THE ANTHROPOLOGIES
OF ILLNESS AND SICKNESS

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INTRODUCTION

Medical anthropology, as a field of research, writing, and professional
activity, has grown remarkably since the *Annual Review of Anthropology*
published its last articles on this subject. When Fabrega (42) and Colson
Selby (21) reviewed the 50 years since the publication of Rivers' *Medicine,
Magic, and Religion* (143) in 1924, they could cite only a handful of
monographs and edited volumes. And although they could refer to the
Transcultural Psychiatric Research Review (Montreal) and *Ethnomedizin*
(Hamburg), there was not yet any major journal devoted to this field.

In the past decade, however, a large number of publications have ap-
peared: specialized collections (71, 76, 96, 99, 103, 105, 146, 150, 167, 170);
comprehensive anthologies (10, 65, 100, 114, 116, 123, 164); theoretical
works (43, 93); medical ethnographies (9, 38, 69, 83, 88, 108, 111, 128, 135,
139, 140, 145, 160, 186); large scale studies (20, 33, 169); textbooks (30, 51,
119, 122, 172; 187 is resolutely biomedical despite its title, however); and
a cross-cultural ethnomedical survey (125). Two monograph series are
under way, the "Comparative Studies of Health Systems and Medical
Care," under the editorship of Charles Leslie (77, 93, 105, 113, 163), and
the "Culture, Medicine, and Healing" series, edited by Arthur Kleinman
and others (19, 35, 97). Three major topical journals are now published:
Culture, Medicine and Psychiatry, Medical Anthropology, and *Social
Science and Medicine's* quarterly issues on medical anthropology. The
Medical Anthropology Newsletter, a quarterly published by the Medical
Anthropology Association, has expanded to include original articles and is
now the best single source of book reviews in medical anthropology. In
addition to these periodicals, there is the *Journal of Ethnopharmacology*
and two new European journals, *Ethnopsychiatrica* (Claix, France) and

Curare (Wiesbaden). Finally, there is a long list of publications which are products of recent conferences and symposia in medical anthropology (1, 2, 11, 39, 41, 63, 65, 76, 79, 80, 96, 103, 105–107, 109, 116, 118, 127, 144, 165, 168, 170, 171, 178).

Publications and conferences are only part of the story. A decade ago, most medical anthropologists were either conventionally trained anthropologists who had become interested in medical issues, or they were physicians and nurses who had taken degrees in anthropology. Formal education in medical matters generally took place outside the anthropological curriculum, if it took place at all. But here, too, there are remarkable changes. By 1980, 20 universities in the United States were offering graduate programs in medical anthropology, three-quarters of these at the doctoral and postdoctoral levels (8, 158).

Why has medical anthropology grown so rapidly? In order to answer this question, it is necessary to know something about anthropologists' motives and methods.

Regarding motives, there are two strong reasons for anthropoligists' increasing interest in sickness and medicine. One is connected with the gradual emergence of a distinctive anthropological discourse on sickness, and with it a compelling set of issues and arguments. Since most of what I have to say in the following pages concerns these arguments, I shall set them aside for the moment and turn to a second reason. Here it is the appearance of new professional opportunities and the decline of old ones that have induced anthropolgists to write and do research on medical subjects. Some of the new opportunities originate in the efforts of clinicians who have grown dissatisfied with the biological reductionism that continues to be the conventional wisdom of their profession (17, 34, 36, 37, 44, 92, 95, 154). Under their aegis, anthropologists have been increasingly invited into clinical settings, particularly in connection with programs in primary care and family medicine. [For recent studies by anthropologists working in Western clinical settings, see (3, 15, 18, 27, 51, 58, 59, 62, 70, 84, 92, 104, 110, 115, 118, 136, 141, 142, 152). For comments on anthropologists *as* clinicians, see (72, 147, 148)]. Other professional opportunities for anthropologists originate in the high level of economic support that has been available in the United States, at least until recently, for social scientists who are interested in medical subjects. This combination of professional incentives helps to explain why much of the recent medical anthropology literature is intended mainly for social and behavioral scientists with clinical interests and responsibilities, and for clinicians and medical educators and planners.

Regarding methods, anthropologists have three more or less distinct ways of writing about sickness and healing. First, some anthropologists

describe medical beliefs and practices by using conceptual systems which were originally intended for studying other phenomenological domains. This is clear if we look back at some classic ethnographies which predate the growth of contemporary medical anthropology: Evans-Pritchard's *Witchcraft, Oracles and Magic Among the Azande* (40), Turner's *Forest of Symbols* (161) and *Drums of Affliction* (162), and Spiro's *Burmese Supernaturalism* (151). In spite of the fact that these monographs contain detailed descriptions of medical beliefs and practices (Evans-Pritchard and Turner even include separate chapters on medical practices), anthropologists usually do not think of them as examples of medical anthropology. Rather, we talk about them as belonging to the anthropologies of religion, comparative modes of thinking, witchcraft, ritual and symbol, culture and psychology, and so on. That is, we think of them as originating in problematics and analytical frameworks where sickness events are only vehicles for understanding other constellations of facts. The point at hand is that many of the books and articles cited in the first paragraphs of this review are examples of the very same ethnographic traditions. This is not intended to be a criticism, since many of these publications are excellent and important in their own right (e.g. 64, 85, 128, 134). Indeed, these kinds of analyses may be the best ways to understand medical events in structurally simple, kinship based societies where the experience of sickness is so thoroughly externalized that the body is reduced to an uninformative "black box" and people's attention is concentrated on the social and symbolic conditions of sickness. [See, for example, Lewis's study of the Gnau of New Guinea (108).]

Second, other anthropologists write about sickness and healing by using methodologies and conceptualizations they have borrowed from medical sociology. As a field of research and writing, medical sociology was established relatively early. It is now thoroughly conceptualized, and its methods and perspectives have been tailored for conducting research in industrial societies. In each of these respects, it contrasts with medical anthropology, and it is no surprise that anthropologists who have only recently begun to study sickness in industrial societies would be tempted to borrow from sociology. Unfortunately, the tendency has been to borrow from empiricist (cf "empirical") sociology. It is unfortunate because social anthropology and empiricist sociology are separated by different premises about their facts. In a nutshell, the empiricist works an epistemology-free social science. He supposes that his language and techniques, once they have been suitably refined, uncover facts about the world rather than produce them. How is this different from social anthropology? Seen from one angle, social anthropology is a science in continuous pursuit of a satisfactory epistemology. What separates the anthropologist from empiricists is that he regards his

own concepts and ideas as simultaneously privileged *and* a part of a cultural system. That is to say, he thinks of his ideas as being suitable for interpreting other peoples' beliefs about the world but, at the same time, knows that his ideas, like the beliefs they are intended to interpret, are products of particular historical and social determinants. The mere fact that he can justify his ideas to the satisfaction of other anthropologists and social scientists or that they seem reasonable to his educated countrymen does not remove them from his scrutiny (181). The anthropologist's willingness to scrutinize his own concepts as a cultural system, to want to know and justify his own context of belief, helps explain both the vitality of anthropological discourse and its "failure" to get beyond what Thomas Kuhn called the preparadigm stage of science.

Given anthropology's cross-cultural field, can it be otherwise? It can when medical anthropoligists incorrectly suppose that epistemological questions are limited to the task of interpreting non-Western belief systems and the field of "ethnomedical" inquiry [an inquiry which ostensibly focuses on beliefs and practices "not explicitly derived from the conceptual framework of modern (allopathic) medicine" (75, p. 99)]. When this happens, and epistemological scrutiny is suspended for Western social science and Western medicine, empiricist-leaning anthropologists are free to adopt, as part of their own conceptual apparatus, the conventional wisdom of the dominant medical culture of their society. I am thinking of concepts such as "stressful life events," "coping mechanisms," "life styles," and "socioeconomic status," which, having been raised out of the culture of the middle classes into the halls of science by empiricist sociology and social psychology, desocialize sickness and the social scientist's knowledge of sickness. Desocializing concepts work by displacing the historical, political, and economic determinants of sickness. In place of social determinants, now reduced to ghostly "situational variables" and "attributes," empiricists establish the primacy of the individual and his values, motives, dispositions, and perceptions.

The danger in empiricist social science is that, having fragmented the social relations of sickness particular to Western society, social scientists will help to reproduce these same social relations (180). For example, Foster and Anderson, in their book *Medical Anthropology* (51), describe the United States as a "complex society, with multiple life choices" (51, p. 196, in the course of a discussion of careers in nursing). In this society, they write, disproportionately few physicians come from "lower socioeconomic levels," because lower socioeconomic parents are unable to sustain the urges and ambitions of those of their children who initially want to be doctors [(51, p. 178) citing (66)]. Writing about regions of the world undergoing rapid change, Foster and Anderson attribute "the diseases of civilization"

to the tendency of people to adopt health destructive practices, such as excessive use of alcohol, and "just plain unhealthy personal life styles" (51, p. 124). Multiple life choices? Lower socioeconomic levels? Diseases of civilization? Of course. But life choices are not randomly distributed in our complex society, nor are the diseases of civilization randomly distributed in less developed countries. Through some mechanism, people at a "lower" level get mainly wretched choices, such as choosing to work where they will be exposed to toxic substances and choosing to be periodically unemployed or chronically underemployed. And this same mechanism distributes to a "higher" level a large number of generally more healthful choices. The two sets of choices do not merely co-occur: each helps to determine the other (cf 51, pp. 21–22). That Foster and Anderson ignore the social relations of sickness is consistent with a position Foster takes in an earlier article, where he refers to the "medical behavioral science specializations" of medical anthropology and medical sociology (50). Anthropologists, he writes, study ethnicity and cultural affiliation, beliefs and practices, values and premises, while subjects such as class and economic differences, and professions and professionalization belong to sociologists.

Allow me to summarize what I have written up to this point. I began by calling attention to the rapid growth of research, writing, and professional activity in medical anthropology over the last decade. To understand these developments, I wrote, it is necessary to know the various incentives anthropologists have had for entering this field and the three options they have had for responding to these inducements: they have had to rely on either (*a*) conceptual systems originally intended for describing and analyzing other phenomenological domains (such as ritual behavior), (*b*) methodologies and conceptualizations borrowed from empiricist medical sociology, or (*c*) an evolving conceptual system centered on the social and experiential particularities of sickness and healing. Finally, I described the limitations of options (*a*) and (*b*). The rest of this review is about (*c*).

THE ANTHROPOLOGY OF ILLNESS

A good place to begin is with Frake's paper, "The Diagnosis of Disease among the Subanum of Mindanao" (52). In this paper, Frake set out five propositions: 1. People depend on cognitive structures to organize their behavior and make their decisions. The cognitive structure of sickness is implicit in utterances which can be elicited from informants by means of standardized questions (e.g. "What kind of illness is that?"). 2. In the case of the Subanum, knowledge of skin diseases is structured by means of a taxonomy, so that each term is distinguished from any other term by at least one unshared attribute or by a different degree of specificity (i.e. by horizon-

tal or vertical contrasts in a taxonomic hierarchy). 3. Although people may disagree about what name to give some actual symptom or set of symptoms, disagreement occurs within the shared taxonomy. 4. Disagreements occur because taxonomic categories are discontinuous but disease and nature are continuous. Disagreements also result from "social role contingencies" (e.g. a speaker's desire to avoid putting his own symptoms into a stigmatizing pigeonhole), dialect variations, and the proclivities of taxonomic hairsplitters. 5. There are no essential differences between the ways in which people organize sickness and other phenomenological domains such as botany.

Frake's paper is a famous example of the formal analysis of ethnographic data, but it belongs to the prehistory of medical anthropology. Sickness, for Frake, is a vehicle for pursuing other interests. I begin with his paper so that we can have a point of reference from which to follow the trajectory of a group of anthropologists, identified with the *explanatory model of illness approach,* whose work does depart in a distinctive way from earlier understandings of sickness. [In a moment, we shall see that explanatory model writers believe that Frake's sort of analysis has only very limited relevance for studying sickness. Not all medical anthropologists share this opinion, however (e.g. 86). In addition, several writers have recently proposed formal models for analyzing how decisions are made during sickness episodes (30, 184, 185).]

A key argument in the explanatory model approach first appeared in a 1977 article by Byron Good, titled, "The Heart of What's the Matter" (61). He elaborated this in a second paper (62), written together with Mary-jo Good. Like Frake, Good stresses the importance of language. Human illness, he writes, "*is fundamentally semantic or meaningful and . . . all clinical practice is inherently interpretive . . .*" (62, p. 175). But there are fundamental differences between Frake and Good regarding how the language of sickness ought to be interpreted. Frake's philosophy of language is the formalist tradition of anthropological semantics, associated with writers such as Ward Goodenough. Good's sources, on the other hand, are writers on the anthropology of ritual and symbol, notably Victor Turner, and contemporary hermeneutic philosophers, among whom he cites Hans-Georg Gadamer and Paul Ricoeur. Good rejects Frake's positions 2 and 3, and argues that a sickness term is not equivalent to a set of defining symptoms, nor is it a "neatly bounded 'category', defined primarily in distinction to other categories." According to Good, each term has a distinctive configuration of meanings, but there are overlapping associational patterns among terms (61, p. 40). He also rejects Frake's positions 4 and 5, on the grounds that sickness terms must be understood in the context of being sick, where "an illness or symptom condenses a network of meanings for the sufferer . . ." (62, p. 76). To argue otherwise, as Frake does in proposition 1, "di-

rects our attention away from the social and symbolic context which gives an illness category its distinctive semantic configuration" (61, p. 38).

Good uses the term *semantic illness network* to label the "network of words, situations, symptoms and feelings which are associated with an illness and give it meaning for the sufferer" (61, p. 40). Good's case studies come from a Turkic-speaking region of Iran (61) and an out-patient clinic in the United States (62), and they are mainly about chronic complaints. For each of his case studies, Good followed the same procedure. Over a period of months, he collected information about symptoms, choice of therapies, social situation, and the etiological circumstances his informant reported to him; special attention was given to how social and situational changes affected the illness's semantic configuration during this time. Good describes his methodology as a kind of "social free association" which enabled him to gain entry to the distinctive reality apprehended by informants (61, p. 39, 62, p. 168). In this way, he reconstructed his informant's semantic illness network. [See Bibeau's elaboration of this scheme (12).]

In his analysis of Iranian semantic illness networks, Good introduced the notion that networks are organized through *core symbolic elements.* The Iranian's heart and heart distress are described as core symbols in this paper. Good's idea of core symbols closely parallels Turner's notion that there are "dominant symbols" which organize the meanings of rituals in preindustrial societies (161; 162, chap. 6). Like dominant ritual symbols, core symbols are polysemic in the sense of linking up with different symbolic domains, and this explains why semantic illness networks include such heterogeneous elements. Good describes an Iranian case study in which the core symbol links up childbirth, miscarriage, pregnancy, blood, pollution, weakness, menstruation, oral contraceptives, infertility, loss of vitality, old age, sorrow and sadness. In another case study, heart distress connects a different, but overlapping, configuration: old age, sorrow and sadness, ritual mourning, worries about poverty, anxiety, interpersonal problems with particular relatives, nerves, madness, and blood problems. Like dominant ritual symbols, core symbols link ideological elements (especially values linked to normative behavior) to emotional-physical ones in such a way that the semantic illness network develops a degree of unity in spite of its complexity and heterogeneity. Within the semantic illness network, core symbols form "a symbolic pathway which links the values and aspirations of purposive interaction, the stresses, shames and disappointments of social contingencies, and the affective and ultimately physiological elements of the personal" (61, p. 39; 90, p. 209). [For other analyses of core symbols in sickness, see (132, 133).]

Semantic illness networks are also described in the work of Blumhagen and Kleinman. Blumhagen's article (13) is about how hypertension is per-

ceived by a population of Americans who suffer from this disease. It would be interesting to compare the descriptions given by Blumhagen's informants with the accounts of Good's Iranians, since the physical location of their core symbols is similar, i.e. the circulatory system, while their semantic configurations are very different, of course. Unfortunately, it is difficult to make the comparison because Blumhagen's approach to semantic illness networks departs from Good's in important ways. First, Blumhagen collected his data from a relatively large number of people, 105 clients of a Veterans Administration hospital. Each semantic illness network was constructed on the basis of a single interview, using an open-ended questionnaire. Second, Blumhagen organized his informants' responses into what he calls "nodes" and "arrows." Nodes consist of reports of symptoms (e.g. dizzy spells), interaction (e.g. family arguments), physiological functions (e.g. ballooning veins), body states (e.g. overweight), pathogenic agencies (e.g. acute stress), and activities (e.g. smoking, eating pathogenic agencies (e.g. acute stress), and activities (e.g. smoking, eating salt). Arrows identify *causal relations* between nodes (e.g. high pressure causes ballooning veins which cause ruptured blood vessels). Blumhagen reduced 1300 individual reports (nodes) to 59 categories, and redrew his arrows to indicate the relative frequency with which his informants reported causes and effects.

But this is very different from what Good proposed to do. What separates Good's work from earlier, more traditional, anthropological views of sickness is the idea that an informant's statements need to be interpreted in the context of his illness experience, and close attention needs to be given to the way his statements change over time in response to his circumstances. In brief, Good says that semantic illness networks are inseparable from the idea that illness is an individualized process. Another difference with Blumhagen is that Good takes the more challenging position that although some of the elements (nodes) in semantic illness networks are linked by cause and effect, others may be associated in noncausal ways.

Now for Arthur Kleinman. Kleinman is probably the most influential and prolific writer in medical anthropology today. In his work, semantic illness networks have been made part of a comprehensive framework. Before taking up Kleinman's use of semantic networks, however, I want to say a few words about his framework.

Like Good and Blumhagen, Kleinman rejects the physicalistic reductionism of the biomedical model and replaces it with the following scheme:

DISEASE refers to abnormalities in the structure and/or function of organs and organ systems; pathological states whether or not they are culturally recognized; the arena of the biomedical model.

ILLNESS refers to a person's perceptions and experiences of certain socially disvalued states including, but not limited to, disease.

SICKNESS is a blanket term to label events involving disease and/or illness. According to Kleinman, medical anthropologists need to remember that their domain is sickness, even though their special contribution will be mainly with regards to illness. [For similar positions, see (34, 42, 43). On the anthropological study of illness and disease, see (4, 46, 81).]

Kleinman's interest in medical beliefs and practices is essentially clinical. For him, this means concentrating on what he calls the "core clinical functions," i.e. how systems of medical knowledge and practice enable people to (a) construct illness as a psychosocial experience; (b) establish general criteria suitable for guiding the health care seeking process and assessing the potential efficacy of different treatment approaches; (c) managing illness episodes through communicative operations such as labeling and explaining; (d) providing healing activities (therapeutic intervention, supportive care); and (e) managing therapeutic outcomes (including chronic illness and death) (93, pp. 71–72).

The clinical process is, then, *a way for individuals to adapt* to certain worrisome circumstances (see also 32, 45). [Kleinman does not believe that every medical practice is necessarily adaptive in the long run, however (93, p. 150).] The adaptation premise is reflected in Kleinman's choice of words: guiding, managing, coping, explaining, negotiating alliances. [For similar perspectives, see (48, 49, 131, 132).] The premise is also implicit in the importance he gives to healing in his theoretical and empirical studies. Although Kleinman does not discuss the term "healing" in detail, he uses it in a way that allows me to gloss it as a process by which (a) disease and certain other worrisome circumstances are made into illness (a cultural construction and therefore meaningful), and (b) the sufferer gains a degree of satisfaction through the reduction, or even elimination, of the psychological, sensory, and experiential oppressiveness engendered by his medical circumstances. Kleinman's conception distinguishes healing from curing in a way which parallels the difference between illness and disease (see Figure 1). But it is important to recognize that he is distinguishing between culture and nature, not between mind and body. In Kleinman's work, healing is *not* a mentalistic activity, although it is bound to the feelings, perceptions, and experiences of the individual. So, for example, pharmacodynamic intervention and its effects on the body are part of the healing process even when they are also part of the *curing process,* i.e. the process affecting pathological organic states. On the other hand, there are occasional instances in which "the construction (or reordering) of cultural meaning may be all that
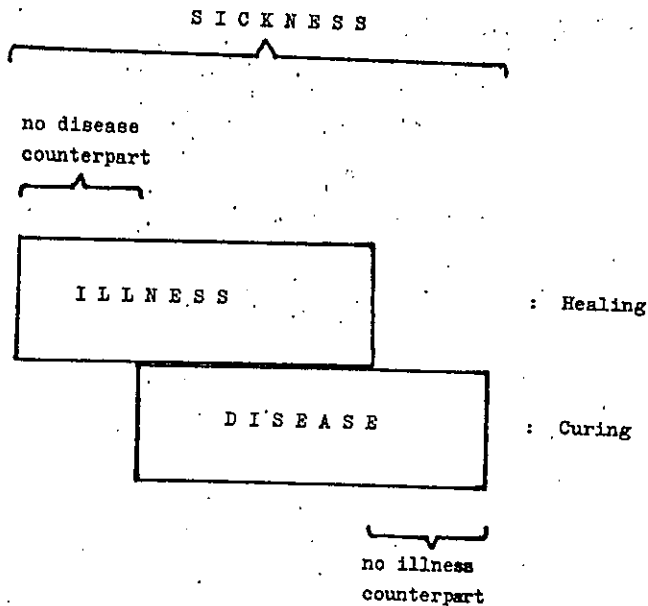
Figure 1   The disease-illness approach.

therapy (and efficacy) consists of' '9. p. 235); see also a detailed treatment of this issue in (98)]. By asserting the complimentarity of mind and body, healing and curing, Kleinman and his associates reject the crude Cartesianism of the biomedical model of sickness. Other anthropologists, notably Moerman (121), have moved far beyond this point to argue that efficacy needs to be understood in terms of the mind's organically mediated effect on bodily processes or, put into other words, the effectiveness of healing upon curing (see also 89). Moerman's radical anti-Cartesianism has been forcefully criticized by Brody in a way that seems consistent with Kleinman's own premises about sickness (16).

Although Kleinman refers to semantic illness networks in his work, another notion looms larger. This is the idea that semantic networks are related to systems of medical knowledge through people's *explanatory models of illness*, a concept which Kleinman abbreviates as "EM". An EM is a set of beliefs which "contain any or all of five issues: etiology; onset of symptoms; pathophysiology; course of sickness (severity and type of sick role); and treatment" (91, pp. 87–88). At first sight, EM seems to be merely a label for the idea that every culture has its particular explanations for sickness. Kleinman is saying more than this, however. First, his EM concept resembles Geertz's idea that cultures provide people with ways of thinking that are simultaneously models of and models for reality (57): EMs

simultaneously create order and meaning, give plans for purposive action, and help to produce the conditions required for their own perpetuation or revision. Second, Kleinman attributes EMs to individuals, not cultures. According to him, EMs are unlikely to be homogeneous even within the same community. Moreover, a person's EM is likely to alter over time, in reponse to his particular medical experiences and to the clinical encounters in which he becomes acquainted with practitioners' EMs. In order to keep track of these various EMs, Kleinman distinguishes them along one dimension in terms of differences between practitioner EMs and lay EMs; along a second dimension in terms of how a Western practitioner's Theoretical or Scientific EM (which he shares with other practitioners; formalized in printed texts) leads to his Clinical EM (particularized by his clinical experiences), and how a layman's Popular EM (shared by a community of laymen) leads to his Family EM (particularized within a family) and ends up as his Individual EM; and along a third dimension in which a Clinical EM or Individual EM passes through its various editions (93, pp. 106–11). In addition to Kleinman's work on this subject, see Helman's papers (73, 74) on how Clinical and Individual EMs of "colds," "fevers," and psychotropic drugs are used and sometimes transformed during clinical transactions; also see Gaines's paper (56) on the differences he found among the Clinical EMs of a group of residents in psychiatric medicine.

Now that we know what an EM is, we can return to the issue of semantic illness networks. In his work, Kleinman refers to both EMs and semantic illness networks, and he even includes them within a single diagram (93, p. 108). The relation between them is not made clear, however, and there are points at which they sound very similar: e.g. "Vagueness, multiplicity of meaning, frequent changes, and lack of sharp boundaries between ideas and experiences are characteristic of lay EMs" (93, p. 107). In spite of this, there is a fundamental difference in the ways in which the concepts are used. Most of the time, Kleinman employs the idea of explanatory models to show how his informants produce their statements about sickness. EMs emerge in his writing as sets of propositions or generalizations. Sometimes they are explicit, but often they are tacit, hidden away in bits and pieces in nonmedical discourses. Also, it seems, EMs are usually about causes and effects, and it is this quality that makes them useful as models for trying to control and predict what is going to happen or to give moral significance to what has already happened. If I am correct, *semantic illness networks are the products of EMs*. That is to say, an informant's semantic illness network refers to a set of statements he actually makes over a given period of time, using EMs to respond to particular contingencies, e.g. an episode of sickness, an anthropologist's questions. When, for instance, Good refers to "core symbols" and describes the Iranian heart as an idiom for expressing emotion in the

course of illness, he is referring to a frequently used element in Iranian EMs. And when Blumhagen uses 105 semantic networks to construct a single arrangement of nodes and arrows and transforms the indeterminacy of many links into causes and effects, he seems to be moving from semantic illness networks (configurations of statements) back towards the Popular EM that produced them.

The exact relation between an EM and a semantic illness network is complicated by the fact that an informant's EM may be changing during the period in which his statements are being made. But the EM idea retains a degree of ambiguity even after its complex and dynamic character has been taken into account. One reason for ambiguity is that it is not always clear how EM writers intend to use the notions. On different occasions, writers have advocated the EM approach in terms of its (a) *analytic* importance, as an instrument for studying the healing process and "clinical transactions" between patients and practitioners; (b) *pedagogic* importance, as a framework for teaching practitioners the meaning of illness and the limitations of the biomedical model for clinical practice; and (c) *clinical* importance, as an instrument for determining the priorities and concerns of patients, exploring problems of noncompliance, negotiating therapeutic alliances with patients (especially when they are ethnically distinct), and choosing and evaluating treatments and methods for managing therapeutic outcomes (14, p. 181; 93, chap. 3; 94).

The issue of EM ambiguity aside, it is important to recognize that this particular combination of claims—analytic, pedagogic, clinical—is intrinsic to the EM approach. EM advocates justify their efforts to develop this framework by pointing to its unique practical advantages (pedagogic and clinical). At the same time, they look at the Western clinic as a place for transforming theory, i.e. a place into which their concepts and methods can be introduced, tested, and refined (e.g. 84, 110). Put into other words, the analytic, pedagogic, and clinical claims of EM advocates are fused through a praxis orientation.

THE SOCIAL RELATIONS OF SICKNESS

While Kleinman and his associates have been establishing the EM approach, other anthropologists have developed a position which gives primacy to the social relations which produce the forms and distribution of sickness in society. It would be going too far to say that these writings present a unified view of sickness, paralleling the work of EM writers. However, it is possible to see in their work the start of a rival approach, rooted in diverse sources but especially indebted to Evans-Pritchard's analysis of Zande medicine (40), Gluckman's study of the social organization

of ritual behavior (e.g. 60), various Marxist writers, and Michel Foucault. In the next pages, I am going to describe this social relations of sickness view by comparing it with the EM approach. The latter sets out in a clear fashion many of the fundamental issues in which a distinctive medical anthropology will be built: the distinction between disease, illness, sickness; the connection between sickness and people's statements about sickness; and the practical (e.g. clinical) role of medical anthropology. For this reason, comparisons with the EM approach are an expedient way to show what these other writers have in common. Moreover, some of these writers are arguing explicitly against the premises of the EM approach.

Sickness and Healing

We begin with the juxtaposition of disease and illness in the EM approach. It will be recalled that EM writers adopted this disease-illness scheme as an alternative to what they saw as the Cartesian dualism of the biomedical view, i.e. in reaction to the view in which disease = sickness and the consciousness of the patient is bracketed out. The usefulness of the scheme has been challenged by Frankenberg (53) and myself (176, 181). In our separate articles, we argue that while the biomedical and disease-illness views clash in one respect, i.e. over the issue of physicalistic reductionism, they resemble each other in another, equally important way. Specifically, both views take *the individual* as their object and the arena of significant events. The point is that the disease-illness view does not require writers to give an account of the ways in which social relations shape and distribute sickness. In this respect, it is not so different from the biomedical model as its advocates seem to believe.

From a distance, this appears to be an unfair criticism, since Kleinman makes several references to social determinants of medical behavior in his monograph on Chinese medicine (93, pp. 24, 202, 288–89) and Good, too, mentions the importance of relations of power in medical practice. But the fact is that neither Kleinman nor Good actually undertakes such an analysis in his own work. What is more to the point, their scheme gives them no compelling reason to make this analysis. Look at it from their point of view: (a) their theoretical interests focus on clinical events and the healing (illness) process; (b) looking outward from the clinical setting, social relations can be seen to radiate dyadically, i.e. between patient and practitioner, patient and individual family members, and so on; and (c) while there are social factors that lie outside this circle, they can be deferred indefinitely without seriously affecting our ability to study the healing process.

According to its critics, there are two problems with the EM view. First, it overlooks the fact that power originates and resides in arrangements between social groups and between classes. Power is merely manifested in

interpersonal relations, and for this reason a focus on dyadic relations fragments and desocializes the true nature of power (180). Second, the path followed by EM writers seems reasonable only because there is a term missing from the disease-illness scheme on which their work is based. To understand this criticism, it is necessary to see how the disease-illness scheme is rewritten by Frankenberg and myself:

DISEASE retains its original meaning (organic pathologies and abnormalities).

ILLNESS is essentially the same, referring to how disease and sickness are brought into the individual consciousness.

SICKNESS is no longer a blanket term referring to disease and/or illness. Sickness is redefined as the process through which worrisome behavioral and biological signs, particularly ones originating in disease, are given socially recognizable meanings, i.e. they are made into symptoms and socially significant outcomes. Every culture has rules for translating signs into symptoms, for linking symptomatologies to etiologies and interventions, and for using the evidence provided by interventions to confirm translations and legitimize outcomes (24, 117, 130). The path a person follows from translation to socially significant outcome constitutes his sickness. *Sickness is, then, a process for socializing disease and illness.* For example:

1. In "pluralistic" medical systems, a single set of signs can designate more than one sickness (137, 138), and social forces help to determine which people get which sicknesses. This happens, for instance, when a particular set of signs can be referred to different kinds of diagnostician-therapists, but the sickness domains of the different practitioners do not overlap (they each own a distinctive set of etiologies and proofs), and the services of the different practitioners are not equally accessible to all sectors of the patient population. In this case, disease and illness are socialized through the arrangements which determine who gets what practitioners and interventions (55, 68, 129, 173, 174).

2. In Western society, the right to translate signs into socially important symptoms is dominated by a single kind of practitioner and a more or less unified set of etiologies. But here, too, social forces determine that individuals with the same set of signs are sometimes allocated different sicknesses. This is clear, for example, if we compare clinical events which are organized through practitioner-client relations (e.g. the physician is paid through a fee-for-service arrangement, and the patient is his client) with clinical events which are organized in terms of practitioner-patron relations [e.g. the physician receives a salary and a position in a bureaucratic career structure, so that his employer is also his patron (29, 82)]. Another example occurs when translations and etiologies are publicly contested and resolved through legal rather than medical means [e.g. diseases of the workplace, such as Brown Lung (6)]. More commonly, though, the social relations of sickness in

Western society take a form in which the same sickness (translation) leads to different illnesses and different cures according to the sufferers' particular economic and social position.

3. Symbols of healing are simultaneously symbols of power. Specific views of the social order are embedded in medical beliefs, where they are often encoded in etiologies and beliefs about the sources of healing power (25, 26, 156). These ideological views are brought into the consciousness of individuals in the ceremony, dramaturgy, and demonstrations of efficacy that make up healing practices. In other words, medical practices are simultaneously ideological practices when they justify (a) the social arrangements through which disease, healing, and curing are distributed in society (e.g. 124), and (b) the social consequences of sickness (e.g. the patient's liability for disease he contracts in the workplace).

Just one more point in this connection. Earlier I wrote that when EM writers turn to clinical events they adopt the premise that descriptions and analyses of medical practices should focus on their *adaptive* functions. [Some anthropologists have adopted a stronger, and probably tautologous, version of this premise: the fact that people choose to perpetuate a particular medical practice is evidence that it *is* adaptive, as a form of healing if not necessarily curing (4).] This issue is complicated by the fact that some societies, e.g. the Japanese according to Lock (112, 113), themselves underline the putatively adaptive function of their medical practices. This premise entails problems when writers see adaptation mainly from the point of view of the sufferer and so neglect the fact that many medical practices develop and persist because they are useful for other people and for reasons unconnected with curing and healing. For example, Strong (155) calls attention to the ways in which physicians employ certain clinical practices to control the emotionality of their clients and themselves, and to control the agendas and lengths of clinical sessions (see also 22, 23). The most important problem, though, is that the adaptation premise marginalizes the fact that *sickness rather than illness* determines the choice and form of many clinical interventions, transactions, etc. In other words, people are sometimes forced into sicknesses that make their situations more difficult for them (173). The fact that patients are liable to interpret such socially determined events in "adaptive" ways (i.e. give them self-serving and self-preserving meanings) is not especially interesting, nor is it a distinctive feature of medical situations (153).

Forms of Medical Knowledge

EM writers are correct when they say that informants' statements about sickness are complex and often ambiguous. They are right, too, to say that the anthropologist's job is to search for the inner logic of these statements, i.e. the reasoning which connects perceptions, beliefs, knowledge, action,

and reflection. But have EM writers drawn the correct conclusions from their own observations? They have pointed out that statements are complex and ambiguous, but can they explain *why* this situation should have come about in the first place?

In recent papers (182, 183), I criticize EM writers for trying to explain the surface meaning of informants' statements in terms of a single set of underlying cognitive structures, i.e. explanatory models of illness. Informants' statements seem complex, I argue, because they often juxtapose different kinds of knowledge (159, 188, 189). A speaker does not necessarily know all of his facts in the same way, and he often gives different, epistemologically distinctive, accounts of his sickness at the same time.

One of my papers (182) outlines a scheme for identifying the different kinds of medical knowledge that appear in people's statements. The scheme describes 1. theoretical knowledge, which organizes discrete events, experiences, etc. into classes (e.g. "This is a case of influenza"); 2. knowledge of events etc. in terms of their empirical particularities; 3. knowledge rendered existentially coherent with the thinker's previous experiences, his assumptions about human nature, man's fate, etc; 4. knowledge which the speaker has transformed in order to make it intelligible to other people; and 5. knowledge he has produced by negotiating meanings with other people. This scheme is not a typology, since each form of knowledge—theoretical, empirical, etc—is a distinctive account which emerges in a process of knowledge production that the thinker is continually undertaking, as he responds first to one contingency and later to another (e.g. choosing efficacious therapies, giving socially acceptable accounts of his behavior). Within this production process, the different forms are linked dialectically so that over time they are continually transforming one another. Thus, no one form is, a priori, the speaker's authentic knowledge of medical events, and no single set of cognitive structures can be said to be the ultimate source of the surface meaning of his statements.

In this scheme, explanatory models of illness are only one of several possible forms of theoretical knowledge. This explains why EM writers are incorrect if they assume that explanatory models are necessarily implicit in all of the statements an informant makes about sickness (101, 177). The scheme describes two other forms of knowledge that, like explanatory models, shape people's understanding of sickness, determine their statements, affect their motives, etc. These are "prototypes" and "chain complexes," concepts borrowed from Hallpike and Vygotsky (67, 166: see Figure 2).

People use *prototypes* just as they would employ an EM, to organize the events and circumstances they are experiencing. Although they are both forms of theoretical knowledge, prototypes and EMs are dissimilar in im-
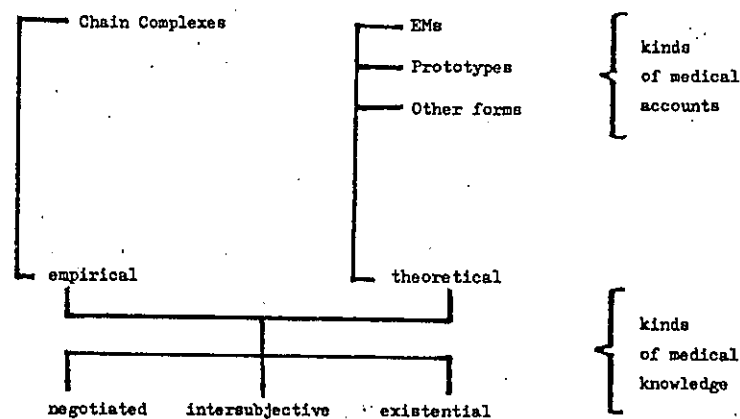
Figure 2 Kinds of medical knowledge and accounts.

portant ways. An EM, it will be recalled, consists of strongly causal propositions, and this is a reason that EMs are so important for medical practices and thinking: they enable people to formulate plans of action. Further, EMs are generally shared by relatively large numbers of people. A prototype, on the other hand, is never more than a string of events and circumstances recalled from the past, usually from an earlier sickness episode. Its elements (symptoms, sensations, outcomes, etc) are connected as causes and effects, but more often they are associated by simple contiguity, chronology, and resemblance. Further, a prototype is characteristically limited to either a small circle of people (a family for instance) or the thinker alone (cf Clinical, Family, and Individual EMs, which are dialects of widely known Scientific and Popular Ems). Because prototypes are neither widely shared nor strongly causal, they are difficult to incorporate into diagnostic and therapeutic practices, at least in large scale societies where internalizing and desocializing medical belief systems dominate people's practices (176). For these reasons, the contents of a prototype are relatively unstable, and elements are likely to be simplified, lost, or transformed as time goes by. *Chain complexes* are superficially similar to prototypes: they, too, are often acausal, limited to small numbers of people, unstable. But whereas prototypes are a form of theoretical knowledge and an instrument for analogical reasoning, chain complexes stand only for themselves. They are products of experience (and possibly unconscious forces): simply strings of empirical events, sensations, symptoms, etc which cohere and persist in the mind because of the salience, contiguity, and chronology of the individual elements in the life of the thinker.

There are two reasons why anthropologists should want to be able to recognize prototypes and chain complexes. First, they explain why some

statements about sickness seem complex and ambiguous: an informants' statements about sickness are occasionally products of a *combination* of loosely connected EMs, prototypical experiences, and chain complexes (see 183 for an illustration). Second, people occasionally use prototypes and chain complexes to express dimensions of sickness which are excluded by EMs. In Western society, for example, people sometimes use prototypes and chain complexes to give accounts of sickness in social and biographical terms. Taussig describes this use in an article (157) based on his conversations with a woman repeatedly hospitalized for degenerative and chronic ailments. Taussig, a physician-anthropologist, characterizes this patient's relations with physicians and nurses as alternating between "alienated passivity and alienated self-assertion," a pattern, he claims, which is often observed among hospitalized patients. As Taussig describes this case, the woman's passivity is a product of the practitioner's Clinical EM and her own, complementary, Individual EM. These EMs, and the practices and ceremonies through which the hospital staff materialize them, reduce the facts of her sickness to a case of intrasomatic betrayal, and transform her from being the subject of her history into a passive object of an ostensibly benevolent medical science. On the other hand, her outbursts of aggressive, purposeless, and sometimes violent self-assertion originate first in her knowledge of a *chain complex* of socially and economically determined events and circumstances that pushed her along the path of misfortune and hospitalization, and second, in her knowledge of her sickness as a *prototype* of the medical events which now seem to be overcoming her own daughter and granddaughter also. In Taussig's account, chain complex and prototype are the idiosyncratic, inchoate, and frustrating counterpoint to the powerful desocializing accounts produced by Clinical and Individual EMs. They are merely facts without a discourse.

In Taussig's case, conventional medical practices marginalized his patient's prototypical and chain complex accounts of sickness. What happens in tribal societies, though, where medical beliefs are socialized and the sick person's widest social group is a small-scale society? An excellent account by Sindzingre and Zempleni takes up this question regarding the Senufo of the Ivory Coast (149). After establishing the point that the Senufo produce heterogeneous medical facts and accounts, Sindzingre and Zempleni describe how Senufo divination-diagnosis actively incorporates prototypes and chain complexes. Here, contemporary sickness events are conceived as the reactivation of earlier events and they are "reinserted and stored in the collective memory of the matrilineage; ... the divinatory device has the function of feeding this memory with its proper constituent instances and recollections ..." (149, p. 279).

Medicine as an Ideological Practice

In his article, Taussig (157) adopts a position, originating with Georg Lukacs, that in Western society the commodity mode of production dominates the social order and creates a situation in which human beings and their experiences are constructed as dehistoricized objects-in-themselves. Taussig's point is that Western medicine is, among other things, part of the ideological apparatus of this social order. Its practices are ideological practices because they produce evidence for the view that society is merely the sum total of its constituent individuals and that social forces are nothing more than the equilibrium which arises from the multitude of these separate wills and fates. Scientific and Clinical EMs, and the practices and ceremonies which they legitimize, achieve an ideological effect by *reifying* signs, experiences, and outcomes as desocialized facts of nature. Taussig compares this situation with what happens in tribal societies, such as among the Azande, where medical facts "are not separated from values, physical manifestations are not torn from their social contexts, and it requires ... no great effort of mind to read social relations into material events."

According to Taussig, it is no accident that sickness has become a focus for ideological practice. Serious sickness interrupts everyday routine and the more or less uncritical acceptance of life. It turns people "into metaphysicians and philosophers," pondering questions such as "Why me?" Thus, when sickness is brought into the clinic, the practitioner is given "a powerful point of entry into the patient's psyche," through the authority of his EMs. It is at this point that the Western clinician becomes the agent of entrenched class interests, medicine becomes a means of social control, and the body is transformed into an instrument for ratifying socially engendered categories and "fabulating reality." By denying the social relations embodied in sickness, these practitioners turn clinical medicine into "a science of (apparently) 'real things', " and transmogrify reality into a "world of a *priori objects* beholden only to their own forces and laws dutifully illuminated for us by professional experts such as doctors." In this way, a political message is transmitted within the clinic itself: "Don't contemplate rebellion against the facts of life for these are ... irretrievably locked in the realm of physical matter."

In the United States, the success of ideological practices such as clinical medicine lies not in their capacity to confront and refute rival views, but in the power to push these views to the margins of reasoned discourse. Through these practices, socialized knowledge of medicine is made to seem not so much "wrong" or "counter-productive" (which would at least suggest some common ground between conventional and socialized views) as "not medicine at all" or an attempt to "politicize" medicine and science.

This is the point at which Taussig turns his critical remarks to EM writers. According to Taussig, the EM approach subverts the possibility of a socialized medicine. While the EM approach claims to give the patient's definition of the problem a privileged place in the medical dialogue, its real effect is to reduce the social relations of sickness to a discourse on illness and adaptation. The EM approach becomes an instrument for co-opting and then subordinating the patient's definition, for leaving his socialized knowledge out in the cold in the form of prototypes and chain complexes, and for wresting control out of his hands.

There are several points in his argument where Taussig sidesteps important issues. First, there is a problem which is signified by his reference to the Azande. In spite of what Taussig implies, Zande medical divination-diagnosis seems to have functioned as a powerful ideological and quasi-juridical instrument through which an aristocratic stratum dominated the mass of commoners. While it is true that Zande medicine did socialize sickness, the main political effect of this was to divide nonaristocrats against each other and to ensure their common exploitation (120). Second, it is not clear exactly what Taussig means by "reification," since he does not distinguish this concept from forms of objectification which seem to be *inevitable*, namely the symbolic processes through which people objectify themselves in particular events, material things, and social relations, and which help constitute the cultural construction of reality in every society.

Are Taussig's problems more apparent than real? After all, he seems to be implying that (a) objectification is "reification" when it *mystifies* the social origins of disease, the social determinants of sickness, and the lineaments of class domination, and (b) when symptoms, etiologies, etc are *authentically* socialized (de-reified) they are instruments for *demystifying* disease, illness, and the social order, and this is true in spite of the fact that in some tribal and traditional societies they are socialized in ways which contribute to structures of domination. But this line of reasoning would lead to another question. How can Taussig or any anthropologist be sure that his own ideas about sickness are correctly demystified, that he is not merely mystifying the social relations of sickness through an idiom of "society" rather than in the conventional way, through "nature" (175)? And this is Taussig's problem. He uses Lukacs's argument about reification to justify his own claim that he can correctly socialize (demystify) sickness. But Lukacs based his argument on highly problematic claims about history, class consciousness, and the emancipatory role of the proletariat (87, chap. 8). Perhaps Taussig could win us over to these claims and perhaps not. The point is that they are tacit and unargued in his article. The result is that his

analysis of how Western medicine mystifies sickness is both convincing and important, but his own epistemological claims are problematic.

A recent article of mine (180) parallels Taussig's paper up to a point. It departs from his thesis by arguing that *all* knowledge of society and sickness is socially determined, and that anthropologists cannot legitimately claim access to demystified facts. What they can claim, and what would set their accounts of sickness off from those of others, is a critical understanding of how medical facts are *predetermined* by the processes through which they are conventionally produced in clinics, research settings, etc. Thus, the task at hand is not simply to demystify knowledge, but to critically examine the *social conditions of knowledge production*. The article itself analyzes how stress researchers produce their characteristic facts about sickness outcomes. Like proponents of the disease-illness model, stress researchers promise to move knowledge of sickness beyond the limitations of the biomedical perspective, but in the end they, too, use sickness to show that socially determined ideas about society (its reducibility to individuals and dyads of individuals, etc) are irresistible facts of nature. In stress research medical knowledge is desocialized through a labor process which (a) recapitulates the social relations of production which are characteristic of the general economy (e.g. separating the intellectual labor of the project manager from the fragmented and mechanical labor of his informants), and (b) displaces the human subject (i.e. the people about whom stress researchers are writing) into an individualistic zone of anxiety, the milieu of "stressful life events," where he is reconstituted as a psychological abstraction. [In this connection, see analogous arguments by Navarro (126) and Assennato & Navarro (7, pp. 224–30) on the social production of knowledge of occupational medicine, and Latour & Woolgar (102) on the social production of biomedical knowledge.]

Efficacy and Productivity

What is the importance of medical anthropological research for the people *about whom* medical anthropologists write? EM writers are quite clear on this point: their practical interest is in the issue of *medical efficacy*. That is, they want to augment the effectiveness of clinical medicine in the context of the healing process. For example, they want to enhance patient education, remedy problems of noncompliance, and challenge maladaptive courses of treatment. On the other hand, the writers I have identified with the anthropology of sickness perspective are oriented to a point beyond the healing process, the inner logic of illness, and the consciousness of the individual. Their practical interest is in what can be called *medical produc-*

tivity. That is, they want to identify the direct and indirect impact of particular clinical practices and perspectives on the levels of morbidity and mortality of the population at large. Among other things, this means that they want to learn whether particular clinical practices, just because they *are* efficacious, also help to determine who is exposed to which pathogens and pathogenic situations, and who controls or has access to which medical practices and resources.

Because a medical system's degree of productivity depends on the effectiveness of its armamentarium and the technical skills and knowledge of its practitioners, it is impossible to talk about productivity without also introducing questions about efficacy. The reverse is not true, however, and this tends to limit the practical importance of efficacy centered approaches, since there are at least three situations in which improvements in efficacy have little or no positive effect, i.e. they are unproductive. 1. Improvements in efficacy are restricted to a small number of people and have a negligible effect on levels of morbidity and mortality for the total population for which they could make a difference. In a similar situation, improvements in efficacy are made available to the population for which they could make a difference, but this means diverting resources needed for improving the health of a larger segment of the total population. For example, this is the situation in some less developed countries where the capital absorbing medical interventions demanded by urban elites siphon resources away from the primary health care needs of a much larger (poorer, rural) at-risk population. The net effect is no change, or even an increase, in overall levels of morbidity and mortality. Situations of this sort are described by Djurfeldt & Lindberg (31), writing about the introduction of Western medicine into a region of Tamil Nadu State (India), and by Frankenberg & Leeson (54), writing about the development of a physician centered medical system in Zambia (see also 28, 179). 2. Improvements are made available to the population at large, but they are efficacious only under carefully regulated conditions. In actual practice, the social relations of sickness make them either iatrogenic or wasteful, e.g. the unregulated dispensing of powerful antibiotics in less developed contries (47). 3. There are situations in which the main effect of improvements in efficacy is to justify the "reification" or desocialization of sickness. As we have seen, this happens when efficacious practices give evidence, in the form of *individual cases* of curing and healing, that desocialized views of sickness merely reflect the facts of nature. But desocialization is intrinsically unproductive because it hides the underlying social determinants of patterns of morbidity and mortality and, in this way, helps to reproduce them. (Taussig's claim is that even if the EM approach were used efficaciously, it would probably fall into the second and third situations.)

CONCLUSION

The following outline schematizes my view of medical anthropology's field.

THE ANTHROPOLOGIES OF ILLNESS AND SICKNESS
  I. Biological Orientations
     A. Biomedicine
     B. Anthropology of disease (Biological anthropology)
 II. Sociocultural Orientations
     A. Empiricist epistemologies
        i. Medical sociology
        ii. Empiricist medical anthropology
     B. Nonempiricist epistemologies
        i. Traditional anthropological approaches
        ii. Anthropology of illness
        iii. Anthropology of sickness

This outline points us in the direction of a last question. Are the anthropologies of illness and sickness bracketed together in complementary or antagonistic ways? The answer depends on one's point of view.

Anthropologists of illness appear inclined to see the development of an anthropology of sickness as evidence of an emerging intellectual division of labor within medical anthropology. For example, this view is implicit in the way Kleinman treats the social relations of sickness in his monograph on Chinese medicine (93). Several times he mentions the importance of recognizing the social and economic determinants of clinical events. At the same time, he postpones the task of actually describing and analyzing these determinants. Thus, he implies that while knowledge of illness does not depend on knowledge of its social conditions, the former will probably be enriched by the latter.

The view from the *anthropology of sickness* is more complicated. On the one hand, writers like Taussig raise the question of whether there is a shared epistemology between the anthropologies of illness and sickness. If there is not, then the relation between the two is antagonistic and predicated on rival claims to the truth. On the other hand, there are writers, including myself, who stop short of this conclusion and recognize the common ground of the anthropologies of illness and sickness.

What all of the anthropologists of sickness share is the premise that social forces and relations permeate medical anthropology's field. When these social conditions are ignored or deferred, knowledge of medical events, including what happens in the clinic, is distorted. [For other approaches to this subject, see Janzen's distinction between microanalysis and macroanalysis (78), and Press's typology of medical systems (138).] What is more,

the anthropologist's knowledge is distorted in ways which are themselves socially significant. The underlying argument here is that the key concepts of the anthropology of illness—i.e. healing, illness, efficacy, explanatory models, and semantic illness networks—cannot be understood merely in relation to each other. By themselves, the concepts do not constitute a system for describing other people's medical beliefs, experiences, events, and behavior, because 1. illness is, among other things, a means through which tacit knowledge of the human subject (including his knowledge of his capacity to know, influence, or change the conditions of sickness) enters into the consciousness of the individual; 2. healing, in addition to bringing satisfaction to sufferers, is also an ideological practice which helps to reproduce the social relations through which illness is made real and both illness and disease are distributed in society; 3. efficacy is practically important because of the socially determined contribution it makes to productivity; and 4. explanatory models, semantic illness networks, together with prototypical episodes and chain complexes, are dialectically related elements within a socially determined process of knowledge production.

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