unconscious infra-system is dynamic and out of balance, composed at once of the legacy of the past and the tendencies of the future not yet realized.

In fact, Saussure had not yet discovered the presence of differential elements behind the phoneme. His position indirectly foreshadowed, on another plane, that of Radcliffe-Brown, who was convinced that structure is of the order of empirical observation, when in fact it is beyond it. This ignorance of hidden realities leads the 2 men to opposite conclusions. Saussure appears to deny the existence of a structure where it is not immediately given; Radcliffe-Brown affirms it but, seeing it where it is not, he deprives the notion of structure of its full force and significance.

In anthropology, as in linguistics, we know today that the synchronic can be as unconscious as the diachronic. In this sense the divergence between the 2 is already reduced.

On the other hand, the *Course in General Linguistics* sets forth relations of equivalence between the phonetic, the diachronic, and the individual, which form the domain of the *parole;* and the grammatical,

the synchronic, and the collective, which are the domain of the *langue*. But we have learned from Marx that the diachronic can also exist in the collective, and from Freud that the grammatical can be achieved entirely within the individual.

Neither the editors of the *Course* nor Radcliffe-Brown sufficiently realized that the history of systems of symbols includes logical evolutions which relate to different levels of structure and which it is necessary first to isolate. If a conscious system exists, it can only result from a sort of "dialectical average" among a multiplicity of unconscious systems, each of which concerns 1 aspect or 1 level of social reality. Now, these systems do not coincide either in their logical structures or in their historical affiliations. They are as if diffracted upon a temporal dimension, whose thickness gives the synchronism its consistency, and lacking which the synchronism would dissolve into a tenuous and impalpable essence, a phantom of reality.

It would thus not be going too far to suggest that in its oral expression, the teaching of Saussure must not have been very far from these profound remarks of Durkheim, which, published in 1900, seem to have been written today:

> Without a doubt, the phenomena which concern structure are somewhat more stable than functional phenomena, but between the 2 orders of facts there is only a difference of degree. Structure itself occurs in the process of becoming ... it is ceaselessly breaking down and being reconstituted; it is life arrived at a certain degree of consolidation; and to distinguish the life whence it derives from the life which it determines would be to dissociate inseparable things (Durkheim 1953:190).

* * *

In truth, it is the nature of the facts we study which leads us to distinguish within them that which belongs to structure and that which belongs to the process. As important as the historical perspective may be, we can only attain it at the end: after long researches which—as radiocarbon dating and palynology demonstrate—are not even always within our competence. On the other hand, the diversity of human societies and their number—several thousand still at the end of the 19th century—make it seem to us as if they were displayed in the present. It is not so astonishing if, responding to this solicitation of our object, we adopt a method of *transformations* rather than of *fluxions*.

As a matter of fact, there is a very close relationship between the idea of transformation and the idea of structure which is so important in my works. Radcliffe-Brown, inspired by the ideas of Montesquieu and Spencer, introduced the latter into social anthropology, to designate the durable manner in which individuals and groups are connected within the social body. For him, consequently, structure is of the order of a fact; it is given in the observation of each particular society. This view proceeds, no doubt, from a certain conception of the natural sciences, but one which would have already been unacceptable for a Cuvier.

No science can today consider the structures within its jurisdiction as reducing to just any arrangement of just any parts. Only that arrangement is structured which meets 2 conditions: that it be a system, ruled by an internal cohesiveness; and that this cohesiveness, inaccessible to observation in an isolated system, is revealed in the study of transformations, through which the similar properties in apparently different systems are discovered. As Goethe wrote:

All forms are similar, and none are the same,
So that their chorus points the way to a hidden law.

This convergence of scientific perspective is very comforting for the semiological science of which social anthropology is a part, since signs and symbols can only play their roles insofar as they belong to systems, regulated by internal laws of implication and exclusion, and since the property of a system of signs is to be transformable, in others words, *translatable,* into the language of another system with the aid of permutations. That such a conception could be born in palaeontology leads social anthropology to nourish a secret dream: it belongs to the human sciences, as its name adequately proclaims; but while it resigns itself to making its purgatory beside the social sciences, it surely does not despair of awakening among the natural sciences at the hour of the last judgment.

I shall attempt to show by 2 examples how social anthropology now endeavors to justify its program.

We know what function the incest prohibition fulfils in primitive societies. By casting the sisters and daughters out of the consanguineal group, so to speak, and by assigning them to husbands who belong to other groups, it creates bonds of alliance between these natural groups, the 1st such bonds which one can call social. The incest prohibition is thus the basis of human society, and in a sense it is the society.

To justify this interpretation, we did not proceed inductively. How could we have done so, with phenomena which are universally correlated, but among which different societies have invented all sorts of strange connections? Moreover, this is not a matter of facts but of meanings. The question we asked ourselves was that of the *meaning* of the incest prohibition (what in the 18th century was called its "spirit") and not its *results,* real or imaginary. It was necessary, then, to establish the systematic nature of each kinship terminology and its corresponding marriage rules. And this was possible only by elaborating the system of these systems and putting them into a relationship of transformations among themselves. From this point on, what had been nothing but an immense disorder was organized in the form of a grammar: terms constraining in all conceivable ways to set up and maintain a system of reciprocity.

This is where we are now. And now, how should we proceed to answer the next question, that of the universality of these rules in the totality of human societies, including contemporary ones? Even if we do not define the incest prohibition in terms of the form it takes among the Australians or the Amerindians, does the form it takes among us still have the same function? It could be that we are attached to it for very different reasons, such as the recent discovery of the harmful consequences of consanguineal unions. It could also be—as Durkheim thought—that the institution no longer plays a positive role among us and that it survives only as a vestige of obsolete beliefs, anchored in collective thought. Or, is it not rather the case that our society, a particular instance in a much vaster family, depends, like all others, for its cohesiveness and for its very existence on a network—grown infinitely unstable and complicated among us—of ties between consanguineal families? If so, is it necessary that the network be homogeneous in all its parts, or must we recognize therein types of structures differing according to environments or regions and variable as a function of local historical traditions?

These problems are essential for anthropology, since the response to them will determine the intimate nature of the social fact and its degree of plasticity. Now, it is impossible to settle this once and for all using methods borrowed from the logic of John Stuart Mill. We cannot vary the complex relationships—on the technical, economic, professional, political, religious, and *biological* planes—which a contemporary society presupposes. We cannot interrupt and re-establish them at will in the hope of discovering which ones are indispensable to the existence of the society as such, and which ones, if it had to, it could do without.

But we could choose the most complex and least stable of those matrimonial systems whose function of reciprocity is best established; we could then construct models of them in the laboratory to determine how they would function if they involved increasing numbers of individuals; we could also distort our models in the hope of obtaining models of the same type but even more complex and unstable; and we could compare the cycles of reciprocity thus obtained with the simplest cycles it is possible to observe in the field, e.g., in regions characterized by small isolated groups. By means of successive trips from laboratory to field, from field to laboratory, we would try gradually to fill in the void between the 2 series—1 known, the other unknown—by intercalating a series of intermediary forms. In the end, we would have done nothing but elaborate a language whose sole merits would be that it would be coherent, like all language, and that it would render an account of phenomena until then thought to be very different by the application of a small number of rules. In the absence of an inaccessible truth of fact, we would have arrived at a truth of reason.

* * *

The 2nd example relates to problems of the same type carried to another level: it will still be concerned with the incest prohibition, but no longer in the form of a system of rules—rather, in the form of a theme of mythic reflection.

The Iroquois and Algonquin Indians tell the story of a young girl exposed to the amorous enterprises of a nocturnal visitor whom she believes to be her brother. Everything seems to point to the guilty one: physical appearance, clothing, and the scratched cheek which bears witness to the virtue of the heroine. Formally accused by her, the brother reveals, that he has a counterpart, or more exactly, a double, for the tie between them is so strong that any accident befalling the 1 is automatically transmitted to the other. To convince his incredulous sister, the young man kills his double before her, but with the same blow he pronounces his own death sentence, since their destinies are linked.

Of course, the mother of the victim will want to avenge her son. Now, she is a powerful sorceress, the mistress of the owls. There is only 1 means of misleading her: that the sister marry her brother, the latter passing for the double he has killed. Incest is so inconceivable that the old woman never suspects the hoax. The owls are not fooled and denounce the guilty ones, but the latter succeed in escaping.

The Western listener easily perceives in this myth a theme established by the Oedipus legend: the precautions taken to avoid incest in fact make it ineluctable; in both cases the sensational turn of events is that 2 characters at 1st presented as distinct turn out to be identical. Is this simply a coincidence—different causes explaining the fact that the same motifs are arbitrarily found together—or does the analogy have more profound bases? In making the comparison, have we not put our finger on a fragment of a meaningful whole?

If so, the incest between brother and sister of the Iroquois myth would constitute a permutation of the Oedipal incest between mother and son. The contingency rendering the former inevitable—the double personality of the hero—would be a permutation of the double identity of Oedipus—supposed dead and nevertheless living, condemned child and triumphant hero. To complete the demonstration, it would be necessary to discover in the American myths a transformation of the episode of the sphinx, which is the only element of the Oedipus legend still lacking.

Now, in this particular case (and hence we have chosen it in preference to others), the proof would be truly decisive, since, as Boas was the 1st to point out, riddles or puzzles, along with proverbs, are rather rare among the North American Indians. If puzzles were to be found in the semantic framework of the American myth, then, it would be not the effect of chance, but the proof of a necessity.

In North America, 2 puzzle situations are found whose origins are incontestably indigenous: (1) among the Pueblo Indians of the southwestern United States, there exists a family of ceremonial clowns who pose riddles to the spectators and whom myths describe as having been born of an incestuous union; and (2) precisely among the Algonquin themselves, there are myths in which owls, or sometimes the ancestor of owls, pose riddles to the hero which he must answer under pain of death; and it will be remembered that the sorceress in the myth summarized here is a mistress of owls. Consequently, in America too, riddles present a double Oedipal character: by way of incest, on the 1 hand, and by way of the owl, in which we are led to see a transposed form of the sphinx, on the other.

The correlation between riddle and incest thus seems to obtain among peoples separated by history, geography, language, and culture. In order to set up the comparison, let us construct a model of the riddle, expressing as best we can its constant properties in the different mythologies. Let us define it, from this point of view, as *a question to which one postulates that there is no answer*. Without considering here all the possible transformations of this statement, let us simply invert its terms by way of an experiment producing: *an answer for which there is no question.*

This is, apparently, a formula completely devoid of sense. And yet, it springs to mind that there are myths, or fragments of myths, which derive their dramatic force from this structure—a symmetrical inversion of the other. Time is too limited for me to recount the American examples. I will therefore restrict myself to reminding you of the death of the Buddha, rendered inevitable because a disciple fails to ask the expected question. Closer to home, there are the old myths adapted in the Holy Grail cycle, in which the action hangs on the timidity of the hero in the presence of the magic vessel, of which he does not dare to ask, "What good is it?"

Are these myths independent, or must they be considered in turn as a species of a vaster genus, of which Oedipal myths constitute only another species? Repeating the preceding step, we will look to see if, and to what extent, the characteristic elements of 1 group can be reduced to permutations (which will here be inversions) of the characteristic elements of the other group. And that indeed is what is produced: from a hero who misuses sexual intercourse (since he carries it as far as incest), we pass to a chaste man who abstains from it; a shrewd person who knows all the answers is replaced by an innocent who does not even know that he should ask questions. In the American variants of this 2nd type, and in the Holy Grail cycle, the problem to be resolved is that of the "*gaste pays*," that is to say, the lost summer. Now, all the American myths of the 1st or "Oedipal," type refer to an eternal winter which the hero dispels when he solves the puzzles, thereby bringing on the summer. Simplifying a great deal, Perceval then appears as an inverted Oedipus—a hypothesis we would not have dared to consider had it been necessary to compare a Greek with a Celtic source, but which is forced upon us in a North American context, where the 2 types are present in the same populations.

However, we are not at the end of our demonstration. We have verified that, at the heart of the semantic system, chastity and "the answer without a question" are in a relationship to that of incestuous intercourse and "the question without an answer"; therefore we must also admit that the 2 stated in sociobiological terms are themselves in a homologous relationship with the 2 stated in grammatical terms. Between the puzzle solution and incest there exists a relationship, not external and of fact, but internal and of reason, and that indeed is why civilizations as different as those of classical antiquity and indigenous America can independently associate the 2. Like the solved puzzle, incest brings together elements sworn to remain separate: the son marries the mother, the brother marries the sister, *in the same way in which the answer succeeds, contrary to all expectation, in rejoining its question.*

In the legend of Oedipus, then, the marriage with Jocasta does not arbitrarily follow the victory over the sphinx. Besides the fact that myths of the Oedipal type by definition always assimilate the discovery of incest to the solution of a living puzzle personified by the hero, their various episodes are repeated on different planes and in different languages and provide the same demonstration which one finds in an inverted form in the old myths of the Holy Grail: the audacious union of masked words or of consanguines concealed from themselves engenders decay and fermentation, the unchaining of natural forces—one thinks of the Theban plague—just as impotence in sexual substance dries up animal and vegetable fertility (as well as preventing a proposed dialogue).

In the face of the 2 perspectives which might capture the imagination—an equally eternal summer or winter, the former licentious to the point of corruption, the latter pure to the point of sterility—man must resign himself to preferring the equilibrium and the periodicity of the seasonal rhythm. In the natural order, the latter fulfils the same function which is fulfilled on the social plane by the exhange of women in marriage and the exchange of words in conversation, when these are practiced with the frank intention of communicatting, that is to say, without ruse or perversity, and above all, without hidden motive.

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I have been satisfied simply to sketch in the broad outlines of a demonstration—which will be taken up again in detail at some future time—to illustrate this *problem of invariance* which social anthropology seeks to resolve. The other sciences are concerned with this problem too, but for anthropology it seems like the modern form of a question which it has always asked itself—that of the universality of human nature. Do we not turn our back on this human nature when, in order to sift out our invariants, we replace the givens of experience with models upon which we are free to perform abstract operations as the algebraist does with his equations? I have sometimes been reproached for this, but, apart from the fact that the objection carries little weight with the expert—who knows with what fastidious fidelity to concrete reality he pays for the liberty of skimming for a few brief moments—I would like to recall that in proceeding as it does, social anthropology is only reassuming responsibility for a forgotten part of the program which Durkheim and Mauss mapped out.

In the preface to the 2nd edition of *The Rules of Sociological Method,* Durkheim defends himself against the charge of having unjustifiably separated the collective from the individual. This separation, he says, is necessary, but it does not preclude the possibility that in the future

> we will come to conceive of the possibility of a completely formal psychology which would be a sort of common ground of individual psychology and sociology . . . . what would be necessary would be to seek, by the comparison of mythic themes, legends, popular traditions, and languages, in what way social representations call for each other or are mutually exclusive, merge with one another or remain distinct . . . (Durkheim, 1960:viii-xix).

This research, he noted in closing, is on the whole under the jurisdiction of abstract logic. It is curious to note how close Lévy-Bruhl could have come to this program if he had not chosen at the outset to relegate mythic representations to the antechamber of logic, and if he had not rendered the separation irremediable when he later renounced the notion of prelogical thought. In so doing, he was simply throwing out, as the English say, the baby with the bath: he denied to the "primitive mentality" the cognitive character which he had initially conceded to it, and cast it back entirely into the realm of emotion.

More faithful to the Durkheimian conception of an "obscure psychology" underlying social reality, Mauss orients anthropology "toward the study of what is common to men. . . . Men communicate by symbols . . . but they can only have these symbols and communicate by them because they have the same instincts" (Mauss 1950*c*:296).

Does not such a conception, which is also my own, expose one's flank to another criticism? If your final goal, someone will say, is to arrive at certain universal forms of thought and morality (for the *Essay on the Gift* ends with conclusions on morals), why give a privileged status to the societies which you call primitive? Shouldn't one in theory arrive at the same results starting from any society? This is the last problem which I would like to consider here.

This is all the more vital since some ethnologists and sociologists who study societies in rapid transformation will perhaps dispute the conception which I seem implicity to hold of primitive societies. Their putative distinctive character, they may believe, verges on an illusion which is the effect of our present ignorance of what is actually going on; objectively, they do not correspond to reality.

Without a doubt, the character of ethnographic investigations is changing as the little savage tribes we used to study disappear; these investigations are coming to rely on vaster wholes in which the problems tend to resemble our own. But if it is true, as Mauss taught us, that ethnology is an original mode of knowledge rather than a source of particular bits of knowledge, we can only conclude that today ethnology is conducted in 2 ways: in the pure state and in the diluted state. To seek to develop it where its method is mixed with other methods, where its object is confused with other objects, is not the course of action resulting from a sound scientific attitude. This chair will therefore be consecrated to pure ethnology, which does not mean that its teaching cannot be applied to other ends, nor that it is not interested in contemporary societies, which, at certain levels and under certain aspects, are immediately relevant for ethnological method.

What, then, are the reasons for our predilection for those societies which, in the absence of a better term, we call primitive, although they certainly are not that?

The 1st reason, let us frankly acknowledge, is of a philosophic order. As Merleau-Ponty has written,

> each time the sociologist [*but it is the anthropologist he means*] returns from the living sources of his knowledge to that which operates in him as a means of understanding the cultural formations most remote from himself, he spontaneously makes philosophy (Merleau-Ponty 1960:138).

In fact, the field research with which every ethnologic career begins is mother and nurse of doubt, the philosophic attitude *par excellence.* This "anthropological doubt" does not only consist of knowing that one knows nothing, but of resolutely exposing that which one thought one knew, and indeed one's very ignorance, to the insults and contradictions which are directed at one's most cherished ideas and habits by those who can contradict them to the highest degree. Contrary to appearances, I think it is by its more strictly philosophic method that ethnology is distinguished from sociology. The sociologist objectivizes for fear of being duped. The ethnologist does not experience this fear, since he is not immediately concerned by the distant society which he studies and since he is not compelled in advance to extract all its nuances, all its details, and even its values—in a word, all that in which the observer of his own society risks being implicated.

However, in choosing a subject and an object radically distant from one another, anthropology runs a risk: that the knowledge obtained from the object does not reach its intrinsic properties but is limited to expressing the relative and always changing position of the subject in relation to that object. It is highly possible, indeed, that so-called ethnological knowledge is condemned to remain as bizarre and inadequate as that which an exotic visitor would have of our own society. The Kwakiutl Indian whom Boas sometimes invited to New York to serve him as an informant was indifferent to the spectable of skyscrapers and streets lined with automobiles. He reserved all his intellectual curiosity for the dwarfs, giants, and bearded ladies which were at that time exhibited in Times Square, for automats, and for the brass balls decorating staircase bannisters. For reasons which I cannot go into here, all these things challenged his own culture, and it was that culture alone which he was seeking to recognize in certain aspects of ours.

In their own way, do not ethnologists succumb to the same temptation when they permit themselves, as they so often do, to interpret indigenous customs and institutions in the light of new charges, with the unacknowledged goal of making them fall into line better with the theories of the day? The problem of totemism, which some of us hold to be transparent and insubstantial, has weighed upon ethnographic thought for years, and we understand now that its importance proceeds from a certain taste for the obscene and the grotesque which is for the science of religion like a childhood disease: a negative projection of an uncontrollable fear of the sacred from which the observer has not been able to disengage himself. Thus the theory of totemism is constructed "for us," not "in itself," and nothing guarantees that in its current forms it does not still proceed from a similar illusion.

The ethnologists of my generation are disconcerted by the repulsion inspired in Frazer by the research to which he had dedicated his life: "tragic chronicles," he wrote of the errors of man, "foolish, vain efforts, lost time, frustrated hopes" (Frazer 1936:vi). We are hardly less surprised to learn from the *Notebooks* how a Lévy-Bruhl considered myths, which according to him "no longer have any effect on us . . . strange narratives, not to say absurd and incomprehensible . . . it costs us an effort to take an interest in them . . . ." Of course, we have acquired direct knowledge of exotic forms of life and thought which our precursors lacked; but is it not also the case that surrealism—an internal development of our own society—has transformed our sensitivity, and that we are indebted to it for having discovered or rediscovered at the heart of our studies a certain lyricism and integrity?

Let us then resist the seductions of a naïve objectivism, but without failing to recognize that, by its very precariousness, our position as observer brings us unhoped-for assurances of objectivity. It is in the degree to which the so-called primitive societies are distant from our own that we can discover in them those "facts of general functioning" of which Mauss spoke, which have the chance of being "more universal" and of having "more of reality" (Lévy-Bruhl 1949:200). In these societies—and I am still quoting Mauss—"one grasps men, groups, and behavior . . . one sees them driven as in piece of machinery . . . one sees masses and systems" (Mauss 1950*b*:276). This observation, privileged by distance, no doubt implies certain differences in nature between these societies and our own: astronomy demands not only that the celestial bodies be distant, but also that the passage of time there have a different rhyhm; otherwise, the earth would have ceased to exist long before astronomy was born.

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Of course the so-called primitive societies exist in history; their past is as old as ours, since it goes back to the origin of the species. In the span of thousands of years they have undergone all sorts of transformations; they have gone through periods of crisis and of prosperity; they have known wars, migrations, adventure. But they have specialized in ways different from those which we have chosen. Perhaps they have, in certain respects, remained closer to very ancient condtions of life, but this does not preclude the possibility that in other respects they are farther from those conditions than we are.

Although they exist in history, these societies seem to have elaborated or retained a particular wisdom which incites them to resist desperately any modifications of their structure which would permit history to invade their midst. Those which have best protected their distinctive character appear to be societies which inspire in their members a predominant concern for persevering in their existence. The way in which they exploit the environment guarantees both a modest standard of living and the conservation of natural resources. Their marriage rules, though diverse, manifest to the eye of the demographer a common function, namely, to set the upper limit on the fertility rate and to keep it constant. Finally, a political life based on consent and admitting of no decisions other than those unanimously arrived at seems conceived to preclude the possibility of employing that driving force of collective life which takes advantage of the contrast between power and opposition, majority and minority, exploiters and exploited.

In a word, these societies, which are "cold" in that their internal environment is near the zero of historical temperature, are, by their limited total manpower and their mechanical mode of functioning, distinguished from the "hot" societies which appeared in different parts of the world following the Neolithic revolution and in which differentiations between castes and between classes are urged unceasingly in order to extract energy from them.

The value of this distinction is mainly theoretical, because probably no concrete society exists which corresponds exactly to 1 or the other type. And in another sense also the distinction remains relative, if it is true, as I believe, that social anthropology responds to a double motivation: retrospective, since the various types of primitive life are on the point of disappearing and we must hasten to cull our lessons from them; and propective, to the extent that, being conscious of an evolution whose tempo is ever increasing, we feel ourselves already the "primitives" of our great-grandchildren, and to the extent that we seek to validate ourselves by drawing closer to those who were—and still are, for a brief moment—like a part of us which continues to exist.

On the other hand, neither do those societies which I have called "hot" have this character in the absolute. When, on the morrow of the Neolithic revolution, the great city-states of the Mediterranean Basin and of the Far East imposed slavery, they constructed a type of society in which the differential statuses of men—some dominant, others dominated—could be used to produce culture at a rate until then inconceivable and unthought of. By the same logic, the mechanistic revolution of the 19th century represents less an evolution oriented in the same direction, than a rough sketch of a different solution: though for a long time it remained based on the same abuses and injustices, yet it made possible the transfer to *culture* of that dynamic function which the protohistoric revolution had assigned to *society*.

If—Heaven forbid!—it were expected of the anthropologist that he predict the future of humanity, no doubt he would not conceive of it as a continuation or a projection of present forms, but rather on the model of an integration, progressively unifying the appropriate characteristics of the "cold" societies and the "hot" ones. His thought would retie the thread with the old Cartesian dream of putting machines, like automatons, in the service of man; it would follow a trail through the social philosophy of the 18th century and up to Saint-Simon. The latter, in announcing the passage "from the government of men to the administration of things," anticipated at the same time the anthropological distinction between culture and society, and that conversion of which the advances of information theory and electronics gives us at least a glimpse: the conversion of a type of civilization which inaugurated historical development at the price of the transformation of men into machines into an ideal civilization which could succeed in turning machines into men. Then, culture having entirely taken on the burden of manufacturing progress, society would be freed from the millenial curse which has compelled it to enslave men in order that there be progress. Henceforth, history would make itself by itself, and society, placed outside and above history, would be able to assume once again that regular and, as it were, crystalline structure which the best-preserved of primitive societies teach us is not contradictory to humanity. In this perspective, utopian as it is, social anthropology would find its highest justification, since the forms of life and thought which it studies would no longer have a purely historical or comparative interest: they would correspond to a permanent hope for mankind, which social anthropology, particularly at the darkest hours, would have a mission to keep watch over.

Our science would not have been able to mount this watchful guard—and would not even have conceived of the importance and the necessity of it—if, in the remote regions of the earth, men had not obstinately resisted history, and if they had not remained as living examples of that which we want to preserve.

* * *

In conclusion, I would very much like to evoke in a few words the very exceptional emotion which the anthropologist feels when he enters a house in which tradition, uninterrupted for 4 centuries, goes back to the reign of Francis I. Especially if he is an Americanist, many bonds attach him to that era, in which Europe received the revelation of the New World and was opened to ethnographic knowledge. He would have wanted to live then—indeed, he lives there every day in thought. And because, remarkably, the Indians of Brazil (where I went through my 1st field campaign) could have adopted as a motto, "I will stay put," it happens that the study of them takes on a double quality: that of a trip to a distant land, and—even more mysterious—that of an exploration of the past.

But for this reason also—and remembering that the mission of the College of France has always been to teach science in the making—I am touched by the hint of a regret: why was this chair created so late? How does it happen that ethnography did not receive its place when it was still young, and when the facts still retained their richness and freshness? For it is in 1558 that one likes to imagine this chair established, when Jean de Léry, returning from Brazil, drafted his 1st work, and when André Thevet's *The Singularities of French Antarctica* appeared.

Certainly social anthropology would be more respectable and more self-assured if official recognition had come at the moment when it was beginning to outline its projects. However, supposing that all had happened thus, anthropology would not be what it is today: a restless and fervent study which plagues the investigator with moral as well as scientific questions. It was perhaps in the nature of our science that it appeared as an effort to make up for lost time and at the same time as a reflection on a backlog to which certain of its fundamental traits should be attributed.

If society is in anthropology, anthropology is itself in society: it has been able to enlarge progressively the object of its study to the point of including therein the totality of human societies; it has, however, appeared at a late period in their history and in a small sector of the inhabited world. More than that, the circumstances of its appearance are comprehensible only in the context of a particular social and economic development: one suspects then that they are accompanied by a seizure of conscience—almost of remorse—that humanity could have remained alienated from itself for such a long time, and above all, that that fraction of humanity which produced anthropology should be the same fraction of humanity which has made so many other men the objects of execration and contempt. "Sequels to colonialism," it is sometimes said of our investigations. The 2 are certainly linked, but nothing would be more false than to hold anthropology to be a throwback to the colonial frame of mind, a shameful ideology which would offer colonialism a chance of survival.

What we call the Renaissance was a veritable birth for colonialism and for anthropology. Between the 2, confronting each other from the time of their common origin, an equivocal dialogue has been maintained for 4 centuries. If colonialism had not existed, the rise of anthropology would have been less belated; but perhaps also anthropology would not have been led to implicate all mankind in each of its particular examples. Our science arrived at maturity the day that Western man began to see that he would never understand himself as long as there was a single race or people on the surface of the earth that he treated as an object. Only then could anthropology affirm itself as what it is: an enterprise renewing and atoning for the Renaissance, in order to spread humanism to all humanity.

Having rendered homage to the masters of social anthropology at the beginning of this lecture, let me reserve my last words for those savages whose obscure tenacity still offers us a means of assigning to human facts their true dimensions. Men and women who, as I speak, thousands of miles from here on some savannah ravaged by brush fire, or in some forest dripping with rain, are returning to camp to share a meager pittance and to invoke their gods together; those Indians of the tropics and their counterparts throughout the world who have taught me their poor knowledge (in which resides, nevertheless, the essence of the knowledge which you have charged me to transmit to others); soon, alas, they are all destined for extinction under the impact of illnesses and—for them even more horrible—modes of life which we have brought them. To them I have incurred a debt which I can never repay, even if in the place in which you have put me I were able to give proof of the tenderness which they inspire in me and the gratitude which I feel toward them by continuing to be as I was among them, and as, among you, I would hope never to cease being: their pupil and their witness.

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OUR READERS WRITE

(*Continued from page 109*)

Review to appear before the actual publication date of the book?

HELMUTH FUCHS
Caracas, Venezuela

[Editor's note: If copies or proofs of a book are available long enough in advance, a review might be developed before the book's publication date.]

The June 1965 issue of CA was extremely interesting, especially from the ethnological point of view. The paper "Alcohol and Culture" by D. G. Mandelbaum (CA 6:281) and the comments on it are very important contributions: I would suggest that research on this topic be organized in individual cultures. In my opinion, the discussion "European vs. American Anthropology" (CA 6:303) should be continued. It would be very beneficial to our science and would promote better understanding among anthropologists (ethnologists) throughout the world.

MILENKO S. FILIPOVIĆ
Belgrad, Yugoslavia

On the debate concerning "European vs. American Anthropology" (CA 6:303) it may interest you to know that at the London University Institute of Archaeology, students take a course called Anthropology for Archaeologists and at University College (just across the way) anthropologists take a course in Archaeology for Anthropologists. As one who took the former, I can pay tribute to its interest and value.

JAMES H. CHAPLIN
Kampala, Uganda

Reprinting Associates' Letter

[Reprinting the original Letters to Associates (6:247) makes relevant new comments on some older matters.]

Because of the importance of such studies for understanding man's cultural history, I think CA should undertake to coordinate research on prehistoric and protohistoric migration routes of hominid groups.

Regarding arguments *ad hominem* (CA 5:77; 6:265), I think that politeness is essential for human relations and should always be maintained, even in heated discussions. The Editor should be able to delete expressions that are not conducive to better understanding.

ARTUR HEHL NEIVA
Rio de Janeiro, Brazil

That the replies from Associates decrease is, I think, normal (CA 6:279). In the beginning there is much interest, but as time goes on many find that they no longer have time enough in their daily schedules for taking part in the work of the community; and the small Reply Letters become a burden. In this way the "real" Associates remain, and those who overestimated their interest lose contact with the Editor. I think there is nothing that could be done about this. After some time there will be more stability when only "real" Associates are left.

The percentage of linguists among Associates is rather low. It is difficult to believe that the linguists of the world could lack interest in CA. If they are not represented adequately among Associates, I feel this must be due to traditional barriers between the disciplines. Outside America CA invited ethnologists chiefly, and to a certain degree archaeologists, physical anthropologists, and palaeontologists. As no complete study of man is possible without linguists, something should perhaps be done about reaching them.

RADOSLAV KATIČIĆ
Zagreb, Yugoslavia

I believe it to be a good idea to publish private travel plans (CA 6:247). I would be glad to see Associates here in Berlin.

I too received racist materials from "The International Association for the Advancement of Ethnology and Eugenics, Inc." (CA 6:250). I proposed in a letter to Mr. McGregor to discuss his attacks publicly by an exchange of articles, and he agreed. I sent such an article, but now he is silent.

B. BRENTJES
Halle/Saale, Germany

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7

Anthropology: Its Achievements and Future[1]

by Claude Lévi-Strauss

R. IX (6)

AMONG MY MANY CHERISHED recollections of the years I spent in the United States, 1 remains outstanding because it is associated with what, due to my inexperience, appeared to me as something of a discovery. This apparent discovery took place quite casually one day, when I stumbled upon a bookstore which specialized in secondhand government publications and where could be bought, for $2 or $3 apiece, most of the *Annual Reports* of the Bureau of American Ethnology.

I can hardly describe my emotion at this find. That these sacrosanct volumes, representing most of what will remain known about the American Indian, could actually be bought and privately owned was something I had never dreamed of. To my mind, they belonged rather to the same irredeemable past as the beliefs and customs of which they spoke. It was as though the civilization of the American Indian had suddenly come alive through the physical contact that these contemporary books established between me and their time. Although my financial resources were scant and $3 represented all I had to spend on food for the same number of days, this sum seemed negligible when it could pay for 1 of these marvelous publications: Mallery's *Pictographs*, Matthews' *Mountain Chant*, Fewkes's *Hopi Katcinas*, or such treasure troves of knowledge as Stevenson's *Zuni Indians*, Boas' *Tsimshian Mythology*, Roth's *Guiana Indians*, and Curtin and Hewitt's *Seneca Legends*.

Thus it happened that, volume after volume, at the cost of some privations, I built up an almost complete set (there is still 1 volume missing) of *Annual Reports* 1-48, which belong to the "great period" of the Bureau of American Ethnology. At that time, I was far from imagining that a few months later I would be invited by the Bureau to become a contributor to 1 of its major undertakings: the 7-volume *Handbook of South American Indians*.

Notwithstanding this close association and the years that have since elapsed, the work of the Bureau of American Ethnology has lost for me none of its glamour, and I still feel toward it an admiration and respect which are shared by innumerable scholars the world over. Since it so happens that in the same year that marks the 200th Anniversary of James Smithson, the life of the Bureau has come to an end (though its activities are carried on under a new guise), the time may be fitting to pay tribute both to the memory of the founder of the Smithsonian Institution and to the Bureau which has been one of its greatest achievements.

Ever since it was founded in 1879 (emancipating ethnology from geography and geology, with which it had until then been merged), not only did the Bureau avail itself fully of the amazing opportunity provided by the presence of scores of native tribes at a few hours' or days' travel from the great cities, but also "the accounts of custom and culture published by the Bureau compare in thoroughness and quality of reporting with modern ethnographic studies" (Lienhardt 1964:24). We are indebted to the Bureau for instituting standards of scholarship that still guide us, even though we but rarely succeed in attaining them.

Above all, the collection of native texts and factual observations contained in the 48 major *Reports* and certain of the subsequent ones, in the 200 or so *Bulletins*, and in the *Miscellaneous Publications* is so impressive that after nearly a century of use only the surface of it has been scratched. This being the case, one can only wonder at the neglect in to which this invaluable material has temporarily fallen. The day will come when the last primitive culture will have disappeared from the earth, compelling us to realize only too late that the fundamentals of mankind are irretrievably lost. Then, and for centuries to come, as happened in the case of our own ancestral civilizations, hosts of scholars will devote themselves to reading, analyzing, and commenting upon the publications of the Bureau of American Ethnology, which preserve so much more than has been preserved of other bygone cultures (not to mention the unpublished manuscripts placed in the Bureau's custody). And, if ever we succeed in enlarging our narrow-minded humanism to include each and every expression of human nature, thereby perhaps ensuring to mankind a more harmonious future, it is to undertakings such as those of the Bureau of American Ethnology that we shall owe it. However, nothing could be farther from my mind than the notion that the work of the Bureau belongs to the past; I believe, on the contrary, that all of us, together with its legal successor, the Office of Anthropology, should seek in these achievements a living inspiration for the scientific task ahead of us.

It has become the fashion in certain circles to speak of anthropology as a science on the wane, on account of the rapid disappearance of its traditional subject matter: the so-called primitives. Or else it is claimed that in order to survive, anthropology should abandon

1 Remarks at the bicentennial celebration commemorating the birth of James Smithson, Smithsonian Institution, Washington, D.C., 17 IX 65. To be published in *Knowledge among Men*, New York, 1966, Simon & Schuster.

fundamental research and become an applied science, dealing with the problems of developing countries and the pathological aspects of our own society. I should not want to minimize the obvious interest of these new researches, but I feel, nevertheless, that there is, and will remain for a long time to come, much to be done along more traditional lines. It is precisely because the so-called primitive peoples are becoming extinct that their study should now be given absolute priority.

It is not too late for anthropologists to set to work. As early as 1908, Sir James Frazer, in his inaugural lecture at Liverpool University, stated that classical anthropology was nearing its end. What have we witnessed instead? Two great wars, together with scientific development, have shaken the world and destroyed physically or morally a great many native cultures; but this process, however disastrous, has not been entirely 1-way. The 1st World War gave rise indirectly to Malinowski's new anthropology by obliging him to share the life of the Trobriand Islanders in a more durable and intimate manner than, perhaps, he would have done otherwise. And as a consequence of the 2nd World War, anthropologists were given access to a new world: the New Guinea highlands, with a population of 600-800,000 souls whose institutions are changing our traditional outlook on many theoretical problems. Likewise, the establishment of the new federal capital of Brazil and the building of roads and aerodromes in remote parts of South America have led to the discovery of small tribes in areas where no native life was thought to exist.

Of course, these opportunities will be the last. Moreover, the compensation they afford is small indeed, compared with the high rate of extinction afflicting primitive tribes the world over. There are about 40,000 natives left in Australia as opposed to 250,000 at the beginning of the 19th century, most, if not all, of them hungry and disease-ridden, threatened in their deserts by mining plants, atom bomb test grounds, and missile ranges. Between 1900 and 1950, over 90 tribes have been wiped out in Brazil; there are now barely 30 tribes still living in a state of relative isolation. During the same period, 15 South American languages have ceased to be spoken. Scores of similar examples could be given.

Yet, this is no reason to become discouraged. It is undoubtedly true that we have less and less material to work with. But we can compensate to some extent for this diminishing volume by putting it to better use, thanks to our greater theoretical and factual knowledge and more refined techniques of observation. We have not much left to work with, but we will manage to "make it last." We have learned how to look for the cultural "niches" in which traditional lore finds refuge from the impact of civilization: language, kinship, ethnobotany, ethnozoology, and the like.

But although the physical disappearance of populations that remained faithful till the very end to their traditional way of life does, indeed, constitute a threat to anthropology, curiously enough, a more immediate threat comes from an evolution that has been taking place in such parts of the world as Asia, Africa, and the American Andes, which used to be considered within the realm of anthropological studies. The population density of these regions was always high, and it shows no sign of decreasing; quite the contrary. The new threat to our studies is not, then, so much quantitative as qualitative: these large populations are changing fast, and their culture is resembling more and more that of the Western world. Like the latter, it tends to fall outside the field of anthropology. But this is not all, for the mere fact of being subjected to ethnographic investigation seems distasteful to these peoples, as though by studying the ways in which their old beliefs and customs differed from our own we were granting these differences an absolute status and conferring upon them a more enduring quality.

Contemporary anthropology thus finds itself in a paradoxial situation. For it is out of a deep respect for cultures other than our own that the doctrine of cultural relativism evolved; and it now appears that this doctrine is deemed unacceptable by the very people on whose behalf it was upheld, while those ethnologists who favour unilinear evolutionism find unexpected support from peoples who desire nothing more than to share in the benefits of industrialization, and who prefer to look at themselves as temporarily backward rather than permanently different.

Hence the distrust in which traditional anthropology is held nowadays in some parts of Africa and Asia. Economists and sociologists are welcome, while anthropologists are tolerated at best and from certain areas are simply banned. Why perpetuate, even in writing, old usages and customs which are doomed to die? The less attention they receive, the faster they will disappear. And even should they not disappear, it is better not to mention them lest the outside world realize that one's culture is not as fully abreast with modern civilization as one deludes oneself in believing it to be. There have been periods in our own history when we too have yielded to the same delusion, only to find ourselves struggling to regain balance after eradicating so recklessly our roots in the past. Let us hope that this dire lesson will not be lost on others. The question is, in effect: What can we do to keep the past from being lost? Is there a way of making peoples realize that they have a tremendous responsibility toward themselves and toward mankind as a whole not to let perish before it has been fully recorded this past which it is their unprecedented privilege to experience on a par with their incipient future? The suggestion has been made that in order to render anthropology less distasteful to its subjects it will suffice to reverse the roles and occasionally allow ourselves to be "ethnographized" by those for whom we were once solely the ethnographers. In this way, each in turn will get the upper hand. And since there will be no permanent privilege, nobody will have grounds to feel inferior to anybody else. At the same time, we shall get to know more about ourselves through the eyes of others, and human knowledge will derive an ever growing profit from this reciprocity of perspective.

Well-meant as it undoubtedly is, this solution appears to me naïve and unworkable, as though the problems were as simple and superficial as those of children unaccustomed to playing together, whose quarrels can be settled by making them follow the elementary rule: "Let me play with your dolls and I shall let you play with mine." To arrive at an understanding between people who are not merely estranged from one another by their physical appearances and their peculiar ways of life, but also stand on an unequal footing to one another, is a different question altogether.

Anthropology is not a dispassionate science like astronomy, which springs from the contemplation of things at a distance. It is the outcome of a historical process which has made the larger part of mankind subservient to the other, and during which millions of innocent human beings have had their resources plundered and their institutions and beliefs destroyed, whilst they themselves were ruthlessly killed, thrown into bondage, and contaminated by diseases they were unable to resist. Anthropology is daughter to this era of violence: its capacity to assess more objectively the facts pertaining to the human condition reflects, on the epistemological level, a state of affairs in which 1 part of mankind treated the other as an object.

A situation of this kind cannot be soon forgotten, much less erased. It is not because of its mental endowments that only the Western world has given birth to anthropology, but rather because exotic cultures, treated by us as mere things, could be studied, accordingly, as things. We did not feel concerned by them whereas we cannot help their feeling concerned by us. Between our attitude toward them and their attitude toward us, there is and can be no parity.

Therefore, if native cultures are ever to look at anthropology as a legitimate pursuit and not as a sequel to the colonial era or that of economic domination, it cannot suffice for the players simply to change camps while the anthropological game remains the same. Anthropology itself must undergo a deep transformation in order to carry on its work among those cultures for whose study it was intended because they lack a written record of their history.

Instead of making up for this gap through the application of special methods, the new aim will be to fill it in. When it is practiced by members of the culture which it endeavours to study, anthropology loses its specific nature and becomes rather akin to archaeology, history, and philology. For anthropology is the science of culture as seen from the outside and the first concern of people made aware of their independent existence and originality must be to claim the right to observe their culture themselves, from the inside. Anthropology will survive in a changing world by allowing itself to perish in order to be born again under a new guise.

Anthropology is thus confronted with tasks which would prove contradictory unless they were undertaken simultaneously in the same field. Wherever native cultures, though disappearing physically, have remained to some extent morally intact, anthropological research should be carried out along traditional lines and the means at its disposal increased to the utmost. And wherever populations have remained physically strong while their culture rapidly veers toward our own, anthropology, progressively taken over by local scholars, should adopt aims and methods similar to those which, from the Renaissance on, have proved fruitful for the study of our own culture.

From the very beginning, the Bureau of American Ethnology has had to face this 2-fold necessity by reason of the peculiar situation of the American Indians, who allied cultural remoteness, physical proximity, and a tremendous will to live, at least among some tribes, despite all the ordeals they have been subjected to; thus the Bureau was compelled from the start both to carry out ethnographic surveys and to encourage the natives themselves to become their own linguists, philologists, and historians. The cultural riches of Africa, Asia, and Oceania can only be saved if, following this example, we succeed in raising dozens (and they themselves hundreds) of such men as Francis La Flesche, son of an Omaha chief; James Murie, a Skidi Pawnee; George Hunt, a Kwakiutl; and many others, some of whom, like La Flesche and Murie, were on the staff of the Bureau. We can but marvel at the maturity and foresight, and hope for the worldwide extension, of what a handful of resolute and enlightened men and women knew should be done in the field of American studies.

This does not mean that we should be content merely to add material similar to that which is already available. There remains so much to be saved that the urgency of the task may make us overlook the present evolution of anthropology, which is changing in quality as it increases in quantity. This evolution, the recognition of which should make us more confident in the future of our studies, can be verified in many ways. To begin with, new problems have arisen which can still be solved, even though they have thus far received but scant attention. For instance, until recently anthropologists have neglected to study the elasticity of the yield of crops and the relationship between yield and the amount of work involved; yet 1 of the keys to the understanding of the social and religious importance of yams throughout Melanesia can probably be found in the remarkable elasticity of the yield. The farmer who may harvest far less than he needs must plant far more in order to be reasonably certain to have enough. Conversely, if the harvest is plentiful it may so widely exceed expectations that to consume it all becomes impossible; this leaves no other use for it than competitive display and social food presentation. In such cases, as in many others, we can render the observed phenomena a great deal more significant by learning to translate in terms of several different codes phenomena that we have been apprehending in terms of 1 or 2 codes only.

A broad system of equivalents could then be established between the truths of anthropology and those of neighbouring sciences which have been progressing at a similar pace: I am thinking not only of economics, but of biology, demography, sociology, psychology, and logic, for it is through a number of such adjustments that the originality of our field will best appear.

There has been much question lately as to whether anthropology belongs among the humanities or among the natural sciences. In my opinion, this is a false problem: anthropology is unique in not lending itself to such a distinction. It has the same subject matter as history, but for lack of time perspective cannot use the same methods. Its own methods tend rather towards those of sciences also synchronically oriented

During his remarks at the bicentennial celebration at the Smithsonian Institution, Washington DC (USA) on November 17, 1965 Claude Levi-Strauss explained the nature of anthropology in the following words:" "

but not devoted to the study of man. As in every other scientific undertaking, these methods aim at discovering invariant properties beneath the apparent particularity and diversity of the observed phenomena.

Will this assignment deter anthropology from a humanistic and historical outlook? Quite the opposite is true. Of all the branches of our discipline, physical anthropology is probably the closest to the natural sciences. For this very reason, it is worth noting that by refining its methods and techniques, it has been getting ever closer to, not farther from, a humanistic outlook.

For the physical anthropologist, to look for invariant properties traditionally meant to look for factors devoid of adaptive value from the presence or absence of which something could be learned about the racial divisions of mankind. Our colleagues are less and less convinced, though, that any such factors really do exist. The sickle-cell gene, formerly held to be such a factor, can no longer be so considered if, as is now generally accepted, it carries a certain measure of immunity to malaria. However, as Livingstone (1958) has brilliantly demonstrated, what appears an irretrievable loss from the point of view of long-range conjectural history can be viewed as a definite gain from that of history as historians conceive it, that is, both concrete and at close range. For by reason of the adaptive value of the sickle-cell gene, a map showing its distribution throughout Africa would make it possible for us to read, as it were, African history in the making, and the knowledge thus obtained could be correlated with that acquired from language and other cultural maps. Therefore, the invariant properties which have vanished at the superficial level reappear at a deeper functional level and, instead of growing less informative, turn out to be more meaningful.

This remarkable process is actually taking place everywhere in our field. Foster has recently given new life to what most of us held to be an exhausted question—the origin of the potter's wheel—by pointing out that an invention is neither simply a new mechanical device, nor a material object that can be described objectively, but rather a manner of proceeding which may avail itself of a number of different devices, some crude and others more elaborate. In the field of social organization, I myself have tried to show that kinship systems should not be described by their external features, such as the number of terms they use or the way they classify, merge, and distinguish all possible ties between individuals. In so doing, all we can hope to obtain is a long, meaningless list of types and subtypes, while if we try to find out how they work, that is, what kind of solidarity they help to establish within the group, their apparent multiplicity is reduced to a few basic and meaningful principles.

Similarly, in the field of religion and mythology, an attempt to reach beyond external features, which can only be described and arbitrarily classified by each scholar according to preconceived ideas, shows that the bewildering diversity of mythical motifs can be reduced to a very small number of schemes, each of which appears endowed with a specific operational value. At the same time there emerge for each culture certain sets of transformation rules which make it possible to include in the same group myths previously held to be markedly different.

These few examples, chosen among many others, tend to show that anthropology's traditional problems are assuming new forms while none of them can be said to be exhausted. The distinctive feature of anthropology among the human sciences is to look at man from the very point where, at each period of history, it was considered that anything man-like had ceased to exist. During antiquity and the Middle Ages, this point was too close to permit observation, since each culture or society was inclined to locate it on its neighbour's doorstep. And within a century or so, when the last native culture will have disappeared from the Earth and our only interlocutor will be the electronic computer, it will have become so remote that we may well doubt whether the same kind of approach will deserve to be called "anthropology" any longer. Between these limits lies the only chance that man ever had or will have to look at himself in the flesh while still remaining a problem unto himself, though one he knows can be solved since it is already certain that the outer differences conceal a basic unity.

Let us suppose for a moment that astronomers should warn us that an unknown planet was nearing the Earth and would remain for 20 or 30 years at close range, afterwards to disappear forever. In order to avail ourselves of this unique opportunity, neither effort nor money would be spared to build telescopes and satellites especially designed for the purpose. Should not the same be done at a time when half of mankind, only recently acknowledged as such, is still so near to the other half that except for men and money, its study raises no problem, although it will soon become impossible forever? If the future of anthropology could be seen in this light, no study would appear more urgent or more important. For native cultures are disintegrating faster than radioactive bodies; and the Moon, Mars, and Venus will still be at the same distance from the Earth when that mirror which other civilizations still hold up to us will have so receded from our eyes that, however costly and elaborate the instruments at our disposal, we may never again be able to recognize and study this image of ourselves.

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1. Smithsonian-Wenner-Gren Conference[1]

■ Planning Conference, Smithsonian Research Program on Changing Cultures, April 10-12, 1966, Washington, D.C. Sponsored by the Wenner-Gren Foundation for Anthropological Research and the Smithsonian Institution.

Organizing Chairmen:

WILLIAM C. STURTEVANT, Smithsonian Institution; and SOL TAX, Editor, CURRENT ANTHROPOLOGY.

Participants:

ASEN BALIKCI, Université de Montréal;
FREDRIK BARTH, Universitetet i Bergen;
STEPHEN T. BOGGS, American Anthropological Association, Washington;
R. N. H. BULMER, University of Auckland;
✓ CHEN CHI-LU, National Taiwan University, Taipei;
GEORGES CONDOMINAS, École Pratique des Hautes Études, Paris;
WILLIAM H. CROCKER, Smithsonian Institution, Washington;
RONALD G. CROCOMBE, New Guinea Research Unit, Port Moresby;
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✓ DUHYUN LEE, Seoul National University;
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SAUL H. RIESENBERG, Smithsonian Institution;
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JAMES WOODBURN, London School of Economics.

Discussion:

For at least a century anthropologists have understood a main purpose of their field investigations to be the recording of data on cultures undergoing change. Even when the focus of their research was different, most fieldworkers have felt that in a sense they were producing primary historical documents on a unique cultural situation which would never again be quite the same if indeed it would not soon be totally unrecognizable. From the beginnings of the field study of human cultures there has been a sense of urgency, an awareness that there are too few anthropologists to keep up with culture change.

In the last 15 or 20 years this sense of urgency has become more intense as it has become obvious that the course of industrialization and "modernization" and the rapid development of means of communication have so speeded up culture change all over the world that the disappearance of a very large part of the cultural variability of mankind can be foreseen within the near future—perhaps even the disappearance of most of the cultural variation which is important to anthropology and crucial for the testing of anthropological hypotheses. At the same time the study of culture and society has advanced to the point where we are more aware of the theoretical importance, actual or conceivable, of the data we are losing. It is no longer only historical or antiquarian interests which are threatened by the rapid transformation or disappearance of ancient cultural traditions. While it is true that all cultures change at all times, it can hardly be denied that the present situation is qualitatively different, and that anthropology is in danger of losing the largest portion of its laboratory just at the time when it becomes able to use it effectively.

In an address in Washington on September 17, 1965 (reprinted in CA 7:124-27) Claude Lévi-Strauss stated the urgency of a great increase in ethnological fieldwork in a manner which caught the imagination of both the anthropological staff and the administration of the Smithsonian Institution. This suggestion for expanded research activity came at a most op-

[1] This conference report, prepared by William C. Sturtevant, was mailed to the 45 conference participants for their comments. The following responded with written comments: Claude Lévi-Strauss, Merrick Posnansky, Hitoshi Watanabe, and J. S. Weiner. The comments written for publication are printed in full after the report and are followed by a reply from Sturtevant.

portune time: anthropology at the Smithsonian had been reorganized a few months before, with the promise of increased financial support and an enlargement of the staff; a month or so later, Sol Tax agreed to play a major role in Smithsonian anthropological programs, while continuing to edit CA.

Although several organizations are already at work on problems of urgent anthropological research, and Robert Heine-Geldern has been particularly active in this field for some 15 or 20 years, additional efforts are required. Therefore, the Smithsonian called an international conference in Washington on April 10-12, 1966. The Wenner-Gren Foundation provided most of the required financing, while the Smithsonian provided the facilities and the supporting staff. Invitations were sent to some 75 anthropologists, chosen rather arbitrarily from among several hundred people probably equally well qualified, representing a wide range of geographical areas (in both research and residence) and a good range of topical interests. In order to increase the geographical range while holding travel costs to a minimum, special consideration was given to those attending an international symposium on "Man the Hunter" in Chicago, April 6-9, and to those known to be traveling via the United States to attend a meeting of the Permanent Council of the International Union of Anthropological and Ethnological Sciences in London, April 13-16. In the end, 48 anthropologists from 22 countries were able to attend.

The purpose of the conference was to explore the proposition that, late as it is, a concerted international effort may yet recover a very significantly greater amount of data than would be preserved without such an effort. Present circumstances may permit a really massive increase in the amount of anthropological fieldwork to be undertaken during the next few years. The anthropologists on the Smithsonian staff hoped that the conference would advise them on the role the Smithsonian might play in encouraging and supporting such research.

Conference participants were provided with some tentative proposals worked out by the organizing chairmen, and with copies of three papers which describe the urgency of the situation (Lévi-Strauss 1966, Heine-Geldern 1957, and Dussan 1965). The present report summarizes the discussions of the conference, carrying out a resolution passed by the participants. The conference also requested Tax to "create an appropriate instrument to prepare for" a larger, more representative conference; in May, 1966, he therefore included the proposal for such a conference in a letter and questionnaire to all Associates in CURRENT ANTHROPOLOGY. The replies are summarized in the report which follows this one.

As Lévi-Strauss has pointed out (1966), the spread of Westernization has a double effect: the rapid disappearance of isolated tribal cultures, and the refusal of other non-Western societies to be studied as objects by Western anthropologists. The conference proposed many methods for increasing our knowledge of disappearing or rapidly changing cultures. But the second effect is more complex and was not squarely faced. The conference implicitly rejected—by failing to consider his reasons—Lévi-Strauss' warning that reciprocal study of Western societies by non-Western anthropologists is not the answer. Nor did the conference accept his conclusion that anthropology, "the science of culture as seen from the outside," will be replaced for these societies by sociology, economics, history, and other studies of culture "from the inside": rather, attention was devoted (although not explicitly in these terms) to means for making anthropology more palatable to such societies, by de-emphasizing its origins in the European political, economic, and intellectual dominance which gives anthropologists, as participants in the European scholarly tradition, the capacity for dispassionate observation of other societies. "Anthropology will survive in a changing world by allowing itself to perish in order to be born again under a new guise" (Lévi-Strauss 1966:126).

Scope

The background materials prepared for the conference stated that it is probably both impossible and unnecessary to attain general agreement on detailed criteria for establishing degrees of urgency of research on different cultures or societies. The conference appeared to accept this position, for there was little discussion of criteria for deciding the relative urgency of different projects. The conferees were not inclined to deny the urgency of any of the many specific examples mentioned by participants, although some felt that many fieldworkers, perhaps because of defects in their training and preparation, are ignoring urgent tasks in favor of other, less demanding research. Anthropologists knowledgeable about an area are likely to agree in general terms about the urgent tasks within that area, and it should be left to such specialists to point out research which needs to be done. One general criterion suggested was the question, "Is this research likely to be still possible 10 years from now?"

The background materials also contained the following statement:

> The most interesting and significant field research is that which is related to theoretical questions. It is not adequate to state the problem simply as a necessity to "salvage" data before it is too late; rather, attention must always be paid to the theoretical significance of the information being collected.

Different kinds of sampling required by five different types of hypotheses were then briefly mentioned. These statements aroused considerable discussion, and the consensus was that the position was erroneous and harmful, or at best, exaggerated. It was pointed out that well-rounded "complete" descriptive studies will serve theoretical needs which cannot be foreseen at the time they are conducted. Some emphasis was also placed on the availability of cinema and other recording techniques which make possible descriptions less affected by observers' biases. Such accounts will surely be needed in the not so distant future, for demonstrating the diversity which once existed for purposes of teaching about cultural differences as well as to serve as raw data for new types of analyses. Furthermore, it is evident that an overemphasis on theory and method in fieldwork will discourage partially trained observers, who may be the only hope for recording many cultural and social variants. The testing of hypotheses will continue without extra encouragement; what is urgent is the recording of all sorts of data on societies which are now rapidly changing. A humanistic view was also expressed in this context: that cultures are important in themselves and deserve well-rounded study for their own sake. There are also practical and political reasons for preferring general studies of the culture of a society or a country, over studies which treat such societies as laboratory objects to be used for testing a general theory.

To call such research "urgent anthropology" is both too broad and too restrictive. All studies of living peoples who carry particular ways of life, especially in situations of rapid cultural or social change, are relevant. Field ethnography and field linguistics are central. The biology of changing human groups is urgently in need of study. The needs and interests of other social and behavioral sciences which require cultural or societal contrasts should be considered. The collaboration of anthropological fieldworkers with research programs in these and other related disciplines is often mutually advantageous, and should be encouraged. The urgency of archeological fieldwork in the face of the increasing destruction of prehistoric sites was felt to be outside the scope of the discussion.

Personnel

A larger proportion of the disappearing data can be recorded and preserved if we can increase the number of anthropological fieldworkers, and if we can increase the possibilities for fieldwork by anthropologists who are already trained.

The following two paragraphs appeared in the materials prepared for the conference, and found general acceptance by the participants.

A good way to increase the amount of fieldwork being done is to make better use of nonprofessionals as fieldworkers. Many of these will also presumably be led to seek further training to become fully qualified professional anthropologists. But there is room for many different levels of amateur fieldworkers and of specialized ethnographic technicians who have less training than fully qualified professional anthropologists. Important characteristics of potential fieldworkers of this type are: that they are in contact with a cultural situation which ought to be studied, have the required intelligence and interest, are willing to accept instruction and supervision, and are literate in one of the world languages. Suitable candidates may be found among local school teachers, district officers and similar local governmental officials, doctors and public health personnel, literate religious functionaries, local businessmen, some military personnel, agents of the central government stationed in or frequently visiting rural areas, members of international organizations such as WHO, FAO, and other UN agencies, and foreign economic development, educational, and other personnel such as Peace Corps volunteers. If there were means for the support and supervision of fieldwork by such people, professional anthropologists would be encouraged to find suitable candidates and start them out, during their own fieldwork.

Possible means of providing training and guidance for nonprofessional fieldworkers include the following: short training sessions in fieldwork, at regional centers or elsewhere (including opportunities for contrastive cultural experience in another region); periodic working sessions or seminars at regional centers or elsewhere (on the model of the Summer Institute of Linguistics); correspondence with a professional anthropologist specializing in the area; sh[illegible] visits by such a professional to the fieldworker at the place where he has been working (again, on the model of the Summer Institute of Linguistics); opportunities to serve as field assistants to professional anthropologists; regional centers to maintain, for distribution as needed, a collection of specialized guides to fieldwork, questionnaires, and sample studies.

Conference participants added to these suggestions an emphasis on the importance of encouraging and assisting anthropological research by members of the societies being studied. It was also pointed out that more attention should be devoted to introducing anthropology, and especially ethnographic field methods, into the normal training curricula of specialists in other fields who may later find themselves in a position to record data of anthropological interest: school teachers, doctors, engineers, missionaries, also other social scientists such as economists and political scientists.

Another possibility worth considering is the development of specialists in techniques such as filming, some types of linguistic work, and the administration of projective tests, who have general training in the language and cultures of a region and can be sent anywhere in that region for short periods of intensive documentation, perhaps in collaboration with general ethnographers spending longer periods in one place.

A shortage of trained fieldworkers is probably everywhere more of a problem than a shortage of financial support for their research—but it would be advisable to see whether there are any countries where there are more well-trained research workers than can at present find employment.

However employed, well-trained anthropologists often find it difficult to conduct as much fieldwork as they would like to do. Lack of financing for field research or time-consuming difficulties in obtaining such financing is a problem in many countries. Some assistance with teaching and administrative duties could be provided by correcting the present imbalance in exchange teaching fellowships, which tends to draw people from areas with lower academic salaries, thereby placing a heavier teaching load on those remaining. The loss might be balanced by subsidizing the salaries for return visitors to raise them to the levels of their home countries.

Improvement of press and public relations for anthropology is a significant way to increase the level of financial support for field research, and, at least equally important, to make it easier for trained anthropologists to obtain leave and other facilities to allow them to conduct more fieldwork.

Well established local institutions are normally very willing to assist foreign fieldworkers, but this often adds to their administrative and clerical work loads. Outside assistance for meeting this problem would increase opportunities for fieldwork by their own staffs.

When money and time are available for field research, they are often lacking for the immediately following period when the material should be written up. One way to increase the recovery of data is to include support for subsequent writing in grants for field research. It would also be helpful to provide grants for anthropologists receiving advanced training abroad, which would permit them to conduct field research and writing when they first return home; otherwise they often, if not usually, must immediately devote full time to teaching and administration in their home institutions.

Publications

The training, assistance, and coordination of new fieldworkers under existing educational systems as well as under new ones, require publications of several types.

The conference agreed that CA can serve for worldwide professional communication about urgent fieldwork. A new journal on a sub-professional or student level might be published in several editions in different languages, but centrally edited, to publish articles on topics relevant to fieldwork and training everywhere, such as surveys of the current state of selected anthropological subjects, news of new developments of general significance, and requests for assistance and suggestions for field investigations applicable in many parts of the world. There might also be separate regional journals or newsletters, with information and inquiries particularly significant for each region, with less theoretical content than the other journals.

There is a need for more and better guides and questionnaires for fieldwork, and for the publication of existing as well as new ones in several different languages. General guides such as *Notes and Queries* (Royal Anthropological Institute 1951; Griaule 1957 is another example) should be issued in improved and up to date versions and widely distributed. Other manuals should be regional, oriented to the specific local field methods and problems. Manuals and field guides should also be produced to teach specialists of other types—teachers, nutritionists, nurses, etc.—the sorts of ethnographic data their previous training best equips them to collect. All general guides should emphasize that valuable fieldwork can be done everywhere, in peasant and urban as well as tribal areas.

To the extent that any fieldworker feels responsible for collecting as wide a range of data as possible in situations of rapid change, he would be helped by the availability of specialized and technical guides and questionnaires on subjects in which he has less competence. On the other hand, questionnaires which are too extensive and complex will tend not to be used.

There is a particular need for translations of good descriptive ethnographies which will illustrate various world areas and different emphases and historical periods in anthropological research, for general teaching purposes and to serve as models for new work. A basic set of such works in perhaps five languages would greatly assist anthropological education. Also, fieldworkers from another area should publish their materials in the national language of the country in which they have collected their data. If there is a marked improvement in the present geographical and national imbalance in the distribution of professional anthropologists, and if there is an increase in the amount and quality of fieldwork done by amateur or lay researchers, there will also arise the necessity for translation of local results into one of a few world languages.

It was also pointed out that the technical terminology of anthropology needs particular attention from the point of view of translation. This terminology is frequently ethnocentric or has a heavy European bias, for example in kinship, dating systems, measurements, animal and plant names, names of cultures and countries. At the least, field guides and teaching materials should provide tables of equivalents. Reduction and internationalization of technical terminology should be considered. Writers in European languages should remember that Latin and Greek etymologies are not transparent for educated speakers of other languages. In translating monographs and guides, particular care should be taken that the bibliography is also revised so as to include the basic sources and references available in the language of the translation. Wherever possible translations should at least be checked by competent professional anthropologists.

For the training of fieldworkers it would be valuable to have a listing of the institutions where particular contact languages are taught.

A useful model for training foreign students was pointed out in the West Germany university practice of formally introducing foreign students to the new language, new culture, and new academic tradition, to prepare them for direct competition with native students.

Finally, the conference noted that publication is not the only means of preserving field data. Archives for unpublished materials must be strengthened and made more numerous, and all fieldworkers must be encouraged to deposit their field notes in them. Many existing archives are in danger of destruction. There is a need for publications on methods for collecting, documenting, and preserving manuscript materials. In addition, facilities and procedures for collecting and preserving still photographs, cinema films, tape recordings, and museum and herbarium specimens need attention both for the fieldworkers who will collect them and for the institutions which will receive and maintain them.

Etiquette

Considerable discussion was devoted to the implications of anthropology and anthropological research in some areas of the world. There are places where anthropologists are thought to be hostile toward social and cultural change; sometimes anthropological field research is viewed as a form of exploitation.

One response to this is to drop the term "anthropology" (or "social anthropology"); substitution of the label "sociology" has been successful in some instances. But this has several disadvantages, among them that it may be taken as a form of subterfuge, and that it may erroneously imply action-oriented rather than fundamental research. Two other nominal solutions to the problem were suggested: use "ethnography" because it is a more neutral term; use names of more specific subdivisions such as ethnobotany, ethnoscience, musicology, etc. But anthropology is not everywhere a bad word; it would be useful to know where it is and where it is not, and to consider the reasons for the associations it does have in various regions, rather than precipitously dropping the term.

The conference kept returning to the "image of anthropology" held by administrators and government officials and by members of the specific societies it is proposed to study, and suggesting ways to improve this "image" and thereby facilitate fieldwork.

For many reasons it is important to draw indigenous people into active participation in anthropological inquiry. The status of informants and field assistants should be raised: wherever possible they should be given deserved credit in resulting publications, particularly when their collaboration takes such forms as typing texts; fieldworkers should more often explain and go over their conclusions with members of the society being studied (this is important for methodological as well as practical reasons); means should be found for deriving more trained professional anthropologists from among informants and field assistants, and this points also to the obligation of fieldworkers to support or help create local institutions where such people can receive further training and some prospect of continuing employment.

The support and strengthening of local institutions is also essential for facilitating fieldwork by foreign investigators. Fieldworkers should always seek some form of association with or sponsorship by local institutions when organizing their research programs—with due attention to local competition between institutions, and to the frequent desirability of developing centers outside the main national centers, interdisciplinary and other research centers (agricultural, nutritional, public health, etc.) should be sought out for cooperation. Visitors should be careful to send copies of all publications deriving from their fieldwork to the local institutions, and it also helps avoid the impression of exploitation if results of fieldwork by foreigners are published (bilingually if necessary) in the national language of the country where the fieldwork was done. Researchers should consult with and report to local institutions at the end of their fieldwork as well as at earlier stages.

Relations with governmental administrations have always been important for anthropological fieldwork, and are certainly no less so now. The problems here are of course complex, and vary considerably in different regions. These and other matters of protocol and ethnographic etiquette should receive more specific and formal attention in the training and preparation of fieldworkers than has usually been customary.

There was some discussion of the responsibility of anthropologists for providing information of practical utility, and for pointing out that pure research often has practical by-products. On the other hand, there are difficulties with the potential use which may be made of information, for example when there is conflict or stress between a majority population and a central government on the one side, and a minority group on the other.

We should squarely face the tensions and prejudices which influence the opportunities for field research. The advantages of fieldwork by a member of a group which is neutral to whatever tensions exist in a given area should be recognized, and efforts should be made to encourage selection and sponsorship of fieldworkers by international organizations. Even so, there may be regions where any anthropological fieldwork by anyone is impossible under present conditions. There are however insufficient people to do all of the urgent research which it is possible to perform.

Organization

The conference devoted a great deal of discussion to organizational forms for promoting and supervising urgent research. It is not difficult to list tasks for such an organization and to propose more or less elaborate abstract schemes for a worldwide organization with central secretariat, separate regional committees and headquarters which would divide up the world for anthropological research, and a complex flow of directives, advice, information, and funds up, down, and across a vast, rational, and neatly hierarchical organization. No firm proposals were made as to how such an organization should be initiated, and by whom, and no formal discussion was devoted to means for arbitrating the inevitable conflicting claims of different institutions and individuals. Informal, more private conversations during the conference did return repeatedly to these problems. The conclusion seems to be that acceptable social control requires the power of acceptable authority, and this does not exist in anthropology considered as a world community of scholars.

While the scope of CA and its Associates, is worldwide, it is doubtful that an attempt to turn it into a formal organization directing and controlling research activities would succeed. However, the conference was in full agreement that CA should serve as an information and news source for urgent research. The list of Associates, with its new punch-card retrieval system, might be expanded as a convenient roster of specialists and institutions and their experience and interests. CA can also collect and publish suggestions for urgent research: this began with the summary of replies to the May 1966 Letter to Associates. Thus, CA can build on its present success as a communications medium and establish a bank of information and ideas on urgent research to be drawn on by any interested individual or institution.

Ways of encouraging urgent research will undoubtedly continue to be discussed at various national, regional, and international anthropological meetings. The Washington conference also gave favorable consideration to a larger international congress to be devoted especially to urgent field research—but did not resolve the problems of financing such a meeting, of deciding on the most appropriate location and sponsorship, or of how to select those who should attend.

Reported by William C. Sturtevant

Comments

by Claude Lévi-Strauss☆

Paris, France. 19 IX 66

I have read with great interest the excellent report by Sturtevant and I wish to comment briefly upon two points:

1) I would not suggest (p. 356) that sociology and/or economics could replace anthropology. In my opinion there is no such thing as sociology, and economics is a completely dishumanized science. Should anthropology dissolve, this would not be for the benefit of any so-called "social sciences" but rather of the humanities: linguistics, philology, archaeology, history, philosophy.

2) The statement (p. 357) that "a shortage of trained fieldworkers is probably everywhere more of a problem than a shortage of financial support for their research" does not hold for several European countries including France. I would say that we have at any time about a dozen trained fieldworkers away from the field for lack of financial support plus probably as many foreign-born living and working in France. This latter group is even more difficult to support because they are not French.

by MERRICK POSNANSKY☆

Kampala, Uganda. 19 x 66

One topic raised at the Conference but not mentioned in the report concerned the need for documenting culture change during the period of the onset of the colonial era in many parts of Africa and to some extent Asia. Much of the "scramble for Africa" took place in the last years of the 19th century and in the first decade of the 20th century. Many of the peoples who witnessed the impact of western society are dead; some still survive but will be gone within half a generation. It is urgent that (1) their oral testimony be recorded about the conditions of their preimpact society, their responses to the colonial era and the new societies with whom they were brought into contact; and (2) the adaptation of their indigenous structures to the changes that took place. In the field of traditional history anthropologists can profitably gain by close collaboration with the historians and political scientists.

On the question of the training of non-professional fieldworkers, the continuity provided by national and regional museums needs stressing. The local museum, if assisted with its registerial duties, can provide documentation, visual instruction centres, and a link between work on the social and material aspects of changing cultures and between the layman and specialist. One barrier to the release of trained anthropologists is not perhaps so much the lack of finance for research but the lack of medium grade personnel to assist with administrative chores. The provision of finance for executive secretaries or even filing clerks puts heavy strains on professional staff who often are unable to exercise their skills in any other way than servicing the expeditions of anthropologists from more adequately financed institutions in the "developed" countries. A greater flexibility in the conditions of grant-aiding bodies over the employment of grants, the encouragement by bursaries of graduate assistants to work in institutions in underdeveloped countries on routine work for which grants are never normally available, could release a large amount of well-trained manpower.

by HITOSHI WATANABE☆

Tokyo, Japan. 6 x 66

The Urgent Anthropology meeting in Tokyo at the Pacific Science Congress gave me the impression that there is some urgent business to be done before the next international conference on Urgent Anthropology. The business is to collect an accurate, simplified inventory or manual on what studies are urgently needed of what peoples or societies, and on the present situations of the peoples in question. General discussions at an international level on the means (fieldworkers, cinema, field manual, etc.) and organizations (national, regional, international) might be easier after a clear outline is compiled. I would like to suggest the following points.

1) It is hoped that CURRENT ANTHROPOLOGY will collect papers on urgent field research such as those presented at the 1961 Research Conference on Australian Aboriginal Studies, asking for comments from specialists concerned in the form of the CA☆ review articles. The authors and commentators may be selected by region (which will be suggested later) as well as by methodological and theoretical interests. (We know CA has been collecting similar data and information by means of the Associate's Reply Letter system. The articles and comments mentioned above would provide us with a little different kind of data or more systematic information on peoples and regions than retrieved through the ARL system. Thus both system would supplement each other.)

2) It is hoped that the Smithsonian Institution, the promotor of the Urgent Anthropology Project, will arrange to have compiled this data and information and, with reference to or on the basis of the data obtained, compile an inventory of field studies urgently needed in different parts of the world and among different peoples.

3) It is hoped that the next international conference on Urgent Anthropology, or any kind of UA meetings, may be held to discuss general problems with reference to or on the basis of that inventory. The inventory will give the participants a bird's eye view of urgent problems in concrete form and help them in thinking and discussing problems in a wide framework.

4) Systematic selection of the authors and commentators of the articles previously mentioned and organization of urgent field research projects necessitate the establishment of ethnographical regions. The most important problem here is how to divide the world into regions useful for the UA project. The division should be practically useful and applicable not only to scientific studies themselves but also to the establishment of effective organizations for planning and carrying out the urgent field research.

Some regions may include various peoples; certain peoples are so distributed as to cover several countries. For example, Southeast Asia includes various groups of settled cultivators, food-gatherers, and shifting cultivators, for instance, the Negrito groups, are distributed over different countries. These situations make any simple divsion on a geographical or national basis dificult. From the above point of view the following geographical-cultural political, three-fold system of division of aboriginal peoples may be recommended:

I. Food-gatherers		
	Africa	Bushmen groups Pgymy groups Other groups, if any
	S.E. Asia	Negrito groups (Malaysia, Philippine)
	Australia	Australian aborigines.
	South America	(predominantly food-gathering) Siriono groups Other groups, if any.
	North America	(or Artic and Sub-arctic Regions) Eskimo groups (U.S.A., Canada, Denmark, U.S.S.R.) Athapaskan groups (Canada) Other groups, if any.
II. Pastoral Nomads.		
III. Shifting Cultivators.		
IV. Settled Cultivators.		

This kind of dividing system may be convenient for selection of the authors and commentators of the urgent research papers and also useful for establishing local organizations on a regional or national basis, and arranging discussions on urgent problems.

5) Practically, it seems to be difficult or impossible to collect "all sorts of data" from all the peoples and societies needing urgent research and to make "well-rounded complete" studies of all of them. In the world there may be so many peoples or societies worthy of urgent research. But time and money are limited. A plan which may be practical and may fulfill the need of UA is to combine the intensive study of some peoples or subgroups of peoples sampled and the extensive wide-range survey of as many of the other groups as possible. How to sample the groups is another thing to be discussed.

6) It seems to me necessary to frame a good definition of the term "changing" or "disappearing." Peoples and cultures always change. One general criterion for establishing degrees of urgency of research on difefrent cultures or societies suggested at the Washington Conference, that looks very useful and practical is, "Is this research likely to be still possible 10 years from now?" But it will need another supplementary criterion. A culture as a whole may not change or disappear this rapidly even though part of a culture or individual elements may do so. Actually, there are many culture elements or trait-complexes everywhere which are about

to change or disappear. From this point of view it is hoped another criterion will be devised.

by J. S. Weiner☆

London, England. 16 ix 66

I have read the report on the Smithsonian Conference on Changing Cultures with interest. I do take some exception to what is said (p. 359) under "Organization" starting "no firm proposals were made..." and ending "...community of scholars." I am surprised to read this. Professor Barth and his group put forward a number of very clear and realistic organizational suggestions, which were in fact endorsed by the Conference. Many people were disappointed in fact that these proposals were not followed up and indeed many preferred action along the lines suggested rather than the holding of a second large conference. Another organizational proposal was that the Smithsonian Institution should act as a clearing office for research activities on changing cultures, which was also endorsed, I believe.

Reply

by William C. Sturtevant

(1) Watanabe's suggestions are partially met by plans to publish lists of urgent tasks in CA, beginning with the results of Associates' replies to Letter 35 and with an index to Prof. Heine-Geldern's *Bulletin of the International Committee on Urgent Anthropological and Ethnological Research* (pp. 362-417). These lists hopefully will serve as a beginning to be expanded and criticized via letters from Associates. It should soon be possible to publish a summary which will be better balanced than presently-available lists; such a summary may also imply some criteria of urgency, which will presumably differ according to the interests of the proposers of, and commenters on, suggested tasks, and also according to the world regions in which the societies are located. Watanabe suggests that such summaries be systematic, that they pay attention to criteria of urgency, and that they be prepared by and commented on by regional specialists. The lists which CA already plans to publish can serve as the basis for such articles. However, I do not believe that the Editor of CA, or the Smithsonian staff, should attempt to divide up the world for this purpose and select authors and commenters for regions thus defined. I would much prefer to follow CA custom and call for volunteers, allowing each volunteer to define his own regional and topical boundaries and permitting (even encouraging) overlapping coverage in different summary articles.

(2) I apologize for having misinterpreted Lévi-Strauss, who clearly specifies that "when it is practiced by members of the culture which it endeavours to study, anthropology loses its specific nature and becomes rather akin to archaeology, history, and philology" (1966:126). He also states that "wherever populations have remained physically strong while their culture rapidly veers toward our own, anthropology, progressively taken over by local scholars, should adopt aims and methods similar to those which, from the Renaissance on, have proved fruitful for the study of our own culture" (p. 126). Personally, and perhaps parochially, I would prefer to interpret this last passage more broadly than does its author, and include the "social sciences" with the humanities as fields which are fruitful for the study of culture "from the inside."

(3) In reply to Weiner's comments, it should be pointed out that the only conclusions formally adopted by the Washington conference were that I should prepare the present report which should be published in CA after circulation to all attendees for comment; that a larger congress on this subject should be held; and that the Editor "should create an appropriate instrument to prepare for the congress, with consultation of appropriate advisers representing various regions." The May 1966 Letter to Associates was the response to this last recommendation.

The discussion group to which Weiner refers consisted of Barth, Bulmer, Boggs, Krader, Palerm, Weiner, Woodburn, and Sturtevant (secretary). The resulting organizational suggestions, while perhaps "very clear and realistic," were nevertheless without suggested means for implementation. The proposals of this group can be summarized as follows: the world-wide organization should consist of four levels. The lowest is made up of professional anthropologists and a few lay fieldworkers sponsored by these professionals. From these people come suggestions for priorities and applications for financial support for research projects. (No suggestions were made on how to define "professional anthropologist," or who should apply such a definition.) The second level consists of regional committees, which decide on priorities within their own regions and match these against applications from individuals, certifying to the qualifications of applicants and to the degree of urgency of the research they propose, and forwarding these recommendations to national and international sources of funds; such proposals could also be forwarded directly by individuals, bypassing the regional committees. The regional committees would not themselves directly dispense funds. The functions of these committees were spelled out in somewhat more detail, but the procedure for choosing them was not fully resolved—it was only suggested that temporary regions be first defined (by whom? how modified into permanent regions?), for example 8 for the world, that a convener be appointed (by whom?) for each region, to call together a temporary regional committee (approved by whom? how converted into a permanent committee?). On the international level, CA should publicize priorities of urgent tasks. Also, existing non-regional organizations such as United Nations agencies, the Smithsonian, "etc." (what is the definition of such organizations?) should coordinate proposals received from anthropologists, directly or through the regional committees, and recommend them to international sources of funds. There should be no single world center for urgent research, other than CA (and the latter has no administrative or financial functions).

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