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Ethnography and Comparison in Legal Anthropology

Legal anthropology is, as I have remarked elsewhere, a small field in which the general quality of the work is extraordinarily high. For that reason it serves well as a microcosm in which to examine some of the problems that plague the more inclusive subject of social anthropology.

One of the recurrent problems in social anthropology is the relationship of ethnography to what is usually called comparison. Differences of opinion about the nature of data and its organization into ethnography and its reorganization into "comparative studies" have been at the heart of much disparateness within the discipline. Within legal anthropology these differences have assumed inordinate proportions.

Given the present state of the art, I find that there are four main areas on which some light can be thrown. I want to take them up in order of increasing importance: the use of native terms in reporting ethnography; the idea of the "folk system" and the way in which unavoidable warping of native ideas can be kept track of; some of the kinds of intellectual gymnastics that have been called comparison; and the difference between the theory and culture of ethnography and the theory and culture of comparison.

In the material that Gluckman added to the second edition (1967) of his *The Judicial Process among the Barotse* (originally published in 1955), he quotes a sentence from Mary Douglas' review of his book (Douglas 1956:369) which seems to touch the kernel of the problem: "C'est précisement dans cette interposition de forms de pensées étrangères que reside l'acte d'interprétation." Roughly rendered, this reads, "It is

in the juxtaposition of previously unconnected ideas that the act of interpretation is to be found."

With this idea in mind, the present article will be seen to deal with four contexts in which novel juxtapositions can be made, and the kind of interpretations that can justifiably be derived from each: the new juxtaposition that comes when an ethnographer struggles to present his ideas in English to his colleagues; the new juxtapositions that arise between the models of the ethnographer and the models of his subject people; the new juxtaposition of ethnographic models with one another when they come from different cultures; and the new juxtaposition of new ethnographic models with those already accepted as theory in the subject.

Native Terms

I have been told on a number of occasions that my Tiv ethnography makes extensive use of native terms in reporting. I always agree that this is true. Gluckman (1955:380–381) takes this position: "I consider that very many of these concepts can, without distortion after careful and perhaps lengthy description and discussion, be given English equivalents, at least out of courtesy to one's readers."

Thus, the overeasy statement would seem to be that an author either uses native terms and makes it tough on the reader but easy on himself, or else he uses no native terms and makes it tough on himself but easy on the reader. This seems to me to be an erroneous way—at least an oversimple way—of looking at the problem. No one doubts that it is difficult to read an ethnographic report that is studded with foreign words. No one questions the fact that one's material should be made as easy to read as possible.

The alternative to using native words is to put a gloss on English words. That means to expand or reduce the meaning such words carry in their own culture so that they can carry an accurate meaning from the subject culture.

It is my firm opinion that the power of the word is always greater than the power of the gloss. If the ethnographer is not concerned with some (unfortunately he cannot be concerned with all) of the delicate nuances of his culture, then he may write without either native terms or glosses, "using words as the dictionaries define them"—but I brand him the lesser ethnographer. It boils down to this question: is it more difficult for a reader to keep in mind a set of native terms or a set of glosses on English words—usages of ordinary English words that will apply in no other context than the immediate one? I agree with William Empson (1951) that to introduce such a gloss is either to make a word mean so much that it becomes too blunt or to create what he calls a

"negative pregnancy" to the meaning of the word—to accept only one chunk out of its ordinary day-to-day meaning. Either procedure, but particularly the latter, is to insert a trap for the reader that no amount of skillful writing or careful reading can keep him from falling into, because he subconsciously reinserts the entire range of meaning.

The result is that good ethnography is hard to read, whichever method one opts for. It is my opinion that every ethnographer owes it to himself, the people he studies, and his colleagues not to blunt the edge of his material. He must, of course, translate as much as possible; he must gauge the point at which difficulty of reading becomes impossibility of reading. But there is an analogous point at which the gloss method leads to even greater difficulty, because it simulates understanding through the use of a familiar word. Such simulation leads—almost inevitably, I think—to an assumption of comparability of everything called by the same word—and this is a difficulty that is almost impossible to correct. The most blatant examples come in the old-fashioned treatment of kinship terminology: translation of kinship terms is notoriously difficult, and we all know what a mess one can get into if he translates as "father" the words from Crow or Omaha systems that include the English "father" and then work in translation. Hocart (1937) understood this and, as far as I know, was the first really to grasp the nature of the problem that today is being attacked by componential analysis and the beginnings of a kind of ordered ethnography that its practitioners call by the unfortunate term "ethnoscience."

Hoebel, Ayoub, and some others have, when reviewing Gluckman's book and my own *Justice and Judgment among the Tiv*, claimed that Gluckman's approach leads to greater theoretical understanding. I am convinced that they have fallen into his language trap, and that in fact greater understanding has been sharply curtailed the very while our capacity to talk in demi-semantic categories gives the illusion of getting somewhere.

I see no virtue in everybody's agreeing on this matter. Differences of purpose and of personality seem to me best left to stand and speak for themselves.

While we are on the matter of language, another question can be taken care of: is the "native language" a reasonable alternative to the language of jurisprudence? Here is, indeed, another red herring. Hoebel's (1960–1961:431) statement is the earliest one I know. Although he tells me he would not stand by the statement today, I nevertheless want to review it, because the precise point was made in Gluckman's 1967 addenda. Hoebel stated that "The Tiv folk system idealizes the Tiv legal process, and it is just as erroneous to slip into an acceptance of Tiv thinking about their system as representing the real thing as it would be uncritically to carry over inappropriate Austinian conceptions to the distortion of

Tiv ideas and behavior." Gluckman (1955:382) puts it in this way: "It seems to me that the refinements of English, and in general, European, jurisprudence provide us with a more suitable vocabulary despite its connotations, than do the languages of tribal law."

Both these statements miss the point. English jurisprudence has developed a vocabulary for talking about English law (and to a lesser extent talking about comparative law and conflicts). Tiv have not developed jurisprudence. Therefore, even to get these two matters onto a comparative level, the ethnographer must do for the Tiv what they have not done for themselves—find a "theory" of Tiv legal action, for what Gluckman aptly calls forensic action. Then the job of comparison can begin. Gluckman is no doubt right that a larger proportion of what we shall below call the culture of comparison will be derived from English jurisprudence than from Tiv (or any other) ethnography. But the two "languages" per se—English jurisprudence and the native language—are not equivalent entities. In short, when you juxtapose one with the other, you are not merely changing language—you are changing media.

Hoebel's statement above has an additional difficulty in the phrase "representing the real thing." One cannot represent the real thing any way except by a grossly ethnocentric viewpoint or else by some kind of technique, such as the folk system or ethnoscience, to get at something less ethnocentric. I will have more to say below about the difficulty involved in "the real thing."

One's knowledge of ethnography and the institutions of other societies must comfortably inform the writing of any ethnographic tract the very while they are not made part of it. That is to say, if I report an institution from the Tiv that is totally similar to another institution that is described by someone else, I must neither omit to report it nor merely say that it is "like the Romans." I must describe it in Tiv terms, so that other scholars can make up their minds whether it is like the Romans. If the ideas that an ethnographer has discovered or has had to invent to explain the situation in another culture are helpful and actually do work, then any ethnographer must use them, or improve on them, and he is at fault if he does not. This is utterly different from the comparative method. It is merely utilizing the full culture of one's own profession (which Gluckman confuses with comparison). In short, Gluckman has treated what I have talked about as the folk system as if it were merely a first step toward some problem that he is interested in. In fact, it seems to me that it would never lead to the problem he is interested in, and he has therefore branded it as "unique" and as "cultural solopsism." That does not mean that it is "not comparative" so much as that he has an egocentric concept of comparison.

Some Chimeras of Comparison

If the core of ethnography is the creation of theory around ideas that are "discoverable" in a folk system, what is the core of comparative anthropology? To put the question another way, if the core of ethnography is the juxtaposition of a set of native ideas with a set of ideas in what we shall call below the "culture of ethnography," what is the new juxtaposition to be achieved in comparative anthropology and what are its methods and its dangers?

As long ago as 1896, Franz Boas in an essay in the pages of *Science* pointed out some of the chimeras that lay waste the fields of anthropology in the name of comparative method. His primary target was the questionable doctrine of the psychic unity of mankind. He argued convincingly that such a doctrine, more or less based on the systematic misunderstanding of Bastian (and, a latter-day investigator might add, of Jung), harbors a lurking hypothesis that the same phenomena everywhere develop in the same manner, represent the same thing, and have the same meaning. As Boas noted (1896:903) "even the most cursory review shows that the same phenomena may develop in a multitude of ways." The question for the comparative method is "when are two things the same thing?"

Boas (1896:904) went on to deplore

> the fact that so many fundamental features of culture are universal, or at least occur in many isolated places, interpreted by the assumption that the same features must always have developed from the same causes, leads to the conclusion that there is one grand system according to which mankind has developed everywhere; that all the occurring variations are no more than minor details in this grand, uniform evolution. It is clear that this theory has for its logical basis the assumption that the same phenomena are always due to the same causes.

The original grand theory of evolution was (temporarily) eclipsed, but the psychic need of some social scientists for a single grand theory

that would explain everything was not. Some anthropologists have a penchant for rejecting any proposition that they can find an exception to, whether that exception leads to a valid refutation of a position or proposition or not. This is what Potter (1951) has called the "But not in the south" technique of one-upsmanship. Indeed, one reason social science has garnered its reputation for never getting anywhere can be laid to some of its most vocal practitioners who demand that theory be universal and reject all theory that is not. I have never understood how they can think their own theories can pass muster better than others.

The "psychic unity of mankind" as an idea did not die—it merely took refuge in the creed of liberalism. And as such it covertly informs much of Professor Gluckman's work. In his first Lozi law book (1955:271) Gluckman states that "it is unfortunately still necessary to demonstrate that Africans . . . use of processes of inductive and deductive reasoning which are in essence similar to those of the West, even if the premises be different." He wants, in short, not merely to study a culture and social structure, but also to prove that Africans are as good as anybody else. The point is later made even more explicit: "I am delighted in every way that his report bears witness to some similarities of social life everywhere, and to the basic similarities of all human beings in very varied conditions." Of course one must recognize that Gluckman knew his book would be read by some whites in southern Africa who might want to refute that position, and one sees his difficulty in writing with two audiences in mind. Yet he has not negotiated this passage very well, and the febrile doctrine of psychic unity stands in a way of a thoroughgoing analysis of what is to be found in his notes and cases. What *is* in question is a difficulty that Durkheim long since warned us against—the danger of a psychological *deus ex machina* in social and cultural analysis.

Boas (1896:907) claimed, and I agree, that the only way to clear up such difficulties is to have the history and ethnography of single cultures completely understood, each in its environment, with its psychic component, its history and other dimensions. "Thus by comparing histories of growth [ethnographies of structure and culture] natural laws may be found. This method is much safer than the comparative method, as it is usually practiced, because instead of a hypothesis on the mode of development, actual history forms the basis of our deductions." The words "history" and "development" meant something different to Boas in 1896 from what they do to most anthropologists in the 1960's. But if we insert "ethnography" and "integration" instead, his meaning is precisely the one I wish to convey.

I have not quoted Boas merely as an ancestral totem—indeed, he is not my totem. But on this point he spoke clearly and aptly—and a long time ago. Obviously his precepts have not yet been fully recognized or digested by the profession.

Another, and almost exactly opposite, mistake is one that is marked by Nader when—casually, so perhaps she does not deserve the brunt—she suggests that mine is a "relativist" approach. "Relativism" has created almost as much misunderstanding in the anthropological profession as has "prelogical" or the "psychic unity of mankind." Nader (1965b:11) writes: "[Bohannan] shirks the problems that such a relativist approach implies for comparison, but then he was not interested in comparison at the time."

"Relativism" seems a shorthand term for a doctrine that holds that there is no absolute. In that sense, surely every social scientist must be a relativist. But relativism also sometimes means to some people "anything goes." Only if we mean by it that no analysis can be made of any culture without some set of premises or other, and that such premises are themselves necessarily culture-bound to some degree, can we see the "relative" problem. The "correct" set of premises is that which informs the action as well as the analysis. Seen so, relativism is not merely the opposite of "absolutism" but is itself relative. Denying psychic unity or denying the omni-appropriateness of one set of assumptions (English jurisprudence, in the case at hand) does not mean that one is, for that reason, a relativist. "Relativism" is valid only if it means that every society, and hence every ethnography, must be understood on its own terms (which may or may not be unique—the point is irrelevant), and only if the theory is forged from the force of creative processes dealing with those terms.

Another "mythical beast" that sometimes masquerades under the name of "comparison" is backward translation. As we have already noted, translation from the language and culture of the subject people into the technical language and culture of anthropology is the task of every ethnographer. The danger is that, instead of starting with the culture in question, we as ethnographers begin with our own anthropological language or even our mother tongues and find equivalents in the language of the people we study. It appears to me that this is what lies behind the difficulty that has led to so much misunderstanding of Gluckman's position. In the new material to the second edition of *The Judicial Process*, he notes (2nd ed., 1967:376) that a "myth" has grown up about his methods, and he takes as his scapegoat a statement by Nader about his using the categories of Roman-Dutch law. I think he adequately defends himself here; however, in the course of the defense, he has missed what it was that led to the myth. It appears to me that instead of starting with Lozi ideas and seeking to translate them for his colleagues, he begins wih his own rather considerable sophistication in jurisprudence and translates as much as possible of it into Lozi. The original flag quote of my book, from Sir Henry Maine, was selected to warn against such a danger. Gluckman does surprisingly well at the translation—but in doing so he misses some of the Lozi point. I am not impugning Gluckman's

cases or his documentation, which we all know to be the very best. I am, rather, saying that he fails to give us Lozi interpretation because his own interpretation enters too early. Here is one of the more obvious examples of Gluckman's backward translation (1955:316):

> The key concept here, equivalent to *law* in which it is embraced, is trial by due process of law (*tatuho kamulao*). The process is based on hearing evidence (*bupaki*) which established proof (also *bupaki*) on the facts (*litaba* = also things). Evidence itself is reduced by concepts of relevance (*bupaki bobuswanela*, appropriate or right evidence; *bupaki bobukena*, evidence which enters); of cogency (*bupaki bobutiile*, strong evidence); of credibility (*bupaki bobusepehala*); and of corroboration (*bupaki bobuyemela*). These types of evidence are tested as direct, circumstantial, or hearsay. The concepts of evidence have a marked flexibility, as contrasted with those of substantive law. First, they are multiple in their referents: they cover evidence given in courts about actions committed, the judge's knowledge of the social and physical world (judicial presumptions: *linto zelwaziba*, things we know), and the judge's inferences from the evidence (*lisupo*, indications, probabilities).

This sort of thing goes on. It reminds me of a witticism Margaret Mead made some years ago at a cybernetics conference, when someone in a flush of enthusiasm claimed that Kant could be translated into Eskimo. Mead's laconic reply was, "Of course, but can you make an Eskimo understand it?"

It is obvious, from the passage given above and from a number of others that might be selected, that Gluckman is translating fundamentally Western ideas into Lozi instead of translating fundamentally Lozi ideas into English. Please note that I did *not* say that the Lozi and the English do not have fundamentally similar ideas, but only that by this method of exposition there is no possible way for a reader to discover whether they have or not.

Indeed, as Gluckman goes on, the combination of the unstated bias of a belief in the psychic unity of mankind and such backward translation (1955:3–57) very nearly becomes an overt bias: "On the whole, it is true to say that the Lozi judicial process corresponds with, more than it differs from, the judicial process in Western society." Of course it does, or Gluckman could not have defined it as judicial. That is not the significant aspect. A final example: "The judges' manipulation of these concepts is *implicit* in their assessment of the evidence of the parties against the standard of 'the reasonable and upright man'" (Gluckman 1955:318). I have italicized *implicit* to make my point. Implicit in what? In Lozi social action, perhaps—in spite of Gluckman's protestations we cannot tell. This is the same kind of reasoning that led Murdock to say that the nuclear family is universal when only its components are universal.

Another bugbear of the comparativist is a tendency not to distinguish between cross-cultural and interdisciplinary. Gluckman deals with the

interdisciplinary problems of communication between law and anthropology under his rubric of "comparison." In *The Judicial Process*, Chapter VII is called "Some Comparative Implications of the Lozi Judicial Process." There is almost nothing in that chapter that is cross-cultural, which is what most anthropologists since Tylor have meant by "comparative." Rather, the chapter is an examination of some jurisprudential writers and the way that some of their problems can be illuminated by Gluckman's readings of his Lozi cases. In *The Ideas of Barotse Jurisprudence* (1965b) Gluckman has added some cross-cultural material, still under his rubric "comparative." In one of the best parts of the book—that dealing with political power and treason—over half the passage deals not with the Lozi at all, but with medieval Europe. But when he does add the cross-cultural material, it is almost totally without his even trying to retain control of it. Here is his own statement (1965b:204):

> Wrongs in Barotse society arise out of aggressive actions by outsiders, or out of failure to fulfill obligations of familial life between persons in specific status relationships. I shall argue that general ideas of wrong are again best understood in terms of reaction to those wrongs arising in status relations. To demonstrate my argument I begin not from the Barotse, and their governmental system with its courts, but from societies where redress was secured by self-help and where vengeance for the killing of a man was enjoined on his kinsmen.

From that statement he goes into a direct discussion of Maitland's article on the history of malice aforethought, and on to the Nuer; he gives an Ibo reference, and then back briefly to the Barotse, with sidelights on the Zulu, Kalinga, Ifugao, Marc Bloch's Middle Ages, Wales, the Yurok, England; back to Maitland's early England, to the Australian aborigines, the Cheyenne; and other subjects that it would be tedious to list and to read.

The point that Gluckman wants to make is that theory is where you find it and that most of it has been thought of already. I agree, but the point I want to make is that if wide-ranging materials are utilized in this way, a theory of comparative method becomes necessary, as well as (in this particular case) a theory of jurisprudence. Comparison must be done in a controlled way, with great awareness and sensitivity to the original meaning, and with a set of methods that allow us to utilize what we are doing toward some specific ends beyond merely buttressing a position.

As many another social anthropologist might, I have come up with a genealogy of the various methods that have been used for comparative purposes.

There are two fundamental types of comparison, which I have called "casual" and "controlled" comparison. Casual comparison is inserted by a writer in order to aid his reader in adjusting the ethnography at hand to the vicissitudes either of the reader's own culture or some other culture he already knows. There are two modes of casual comparison. One I have called "ethnocentric" if the comparative examples are taken only from the culture of the writer and reader. The other I have called "selected counter-illuminative information," because it is chosen out of a very wide range of possible ethnography specifically because it illuminates the case in hand. This latter is the type of comparison Gluckman has done in the section of his book mentioned just above.

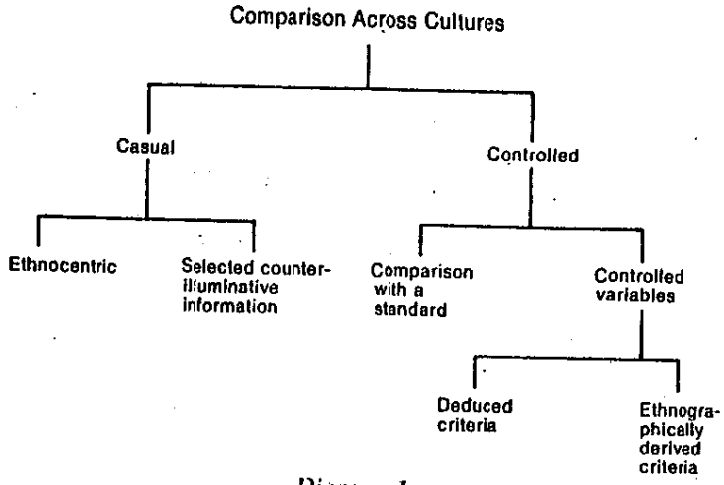

Diagram I

"Controlled" comparison is more complex, and for purposes of the present exposition I have oversimplified it into two sorts. The simplest form is comparison with a standard extrinsic to the particular culture in hand. The source of this standard is of considerable moment insofar as the better it is chosen, the further one can go before bogging down. However, bogging down is, I think, the inevitable end. Ultimately one can say only that something is or is not like something else—that helps, but it is limited.

The "standard" method of comparison is similar to the "application" of "general theory." It is possible to take an idea, like the overlapping spheres in Pospisil's book (1958b:257–271) or Nadel's (1947:504), or the set of boxes in the final chapters of my own *Justice and Judgment* (1957: 150), and utilize that idea as a general theory of law. That is to say, the "facts" of any system can be squeezed into any such classification with greater or lesser force required. That is, however, precisely the same mistake as forcing the data into the boxes of English jurisprudence. All these are way stations on the road to better understanding of legal an-

thropology; we can forge on from them only by new ethnography or new syntheses. The irony is that we never arrive at our goal—or, if we do arrive, it is at the wrong goal or else it proves to be trite. That is the fate of all science—Kingdom Come is a mirage, but the world of here and now must nevertheless be organized to deal with mirages.

The other method of controlled comparison is the method of controlled variables. These variables may either be derived from a "mass-theory," such as the *Outline of Cultural Materials* trait list, or they may be carefully selected and refined in the variation of the method Boas expounded (1896:905) and that Eggan later called "controlled comparison" (Eggan 1954). The criteria for comparison either may be deduced or may be ethnographically derived. Again, to overstate for purposes of exposition, "deduced" criteria are a prioristic. The mode of "ethnographic derivation," on the contrary, is one that is similar to the linguist's mode of determining phonemes in any language: if there is a difference in meaning in two sounds, there are two phonemes and hence a legitimate distinction. The ethnographically derived criteria are all the legitimate distinctions that are made in the culture of the sample—they will not all be ethnographically relevant in all situations, although these distinctions may sometimes be instructive by their very ethnographic absence. Therefore, the "bank" of distinctions is constantly enriched by ethnography and must be simplified and arranged and theorized about by students of cross-cultural comparison. This category of "ethnographically derived criteria" resembles what might be called, in presently fashionable lingo, "comparative ethnoscience." It is the sort of thing that Goodenough is doing with residence patterns, and the sort of thing that Lévi-Strauss began (but did not finish) in *Les structures élémentaires de la parenté* (too early he turned one brilliantly delineated model into his standard —that of kinship based on a model of economic reciprocity—never getting to the other modes of economic distribution—redistribution and market —that also obviously function in the kinship sphere).

My objection to Gluckman's "comparative" is that he swings uneasily between what I have called the "single standard" and what I have called "selected counter-illuminative information." With the first he lays himself open to the possible charge of imprinting an a priori set of categories and concepts; with the second he lays himself open to the charge of selecting only the positive cases. Both may be unjust—but we cannot be sure.

The Culture of Ethnography and the Culture of Comparison

We have now brought ourselves squarely up against the main epistemological problems in anthropology. First of all, how can we secure the

advantages of juxtaposing ideas and analyses from the corpus of ethnographic works with the materials of our own subject cultures without implying that these ideas are in the subject culture? Put another way, if we consider the ethnographic record as a subculture, how do we utilize that subculture in our comprehension of the culture we are reporting and analyzing? The second problem is no less difficult: what is the process by which we can derive from ethnography the variables that are to be used for controlled culture of comparison, utilizing the very material that is the substance of the culture of ethnography?

The key to the first problem, it seems to me, lies in exercising the greatest care in making our own assumptions overt and, perhaps most important of all, in not confusing the bizarre and unstructured totality of ethnography with some kind of logical construct. The organization of an ethnography, on the one hand, focuses on a people—and that includes the special ethnographies that focus on particular problems of a specific people. The organization of a comparative study, on the other hand, focuses on the logical interconnectedness of propositions. The fact that the cultural items may be the same must not be allowed to mislead us. What I am in fact saying, obviously, is that Gluckman is not comparative at all—he is merely calling on an admirably wide arc of the culture of ethnography.

It seems to me that the key to the second problem—how to create a controlled culture of comparison—lies all around us; it is included in a statement by Gluckman (1965b:183) that he meant as a criticism: "If [Bohannan] were correct [about a folk system being raised to the level of an analytical system] we would have to be 'cultural solipsists,' unable to compare or to generalize widely—unless we were to develop a whole new independent language without national home."[1] Gluckman, both here and in his 1967 "Reappraisal," thrusts this idea away as if one could not possibly have meant *that*. However, I did mean that, and it cannot be said better. In fact, the name of the "whole new independent language without national home" will probably be Fortran or some other computer language.

The question, obviously, is how we go about developing "an independent language without national home." I have suggested above that the data to be worked with in constructing a comparative system are

1. This investigation creates for me a direct confrontation with some of the statements that Max Gluckman has made in the last decade or so. I have not heretofore answered any of his statements. I do so now without polemic—and I do not read what he has said as polemic. We are using each other's statements as aids to express our own points of view the more clearly, for we do have some differences of opinion that I, at least, think of some importance to social anthropology. Arguments identical with this one are going on in economic anthropology, with a different cast of characters, and in psychological anthropology with a constantly shifting cast of characters. Obviously all this has something to do with communication among scholars who have basically different philosophical foundations. It is a healthy sign in a healthy discipline.

the folk models that were found to be necessary to explain and describe specific cultures. The comparatists, from Tylor to Gluckman, want to compare substantive material; I want, rather, to compare viewpoints or theories of substantive material. So, obviously, did Boas (1896:908): "The comparative method, notwithstanding all that has been said and written in its praise, has been remarkably barren of definite results, and I believe it will not become fruitful until we renounce the vain endeavor to construct a uniform systematic history of the evolution of culture, and until we begin to make our comparisons on the broader and sounder basis which I ventured to outline." That is, on the basis of full and understood ethnographic accounts, each sufficiently theorized.[2]

In the creation of this "independent language without national home," there are certain things we cannot do. Whether it be a computer language or a "people language," we cannot assume that it will be without bias or known culture. I have already pointed out here that many cultures of completely different types share a good many ideas without that making them similar in origin, or use, or psychic proclivity. Obviously we cannot become biasless—rather, we must investigate our biases and institute controls for them. Obviously human beings cannot compare (or do anything else) without culture; rather, we must control the logic of the culture of comparison.

What, then, is the origin of the propositions in this new logical and independent language—this culture of comparison? We go to the ethnographies. In the best of all possible worlds, we include all of them (though some will, of course, prove useless). We must work somewhat in the linguist's mode of determining the basic phonemic system (by the comparison of controlled pairs), and then by a process that ultimately allows us to find axioms that enable us to deduce the specific folk systems (today this is called "generating" the system in a simulation project), and then by a process that allows us to compare the axioms of one society with those of another, and to investigate the ways in which they bunch, as vectors, and hence are culturally correlated.

These are the neologisms that Gluckman (1955:381) is waiting for. It will take about ten years, it seems to me. We must learn to put up with the theoretical "weightlessness" that comes from escaping from our own gravity. As a matter of fact, when he is not aware of it, Gluckman does a lot of this sort of thing. Certainly his books will be some of the major sources of the propositions for the new language. It is in his admonitory

2. Although Boas is saying all the right things, he did not himself in his ethnography of the Kwakiutl do all the right things. His ethnography is full of detail, but there is very little ethnographic theory to inform it, which is one of the reasons that it takes so much effort to use it today. Boas' Kwakiutl material can be used—but only if one puts almost as much effort into it as Boas himself did. Had he been more willing to create a theory of the Kwakiutl, this would not be so, and Boas instead of Malinowski would be the father of modern field studies.

passages that he narrows down and hides from his right hand many of the interesting and imaginative things that his left hand is doing.

To elucidate the immediate task, I shall revert to a diagram that I first published in the Letter pages of *Science* in 1959. Box A contains the mother culture of the social scientist. Box B contains the models or structures with which the scientists and the other people of his culture deal with in their own lives and their environment—the common-sense "the-

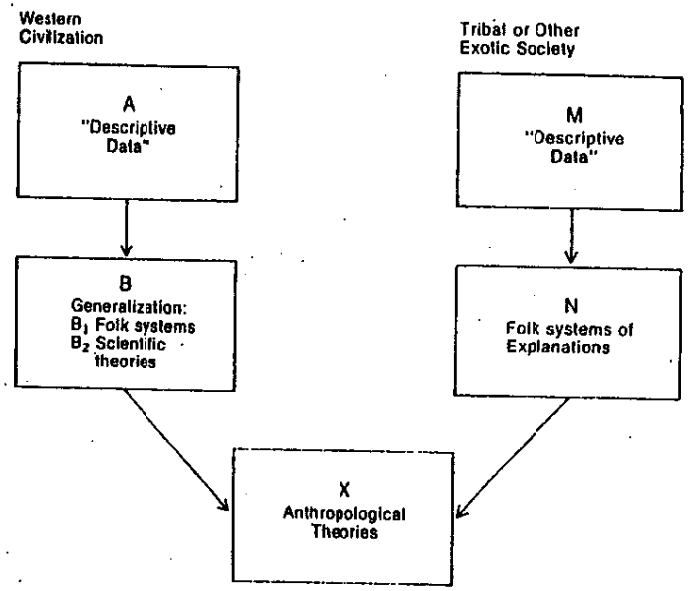

Diagram 2

ories," the myths, the scientific theories, and all the rest. Box M contains the culture of some other society (or of some other subgroup, for one cannot sensibly draw lines between cultures, except in terms of specific problems). Box N is the folk system of explanation that the anthropologist has learned and arrived at in order to explain the arrangement of the data in Box M. The "theories" or generalizations or models in Box N are sometimes also supplied, in writing or other "scholarly" modes of communication, by persons whose mother culture is M. There are, obviously, many boxes M and N—one might say $M_1 \ldots M_n$. And obviously Box A is a type of Box M; Box B is a type of Box N.

Now the new language that we are creating must take its *comparata* from all the Boxes N and including Box B. This new language, called X in the diagram, is a logical structure of interrelated propositions about

the working of society and culture. We can learn a lot about it from any culture, and we can test some of the propositions in Box X against some of the models in the various Boxes N—using, of course, overt and stated criteria (hence open to criticism) of suitability for the selection of those we use.

It is in the theoretical structure in Box X that we are all ultimately interested as social scientists. We must be able to call on all the scientific theories of all the other disciplines (Box B) as well as on the folk explanations (all the other Boxes N) of other peoples of the world. May I repeat: in the immediate case in hand, that of comparative legal anthropology, I have no doubt that more of our theory can be drawn from Box B than from any other box included in the N series of boxes. But I also have no doubt that to confuse Box B with Box X is a logical error leading to a dead end.

This proposition is programmatic, I am the first to recognize. But given a decade or two, it will be well launched in the "real world"—whatever that may be.

MAX GLUCKMAN

Concepts in the Comparative Study of Tribal Law

This is how Nader (1965a:11) summarizes the controversy:

Bohannan's contribution to methodology of description lies in his formulation of the folk versus the analytical system—while not "new," an argument that

strikes at the very core of the translation and meaning problem in anthropology. He raises for inspection statements made by Hoebel (1954:46) and more recently by Gluckman (1962) that encourage anthropologists to discuss tribal law using the principal concepts of Western jurisprudence. Gluckman states, "The very refinement of English jurisprudence makes it a better instrument for analysis . . . than are the languages of tribal law" (1962:14). Bohannan (1957), on the contrary, contends that to describe and analyze the system of justice and judgment of one culture through the interpretation of a system indigenous to a second society can only lead to confusion and distortion. He implicitly charges Gluckman with having converted the Western legal folk system into an analytical system and with having forced the Lozi folk concepts into a Western model. He presents a variety of instances illustrating for example how the Tiv concept of "correction" when translated to "sanction" may misconstrue what final action does actually involve for the Tiv. Ayoub (1961), Gluckman (1962) and Hoebel (1961) discuss the problems inherent in Bohannan's approach, Nadel (1956) and Ayoub (1961) discuss the perplexing questions inherent in Gluckman's assumptions, as does the whole body of literature in ethnoscience. See in this volume the paper by Metzger and Black. The answer probably lies solely neither with Gluckman nor Bohannan, for how an ethnographer goes about laying bare or describing his society is intimately related to what use he believes can be made of such a description. This brings us to the question of comparison. Bohannan's intention is to discover and portray the Tiv view of justice and judgment. He shirks the problems that such a relativistic approach implies for comparison, but then he was not interested in comparison at the time. Gluckman's work has been characterized as analogous to that of a linguist who attempts comparison by jamming Barotse grammar into Roman Dutch categories.

Nader then discusses more seriously some of my proposals on how to set about comparison. Unfortunately Nader does not state who has characterized my work "as analogous to that of a linguist who attempts comparison by jamming Barotse grammar into Roman Dutch categories."[6] This is a vague and imprecise—and an unvalidated—indictment, which would be thrown out in any court of law as not specific enough to enable the defendant to answer. I do make some explicit comparisons of Barotse law with some Roman-Dutch and Roman, as well as with English, legal institutions and rules, but these are explicit attempts made in order both

5. Nader may have noted a statement in Vansina (1965:117) about a Congolese tribe: "Kuba law is thus very different from any European legal system, and to try to define it in terms of European legal concepts is like trying to fit a Bantu grammar into a Latin model of grammatical categories, something that was actually done until descriptive linguistics taught us better." The analogy is not developed, and it is worth noting that the Kupers in their editorial "Introduction" to the book in which Vansina's essay appears, comment: "In fact, Vansina is able to describe the Kuba system in terms of the concepts of Western jurisprudence" (1965b:5). Vansina's footnote reference is to Bohannan's book, not to mine.

to illuminate by contrast and to try to generalize. I shall return below to some of these comparisons.

What I did have to do, writing in English, was to use words like "evidence," "agreement," "right," "duty," "obligation," "law," "tort," and "delict," "injury," "wrongdoing," and so forth; but I attempted on each occasion clearly to specify the Barotse word that corresponds and to set out with examples its connotations and limitations. For of course once we write in English, we have at some point to translate, and this involves using English (or Roman-Dutch) categories. Even Roman-Dutch words, Latin or Dutch, are commonly translated into English save where the Latin is part of our legal vocabulary. But the alternative for an ethnographer, even at the first descriptive level and before analysis, is to remain in the vernacular; and he can then never escape from that vernacular to get into analysis. For at some point in an English work vernacular categories—whether Roman-Dutch, Tiv or Barotse—have to be expounded in a series of English words, whether commonplace words or social-anthropological technical terms or jurisprudential technical terms. In my opinion, indeed, it is the use of the simplest English words, such as connecting prepositions and conjunctions, that is potentially most dangerous.

Hence I do not consider that even Bohannan is able to escape the difficulties of translation, and at many crucial points he uses English terms. But I do of course agree with him that the first task of an ethnographer," or of any recorder of a system of ideas or practices, is to describe as fully and subtly as he can, taking account of all the problems of translation, what Bohannan has called the "folk system." One of the effects of this accumulating, falsely based controversy between Bohannan and myself has been to suggest that I do not agree with Bohannan on this point. On the contrary, whenever I have argued that to stop at the folk system stultifies further analysis, which I consider to depend on comparison, I have at the same time stated how much I admire—and at times envy—the skill, subtlety, and comprehensiveness with which Bohannan reports on Tiv "folk-concepts." (I have added, however, that fortunately he does not abide by his precept and that he continually does compare in words drawn from outside the folk system.)

Despite these statements of mine, the development of the controversy in other hands has made my opposition to Bohannan appear to condemn altogether his procedure; and I am therefore taking this opportunity not only to defend the procedure of comparison through Western jurisprudential words and concepts, but also to try to dissipate this other confusion and to make clear where Bohannan and I are in agreement.

6. I made clear in my preface to the first edition of *The Judicial Process Among the Barotse* that "my first duty was to describe Lozi legal institutions clearly" (1955:xix).

In this light I do not feel that my use of words in common as well as in technical use in English was misleading. In a study of government one may use the word "legislature" to cover British Parliament, American Congress, German Bundestag and Reichstag, French Chambre des Députés, Japanese Diet, all for purposes of general discussion in order to draw attention to similarity while insisting on differences. Some word is necessary for purposes of general discussion. In discussing several systems of law, therefore, one may speak of ownership, contract, property, succession, marriage, betrothal, judge, decision, all to draw attention to a core of similitude while defining differences. It must be a very blinkered mind that, when an anthropologist speaks of contract in an African tribe, immediately thinks of the English or French or Roman-Dutch or Roman contract. "Contract" has a general meaning of enforceable agreement; and one can use it—and must use it—to discuss the different conditions, forms, incidents, and remedies for breach of contract in various social and economic conditions (Gluckman 1965b:Chapter VI).

I used English words because I was writing in English, and I have always felt that it is unfair to readers to ask them to carry in their heads a large number of vernacular terms. Bohannan uses many Tiv terms. And when he does translate, he sometimes avoids obvious English parallels; for example, "Anongo *calls* Iyorkyaha" avoids "summons," with its many implications of complicated processes, though the "call" is apparently authoritative to come before a court, since there is presumably a penalty if the defendant fails to attend; "calls" appears to me to be too weak, and hence misleading. Ayoub (1961:248) considers that Bohannan's method makes for greater conceptual clarity than does my use of English terms. But in fact Bohannan, since he writes in English, can avoid the use of only a certain number of English jurisprudential terms, such as tort, contract, sanction, court, judges. He nevertheless makes

considerable use of English words that have many technical jurisprudential connotations, such as right, duty, obligation, and, in the end, crime. He avoids "contract" and "tort" because Tiv categorize actions akin to these as "debt." But "debt" itself is a very complex conception: and I have argued in the final chapter of *The Ideas in Barotse Jurisprudence* that we can only understand what this Tiv category of "debt" means if we examine also this particular type of categorizing as it occurred in early English law and in a variety of other tribal and early legal systems (Gluckman 1965b:Chapter VII). I cite this example to show here, first, that however determined one is to present a folk system in its purity, one cannot escape from the use of one's own language. Second, and more importantly, it emphasizes that, for deeper understanding, the presentation solely, allegedly, of the ideas of a folk system, restricts one's analysis severely. In sociological jurisprudence, comparison puts problems in more illuminating perspective. If Bohannan insists that Tiv do not distinguish tort and contract but see them as debt only, neither did Roman law until postclassical times (Jolowicz 1939:285–286, 531–535), while only in the seventeenth to eighteenth centuries did the distinction begin to become clear in England (Winfield 1931:20 f.). We may well ask why these several societies had similar categorizations.

Insistence on uniqueness constantly obscures problems. The following example illustrates this point clearly. When the Tiv conduct an autopsy on a man to see if he has practiced witchcraft, guilt is shown by the presence near the heart of colored sacs—presumably collections of arterial and venous blood—called *tsav;* hence Tiv speak of innocence as "empty-chested." Bohannan (1957:199) says that "empty-chested" in Tiv means two things: "a man of no talent and consequence, if he is alive and healthy. Applied to a dead man, it means a person who died for some reason other than his own evil propensity [his witchcraft bringing his doom through the operation of good ritual]. It resembles our own word 'innocent' in that it has connotations of good, but in other usages the derogatory connotations of lack of experience are dominant."

There is, of course, nothing unique in this Tiv idea that to be innocent is to be guiltless, and therefore in some way to be guileless, naive, simple, and foolish, as any dictionary shows. The existence in such widely separated societies of this feeling that the guilty are somehow clever and that the weak and poor are somehow innocent, while the innocent are also somehow foolish, poses a general problem. The same identification is found in Hebrew, Arabic, Latin, and in at least some Bantu languages. There are also differences. Tiv "empty-chestedness" involves beliefs about the responsibility of persons for their kin's misfortunes and successes quite different from our own, and comparison of these differences leads us into fundamental problems of social relations and dogma (see Gluckman in press).

Again, Bohannan has a brilliant and subtle discussion of Tiv concepts

of truth (1957:47–51). To oversimplify, this discussion focuses on two words, *mimi* (more or less our truth) and *yough* (to maintain the even flow of social relationships), as against *yie*, which covers lies, broken promises, and disturbing the even flow of relationships. The weakness comes in the explicit statement that here the Tiv differ fundamentally from the Western judicial systems' view of "truth" as "verifiable 'facts' of what took place." We have only to think of our difference between "white lies" and "black lies" to see that there are similarities; and the fact that one should not tell "white lies" in court against the verifiable facts shifts the focus of comparison to the role of Tiv courts in maintaining permanent social relationships as against the role of our courts. This comparison draws attention to the possible existence of equivalent valuations in other institutions among ourselves, such as the manner in which lawyers, marriage guidance counselors, and similar "adjusters" of disturbed relations work and urge clients to speak the truth. Bohannan's example of a woman who had to be compelled by an oath to give evidence that she had seen her co-wife becoming involved in an adulterous relation could be paralleled, too: in personal relations, the subpoena and the oath excluding perjury may similarly excuse a person for not lying to maintain smooth social relationships.

There are, of course, specific concepts in particular systems that are unique. For these, the vernacular term has to be used, or some descriptive phrase has to be coined. Thus, after the Barotse liberated their homeland from Basuto conquerors, their king allowed returning Lozi to *litulula* —reclaim or liberate—their ancestral lands. In 1927 his successor barred these claims and made it an offense to bring one. The offense is called *mulitu* and I either speak of *mulitu* or of the offense of making an ancestral claim (Gluckman 1955:54, 56 f., 287–288). But putting cattle out to be herded, though it has somewhat different incidents from agistment in Anglo-Saxon law, can be called agistment. The Tiv ideas involved in *tsav* (Bohannan 1957:passim) are so complex that it may be wiser to use *tsav*; "witchcraft substance" is misleading, but "mystical heart-blood" might do.

I feel therefore that everyone will agree with Bohannan that the first task in reporting a legal system is clearly to describe its "folk-concepts." I consider that, after careful and perhaps lengthy description and discussion, very many of these concepts can, without distortion, be given English equivalents, at least out of courtesy to one's readers. But some such step is essential at the next stage, when one essays comparative work. The problem must be tackled at several levels: first, delineation of the tribal conception; second, its comparison with other conceptions of similar type; third, determination of some general word, perhaps with a qualifying adjective, to arrive at a word for comparative analysis. In some cases it may be better to use neologisms, which Ayoub (1961) favors though he does not suggest any. But his discussion of the problems involved is clear and stimulating. Goodenough, Sheddick, Holleman, the Dutch adat lawyers, and I have done so for land tenure. I did so in making a distinction between "tribute" and "things of kingship" among the Barotse, in *The Ideas in Barotse Jurisprudence* (1965b:Chapter V).

Failing the use of neologisms, a research worker writing in his own language is, in my opinion, entitled to try to specialize by stipulation the riches in the vocabulary of that language; and it seems to me that the refinements of English, and in general European, jurisprudence provide us with a more suitable vocabulary than do the languages of tribal law. It might be helpful to use heavy black type or block letters or other printing devices to mark when a word like "law" or "reasonable" is used for general analytic comparison as against reporting.