ALLAN YOUNG

RATIONAL MEN AND THE EXPLANATORY MODEL APPROACH

ABSTRACT. The previous issue of *Culture, Medicine and Psychiatry* (Vol. 5, N. 4) included my article "When Rational Men Fall Sick: An Inquiry into Some Assumptions Made by Medical Anthropologists" together with a series of comments. This paper consists of my replies to some of the commentators and a case study illustrating my points.

The comments of Arthur Kleinman, Robert A. Hahn, E. Valentine Daniel, Howard F. Stein, Arthur S. Elstein, and Margaret Holmes stand on their own. Additional remarks by me would be superfluous. Three other commentators raised specific issues regarding my argument and its premises, and I would like to respond to them here.

REPLY TO DAN W. BLUMHAGEN

Blumhagen's main point is that I misrepresented the explanatory model (EM) concept in my article. He writes that, "Young defines explantory models as 'sets of generalizations which enable the thinker to produce information about particular sickness episodes and events', whereas Kleinman defines them as 'the notions about an episode of sickness and its treatment that are employed by all those engaged in the clinical process' (Kleinman 1980: 105)."

I think that my definition *is* consistent with Kleinman's. On the other hand, Blumhagen believes that an EM refers to a *specific* application of a 'knowledge base' to a *specific* episode of sickness, and not to a set of generalizations. He quotes Frake 1961 to illustrate his point: "The 'real' world of disease presents a continuum of symptomatic variation which does not fit into conceptual pigeon holes [i.e. the Subanum disease taxonomy]. Consequently the diagnosis of a particular condition may evoke considerable debate." Blumhagen identifies the debaters' utterances, which he refers to as 'knowledge application', with their EMs.

There are good reasons for rejecting Blumhagen's position, though. First, if we were to accept Blumhagen's concept, and regarded EMs as equivalent to emic accounts of particular sickness episodes and experiences, why would we want to call them 'explanatory models'? I think it would be sufficient to refer to them simply as 'explanations'. After all, the term 'model' implies something fairly specific which is excluded by Blumhagen's concept. The most common meaning of 'model' is "a pattern of something to be made" (Webster's New

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Collegiate Dictionary 1974 ed.), and social scientists have tended to use the term in ways broadly consistent with this definition. There is implicit in this meaning *a distinction between the model and what it is a model for*. Given this usage, a model is an example of those sets of generalizations which Blumhagen wants to bracket out of the EM concept. (See Jenkins 1981 for a recent discussion of anthropologists' models. Jenkins' distinction between 'models' and 'cognitive networks' parallels, part of the way, Kleinman's distinction between explanatory models and semantic illness networks, but his conclusions argue against Blumhagen's interpretation.)

Let me illustrate my point, using an imaginary informant, Jones. We meet Jones and tell him that we are interested in his beliefs about influenza. We ask him to answer the list of questions Kleinman employs for eliciting EMs. Although Jones has never had an occasion to think or talk about influenza, he is able to discourse on the sickness briefly: it is caused by germs, it is half-way between a bad cold and pneumonia, etc. This is Jones' EM_1 for influenza. Subsequently, he himself contracts influenza and we interview him after he recovers. His comments are more detailed now and some of them are inconsistent with his earlier statements. But there is a fundamental similarity between the accounts, and it is obvious that EM_1 helped shape his comments about his own sickness. During his sickness, he acquired new information from his physician and also came to reject some of his previous beliefs. Jones has progressed to EM_2. Later, his cousin contracts influenza. We overhear Jones at the cousin's bedside, where he is making prognostications and recommendations. His comments are consistent with his account of his own sickness, and we conclude that EM_2 has shaped these also. His prognostications prove incorrect. We confront Jones. He sticks to EM_2, saying that there are things that make the cousin 'special', but they are unrelated to influenza *per se*. (Jones' EMs, like all theoretical knowledge, include a *ceteris paribus* clause.) Next, Jones' aunt contracts influenza, and he gives us an EM_2 account of this case. After our interview, the attending physician speaks to Jones and points out some of his misconceptions. Jones uses this authoritative information to proceed to EM_3. He is now ready with EM_3 for the next case of influenza that comes to his attention.

It seems to me that this is the way in which Kleinman uses the EM concept. (Although it is not necessarily the way a *physician* would use it in the course of a particular clinical encounter.) Further, it is consistent with the common meaning of 'model', since a distinction is being made between the model and what it is a model for. This is what Kleinman implies when he writes that EMs are "marshalled in response to" particular sickness episodes, "the main vehicle for the clinical construction of reality," and "rarely invalidated by experience" (Kleinman 1980: 106, 110). Blumhagen's point is that EMs are *equivalent to* meaningful experiences and the clinical construction of reality. But does it make sense to say that a meaningful experience can be marshalled in response to itself, or that a clinical reality can be a vehicle for itself? Let me give one more example. In the same book, Kleinman writes that Popular Culture EMs are a source of Individual's EMs, and they are different from Scientific, Clinical, and Theoretical EMs (Kleinman 1980: 109, 110, 131). Blumhagen's concept seems to refer to only an Individual's EM, however, since this is what each person actually uses to interpret sickness episodes. Because Blumhagen does not separate EMs from their productions, it is hard to see what epistemological space a Popular Culture EM, as distinct from an Individual's EM, would take up in his scheme.

REPLY TO BYRON J. GOOD

According to Good, my article continues a theme I began in an earlier paper on stress research (Young 1980). He writes that I am trying "to demonstrate that anthropological theory reproduces the same conventional knowledge of the abstract individual . . . " that stress theory reproduces. Good is twice mistaken. First, in the 1980 article I explicitly made the point that social anthropologists *do not* reproduce the stress researcher's conventional knowledge (Young 1980: 139–40). Second, I don't believe that 'theories' reproduce anything. Writers reproduce knowledge. Researchers reproduce knowledge. So do readers and other people engaged in practice. It is *in this connection* that the two articles have something in common, since both try to analyze the knowledge of informants, anthropologists, psychologists, etc., with a view to *how it is produced.* And so I wrote in the earlier article, that "the social relations needed to produce the psychoanalyst's or anthropologist's knowledge are very different from the stress researcher's, since the latter's productive relations are an instance of the general commodity mode of production in Western society (Young 1980: 140).

Good calls attention to another connection when he writes that "in the two pieces, Foucault's studies of medicine, psychiatry, and sexuality provide (at least in part) the methodological frame for Young's approach." Good follows this with citations from Foucault's writings, and concludes with his disappointment that my present article, which does not reference Foucault, "barely begins to exploit the potential of this [Foucaultian] approach. . . . " Good is correct, but for the wrong reasons. The bibliographies attached to these two articles indicate the writers to whom I am directly indebted for my arguments. Perhaps Foucault is a great man, but neither his approach nor his conclusions are so unique as Good seems to suppose. And if I must be exposed as an ungrateful and failed Foucaultian, what of my equally real debts to the other great men I've barely begun to exploit – Marx, Lukacs, Habermas, Bachelard, and so on – in this connection? Why not demand a full and immediate accounting?

Good is also correct when he says that my argument about Rational Man theory does not take account of "the complexity of the anthropological theories involved." I, too, called attention to this fact in my article. (I should also point out that this disregard for complexities is authentically Foucaultian, though.) The use of the term 'Rational Man theory' is justified, however, because it identifies a set of *tendencies*, and the tendencies are not equally developed among the writers I lumped together. This is all the term is intended to convey and, given the direction of my argument (it is about ways of interpreting informants' statements), this is all it has to convey.

At the same time, I can understand Good's disapproval. His own work on semantic illness networks puts him at a great distance from the ethnoscientists and uncritical positivists of medical anthropology. At first it seems absurd to suggest that there is a common ground between Good and these others. Yet while the semantic illness network concept establishes the possibility of transcending Rational Man perspectives, the fact is that Good has barely begun to exploit its potential. For example, when Good writes that the utterances which constitute a semantic illness network are elicited through a process of 'social free association' and that symbols within networks are condensed and polysemic (a symbol has multiple meanings), he makes it possible to show that utterances about sickness are not always reduceable to the speaker's beliefs about causes and effects or to inductive and deductive forms of reasoning. His semantic illness networks also make it possible to see that many utterances are epistemologically heterogeneous (e.g., combining theoretical and rationalized knowledge). In short, he seems ready to move beyond the horizon of Rational Man. For the time being, however, this is only the unrealized potential of the semantic illness network concept.

Am I being unfair to Good? In my article, I wrote that Rational Man researchers tend to *assume*, among other things, that cognition and language share underlying structures, and these structures are characterized by an underlying logic. Also, these researchers tend to textualize the statements from which their knowledge of the structures and logic is obtained, e.g., they treat utterances as if they were statements within a written text. What does Good say? In his most recent article (Good and Good 1981), he writes that "all reality happens in language" (p. 174), "human illness is fundamentally semantic and meaningful" (p. 175), and "the integration of an illness syndrome is logico-meaningful" (p. 176). Further, in at least two places Good uses the term "texts" to refer to utterances and symptoms (pp. 180, 192).

Given Good's observations on the logico-meaningful integration of syndromes though, it is puzzling to read the final sentence of his comments on my article: "It is the recognition that sick men are seldom rational that provides the impetus to develop sophisticated semantic and hermeneutic analysis of clinical discourse.

. . . " Is there a contradiction here? No, if Good means to imply that the irrationality of sick men is apparent but not real, i.e., it is merely an impetus for the anthropologist's discovering their underlying rationality. (He has already made this point in analyzing Case Study IV, in Good and Good 1981.) If Good means to imply something else – i.e., while sickness episodes are logico-meaningful, sick men are really seldom rational – we are in murky waters, since 'logico' can have at least two different meanings. If 'logico' is being used in the conventional sense – to refer to what goes on when a thinker reasons in a conscious, orderly, and cogent fashion – Good's comments on irrationality and logic *do* seem to contradict one another. On the other hand, he may be using 'logico' in a special, weak sense, to mean that an informant's utterances, experiences, etc. are connected in a variety of conscious and unconscious ways. If this is his intended usage, and logic and connection are seen as equivalent, Good's comments are reconciled and, what is more, he is successfully defending himself against being labeled a Rational Man theorist. But if this is what Good believes, it is surprising that he hasn't shown more interest in distinguishing among different kinds of connections – i.e., the subject of my article.

Which of these three interpretations is correct? Unfortunately, there is no way to tell exactly what Good intends when he writes 'seldom rational' and 'logico-meaningful'. All we have to conjure with is his avowal of a "sophisticated semantic and hermeneutic analysis of clinical discourse", and this, in spite of Good's references to Gadamer and Ricoeur, is simply too sweeping and abstract to help us.

Now I come to Good's last point. According to him, my argument is forfeit because it fails to distinguish between, one, the processes involved in knowing and, two, symbolic and cultural structures. In this instance, Good has got things backwards. He seems to believe that I am saddled with a faulty argument because I neglected to distinguish process from structure. In reality, it is because I am convinced by this argument that I am unsure if it is valid to distinguish process from structure in the conventional (Good's) way. In a nutshell, this is my argument:

(1) Let us accept, without argument, that it is possible to distinguish between an informant's representational knowledge (his knowledge *of* something) and his practical knowledge (the knowledge he produces *in response to* something). (I am assuming that practical knowledge is what Good means when he refered to "the process involved in knowing".)

(2) Anthropologists know the former mainly through the latter, so that an informant's statements are always in a form of practical knowledge, even when he is talking about representational knowledge.

(3) What is true of the anthropologist can also be said to be true of his informant. That is, the informant knows his own representational knowledge

as a form of practical knowledge. This is the case because a person generally recalls knowledge (i.e., he becomes aware of it and, perforce, alters it) in response to some need or motive, e.g., in order to resolve some psychological tension, to plan some instrumental act, to answer a question. (Even the 'spontaneous' recall of knowledge, in dreams etc., may be explained in terms of meeting intra-psychic demands.) Put into other words, a person knows his knowledge mainly as a form of practice.

(4) By admitting the existence of representational knowledge, however, we have allowed that knowledge can exist independently of practice. What are the epistemological and cognitive grounds for holding the position that knowledge is both representational and practical? What do the arguments for and against this position imply about anthropologists' conceptions of cognitive, symbolic, and cultural structures?

This, then, is my argument. It defines a problematic which has received considerable attention from anthropologists over the last decade (e.g., Needham 1972 on 'beliefs', Sperber 1975 on 'symbols', and Barth 1975 on 'knowledge'), and from philosophers before them (e.g., Wittgenstein 1958 on 'language games', Barnes and Law 1976 on 'indexicality'). Why does Good use 'failure' to describe a legitimate problematic? Is it because, not unreasonably, he is unwilling to argue propositions which he believes are axiomatic?

REPLY TO RICHARD A. SHWEDER

The context for Shweder's criticisms is that I have adopted an 'irrationalist' position and this position should be rejected. According to him, an irrationalist compares the canons that govern the language and thought of the ideal scientist, statistician, and logician with the canons of everyday language and thought, and concludes that the canons are different and unequal. Everyday thought is deficient. But how did Shweder come to conclude that this is my position? There is no evidence for it in my article, where I focused on a comparison between the way medical anthropologists usually interpret informants' statements and an alternative interpretive framework. I described the medical anthropologists' human subject as he is tacitly embodied in their interpretations and methodologies, and I labeled him 'Rational Man'. However, I defined 'rational' in a special way and, what is to the point, in a way that is clearly at odds with Shweder's distinction between scientific-rational and everyday thinking. According to my definition, Shweder's ideal scientists, etc., would constitute only a tiny minority of a community's Rational Men. Further, in a recent article I argued explicitly against the position that Shweder now attributes to me (Young 1981).

Shweder also describes me as joining a "rush to irrationalism", together with certain unidentified writers. My rush should seem rather leisurely to anyone who reads in my article that "it would be impossible to understand what people say and do if we did not take into account their strong rational and pragmatic tendencies. My aim is to put these tendencies in their place, not to deny their existence".

According to Shweder, my article is an attempt to solve a non-problem. He points out that people's statements seem puzzling or problematic when observors mistake an informant's tacit knowledge for ellipses in reasoning, or confuse his performance errors and tautologies with irrational modes of thinking, or fail to distinguish statements which have no truth value (Searle's 'constitutive' utterances) from statements with truth value. He implies that my arguments about transductive reasoning, etc., are unnecessary if one takes account of observer errors and misunderstandings. Now, Shweder is right that observers can make these mistakes, and I called attention to this possibility in my remarks on the ways people discourse to anthropologists. But what do we, anthropologists, say about problematic utterances *after* we fail to uncover evidence of these determinants? Do we adopt a *principle of cognitive generosity*, and simply assume that if we knew enough about our informants or pressed them hard enough, their statements would be shown to be consistent with Rational Man premises? Or, as I proposed, is it more fruitful for a researcher to reject the principle of cognitive generosity and entertain a set of alternative hypotheses, about epistemologically heterogeneous forms of medical knowledge, prototypical episodes, and so on?

Shweder proceeds on from these criticisms to a variety of misunderstandings and misreadings:

At one point, he attributes to me some remarks about "the scientific language and thought" of physicians in clinical encounters. But would anyone familiar with the exigencies of treating illness or the political-economy of disease want to describe clinicians' thinking as 'scientific'?

He writes disapprovingly that I believe "Medical scientists . . . have difficulty understanding what it all means when a patient says she is 'anxious' or 'depressed'." I am not sure what is the source of Shweder's dissatisfaction here, but if he is saying that I think most physicians would find such utterances puzzling, he is wrong. Most practitioners aren't puzzled, because they interpret these statements according to *a priori* schemas. If he is implying that there is nothing puzzling about the ways in which patients and physicians use these terms, he is also wrong (see, e.g., Marsella 1978).

Shweder says that I identified 'textualization' (see above) with literacy and, then, I invoked illiteracy as a determinant of less-than-systematic thinking. This is a remarkable conclusion, since my article explicitly disassociates textualization from literacy *per se* and points out that textualization can occur in pre-literate societies.

According to Shweder, "By understanding the deficiencies in the rationality of our informants, Young believes we will be in a better position to understand ordinary medical discourse." Maybe, but maybe not. All that I am willing to say is that the task of interpreting the clinical utterances of patients *and physicians* is more complicated than is generally appreciated. All that I can point to so far are some limits of what can be legitimately inferred from informants' statements.

Next, Shweder is correct to say that polythetic classifications occur in science. Needham (1975), who is referred to at length in my article, makes this clear. But Shweder's observation is beside the point, since I raised the issue of polythetic classifications around the question of whether speakers *recognize* them as being *distinctive* classification (as scientists do) and, if they do not, what this implies about the utility of explanatory, i.e., causal, models of sickness.

At the end of his comments, Shweder concludes that my argument is trivial, since I "have merely imported a few concepts from the literature on children's minds and then declared its relevance to medical anthropology." Really? But only part of my argument refers to the notions of Piaget, Vygotsky, and Hallpike. As for the rest of the argument, does Shweder see Needham, Goody, Latour and Woolgar, Lazarus, and Leventhal as Piagettians? Second, I can only assume that either Shweder has not yet leafed through Hallpike's documentation, or he thinks that Hallpike is another causal importer of damaged goods. Third, I did not 'declare' anything on behalf of medical anthropologists. What I want to do is raise a set of questions and propose a framework for answering them. The questions and framework are products of how I have read the medical anthropological literature, reappraised my own Rational Man researches in Ethiopia and, more recently, studied clinical encounters in a family practice clinic in the United States.

A CASE STUDY ILLUSTRATING MY POINTS [1]

This is an account given to me by a woman in an out-patient clinic in an Israeli development town. She is an Ethiopian Jew (Falasha) in her early thirties, a recent immigrant to Israel and, at the time of the interview, four months pregnant.

In the following paragraphs, I try to distinguish the ways in which she connected particular events, symptoms, and circumstances. Where she described an explicit cause-and-effect relation, I use words such as 'caused' and 'resulted from'. Where she did not know how or why a particular effect occurred but, at the same time, believed it was associated with some factor she had already mentioned, I use weaker terms, such as 'connected'. My annotations within her account are set off in brackets.

The woman, whom I'll call Sharona, came to the clinic because she was having problems sleeping. Her difficulties had begun a few weeks earlier. Because of her unusual pregnancy, she was forced to lie on her right side; the continuous pressure of her body caused pain and this made it impossible for her to sleep through the night. Sharona believed that if she could again switch sides during the night, the pain would probably go away and she would resume sleeping normally.

Sharona spoke of her pain and sleeplessness in the context of other symptomatic complaints. Among other things, she reported being constantly tired, overcome by a feeling of having no strength. Some of her fatigue resulted from lack of sleep, but it was also caused by the condition of heart, which was often 'closed up'. [Ethiopians believe that a person's heart is a source of the body's vital energy. 'Closed up' describes the bottling up of vitality, its failure to reach other parts of the body (see Young 1976).] Sharona was usually so tired that it was difficult for her to get out of bed in the morning, rise from her chair, or walk to the market. She also felt pressure under her left shoulder blade. This, too, was caused by her heart, and was most noticeable when she walked. Then, the pressure pushed in on her chest and made her breathless. In addition to her heart problems, Sharona had occasional headaches and joint pains; she found it difficult to eat very much; and she needed to micturate frequently, releasing only a trickle of reddish urine at a time, causing a painful, burning sensation.

Sharona had several explanations for her complaints. First, there was her unusual pregnancy. In a normal pregnancy, the fetus moves around in the womb; in her case, the fetus was confined to the right side. This was the cause of her painful position in bed: whenever she rolled onto the left side, the extra weight on the right side forced her to change positions. Sharona was sure that the fetus was holding on with its hand to the side of her womb. She had no ideas about why the fetus was behaving in this way, though. She had never heard of any other women having this problem, and did not think that there is a distinctive name for this condition.

Another explanation for her complaints was a sickness she contracted on the way to Israel. On this journey, she spent several months in the hot and feverish Nilotic lowlands. [Sharona, like other Falashas, emigrated from the cool highlands, lying above the range of the anopheles mosquito.] Conditions here were very difficult and she and her companions suffered greatly. While traveling in the lowlands, there was nothing to drink but warm, stagnant water, and this caused her to fall sick with a high fever. During this sickness, her entire body ached, and she recalls that her head and teeth were particularly painful. She does not know if this syndrome has a special name, but it can be called 'fever sickness'. When Sharona arrived in Israel, she was treated for her ailment, and the fever and pains subsequently disappeared. The treatment consisted of pills, an injection, and the extraction of her wisdom teeth. Sharona does not know the mechanism through which the lowland waters caused her sickness, but she does know that the sickness is connected to some of her current heart problems. For instance, she recalls that before her teeth were extracted, cold liquids pained her teeth and head. Now, whenever she drinks cold liquids, her heart closes up and she becomes weak. Sharona also connects the lowland sickness with her unusual pregnancy, even though she became pregnant several months after her arrival in Israel and after being treated for the sickness.

[Sharona's 'fever sickness' was probably malaria. Her dental problems were unrelated to this disease. She received perinatal examinations at the clinic, and her physician told her that the pregnancy is normal. Sharona has also been examined by an obstetrician at a local hospital and he, too, said there is nothing unusual about her pregnancy. Sharona remained unconvinced. Her pregnancy is further complicated by the fact that she is not married to her consort.]

Sharona gave another explanation connected with her heart. She reported that her heart made it difficult for her to swallow, or for food to pass into her stomach. [The distinction we make between 'her' and 'her heart' suggests a greater distance than it would to Ethiopians like Sharona. A very rough analogy with the Ethiopian heart is the lack of separation that we see between volition attributed to 'her' and 'her brain'.] On the occasions when she succeeded in eating normal amounts, Sharona felt too weak afterwards to stand up. She does not know the mechanism through which eating affects the heart, however.

Sharona's last explanation concerned her frequent and painful urination. Her need to urinate was caused by a pressure in the lower abdomen, pushing downwards towards the genitalia. The pressure was unrelated to the weight of the fetus, however, and could have occurred even if she had not been pregnant. Sharona did not spontaneously make a connection between the lowland sickness and her problems urinating. When I mentioned the sickness in this connection, she replied that it was a possible source of these problems.

[There is an explanation that Sharona did *not* give, but which is worth mentioning in the light of her anxieties about this pregnancy. I interviewed several other pregnant Falashas at the clinic. Each woman reported that she was frequently tired, and each described her fatigue in terms of the heart. But these other women attributed heart fatigue to pregnancy (there were other causes, too), and they said that they expected their hearts to be spontaneously restored after their pregnancies were over. Sharona, on the other hand, said that her heart fatigue was not linked to her pregnancy.]

Although Sharona knew no other woman who had a pregnancy like her own, she did know another instance of it. Four years earlier, while living in a village in northern Ethiopia, Sharona had been pregnant for the first, and only other, time. That pregnancy had lasted a full nine months, but her womb had become silent several days before parturition and the baby had been delivered stillborn.

According to Sharona, the fetus in the first pregnancy had also been confined to the right side of her womb. In that pregnancy, however, there had been no earlier event, such as the lowland sickness, which would have explained the fetus's position.

On the visit I observed, Sharona asked the physician to treat her for sleeplessness and pain. She herself had no ideas about what sorts of interventions the physician might find useful. She said that, in this particular case, it was the physician's responsibility to identify an appropriate course of treatment. Sharona would also have liked the physician to treat her painful urination, breathlessness, etc., but she did not have a chance to bring these complaints to the clinician's attention.

Sharona said that if she continued to sleep only on her right side, the fetus might eventually be harmed. She was unwilling to speak about specific possibilities, such as miscarriage and stillbirth, though. She said that her other complaints were bothersome in themselves, but that none of them seemed to point in the direction of future problems or complications.

THE MEANING OF THIS ACCOUNT

Now, I want to ask two questions about Sharona's account. How did she come to make these particular statements? Why is it important to know how she made them?

How the Statements Were Made

My questions to Sharona were an important determinant of what she said, of course. Her replies also reflected her efforts to make them intelligible for me, her assumptions about what I could or should know, her discursive style, and her selective attention to my questions. I shall skip over these facts (intersubjective and negotiated knowledge, according to my scheme) in order to concentrate on the other ways in which she organized her statements. Figure 1 schematizes this organization.

(1) *An Explanatory Model.* Sharona's statements about the causes and expressions of fatigue, loss of appetite, and breathlessness can be traced to an Ethiopian EM for heart distress (*yilib himem*, see Young 1976). Thus, there is a strong family resemblance between the statements made by Sharona and other Falashas regarding the connection between one's heart and vitality.

(2) *A Prototype.* Prototypes and EMs are forms of theoretical knowledge (see my reply to Blumhagen, above). But a prototype, unlike EMs, is created from a string of empirical circumstances and events, especially from a sickness episode. A prototype for Sharona's statements is her first pregnancy.

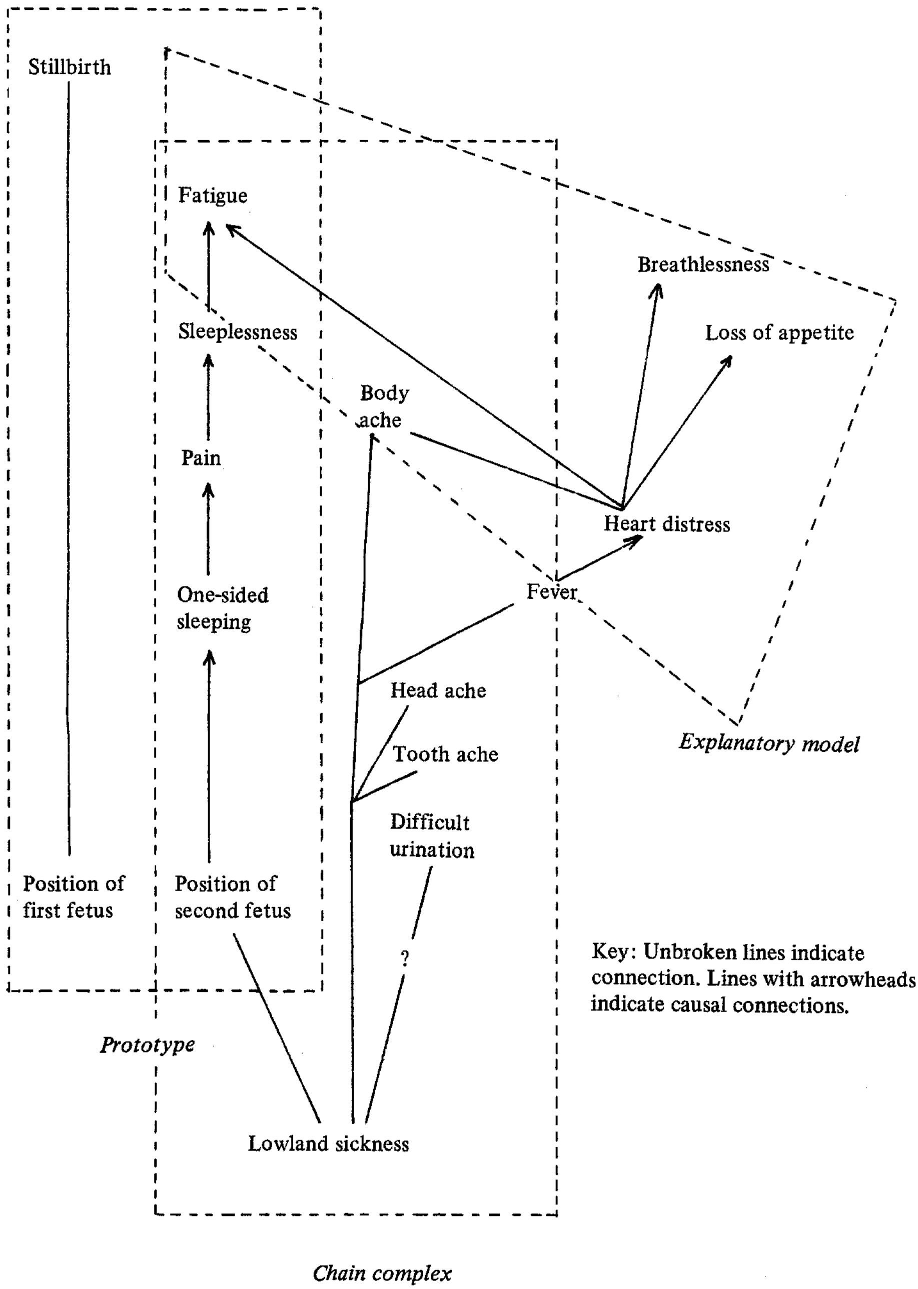

Fig. 1. A schematization of Sharona's account.

A prototype's particular events, symptoms, organs, etc. are connected by heterogeneous links, such as contiguity (e.g., between events, adjacent body parts), resemblance or analogy, and causality. EMs, on the other hand, depend on notions of causality, and it is because EMs are strongly causal that they are also integral to diagnostic and therapeutic thinking and practice. Compared with EMs, prototypes are less constant over time and more susceptible to dialectical revisions in practice. This is because individual prototypes are characteristically limited to small circles of people, e.g., to family members or even to a single thinker. Also, because prototypes are neither widely shared nor strongly causal, they are difficult to incorporate into diagnostic and therapeutic practices. But it is these practices, and the notions which are embedded in them, that stabilize the meanings of medical knowledge. (For a description of how knowledge is materialized and reproduced through cultural practices, of which medical practices are a sub-set, see Young 1980.) These facts help to explain why elements within a prototype (e.g., the position of Sharona's fetus) often function as omens, in the sense that they merely portend events and outcomes without providing any plan for controlling them.

(3) *A Chain Complex*. Chain complexes share many characteristics with prototypes. They, too, are often created out of strings of contiguous and salient events, sensations, symptoms, etc. (The formation of chain complexes and prototypes is also influenced by psychological determinants, as in conversion reactions, and by the propensity of thinkers to preserve ontological security by symbolic means.) For instance, Sharona's chain complex is an inventory of her major traumas over the last year: an arduous and dangerous journey, malaria, hospitalization and tooth extraction, separation from family, life in a strange land, a socially problematic pregnancy, chronic sleeplessness and malaise (depression?). Linked into these events is a part of her chain complex consisting of her unusually placed fetus, her changed sleeping position, and the subsequent pressure, pain, and fatigue. Loosely connected to these other elements in the complex are her problems micturating.

Unlike prototypes, Chain complexes are not instruments for analogical reasoning. Their elements, standing only for themselves, are mute and portend nothing. When a chain complex becomes an apparatus for organizing other sets of events, etc., we can start calling it a prototype. (In the vocabulary of my article, prototypes are the theoretical-rationalized knowledge, and chain complexes are empirical-rationalized knowledge.)

Why It Is Important to Know How Statements Have Been Formed

Informants and patients often present obscure and ill-defined complaints. For example, what did Sharona mean when she said that she is 'too tired' to rise from her bed or chair? What did it mean when she said that her fetus is confined to half of her womb, even after two competent physicians told her that she was having a normal pregnancy? What course of action should a clinician consider for Sharona's fatigue and obstetrical worries? No Rational Man approach can answer these questions satisfactorily.

For the time being, not even the EM concept can do this job. EM writers tell us that their informant's statements are sometimes contradictory and ambiguous, his symptomatic reports are often polysemic, and his personal EMs need to be analyzed in relation to clinical and popular culture EMs. Good. Each point alludes to the fact that an informant's statements are often complex and ambiguous. But these points are also abstract and even rhetorical. They make it possible to raise the formal definitions of 'explanatory model' and 'semantic illness network' to a degree of abstraction where they include, *but do not effectively discriminate among*, the varieties and forms of medical knowledge.

What we really need is a conceptual apparatus for saying how an informant's statements become 'contradictory' in the first place, and how his EM articulates with other orders of medical knowledge, e.g., with prototypes and chain complexes. It should make a big difference to a researcher or clinician whether a presenting complaint is the product of an EM (in which case it may be a clue to a course of action), or the product of a prototype (where it may be only an omen and a source of either anxiety or security, but not of action), or an element in a chain complex (where it may have little practical meaning) – or, as in Sharona's case, where the informant's utterances and complaints are simultaneously the products of an EM, prototype, and chain complex.

Dept. of Anthropology,
Case Western Reserve University

NOTE

1. My collaborators in this research were two physicians, Dr. Robert Like, of the Department of Family Practice of Case Western Reserve University and Dr. Rivka Plotkin of the Ben-Gurion University of the Negev (Israel). Also, I want to thank Avraham Blidstein for his invaluable assistance.

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