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PSYCHOLOGICAL VERSUS STRUCTURAL VALIDITY OF KNOWLEDGE: THE CASE OF ETHNOSCIENCE

'No sovereign scientific method or ethical stance can guarantee the truth of (anthropological) images. They are constituted — the critique of colonial modes of representation has shown at least this much — in specific historical relations of dominance and dialogue' (Clifford 1983:119).

Introduction

As is well-known, the absolutism-relativism debate is concerned with the nature of knowledge, its production and reproduction. Core questions are: (1) is there 'some permanent ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality, goodness or rightness' (Bernstein 1983:8) and (2) how can this framework — if present — be discerned and reproduced? Consequently the heart of the matter is not merely whether we can postulate a correspondence theory of knowledge and reality, but how we can ground it. In anthropology, this issue has been the preoccupation of the ethno-science tradition. Here the problem was reformulated in terms of psychological versus structural validity of analyses. 'A psychologically real description of a culture is a description which approximately reproduces in an observer the world of meanings of the native users of that culture. Structural validity on the other hand is a world of meanings as applied to a given society or individual which is real to the ethnographer but it is not *necessarily* the world which constitutes the mazeway of any other individual or individuals' (Wallace and Atkins 1960:75; see for the concept 'mazeway' Wallace 1961). As such, it is part and parcel of the problem of 'model of' versus 'model for' reality.

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In this contribution I discuss ethnoscience with respect to this topic, at the same time — indirectly — addressing the question of what the absolutism-relativism debate can learn from the ethnoscience controversy about psychological versus structural validity. In my opinion, this outline of developments in ethnoscience not only reveals the intellectual bankruptcy of a purely formal semiotic approach to — or code-model of — culture, cognition and communication (see especially Sperber and Wilson 1986), but also sheds light on the problems that philosophers of science will meet in grounding a correspondence theory.

A subsidiary aim is to show that each perspective harbours and, in fact, cultivates the seeds of its own demise. While the central postulates are provided with a foundation, are elaborated and are applied, the extent to which these postulates can be defended also becomes clear at the same time. In other words, their application leads to their development as well as their destruction. There appears to be a point of saturation after which progression turns into regression, and it finally ends with deliberate neglect or fundamental change (see Lakatos 1970). Then a new configuration of basic assumptions and rules of argument comes into existence. In the case of ethnoscience this is particularly clear. Within a period of twenty years deeply rooted and highly cherished postulates have been given up. If we stick to the linguistic metaphor, we no longer have syntaxis but pragmatics. As a result ethnoscience as such is history nowadays.

The Hard Core of Ethnoscience

Essentially, ethnoscience is based on some simple assumptions regarding the aim, scope and method of anthropological research. These presume a specific relationship between language, cognition and behaviour. One basic postulate states that even the most concrete physical object is embedded in an articulated, meaningful system. Thus it cannot be identified independently of a culturally defined system. Its place within this conceptual or symbolic system defines the meaning of an object or event. So ethnographers should strive to define objects and events according to the conceptual system of their informants. This means that a description of a given culture should be couched in terms of 'native' cognitive principles and categories rather than in terms of the conceptual categories the ethnographer has learned during his training as an anthropologist.

Beginning with the observation that a particular group successfully intercommunicates in a manner not understandable to an outsider, the ethnographer has to ask himself what knowledge is necessary to interpret the verbal and nonverbal messages correctly. 'Whenever we wish to know what people are doing and why, or what they are likely to do, we must know what phenomena they see, for these are the phenomena to which they respond. And we must know what they believe to be the relations among these phenomena and what they perceive as the possible courses of action for dealing with them' (Goodenough 1970:104). An ideal eth-

nography therefore ought to include all the rules, principles and categories that the native himself must know in order to act appropriately in his sociocultural system.

Because ethnoscience aims to understand native conceptions of the universe, how they are formulated and how they are used, it has to develop a mode of study free from the ethnocentrism imposed by the cognitive processes and interpretations of the culture of the investigator. To ethnoscientists, this means an avoidance of *a priori* definitions, because these are thought to distort or mask the realities of foreign cultures.¹

Another important premise of ethnoscience is that cognitive categories are to a large degree encoded in the linguistic distinctions and structures used by a group of people. It is believed that investigating linguistic designations and classifications will provide an unambiguous entry into the cognitive categories employed by the members of a society in ordering these domains (Mathiot 1962:343). It is clear that language is accorded a privileged position. The fundamental reason is that language is thought to govern and constitute perception and cognition; the more pragmatic reason is that language is a relatively simple system: language is an organized whole consisting of relatively few units and levels, which can be used as a model to discuss several domains of human existence. It permits the search for organizing principles which underlie a concrete classification. The aim is to give a description that goes beyond mere observation and recording, while overcoming the tendency to superimpose one's own analytical categories. Such an investigation is based on the view that human cognition and behaviour follows certain rules, in the sense that behaviour can be generated by the application of a set of structure-defining and structure-manipulating rules or principles. In practice this often boils down to the formal analysis of a lexical domain.

A subsidiary presupposition is the idea that culture is a homogeneous code. Ethnoscientists tend to assume that a restricted number of informants can somehow encapsulate the conceptual principles and cognitive categories of a whole culture. Interwoven with this assumption is the almost exclusive concern with group cognition and the corresponding neglect of individual cognition. As a consequence, cognitive anthropolo-

¹ However, ethnoscience is not primarily interested in the understanding of a particular culture. It subscribes unequivocally to the comparativistic perspective in anthropology, as it considers comparison an essential task for the purpose of getting a better view of the general characteristics of culture and man. Intercultural comparison of systems of classification relates to an ultimate goal; ethnoscience seeks insights into the cognitive unity of mankind, and as Gamst and Norbeck (1976:175) remark, 'continuity with the anthropological past is clearly evident in this goal since "cognitive unity" may be seen as a modern version of the idea of psychic unity'. To discover in a reliable and valid way the regularities in the ways in which man segregates and classifies the phenomena of the universe and perceives relations between categories of phenomena, one has to obey the canons of inductivism, which means first describing and finally generalizing and theorizing.

gists concentrate on collective representations or other products of cognition.

The Question of Validity

Anthropologists who advocated the use of formal linguistic methods seem to have had two objectives. The first goal was to specify the conditions under which each term should — and would — be used. The problem then is to determine what one needs to know in order to be able to say that some object is to be designated by a given term. The second goal was to detect the criteria by which speakers of the language themselves decide what term to use for a particular object or event. This is far more ambitious because it implies that one can duplicate the inner mental processes of classification and decision of the native. The difference between the two goals can be expressed as aiming at structural or psychological validity respectively.

Right from the start several leading ethnoscientists claimed that through linguistic analysis one could discover the criteria by which the native speakers themselves classify phenomena (e.g., Frake 1962, Goodenough 1956, Lounsbury 1956). The kind of meaning they hoped to reveal was not only a referential or definitional meaning — the minimal information about the object to which a term referred, either sufficient to justify the utterance of the term in reference or necessary to infer from its use — but above all, the intentional meaning of terms for their native users (Wallace 1965:229). Generally, it was believed that this would enhance the explanatory power of the analysis. The methodological promises of linguistic analysis and a large part of the reason for its popularity lay precisely in its claim to be a systematic, reliable technique for revealing what words mean to the people who use them, for getting inside the informant's head. The objective was to understand native mental activities, to grasp the native point of view, to share the native's inner thoughts, to be able to go through the native's reasoning in one's own mind. This concept of internal understanding, involving a duplication of the native mentality, stems from a Cartesian theory of mind (Hanson 1975:58).

As can be seen, the basic idea is that cultural differences in thought processes are reducible to differences in classification. However, because no one has direct access to other men's minds, cognitive principles are in the final analysis only inferences drawn by the ethnographer: 'they are his conceptions of what the informant's conception might be' (Kaplan and Manners 1972:185).

Consequently an important question is whether a classificatory system constructed by the anthropologist is anything more than a heuristic device, i.e., whether it does have any cognitive reality outside the anthropologist's own mind. How can one know for certain that the thoughts the anthropologist 're-enacts', in fact, are the thoughts entertained by the natives? The ambition to give a psychologically real description requires that not just

any model which predicts some overt class of action, but only that model which is used by the actor be accepted (Wallace 1962:356).

Now ethnoscience is confronted with two problems. Firstly, the relationship between elicited category systems and the contents and processes of men's mind has been a point of enduring controversy, as is manifested in the debate on the Sapir-Whorf hypothesis. From a psychological point of view the purely linguistic evidence may be a necessary requisite for any consideration of cognitive processes, it certainly is not sufficient (Price-Williams 1980:596). Besides, non-linguistic approaches to the study of cognition, such as sorting, choosing and matching tasks, often reveal mental operations that are not expressed in language. People can often sort reliably without having explicit knowledge of the principles they use. They may even, when asked, give linguistic information about principles which they are simply not applying at all (see also Hammer 1966). Secondly, the method of componential analysis — the most prominent kind of linguistic contrastive analysis used by ethnoscientists — does not automatically yield a unique description of a native semantic system. Several times different but equally workable ways of treating a set of terms can be devised. In other words, it can be demonstrated that two different systems of classifying will yield the same result in overt behaviour. In his exemplary article on Trukese kinship, Goodenough (1956) himself provided different paradigms, and the same goes for Lounsbury's paper on Pawnee kinship (1956).

In the following years, several different componential analyses of the same terminological sets appeared.² So, from the very start the serious question about the usefulness of contrastive analysis as a means of constructing psychologically valid representations of the ways in which people see things was raised. This issue became the subject of a heated debate in the first part of the sixties.

After Wallace and Atkins (1960:76) had identified several major sources of indeterminacy of componential analysis³, a bombshell was thrown into the ethnoscientific ranks by Burling. In 'Cognition and Componential Analysis: God's Truth or Hocus-Pocus' (1964), he states that if alternatives are found, the idea that one particular analysis is the actual representation of informants' cognitions becomes improbable. He bases his case on the

² In this context one can think of, among other things, the alternative solutions of English-American kinship terminology (e.g., Wallace and Atkins 1960, Romney and d'Andrade 1964, Goodenough 1965, Burling 1970) and of Njamal kinship nomenclature (Burling 1962, Epling 1961).

³ They stressed the lack of finite boundaries of a set of terms; the variation in: (a) the universe of denotata chosen, (b) the number and identity of terms selected from a lexicon for analysis of the same semantic space, (c) the identity of the dimensions chosen, and (d) the type of class-product space; and the variation as to the use of logical operators (class or relative products) as well as to the inclusion or exclusion of 'connotative' dimensions (Wallace and Atkins 1960:76).

astounding number of theoretically possible alternative orderings of any given set of terms. Whereas for a set consisting of three terms, the possibilities number 6, for any given four-term set they number 24. Technical problems such as homonymy, empty semantic spaces, non-binary components, parallel components and redundancy complicate the analysis further. 'In principle, the number of possible analyses becomes infinite' (Burling 1964:21). So, Burling's main conclusion is obvious: one has to abandon the goal of psychological reality.

Burling's paper provoked several reactions which strengthened and accelerated already existing tendencies. In my opinion most important are, first of all, the emphasis on situational or contextual clues in evaluating the adequacy of different formal analyses, next the dismissal of the goal of psychological validity by a number of ethnoscientists, and finally the appeal to psychological tests to complement linguistic analyses. The overall tendency was to develop new insights concerning ways in which the relation between cognition and culture had to be viewed.

The first — and I think most important — opponent of Burling was Hymes.⁴ Starting from the embeddedness of linguistic utterances in culturally defined situations, he rejects Burling's appeal to formal logic as irrelevant. Formal logical analysis gives no recognition to the fact that terms are intrinsically bound up with an external reality. Hymes points out that the total number of logical possibilities is fully pertinent only if all solutions have an equal chance of being arrived at. This is not the case. There is a fundamental difference between a semantic structure — the discovery of which is the objective of ethnoscience — and an arbitrary arrangement.⁵ To contrast these, one has to find out what questions members of the cultures themselves ask in categorizing experience. Consequently, 'the analyst who follows such a strategy is never in the situation of having to consider all logically possible sortings' (Hymes 1964:431).

Hammel, too, stresses the irrelevancy of formal logic. However, he does not appeal to the context of speaking as Hymes did, but to the simplicity or formal elegance of the analysis. Like Hymes, he starts with the comparison of overt expressions and behaviours of the natives. After describing the correspondences and differences between native sets, the analyst con-

⁴ At exactly the same time Frake, too, reacted to Burling, but while Hymes concentrated on relevant contextual clues, Frake pointed to behaviour as the means of discovering cognition: the structure of a cognitive system can only be approached through the observable behaviour of its possessors. Consequently, he stated that 'if two statements differ in their implications for behaviour, then a choice between them can only be made in one way: by testing them against the behaviour of the people being described' (Frake 1964:119). However, the appeal to behaviour does not solve the problem of alternative solutions, because similar behaviour can be the result of different cognitions (Wallace 1961).

⁵ In this context, Hymes (1964) makes the important distinction between: (a) making the right sorting, which can be formal and cognitively empty, since it involves only putting the discriminated items into relation with each other, and (b) making the right assignment of semantic features to the dimensions of sorting.

structs that set of his own 'which most closely corresponds to the largest number of native sets or to their central tendency in sorting' (Hammel 1964:1167). In this construction important criteria are: replicability, internal consistency, correspondence with observation, and economy of statement. These criteria together may be referred to as an 'elegant' solution (Hammel 1964:1167-1168). However, there are ambiguities in his position. By his coupling of 'elegance' with 'close correspondence to the largest number of native sets' he creates the impression not only that he still aims at psychologically real descriptions, but also that he presupposes that the most 'elegant' analysis is the psychologically real one. However, the least elegant solution may be the correct one. As a matter of fact, such a position can be supported by empirical evidence (e.g., Saltz 1971:253-341; Neisser 1967). Indeed, it appears that cognitive systems and strategies quite often are surprisingly clumsy and indirect. Hence 'the dictum of parsimony will not suffice as a general rule for resolving the problem of psychological reality' (Rubinstein 1981:6). Besides, it is often very hard to determine which of several alternative analyses is most elegant, as is shown by Burling (1965) in his article on Burmese kinship terminology. He provides a conventional componential analysis and a relative product analysis which can both be used without sacrificing formal precision.⁶ He concludes (1965:117) that it is impossible to give a definite answer to the question of which description is simpler or superior.

One must conclude that the classic method cannot answer the question of which one, if any, of various alternatives validly describes not just the distribution of terms over denotata but also the criteria by which the native speaker classifies these denotata. In other words, there is no way within the method of componential analysis itself to determine which of several arrangements of sets of terms is more psychologically real, even if one accepts the necessity of using contextual clues as Hymes recommended. In any case, Hammel became convinced that the relationship between formal elegance and cognitive validity is problematic, and renounced the claim that a componential analysis or a description based on transformational linguistics represents a native speaker's cognitions. He has withdrawn to the bastion of structural validity, together with several other practitioners of the art, among them the first advocates of this approach in cultural anthropology, Goodenough and Lounsbury. Nowadays several scholars assert that to dwell on psychological validity only distracts the

⁶ In the case of relative product analysis use is made of terms that have been defined previously to build up the meaning of additional terms. 'Uncle', for instance, can be defined as parent's brother, once the meaning of 'parent', 'brother' and 's' is known. This means that no description can depend exclusively upon relative product definitions, since a start must be made with terms whose meanings are known in some other way. Consequently, relative analysis is inevitably mixed, for it relies both upon certain primitive terms which are defined much as they are in componential analysis and upon relative products that build upon the primitive terms

attention from the establishment of a system that explains, and not necessarily replicates, human behaviour. Although Werner (1972:272) still postulates that 'mindlike mechanical verbal behaviour seems best suited for the validation of assumptions', we are left wondering 'how to know when a given case of mechanical behaviour is "mindlike"' (Johnson 1978:163).

As a result of these debates it became increasingly clear that techniques based on language could not provide a definitive answer regarding the cognitive validity of the results of this kind of research. Those ethnoscientists who had not given up the claim to be able to discover 'psychological reality' had to resort to other, additional techniques to deal with the problem of testing the psychological validity of data acquired through componential analysis or other linguistic methods, because they had to admit the futility of determining what is inside people's heads on a linguistic basis alone.⁷ Accordingly, several other techniques were developed to compare the results of linguistic analyses for 'degree of fit' with some other body of data. If this is done, the dispute as to whether properly conducted componential analysis yields only one true analysis or a multitude of equally true analyses is beside the point: 'one or many, their psychological validity ... must be established by means independent of the mechanics internal to componential analysis itself' (Wallace 1965:231). An important development was the use of psychological sorting or matching and explication tasks. Well-known is the triad test of Romney and d'Andrade (1964), in which informants were requested to select in every combination of three terms (e.g., father, son, uncle) which two were most alike or which one most different (e.g., father-son versus uncle).

Although there may be minor differences between the specific tests employed, the general principles on which such tests are based are similar. In view of the question of how the native, confronted by a reference object, proceeds to choose the appropriate reference term, the test must require the native speaker to indicate — verbally, in writing or in visible action — the reasoning processes which he employs. This reasoning process, or calculus, involves three stages:

- 1) the selection of sufficient relevant information about the reference object;
- 2) the organization of the data so as to define the object's class or relationship;

⁷ Ironically, this is especially clear in the case of componential analysis — introduced to achieve that goal — because here from the start the analysis is limited to a minimum: only denotata are considered relevant, and connotata — essential to obtain a full psychological meaning — are left aside. Not only is the distinction between connotatum and denotatum operationally speaking unclear — what, for example, is the denotatum of the word 'god' as distinct from its connotatum — but also 'the theory of meaning it entails has been systematically rejected in philosophy' (Leaf 1971:545).

3) the choice of the term appropriate to that class (Wallace 1965:235).⁸ The issue of structural versus psychological validity not only led to the above-mentioned developments, viz.:

- a) emphasis on the importance of the context in which terms are used, leading to an 'ethnography of speaking';
- b) denial of interest in psychological matters and rigorous confinement of attention to formal models in which parsimony, elegance and accuracy in predicting terminological usages are the criteria by which the relative merits of alternative analyses are to be judged; and
- c) introduction of new psychologically oriented techniques to supplement and test data acquired by linguistic techniques.

Other major consequences were:

- 1) a growing awareness that descriptions of 'native cognition' depend heavily on the methods employed; and
- 2) the introduction and subsequent development of new ways of viewing cognition, leading to a rethinking of the concept 'culture'.

As far as the first point is concerned, it became clear that 'cognition is not a monolithic thing waiting to be discovered, but rather a loosely conceived domain' (Johnson 1978:166), and that different methods of collection and analysis of data result in different representations of cognitive structures and processes. Although a great many studies can be called upon to support this statement, an especially revealing example is provided by the study of Cole and his associates, who did research among the Kpelle of Liberia (see Cole and Scribner 1974:52-55). They obtained a long list of Kpelle terms by using eliciting frames. They also gave their informants the opportunity to associate terms freely with other terms, objects and events, e.g., 'what do you think when I say "shirt"?' The researchers also used sorting tasks. In a separate experiment, the objects which had been named were placed in front of the informants, who were then asked to group the objects into a limited number of piles. Next, the results obtained by free-association on the one hand and by sorting on the other were compared. It appeared that at the level of gross categories, both approaches led to similar 'cognitive maps'. However, within categories the objects were re-

⁸ According to Wallace (1965:235-236), there are two kinds of test, corresponding to the diametrically opposed situations of (a) choosing a term from knowledge of the reference object's characteristics, and (b) inferring from the use of a term the characteristics of the class of reference objects to which it refers. In the first type of test, one has (1) to supply in series different sets of restricted information about the reference object in order to discover what dimensions the native speaker considers to be relevant and sufficient to reckon the choice of term; (2) to require the native speaker to make manifest the procedures he uses to reckon with the information supplied in order to discover the logical operations he performs. In the second type of test, one has (1) to supply different sets of terms, in series or simultaneously, in order to discover what dimensions of information the native speaker can infer from the terms; (2) to require the native speaker to make manifest his reasoning processes.

arranged. Besides, the degree of ambiguity or regularity in informant responses varied according to research techniques. When the task emphasized order among terms, the results fell into the familiar taxonomic pattern, but when other techniques were used, such as informally asking informants to list 'things' and then to group them into subgroups, another kind of order resulted, this time based on use and function rather than on formal semantic order. Accordingly Cole et al. concluded that semantic classes can serve as a means of organizing verbal behaviour. However, the extent to which this happens in naturally occurring contexts is very much open to question. 'It is quite possible that in our desire to find "the" classification of Kpelle nouns, we overlooked situations where quite different kinds of classifications would dominate' (Cole et al. 1971:90).

Regarding the second point, an important development was the replacement of component and relative product analysis by techniques and concepts derived from the information-processing approach to human thinking advocated by Keesing (1972), Sanday (1968) and Sarles (1966), among others. In this kind of analysis careful attention is given to the order in which terms are listed by informants, on the assumption that the decision to list a particular item is related to the listing of the previous item by one of a set of operators. The approach involves the manipulation of terms. Concretely, it concerns problem-solving tasks, and a problem is any situation where the subject is presented with a desired end without being told what path to take to attain that end. Inferences about cognitive structure and process are made through the analysis of the path the individual selects to attain the end set by the task. The 'psychological' reality of terms is conceptualized in terms of:

- a) the set of elementary discriminable units stored in the memory (i.e., the terms an individual remembers); and
- b) the set of elementary operators and decision rules also stored in the memory, which are the cognitive processes by which terms are generated.

For example, Sanday (1968), in one of her investigations on American-English kinships terms, gave each individual a pencil and paper and asked him to 'list all the names for kinds of relatives and family members you can think of in English'. She paid attention to the order in which terms were listed in order to detect the operators. Thus if the term 'brother' followed the term 'sister', the sibling operator was assumed to have been used. The variation in cognitive structures and processes revealed in this way was supposed to be the outcome of life-cycle and social role variables and of the individual's experiential knowledge of the domain under study.

A slightly different approach is propagated by Burling. In his 'American kinship terms once more' (1970) he gives an account he considers to be compatible with children's usages as well as with adults' alternatives. He believes that one can study the kinship terminology as if it were a sequence of principles which reflects the route by which we have all learned our

system. The advantages of this approach are threefold, according to Burling. Firstly, it seems to harmonize with ontogenetic development, or at least to be more readily harmonizable with it than a simple componential analysis. Secondly, by recognizing some areas of kinship as being more central than others, we can deal with the fact that parts of our terminology are very uniform while others are much more variable. We can see how people can agree completely about the core of their kinship system and easily understand each other here even when they differ considerably in the more peripheral parts of their system. A componential analysis which places all terms on the same level and defines them all by the same procedures gives us little basis for supporting our intuitive feeling that 'mother' is, in some way, a more standard, central, and basic term than 'second cousin once removed'. Thirdly, it is difficult not to feel that this treatment is rather more natural, possibly even more 'cognitively real', than other more elaborate treatments. We need no fancy notions like 'ablineal' or 'second degree of genealogical distance'. Such concepts have little place in American culture except for the esoteric subculture of the anthropologist, and one would suppose that kinship terminology ought to be describable in the terms the natives use themselves (Burling 1970:15-24).

This information-processing approach strengthened the tendencies already present to redefine the concept of 'culture'. Traditionally, in ethno-science culture was seen as a cognitive code shared by the members of that culture. There existed a clearly uniformist orientation in which homogeneity of values, goals, attitudes and other cognitive patterns was viewed as being essential to the functioning of society (e.g., Pelto and Pelto 1975:2). Of course, it was recognized that individual beliefs and actions do not exactly replicate cultural standards as set by the community, but nevertheless, standard expectations and behaviour were accorded prime importance (Goodenough 1970:101). Because it was believed that the members of a single society utilize a single monolithic cognitive system, the explication of cognitive systems by a very limited number of key-informants — the 'authorities' — was felt to be sufficient for the explication of the cultural cognitive system. In other words, this assumption justified the ethno-science practice of obtaining descriptions of whole areas of cognition from a few informants and presenting these in absolutistic formal analyses. Yet, in spite of this research practice, it proved to be possible to construct several alternative arrangements of sets of terms. So one began to wonder whether this variability reflected not only indeterminacy of the methods used but also the existence of intracultural cognitive diversity. After all, not all of the individuals who make up a particular culture share or participate in their culture in the same way: the cultural reality may be different for chief and commoner, adult and child, rich and poor, handsome and ugly. 'An anthropologist encounters not a culture but particular human beings who are liable to have different views about their

society. We cannot "generate" culture simply by erasing all these differences' (Crick 1976:75). There is no single cultural reality, but there may be a host of differing 'cultural realities' (Kaplan and Manners 1972:185; see also, e.g., Pelto 1966:201).

So the premise that a society's culture is 'shared' by its members was called into question and Wallace's view of culture came to be increasingly accepted. The fundamental question Wallace asked was whether a typical society is 'held together' by sharing of or consensus on values, or whether it is an organization of diversity, consisting of different but congruent cognitive maps. Wallace believed that there is no need for cognitive sharing. He even stipulated that cognitive non-sharing is essential to the continued success of interaction. 'Many social subsystems would not "work" if all participants share common knowledge of the system ... for cognitive non-uniformity subserves two important functions: (1) it permits a more complex system to arise than most or any of its participants comprehend; (2) it liberates the participants in a system from the heavy burden of learning and knowing each other's motivations and cognitions' (Wallace 1961:35). Instead of cognitive sharing, Wallace suggested that social systems are integrated in terms of equivalence structures or complementary behavioural expectations.

The acknowledgement of the validity of this argument contributed to a shift in the dominant research design. No longer could a researcher limit himself to a few well-trained informants or consultants; he had to rely on large and complex samples to do justice to the possibility of intracultural diversity.⁹ When these more complex, composed samples were really used — starting from about the second half of the sixties — the idea of a uniform cognitive code soon proved to be a myth. The moment research turned towards statistical analyses of data drawn from samples of community members, it appeared that several distinct codes existed within a single

⁹ The fact that many ethnoscientists have not utilized psychological tests and statistical sampling techniques has prompted M. Harris (1968:583) to characterize much of their work as 'social psychology shorn of its statistical base'.

society or culture.¹⁰ The picture that emerges from these studies is that cognitive systems — terminological systems — are shared only in the broadest possible sense. This in turn has led to the recognition that 'the concepts people use in talking about the world are not as perfectly bounded and discrete as the contrast sets and paradigms of cognitive analysis have assumed; instead, concepts are found to have core meanings that are widely shared and peripheral meanings that vary to some degree among informants' (Johnson 1978:170).

As was said earlier, a consequence of this growing interest in cultural diversity was a 'rediscovery' of Wallace's definition of culture as a means

¹⁰ One of the first to carry out interesting research in this context was Tyler (1966), who found that among the Koya in India differences in social and linguistic context are reflected in variations in the use of kinship terminology. He showed that variations in kin term usage depend on 'social setting, audience composition, sex and age of speaker/hearer, linguistic repertoires of speaker/hearer and most difficult of all — something that might be called the speaker's intention' (Tyler 1966:704-705). This line of research was followed by a large group of anthropologists who at about the same time presented evidence of the existence of intracultural cognitive diversity related to differences in age, social role, and the like. These studies, which for the most part made use of decision-making or information-processing models — whether or not formalized in terms of probability theory and therefore allowing for 'the interpretation of differences in cognitive maps as being both of degree and of kind thereby escaping the sterility of an extreme stand on the issue of sharedness of culture' (Sankoff 1971:407) — strongly suggest that most published componential analyses may well have used ideal types as their sets of data. 'With one or few informants it is easy to argue that inconsistent usage is the result of mistakes. If data are collected from a larger group of individuals, these same mistakes may crop up often enough to suggest that they are actually relevant patterns of optional labeling. These patterns may be complementary, or contradictory. Both kinds of patterns, but especially the latter, argue also against facile association of kin terminology and social behaviour' (Fjellman as cited by Johnson 1978:171). Outside the ethno science tradition the untenability of the assumption of a single clearly definable cognitive code shared by all members of a community was also shown. One can think here of, e.g., Marvin Harris, who remarks that ambiguity may be one of the salient characteristics of terminological systems, even of such a relatively closed and limited domain as kinship (Harris 1968). In his study on Brazilian racial terms using a series of stylized illustrations reflecting racial differences along a number of dimensions — skin colour, hair type, lip and nose shapes — he discovered that 100 respondents, responding to 72 different drawings, produced as many as 492 different racial terms. He concludes that the 'ambiguous output of the Brazilian racial calculus casts doubts on the assumption that the codes or rules associated with the abstract distinctions and actual identification of many classes of phenomena constitute intersubjectively uniform sets. Equally plausible is the assumption that actual classificatory performance is the expression of indeterminate and variable "competence".... This assumption is especially attractive if the prime social function of the rules is not the maintenance of orderly distinctions but the maintenance or even maximization of noise and ambiguity. Brazilian racial categories appear to constitute such a domain' (Harris 1970:12). Although this ambiguity does not preclude a certain degree of order, at the same time it is obvious that the concepts people use in talking about the world are not as perfectly bounded and discrete as the contrast sets and paradigms of cognitive analysis have assumed (Johnson 1978:170). It may well be that this ambiguity and variability act as dynamic forces in culture. These factors can generate and canalize cultural changes (see Pelto and Pelto 1975:4).

of organizing cognitive diversity. Bearing in mind this view, which suggests that for each set of relevant phenomena there may be several alternative cognitive organizations, it became the task of the anthropologist to construct out of these several alternatives an overall model which incorporates all the variants: 'It is highly unlikely that the members of a culture ever see their culture as this kind of unitary phenomenon. Each individual member may have a unique, unitary model of his culture, but is not necessarily cognizant of all the unique unitary models held by other members of his culture. He will be aware of and use some, but it is only the anthropologist who completely transcends these particular models and constructs a single unitary model. This cognitive organization exists solely in the mind of the anthropologist. Yet to the extent that it will generate conceptual models used by the people of a particular culture, it is a model of their cognitive system' (Tyler 1969a:5). However, this line of arguing, representing the position of the second-generation ethnoscientists, is in some ways unsatisfactory. Kaplan and Manners (1972:146) rightly conclude that the relationship between the anthropologist's overall model and the models actually employed by different individuals is unclear, so that it is hard to understand how exactly the hypothetical construct of the anthropologist may be capable of generating conceptual models used by the people under study. It remains a *deus ex machina*. In any case, this meta-calculus, which deals with the diverse calculi of particular individuals or subgroups cooperating to maintain stable systems of relationships, does not have any psychological reality for anyone, except perhaps the ethnographer.

Conclusion

The combined developments of rethinking the relationship between language, thought and culture as well as the above-mentioned results of research on cultural variability have led to the recognition that linguistically based techniques are insufficient to determine the cognitive validity of analyses. As a result more and more ethno-scientists have abandoned the psychological validity claim altogether. However, others have not given up this goal, although they have redefined it. They have stressed the need to use a number of different techniques of data collection and interpretation. Apart from this extension of their research tool-kit, they are switching to a more mundane, or, preferably, a more realistic, vocabulary. Within an observational and empirical methodology, which is very much characteristic of the classic period of semiotic-cognitive anthropology discussed here, they are no longer speaking of isomorphy between rules inferred by an observer from the behaviour of an actor and the rules employed by that actor (Spradley 1972:25), but of 'analogy' or 'the doctrine of nearer approximations' (Tyler 1969b:71-78). What they have in mind here is something analogous to limit theories in calculus (although in this case a more pragmatic or behavioural theory of limits seems to be

required). In this sense, the conflicting ideologies of psychological reality and ethnographic predictability are simply different kinds of limit theory. Psychological reality represents a limit theory that asserts that convergence of results from independent approaches (i.e., formal analysis and psychological tests) to a common problem provides a nearer approximation to native cognition. Ethnographic predictability represents a limit theory in which the imputed ability of the ethnographer to behave appropriately in some culture is a nearer approximation to the rules of appropriate behaviour in that culture. The important difference between these two limit theories is that they call for nearer approximations to quite dissimilar ends, unless it is assumed that there is an additional convergence of native rules and native cognition. In that case, both ethnographic predictability and psychological reality could be nearer approximations to native cognition and the two limit theories could be simply different expressions of the same thing, albeit involving different methodologies.¹¹

In this context the debate on the problem of psychological validity is shifting its focus more and more. The classic way of posing the question turns out to be spurious, not so much because the hypothesis of only one psychologically valid set of rules has become untenable, but because the idea that the native follows the same rules as those which the anthropologist formulates 'is taking a conscious ratiocinative model of the subconscious mind too literally. Bluntly put, whatever is "in" the subconscious mind, it is not a bunch of propositions and it therefore cannot be literally duplicated by propositions (see Spradley 1972:8-18, 25-27). Hence it is simply absurd to think that the propositions and logical steps of our analyses could ever duplicate subconscious cognition, for they are a different kind of thing' (Hanson 1975:64). The anthropologist only gives statements of the rules applied by natives. Insofar as they account equally well for observed behaviour, they are equally right. No longer is the claim to duplicate that which is going on in native minds at issue (as the Cartesian theory of mind holds), but only that to duplicate overt intelligent native performances. Understanding is just a matter of knowing how, to use Ryle's terminology (see Hanson 1975:61). Understanding cultural institutions internally is nothing other than knowing their implicational meaning (Hanson 1975:65).

The above-mentioned development in the psychological validity debate from intentional to implicational meaning seems to be only a temporary refuge. Since the second half of the seventies there has been a shift from the observational-empirical methodology towards what might be called a

¹¹ This doctrine of nearer approximations is not intended to suggest that such limit theories facilitate discovery of actual (native) cognition. I have strong doubts that native cognition can ever be known, yet this should not obscure the fact that the goal of understanding native cognition is a little like ideal objects (mass, points, frictionless surfaces) in classical mechanics. Both are useful, but 'mythical'.

more dialectical, interactive and constructivist methodology, resulting in the decline of ethnoscience. Now, more emphasis is being put on the nature of 'cultural discourse', in which questions are asked about the constitutive effects of context on ethnographic description and analysis, more specifically regarding the specification of discourses: 'who speaks when and where, with or to whom, under what institutional and historical constraints' (Clifford 1986:13). The renunciation of psychological validity fits in with this development towards an 'ethnography of speaking' or a semantic-praxeological anthropology, because its basic assumption is that analysing cultures is a constructivist activity influenced by many variables, the outcome of which is a product of negotiation rather than observation. Post-modern ethnography is evocative and normative rather than merely analytic and descriptive. 'The whole point of "evoking" rather than "representing" is that it frees ethnography from *mimesis* and the inappropriate mode of scientific rhetoric that entails "objects", "facts", "descriptions", "inductions", "generalizations", "verifications", "experiment", "truth" and like concepts that have no parallels either in the experience of ethnographic fieldwork or in the writing of ethnographies' (Tyler 1986:130).

For that matter, I have a definite feeling that the post-modern tendency is going too far. It leads to an absolutist relativism. In this perspective, definition and acquisition of knowledge are not delimited by the demands of an external, albeit not directly knowable, world, but by group norms, or conventions, which are specific for a certain place and time. This position is too extreme. Of course, knowledge cannot simply be viewed as mirroring the way reality is. One has to admit that it is 'the outcome of long term interactions with the world that have been shaped into stylized fabrications' (De Vries 1987:219). However, this does not imply complete freedom in conceptualizing the world. Reality may not speak for itself, but, as Gieryn (1982:288) has pointed out, science, contrary to art and literature, is a self-corrective learning device, and cannot be viewed as completely arbitrary and conventional (see Raven 1988 for a concise and detailed analysis).

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