2. Two methodological problems

In part one we summarized certain sorts of semantic information which, we have claimed, can enter usefully into ethnography. Two questions immediately arise:

(i) How can we discover semantic facts?

(ii) How can we represent semantic facts, particularly within the framework of an ethnography?

In this chapter we shall consider some approaches to both problems.

First we must understand what we are asking. Any general answer to questions (i) and (ii) constitutes a theory about the nature of semantic facts. Nowhere is the interdependence of methodology and result more evident than in the study of 'meaning'; we have remarked on the fact that, since the technique of componential analysis calls for the elicitation of the referential denotata of terms, the analysis bases 'meanings' on referential features. (It is, in fact, commonly claimed that the sterility of traditional ethnographic semantics is largely due to the rigidity of its methodology.) Nor need we expect that a general methodology be possible at all. The descriptive semantics of physical object words may not resemble, in discovery technique or in output, the descriptive semantics of words denoting social situations or actions.

A more fortunate formulation of question (i) might be: how do we know when we have got the semantic information we want? By what criteria are we satisfied that our data are correct? A realistic fieldworder will take what data he can get. It is not always possible to insist on formal interviews, un-tainted observation, etc.; the evils of non-scientific ethnography may

creep in unwanted, if not unexpected. A pragmatic methodology, then, may well resemble a filter: we scan our observations, and submit proposed ethnographic statements to severe tests of adequacy. We adopt a strategy of hindsight. Such a procedure is familiar in anthropology, and explicit in linguistics.* What

*In linguistics, though we may have methodological hints about how to gather data, how to train informants, how to begin analysis, etc., the real epistemological decisions are made when we try the adequacy of our grammar. Slavish loyalty to a particular theory of grammar often leads us to commit the primary sin: to miss significant generalizations.

we give up in simplicity and 'replicability' we may regain in depth, elegance, and explanatory power.

The answer to question (ii) requires more than a list of techniques for describing semantic facts (e.g., componential analysis, translation to a neutral language, dictionary entries, etc.) Many theorists demand more of semantic descriptions than a formal rendering of certain facts. Katz (1966), for example, faults anthropological componential analyses and ordinary language philosophy for failing to integrate the semantic facts they present into an overall theory of linguistic semantics. Berlin requires that a semantic description allow "a naive observer to perform correctly (culturally speaking) in all contexts circumscribed by the description." (1968: 40)* We might, in another vein,

*We recall Quine's suggestion that even a bi-lingual linguist faces severe descriptive problems: observation is merely replaced by introspection. (1960: 47) Berlin's criterion implies that the analyst must not only know the semantic facts (and be able to act on them), but that he must be able to instruct others in that knowledge.

demand that a description of semantic phenomena of a particular language provide rules to translate native expressions into English (or some suitable target language). These considerations

show that question (ii) presupposes the seemingly wider question: what do we have to be able to do with semantic facts before we know we have them?

An ethnographer can presumably do with less detailed semantic information than would be required in, say, a linguistic description of a language.* Yet it will be convenient to hold our linguistic data in a form that can be eventually incorporated into wider theories of linguistic meaning. We may now turn our attention to questions (i) and (ii).

*There are conflicting versions of how semantics is integrated into grammar. Early attempts to incorporate semantic facts into generative grammar (Katz & Fodor (1963), Katz & Postal (1964), and Katz (1966), as well as Chomsky (1965)) viewed syntax as an autonomous component of grammar, intermediate between phonology and semantics. Syntactic descriptions (of sentences) were input to both components, resulting in the one case in phonological representations, and in the other in 'semantic interpretations.' More recent theorists within the framework of generative grammar suggest that the rules at the deepest level of syntax may themselves be built of semantic primitives -- hence, that in some sense it is semantics (or some system of meanings) that provides the deep-structure input to syntax. (These are my interpretations of such papers as Fillmore (1968) and of Lyons' remarks about "notional grammars" (1968: Chs. 7 & 8).) A third possibility is suggested by the axiom that "meaning implies choice." We may, that is, look for 'meaning' wherever options appear in the syntactic derivation of a sentence. It is possible to view the kinds of semantic information we discussed at the end of part I in terms of options offered in the derivation of sentences: verb aspects alternate at a certain syntactic level; vocabulary choice occurs at a relatively more superficial level, and so on.

2.1 Discovery

We have claimed that a statement of methodology often represents a decision about what we want to discover. The relationship between discovery procedure and result is especially evident in the study of meaning. For despite any intuitions we might have about 'meaning' (as a word), the procedures of

Semantic Analysis may often force us to take contentious stands on The Meaning of a Word. Some reflection on the facts and phenomena which will be relevant to Meaning will take us a step towards explicit methodology.

Traditionally ethnographic semantics has assumed that a semantic analysis of a set of terms gives the rules for the application of those terms to observables. It has been a theory of reference. Lounsbury-type reduction rules, for example, "define the meanings" of kinship terms by providing an explicit characterization of the extension of each term. (See Bibliography for Lounsbury references.) Accordingly ethnographic semantics relies methodologically on the assembly of the denotata of terms. Lounsbury enumerates the following phases of semantic research:

> "...(a) the compilation of raw lexicographic data on particular denotations, to (b) the assembling of the denotata of each single linguistic form as a semantic class of objects, or designatum, to (c) the discovery -- when possible -- of the classificatory dimensions imposed upon the field by native linguistic usage, and (d) the specification of the distinctive features defining each of the constituent semantic classes as a kind to (e) an ordering of the semantic units of the various hierarchical levels within the total structure of the system." (1963: 574)

The primary concern in the early stages of research is training informants to exhibit the denotata of terms in some reliable way -- a task which is, of course, only possible when the terms themselves have been determined satisfactorily. Analysis proceeds by extracting from the denotata the criterial features which allow certain objects and not others to bear the label of a particular term.

The point of view of Quine (1960) serves as a useful critique of the unexamined epistemological assumptions of this traditional

approach. Quine's theory of the interconnections between language and the world (i.e., his theory of reference) leads him to posit a certain "indeterminacy of translation"; according to Quine, "that it (the alleged indeterminacy) requires notice is plainly illustrated by the almost universal belief that the objective references of terms in radically different languages can be objectively compared." (1960: 79) Quine takes the extreme case of radical translation to demonstrate the epistemological difficulties involved in discovering the referents of words.

2.1.1 Reference

First, the notion of reference could stand some scrutiny. The traditional idea that words name things in the world, or that the semantics of natural languages resembles the semantics of formal languages (and hence represents a mapping of the symbols of the language onto a set of objects in some universe), has been well criticised by philosophers. Frege's classic paper "On Sense and Reference" demonstrated that the reference of a word or expression (the thing for which it stood) was not the whole of its meaning. Furthermore, Wittgenstein's Investigations devote considerable attention to the seemingly obvious facts of reference. We may summarize some results.

(i) Words function in so many different ways that to think of the relation between word and the world as one of naming is overly simple. Wittgenstein asks us to consider such single word expressions as: Water! Away! Ow! Help! Fire! No! He asks: "Are you still inclined to call these words 'names of objects'?" (1953:§27)* Some words in language simply do not

*Of course, we still might want to claim that, e.g., 'water' does refer to the world, or that it has a referent. In the example given, the lexeme 'water' occurs in a particular utterance, and furthermore, in a particular syntactic frame (given by the stress pattern indicated by the exclamation point). We have, instead of a word, a whole sentence. Quine, in fact, argues from such sentences and not from some theory of the meaning of individual words.

name in any usefully definable way. Similarly, when words do refer to "things" in the world, it is reasonable to suppose that the relationship between word and object varies from word to word.

(ii) The unexamined belief that we can elicit the denotata of terms in a foreign language is, at least, brought into question by the fact that words need not all refer in the same way. Wittgenstein's remarks on ostension are relevant; for in ethnographic research we largely learn the denotata of terms by ostensive definition (or its reverse: we point; they tell us the name.)*

*We rely less and less heavily on ostensive definition (a) as we ourselves learn the language and its own resources for definition, or (b) if we use bi-lingual informants. Quine (and Wittgenstein) consider the case of radical translation with no bi-lingual informants. Yet there may be no reason to trust the intuitions of informants who have only imperfectly learned the second language.

Wittgenstein is concerned to show that taken alone ostensive definitions can typically always be misunderstood.

> "Now one can ostensively define a proper name, the name of a colour, the name of a material, a numeral, the name of a point of the compass and so on. The definition of the number two, 'That is called two' -- pointing to two nuts -- is perfectly exact. -- But how can two be defined like that? The person one gives the definition to doesn't know what one wants to call 'two'; he will suppose that 'two' is the name given to this group of nuts! -- He may suppose this; but perhaps he does not. He might make the opposite mistake; when I want to assign a name to this group of nuts, he might understand it as a numeral. And he might equally well take the name of a person, of which I give an ostensive definition, as that of a colour, of a race, or even of a point of the compass. That is to say: an ostensive definition can be variously interpreted in every case." (1953: §28)

How do we know what the native is pointing to, when he points to an object (or what we take to be an object) and utters a word?

The indeterminacy Quine mentions has a similar origin, though its ontological roots are, as it were, deeper. In his example, a given native word seems to have the English translation 'rabbit'; but there are difficulties which cannot be solved by pointing, nor, indeed, by any appeal to reference.

> "Point to a rabbit and you have pointed to a stage of a rabbit, to an integral part of a rabbit, to the rabbit fusion, and to where rabbithood is manifested." (1960: 52-53)

We cannot decide which option to adopt without penetrating beyond observed correlations between non-verbal circumstances and verbal behavior. The situation is similar with respect to the translation of certain logical primitives, notably the Aristotelean categoricals, the identity marker, etc.; for "... the categoricals, like plural endings and identity, are part of our own special apparatus of objective reference, whereas stimulus meaning ... is common coin." (1960: 61)* Quine concludes that the ontological

*'Stimulus meaning' Quine defines as the class of all stimulations (of a certain length etc.) which would prompt assent to the question "X?" where 'X' is the word in question; (there is also an appropriate negative criterion.) Hence, we may view the stimulus meaning as a sum of the objective circumstances which define the use of the word -- we have gotten somewhat beyond ostension. Cf., particularly, §§8-10 for a precise statement of these notions.

indeterminacy he discovers is inevitable; that

> "there can be no doubt that rival systems of analytical hypotheses (i.e., hypotheses about the translation of particular native words and phrases and grammatical categories) can fit the totality of speech behavior to perfection, and can fit the totality of dispositions to speech behavior as well, and still specify mutually incompatible translations of countless sentences insusceptible of independent control." (Ibid., p. 72)

We must yet consider whether such indeterminacy is troublesome

for our ethnographically inspired semantic research.*

*Lyons suggests that a somewhat grosser sort of indeterminacy, created by such phenomena as the referential overlap between 'girl' and 'woman', "far from being a defect... makes language a more efficient means of communication. Absolute 'preciseness' is unattainable, since there is no limit to the number and nature of the distinctions one might draw between different objects; and there is no virtue in being obliged to draw a greater number of distinctions than is necessary for the purpose in hand." (1968: 427) The moral for our study is that indeterminacy which does not impair communication (for our ethnographic purposes) is no problem; perhaps it only saves us work.

(iii) If we consider reference in reverse -- i.e., if we try to find out the names for things ("what is that called?", pointing) -- we may encounter disconcerting disagreement. Not only do single things have many names (and not simply names from different taxonomic levels); but people may differ in their identification of things, and hence in the labels they are willing to apply.* Such disagreement may result from differential

*See, in this connection, Laughlin (1967), which deals with the uncertainties in lexicographic research based on the search for names of things. The empirical fact of disagreement can interfere with our naive model of meaning in other related ways: the notable inability of, e.g., most English speakers to match words (even words in a lexical domain) with their definitions (taken, albeit, from relatively unmotivated dictionaries (cf. Weinreich (1964))) speaks ill of the consistency of sense systems across speakers.

knowledge about the world, which manifests itself in different understanding of words.

(iv) We can point to a red ball. Can we point to its color? its shape? In certain circumstances, we can. Wittgenstein remarks that ostensive definition is useful "when the overall role of the word in the language is clear. Thus if I know that someone means to explain a colour-word to me the ostensive definition 'That is called "sepia"' will help me to understand the word."* (1953: §30)

*In practice it may be difficult to convey the idea of 'colour-words.' Conklin's classic study approached the problem in Hanunoo, which lacks an equivalent of our word 'color', as follows: "Except for leading questions (naming some visual-quality attribute as a possibility) only circumlocutions such as ..." 'How is it to look at' are possible. If this results in a description of spatial organization or form, the inquiry may be narrowed by the specification... 'not its shape (or form).' " (1955: p. 190 in Hymes (1964)) Such a process of elimination probably seems riskier than it is; for in a controlled situation researchers do get down to true color words.

Lyons goes a step further: to learn the meaning of a color term, one needs to know not only that it is a color-term, but also what the other terms of the system are.

> "It seems evident that the meaning of each of the colour-terms is learnt together with the meaning of the others. The child learning his first language does not first learn the meaning of one term, say green in English, and then another, blue, and so on -- so that at a particular time he can be said to know the meaning of the one, but not the other. Rather, it must be supposed that over a certain period he gradually learns the position of green with respect to blue and yellow, of yellow with respect to green and orange, and so on, until he has learnt the position of each of the colour-terms with respect to its neighbors and the approximate location of the boundaries of the area in the continuum of the denotational field covered by each term. Only then can he be said to know the meaning of any one of the colour terms." (1963: 39)

Reference is, in R.M.W. Dixon's words, relative to sense. To identify the referent of a word when our informant points at it, we must already know something about the position of the word in the language -- about its formal properties, its relationships with other words, and so on. Wittgenstein generalizes these remarks: "Understanding a sentence means understanding a language." (1958: 5)

(v) Ostensive definition will not take us much beyond words which do name in straightforward ways -- peoples' names, names for common objects, etc. We soon begin to rely on what Lyons calls "cultural overlap." (1963: 41, 1968: 433ff.)

> "Practical experience of learning foreign languages (in the normal conditions in which these languages are used) suggests that we quickly identify certain objects, situations, or other features in the area of cultural overlap and learn the words and expressions that apply to them without difficulty. The meaning of other words and expressions are learned less readily and their correct use comes, if ever, only with long practice in speaking the language." (1968: 433)

Presumably the vocabulary and grammar that we learn in the "areas of cultural overlap" allow us to break into other parts of the language more efficiently than we could if we stuck to observed correlations between verbal behavior and non-verbal phenomena. To take a familiar example, though such words as 'sad', 'morose', 'saturnine', 'sullen', 'gloomy', 'glum', 'grumpy', etc. may well refer to observables (i.e., have stimulus meanings in Quine's sense), the easiest way to learn such words involves explicit appeal to 'sense relations' between words -- e.g., through definition, contextual example. We will acquire sufficient competence in the native language to understand our informant's explanations of meaning; and we will catch on better if he tells than if he shows.

We are plainly treading on epistemologically shaky ground. For in ethnography we must make ourselves doubt the evidence of cultural overlap which, for the casual language-learner, is pragmatically assumed. Moreover, we have not shown how to overcome the indeterminacy of ostensive definition, except by learning the language (learning the "overall role" of different words). On Quine's account, however, the indeterminacy holds even if we become (by whatever mysterious means are necessary) completely bi-lingual; hence, the "translation" (or interpretation) of the words of other speakers of our own language is equally indeterminate. Quine, of course, recognizes this consequence:

> "Our advantage with a compatriot is that with little deviation the automatic or homophonic hypothesis of translation fills the bill. If we were perverse and ingenious we could scorn that hypothesis and devise other analytical hypotheses that would attribute unimagined views to our compatriot, while conforming to all his dispositions to verbal response to all possible stimulations." (1960: 78)

When we reach the point that no empirical evidence will allow us to choose between two translations (in a particular case), it makes no sense to complain that we are uncertain; we don't know what certainty would be like. Or, we should like to say, if indeterminacy interferes with our understanding of foreign languages in principle no more than with our understanding of other speakers of our own language we have very little to worry about (as practical ethnographers and linguists).

Yet there remains a certain discomfort, a doubt about how we can ever get to the meanings of words. This discomfort stems, ultimately, from the assumption that the empirical study of meaning must be confined to the study of reference. Traditional ethnoscience is crippled, when we come to complex terminological areas, by its first methodological step: assembling the denotata of terms. What do we do if there are no obvious denotata to assemble? Quine's formulation is more ingenious: he is concerned, not with individual words in isolation, but with whole sentences and the situations or stimulations which (would) prompt assent or dissent to them. Quine restricts his attention to "language as the complex of present dispositions to verbal behavior." (1960: 27) The translator's job is to crack these dispositions.

> "One is so taught to associate words with words and other stimulations that there emerges something recognizable as talk of things, and not to be distinguished from truth about the world. The voluminous and intricately structured talk that comes out bears little evident correspondance to the past and present barrage of non-verbal stimulation; yet it is to such stimulation that we must look for whatever empirical content there may be." (1960: 26)

That is, "what is before us is the going concern of verbal behavior and its currently observable correlations with stimulation." (1960: 28)

An important consequence of this formulation is that language, belief systems, bodies of cultural knowledge and theory are inter-related through a complex of sentences and a shared history of non-verbal stimulations, linking sentences to the world. Quine refers to this inter-relationship as "the interanimation of sentences." (1960: §3) There are, according to Quine, two primary modes of learning sentences:

> "(1) learning sentences as wholes by a direct conditioning of them to appropriate non-verbal stimulations, and (2) producing further sentences from the foregoing ones by analogical substitution (of other words into new frames, as, e.g., from the sentences 'This is my foot', 'This is my hand', and 'My foot hurts' to derive 'My hand hurts.')" (p.9)

To explain the vast range of our verbal dispositions (our language? our linguistic competence?) -- "if we are to exploit finished conceptualizations and not just repeat them" (p. 10) -- we require mechanisms to associate sentences with sentences (as well as sentences with non-verbal stimulation.) Quine's example deserves full quote:

> "... the power of a non-verbal stimulus to elicit a given sentence commonly depends on earlier associations of sentences with sentences. And in fact it is cases of this kind that best illustrate how language transcends the confines of essentially phenomenalistic reporting. Thus someone mixes the contexts of two test tubes, observes a green tint, and says 'There was copper in it.' Here the sentence is elicited by a non-verbal stimulus, but the stimulus depends for its efficacy upon an earlier network of associations of words with words; viz., one's learning of chemical theory. Here we have a good glimpse of our workaday conceptual scheme as a going concern. Here, as at the crude stage of (1) and (2), the sentence is elicited by a non-verbal stimulus; but here, in contrast to that crude stage, the verbal network of an articulate theory has intervened to link the stimulus with the response." (1960: 10-11)

This bit of philosophy of science is extraordinarily suggestive

for ethnography: it indicates how language, truth, and belief are all bound up together in terms of a complex network of sentences.*

*Paul Kay suggests that the "proposition" is the "appropriate unit" for analysis of culture, viewed as a set of rules or a code. (1966: 108) He refers specifically to the work of Metzger and Williams (cf. Bibliography) as "presenting the model of an individual culture -- the so-called cultural grammar -- in terms of propositions that are held by the carriers of that culture, their internal structure and their mutual interrelations. According to our present working hypothesis, a culture consists of interrelated propositions." (Ibid., p. 109) The kinship with Quine's view is clear, except that Quine makes explicit the degree to which beliefs and language are themselves intertwined, and related at the edges to non-verbal stimulation. Such recent studies as Werner (1969) consider "ethnographic ethnoscience" to involve description of a subset of "semantically interpretable sentences" which are "culturally appropriate." Such sentences form the subject matter for "linguistic ethnographic ethnoscience" (whew!) which "deals with that part of cultural knowledge which is accessible through the language of the informants." (1969: 330)

This characterization of a type of ethnography relies on the now fairly common distinction between different kinds of anomaly in sentences: ungrammaticality (syntactic anomaly), semantic anomaly (nonsense), situational anomaly (cultural or factual inappropriateness). (The term "situational anomaly" is due to Dixon; cf. McCawley (1968: 129-30).) Chances are that culturally inappropriate sentences will be more ethnographically revealing than culturally appropriate ones, which are, after all, just ordinary.

Still, the emphasis on stimulus and response in this account makes us uneasy. We have become accustomed to viewing language as essentially creative or generative. The heart of a language seems to lie not in the actual speech behavior (the stimulation of which we can presumably observe, if superficially), but in its (finite) resources for potential speech behavior which is itself infinite. Characterizing all speech behavior as response seems inaccurate; and the search for the stimuli which prompt verbal behavior seems empty.* Quine's emphasis on dispositions

*Many of the criticisms Chomsky (1959) makes of Skinner's Verbal Behavior apply to Quine's formulation of 'stimulus meanings'; Quine's account, though designed to describe the situation of the linguist in the field working on a totally unfamiliar language, seems to apply equally to the acquisition of one's own language.

(That is, the child is not supposed to have any advantages over the linguist.) Chomsky's remarks I take to be conclusive refutations of a stimulus/response theory of language acquisition or use. We must not oversimpligy the linguistic facts. Chomsky asserts "that the compsoition and production of an utterance is not simply a matter of stringing together a sequence of responses under the control of outside stimulation and intraverbal association, and that they syntactic organization of an utterance is not something directly represented in any simple way in the physical structure of the utterance itself." (1959, in Fodor & Katz (1964: 575)) The efforts of current grammarians demonstrate the truth of this assertion. We may, as a result, doubt Quine's "two primary modes of learning sentences"; for "associations of sentences with sentences" may be more complex than we had imagined.

to verbal behavior represents no improvement. Note the conditional in the following definition of stimulus-meaning:

> "We may begin by defining the affirmative stimulus meaning of a sentence... for a given speaker, as the class of all the stimulations... that WOULD prompt his assent." (1960: 32) (Final emphasis, in caps, added.)

We have here something very far from observables.* Moreover, to

*Recall Weinreich's impassioned plea: "... let us have behaviorism rather than a new scholasticism operating with inaccessible 'dispositions to respond' ..., let us have the observable, publicly verifiable performance of human beings charged with the metalinguistic task of manipulating signs for the disclosure of their intensional structure." (1963: 192)

think of the interpretation of hitherto ungenerated sentences in terms of a hypothetical class of assent-prompting stimulations seems misleading at best. We want, as it were, to get more deeply into the phenomena of meaning, into the resources of the language.

2.1.2 Sense and Non-referential Meaning

We have raised doubts about a referential treatment of linguistic meaning, even a sophisticated treatment which equates referent with stimulus-meaning. In this section we will explore some methods for probing 'sense' and other dimensions of linguistic meaning of ethnographic interest. We hope to represent the semantic structure of vocabulary and grammar as a creative system

going beyond a set of correlations between word and the real world.

Following a suggestion of Quine (1951), Lyons offers two theoretically important definitions:

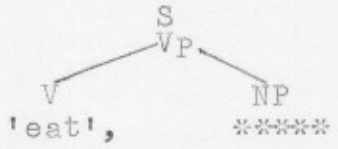
> "I propose to define the notion of 'semantic structure' in terms of certain relations that hold between the items in a particular lexical subsystem. They include such relations as sameness and difference of meaning, incompatibility, antonymy, etc., which are customarily held to fall within the scope of the theory of meaning." (1963: 57)

> "I consider that the theory of meaning will be more solidly based if the meaning of a given linguistic unit is defined to be the set of (paradigmatic) relations that the unit in question contracts with other units of the language (in the context or contexts in which it occurs), without any attempt being made to set up 'contents' for these units." (1963: 59)

Without detailing the semantic relations which Lyons discusses, we can appreciate the fact that the priority of sense-relations over meaning implies a research strategy different from that suggested by a referential theory of meaning. In investigating terminological sense systems, for example, we begin not by discovering denotata of terms but by relating lexical items to other lexical items: defining the appropriate lexical subsystem and identifying particular networks of semantic relations.

These relations are both paradigmatic and syntagmatic -- that is, they can hold between units which can contrast in the same environment, and between a unit and elements of its environment. Analysis of the semantic structure (in Lyons' restricted sense) of a language reduces the complexity of the lexicon and makes specific the limits on many options in lexical selection, etc., in grammar. Some examples will clarify these last remarks. (a) Lyons describes such paradigmatic sense-relations as hyponymy (class inclusion) -- "We will say that scarlet, crimson, vermillinn,

etc., are co-hyponyms of red, and tulip, violet, rose, etc. co-hyponyms of flower. (b) Syntagmatic relations between lexical items frequently limit the possibilities for insertion of lexical items in syntactic frames. The strong inclination to fill in the blanks in "I bit it with my _____" and "I kicked it with my _____" with 'teeth' and 'foot' respectively is good evidence for some such relationship (called 'instrumental' presupposition by Lyons (1968: 440)) in the systems bite:teeth, and kick:foot. In Tzotzil and Tzeltal verb-object presuppositions exist between eating-verbs and food items. (Berlin (1967)) It might prove desirable to have a deep structure configuration like this:



Filling in the asterisks under the object NP by selecting a particular food (say, for Tzotzil, vah 'tortilla') would in turn trigger a re-selection of a particular verb (in this case, -ve7 'eat (of tortillas and bread-like foods)) to fill the V-node. The syntactic realization of such facts is difficult; but the semantic facts are clearly available and general. (c) McCawley (1968a) cites a special sort of semantic (or lexical) relation.

> "... probably all languages have implicational relationships among their lexical items, whereby the existence of one lexical item implies the existence of another lexical item, which then need not be listed in the lexicon." (p. 130)

There is nothing surprising in this claim: derivational morphology may be productively viewed as a mechanism by which a language regularly extends its lexicon given certain 'roots' which provide, as it were, the semantic raw material. Much derivational morphology represents the intrusion of syntax into the lexicon. For

example, we can predict the word jailbreaker from a sentence like "he breaks jail"; or, in Tzotzil, we can produce a word like h-cuk-el (prisoner) from a transitive stem cuk (tie someone up, jail someone). McCawley's examples do not involve syntax in the same way.

> "... the English sentence... 'This coat is warm' ... is ambiguous between the meaning that the coat has a relatively high temperature and the meaning that it makes the wearer feel warm... I propose then that English has two lexical items warm, of which only one appears in the lexicon, the other being predictable on the basis of a principle that for each lexical item which is an adjective denoting a temperature range there is a lexical item identical to it save for the fact that it is restricted to articles of clothing and means 'producing the sensation corresponding to the temperature range denoted by the original adjective'." (1968: 130-131)

McCawley argues that the second lexical item is not derivable from the usual "causative transformation." In fact, such implicational relations within the lexicon may be both paradigmatic and syntagmatic, and may or may not represent morphological markers of the transformational history of embedded sentences (from which individual words in surface structure are derived).

We have claimed that the methodology of ethnoscience has been built around a referential theory of meaning (and that the translation sequence of Quine (1960: Ch. 2) is a more sophisticated account of a referential theory). Serious empirical study of semantics must, to be useful to ethnography in ways outlined in Part 1, devote equal attention to techniques of eliciting sense through sense-relations, of the kind we have just discussed.*

*Note that Quine is concerned to formulate meaning relations in terms of stimulus/response. Hence, it is quite possible to state the notion of "stimulus-synonymy" by reference to the 'dispositions to verbal behavior' that he works with. (Cf. §§11-15 of Word and Object.) What we are after, however, is something deeper: the meaning relations in question are conceptual, not, as it were, statistical. How do we get to them? We never can within Quine's framework; so we need something else.

Our methods, moreover, must extend to other dimensions of meaning of ethnographic interest.

(a) We have seen the importance of 'emotive' dimensions of meaning as well as those elusive factors which label some words obscene or colloquial. Such factors may belong not to the competence notion of 'meaningfulness', but rather to the performance notion of 'acceptability'. The traditional methods of, e.g., componential analysis, are helpless in such domains. (How, for example, should we construct an "eliciting frame" to isolate curses?) (b) We saw in §1.4.2 that it is clearly possible to speak of grammatical categories as 'having meaning' in particular contexts. What is the 'meaning' of the choice of a certain tense or aspect in a given context? of a certain part of speech?* The resources a language has for such alternation are part of its semantic structure.

(c) We shall be interested in understanding what I have called "meta-cultural words" -- words which refer to rules or elements of the cultural code. Evaluative words characteristically belong to this class, as do many adverbs (at least in English: carefully, conscientiously, etc.) Consider the word good. A good chair is a chair which can be sat upon, a good tool a tool which does its job. But

*Lyons gives an example from a poem by Lermontov in Russian (1968: 436), in which a root which we may represent as bel- ('white') appears as a form of the verb beletj (only roughly equivalent to a hypothetical English verb 'to white', i.e., 'to stand out white', 'to shine forth whitely') instead of as adjective belyj, 'white'. In traditional terms it is thus correct to say that "In Russian this difference of 'grammatical meaning' is 'superimposed' upon the 'lexical meaning' which is common to both the 'verb' beletj and the 'adjective' belyj." (Ibid., p. 437)

we do not say that, because 'good knife' means 'knife that cuts', 'good' means 'cuts'; and so on. Good acts as a kind of pointer (as does, e.g., baz'i in Tzotzil); the semantics of the word are, therefore, peculiar.

How, then, are we to elicit semantic information of the kinds required? How do we come to recognize a simple meaning relation (e.g., antonymy) between two lexical items? How do we learn the nuances involved in the selection of one word over another, one verb form over another? A clue to methodology is contained in Lyons' remark that "any meaning relations that are established are established for particular contexts or sets of contexts, and not for the totality of the language" (1963: 80) Lyons has in mind a particularly wide notion of context, including not only the non-verbal Malinowskian 'context of situation', but also the verbal network (Quine's inter-animated sentences) which lies behind an utterance, and "all the conventions and presuppositions accepted in the society in which the participants live, insofar as these are relevant to the understanding of the utterance." (Lyons (1963: 83)) Such 'context' seems frighteningly unsusceptible to empirical study. Yet we claim that language typically has resources for placing utterances precisely in context through the device of giving examples, explaining the usage of words.

We have in mind, of course, the methods of "ordinary language philosophy" -- what Austin (1961: 130) suggests might be more aptly termed 'linguistic phenomenology.' The method of giving examples is clearly kin to Wittgenstein's investigations of 'the grammar' of words, his suggestion that we can get out of linguistic muddles by looking at the "special circumstances" of actual use. (1953: §117) Austin characterizes the methodological appeal to 'ordinary language' as an examination of

"what we should say when, and so why and what we should mean by it."* For philosophers there are good reasons to be clear about

*This quote and others of Austin are taken from his paper "A Plea for Excuses", a methodological gem which is reprinted in Austin (1961), pp. 123-152. What we say here is a woefully inadequate and inelegant recapitulation of some of the ideas in that essay.

the potentials of the primary verbal tools of the trade; for ethnographers, getting clear about words is part of avoiding ethnocentrism. So the thrust of the following remark is different for ethnographers and philosophers:

> "... words are not (except in their own little corner) facts or things: we need therefore to prise them off the world, to hold them apart from and against it, so that we can realize their inadequacies and arbitrariness, and can relook at the world without blinkers." (p. 130)

The philosopher's moral is: do not let words blind you to reality. The ethnographer's moral is: do not let your idea of reality cause you to misperceive the native's words or world. In particular,

> "When we examine what we should say when, what words we should use in what situations, we are looking again not merely at words (or 'meanings', whatever they may be) but also at the realities we use the words to talk about: we are using a sharpened awareness of words to sharpen our perception of, though not as the final arbiter of, the phenomena." (Ibid.)

This is a clear expression of the motives of linguistic anthropology as we have construed them.

Briefly, the method requires us to discover what the native would say. Given a word or expression on whose meaning we should like to shed light, we must elicit from our informant (who may, if we are competent in the language, be ourselves (cf. Quine (1960: 71))) descriptions of situations in which that word or expression would be the thing to say. We find out how the expression is used. Contrast between expressions is mapped onto

contrast between situations (examples). And how do we specify a situation? Typically, in words. There is ordinarily no difficulty in talking about situations; and arguing from examples to meanings tends to produce descriptions of situations which specify the relevant aspects of the contexts of utterances. Certainly not all beliefs, not all actions, not all facts of context are relevant to a particular utterance. Building the example around the utterance incorporates just the relevant things into the example.* There is nothing novel about this

*A beautifully concise sample of the technique may be found in Austin's footnote which draws the distinction between the locutions 'by accident' and 'by mistake.' He writes that these expressions may readily "appear indifferent, and even be used together. Yet, a story or two, and everybody will not merely agree that they are completely different, but even discover for himself what the difference is and what each means." (1961: 132-133) The story of the donkeys illustrates the point:

> "You have a donkey, so have I, and they graze in the same field. The day comes when I conceive a dislike for mine. I go to shoot it, draw a bead on it, fire: the brute falls in its tracks. I inspect the victim, and find to my horror that it is your donkey. I appear on your doorstep with the remains and say -- what? 'I say, old sport, I'm awfully sorry, etc., I've shot your donkey by accident'? Or 'by mistake'? Then again, I go to shoot my donkey as before, draw a bead on it, fire -- but as I do so, the beasts move, and to my horror yours falls. Again the scene on the doorstep -- what do I say? 'By mistake'? Or 'by accident'?" (1961: 133 note)

suggestion: fieldworkers commonly exploit their informants' skills at exemplification to explain confusing points of usage. We are urging, moreover, that a systematic use of examples, carefully drawn to highlight the relationships between words and expressions (and grammatical categories) which fall into common paradigms, will successfully uncover and inform us about semantic structure of the kind we have discussed.

To anticipate an objection which is commonly raised against philosophizing from 'ordinary language' and which seems applicable

here, we may ask: isn't it commonly the case that different people disagree about what we should say when? Austin calls this the "snag of Loose Usage."

> "Well, people's usages do vary, and we do talk loosely, and we do say different things apparently indifferently. But first, not nearly as much as one would think. When we come down to cases, it transpires in the very great majority that what we had thought was our wanting to say different things of and in the same situation was really not so -- we had simply imagined the situation slightly differently: which is all too easy to do, because of course no situation (and we are dealing with imagined situations) is ever 'completely' described. The more we imagine the situation in detail, with a background of story -- and it is worth employing the most idiosyncratic or, sometimes, boring means to stimulate and to discipline our wretched imaginations -- the less we find we disagree about what we should say." (1961: 132)

We may, in fact, exploit loose usage to pursue the details of an example that bring out the differences between two words or expressions we are investigating. If disagreement occurs, try as we may to eliminate it,

> "then you use 'X' where I use 'Y', or more probably (and more intriguingly) your conceptual system is different from mine, though very likely it is equally consistent and serviceable: in short, we can find why we disagree -- you choose to classify in one way, I in another... A disagreement as to what we should say is not to be shied off, but to be pounced upon: for the explanation of it can hardly fail to be illuminating." (Ibid.)

This last remark sheds important light on the valid empiricism of the approach we are advocating. We may want to use many informants in our survey of usage*, but the results are not amenable to statistics. We are displaying conceptual structure, and two conflicting systems (or, more hopefully, divergent systems) may be equally valid, equally much in the data.

*And note, here, that we require the well-informed informants at whom Harris (1968: 585ff.) sneers. We require the native equivalent of the linguistically acute philosopher to be our instructor in such investigations. There seems no danger in this. There is, in fact, no other way.

Note, finally, that giving examples which help us to discover meanings or to understand semantic facts is far from having some descriptive device by which to represent those semantic facts in ethnography.* It is to the problems of representation that

*A feeling one gets from much ordinary language philosophy, and explicitly in Wittgenstein's Investigations, is that there is no simpler way to represent the facts than the examples. The argument seems to run as follows: her is an example. If you follow it you will get the point -- and that is as far as we can go. You must be drawn to see for yourself (understand for yourself).

we now turn briefly in the next section.

2.2 Representation

In the introduction to this chapter we remarked that devising ways to describe semantic facts was equivalent to deciding what we must be able to do with such facts to be sure we have them. It is all very well to sensitize oneself, in whatever insidious ways one can, to the nuances of speech and behavior in the native village. But we have hardly got hold of useful information until we know how to pass it on to someone else without bringing him into the field. These claims need no justification; everyone objects to the ethnographer who says "You had to be there." In this section, however, our aims are somewhat deeper. For to a certain extent it is true that with respect to the understanding of the semantic structure of allanguagesone does have to be there. If as we have urged one's linguistic habits are intertwined with one's perception and beliefs, the full facts of word meaning are inseparable from innumerable other facts of life. Deciding how to represent the results of semantic inquiry is a matter of (a) the selection of certain aspects of meaning-phenomena which we wish to convey, and (b) choosing some wider goals for the semantic

description: what are we training the reader to do?

2.2.1 Old ways

Ethnographers have systematically presented semantic facts in three ways: (i) by giving translations of texts, sometimes accompanied by commentaries, as in Malinowski (1968); (ii) by giving ethnographically oriented word-lists for, say, terms denoting sociologically important categories (as typically in sociological treatments of kinship), or more ambitiously, for the whole vocabulary of a language -- giving, in other words, some parts of an ethnographic dictionary; (iii) by offering componential analyses of small lexical or grammatical domains, or more generally, taxonomic or paradigmatic studies of folk classificatory systems, ordinarily in terms of a referential formalism. Each method has drawbacks which, though they are obvious enough, bear mentioning.

(i) Is our aim in studying semantic phenomena at all is to exploit the uniqueness of a language's semantic categories, its own lexical idiosyncrasies, and the queerness of its idiom, then translation defeats our purpose. The pitfalls of translation are well-known. (See especially Nida (1964, 1945)) Moreover, translation accompanied by lengthy ethnographic commentary is not only tedious but highly episodic: we learn very little about the capacity of a language to express, and only a bit about particular morsels of text. Clearly, Bible-translators have more reason to convert a given message from one language to another than do ethnographers; unless, therefore, we wish to strive for mechanical translation of our natives' language, representing the results of our semantic research by

translating will be non-productive.*

*Ethnographies will no doubt contain translations of bits of native text; we claim here only that such translation will be an insufficient vehicle for portraying these semantic facts which have been the subject of this paper.

(ii) Philosophers have puzzled over definition at least since Plato. The anthropological approach to definition, especially in British social anthropology, derives from what Malinowski thought of as "defining a term by ethnographic analysis."

> "All words which describe the native social order, all expressions referring to native beliefs, to specific customs, ceremonies, magical rites -- all such words are obviously absent from English as from any European language. Such words can only be translated into English, not by giving their imaginary equivalent -- a real one obviously cannot be found -- but by explaining the meaning of each of them through an exact Ethnographic account of the sociology, culture and tradition of that native community." (1923: 299-300) (and cf. above §1.2)

Ethnographers have been hard put to meet these lofty requirements. Instead we are apt to encounter native words sprinkled throughout ethnographies, with only informal explanation. This is a matter of economy: we use a native label for an institution, a ritual, or a set of beliefs, to avoid a cumbersome English locution.

Dictionaries which try despite Malinowski to define individual native words in terms of other words (either in the same or in some other language) are of more general interest. Ethnographic dictionaries, like Laughlin (n.d.), attempt to define large segments of the vocabulary of a language while at the same time providing ethnographic tidbits in the form of notes about usage or verbal examples. For the anthropologist interested in language such dictionaries can be informative and suggestive.*

*Casual inspection of Laughlin (n.d.) reveals (i) unexpected etymological relationships between lexical items, (ii) much of

the extraordinary productivity of derivational morphemes, (iii) the startling proliferation of shape and position words in Tzotzil, (iv) much concern (in the form of lexical development) with certain topics, (v) special speech modes (ritual, scolding, humor, etc.) characterized by special vocabularies, and so on.

Dictionaries have, unfortunately, other characteristic (but perhaps unnecessary) deficiencies.* (i) They are unavoid-

*See especially Weinreich (1962, 1964). Our remarks benefit from Weinreich's observations about "the notoriously anecdotal nature of existing dictionaries." (1966: 406)

ably circular: we can never break into a circle of synonyms without already knowing one of them. (ii) Furthermore, most dictionaries are organized along semantically irrelevant lines: namely, alphabetically. (There is an obvious reason: when we look a word up we know how to spell it, not what it means.) A semantically well-motivated dictionary might instead treat words from contrast sets, words with similar (?) meanings, together.*

*Thesaurii do, on the other hand, try to group words together according to meaning. A compromise between Thesaurus and Dictionary might be a dictionary-type list in which words derived from a single root are shown together -- at least in languages which do derive words productively.

(iii) Weinreich charges, moreover, that dictionary definitions typically contain much irrelevant material; that dictionaries should instead confine their definitions to the criterial conditions for a word's use. (iv) Finally, dictionaries try to frame all definitions in the same syntactic shape as the word they are defining. (Nouns are defined by noun phrases, verbs by verb phrases, etc.; dictionaries, like fifth-grade teachers, do not allow us to define 'fight' by beginning, e.g., "that's when two people ...") It would often be easier and more informative to define words contextually, thereby providing extra syntactic

information. We shall return to these problems below.

To overcome the inherent circularity of dictionary definitions we might ultimately hope for a totally neutral (which is to say, universal) meta-language in which to frame definitions. We should need some inventory of universal semantic primitives. That such primitives can be found is an assumption current in some circles (see Katz (1967)), though claimed examples are decidedly unconvincing. In any case, lacking such universal semantic primitives, we must be content with improving definitions by reducing the seriousness of circularity. R.M.W. Dixon suggests that one way to approximate a neutral meta-language is to frame definitions in a deliberately impoverished language. For example, if we can define a large set of words in terms of a smaller set of common words, our definitions are likely to be successful. This suggestion, notably, has two clauses: (i) we need a small set of words (lexical resources) in terms of which the rest of the words can be defined; (ii) we need finite syntactic resources through which to frame definitions, not necessarily the whole syntax of the language. (I.e., we shall not want to introduce into definitions gratuitous syntactic complexity which itself requires semantic interpretation.) Perhaps we can get by with a small basic vocabulary and a small number of definition-schemata as primitives.

For linguistic semantics, devising a well-motivated and highly structured system of dictionary-type definitions would have the advantage of relating lexical items to syntax in a fairly direct way. For our purposes, a sufficiently explicit dictionary would be an ideal vehicle for the semantic information

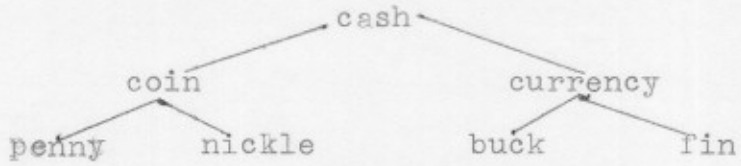
which we hope to build into ethnography. Definitions suitably framed can represent contextual information (viz., verbal environments arising from the examples whose elicitation is described in §2.1.2). Moreover, dictionaries, though not terribly graceful themselves, are eminently more readable than componential analyses -- another reason to opt for definitions over class-products in our ethnographies.

(iii) An alternative to definition which seems to have desirable formal properties is componential analysis. Conklin (1962b) formalizes the technique in demonstrating the taxonomic structure of folk classificatory systems -- insofar as those systems are accurately represented as lexical systems.* The formalization depends on the postulation of analytical entities known variously as 'semantic components', 'semantic markers' or 'semantic features.' Traditionally, one defines a term by identifying its superordinate category, then giving the features which distinguish it from other terms in the same category.*

*Berlin, Breedlove, and Raven (1968) object that nodes on a taxonomic tree which are necessary to accurate treatment of folk classification may not be lexically labelled. There may be covert classificatory categories which are demonstrable but which are not represented in lexical systems.

*If we are talking about nouns, the categories are categories of things (referents) and the distinguishing features are referential features. If we try to organize verbs taxonomically the features upon which the classification rests are less clearly referential, as are the hierarchical relations. E.g., to distinguish 'punch' from 'pummel': is pummeling a species of punching? Or are both subspecies of hitting, the difference being that pummelling is repeated punching? What do we do?

The procedure is a familiar one, which may be illustrated by a simple example. Given the terms cash, coin, currency, penny, nickle, buck, and five-dollar-bill, we may arrange a simple taxonomic tree in the obvious form:



A penny is a coin; it is also cash, but it is not a nickle, a buck, or currency. It is a kind of coin. If we construe each fork in the tree as a choice between semantic components, or between features on some semantic dimension, we may perform the typical abstraction of componential analysis. Hence imagine the following feature systems: a (money/non-money), b (coin/bill), c (1¢/5¢/10¢/25¢/50¢/$1), d ($1/$5/$10/$20/$50/...) If we chose, we could represent penny as x(a(money), b(coin), c(1¢)). Currency would, using the same (deliberately odd) notation, come out as (a(money), b(bill)).

Kay (1966b) pointed out that, formally speaking, a similar procedure could be used to represent the meanings of terms which are not taxonomically structured but form a perfect paradigm. (Under Conklin's analysis the eight pronouns in Hanunoo are described in terms of three binary componential dimensions. (1962a) Hence, since 2^3 equals 8, the pronouns form a perfect paradigm.) The difference may be expressed as a matter or ordering the classificatory cuts; or, we may observe that in a taxonomy, classificatory cuts only govern a portion of the lexical set (e.g., we cannot apply system c(1¢/5¢/...etc.) to the term buck), whereas in a paradigm each term is marked by a feature from each system. Most actual lexical domains fall somewhere between the two formal extremes; therefore it is important to notice that componential analysis is not simply a matter of "factoring" the components from terms, but also of ordering a system of

dependencies between componential dimensions or systems.*

*Nida (1964: 73ff.) has a good discussion of the properties of taxonomically structured sets. He repeats the common claim that "the abundance of terms and the complexity of classification for any semantic area usually depend upon the focus of the culture as a whole, or upon the concentration of attention by persons forming a distinctive subculture." (p. 78)

In §1.3 we review the most frequently voiced criticisms of componential analysis. As a possible representational method for ethnographically interesting semantic facts componential analysis seems doomed as we proceed to wide areas of vocabulary: the problem stems from the essential inefficiency of components. Bolinger (1965) demonstrates how components beget components without justifiable end: we may easily end up with many more semantic 'primitives' than lexical items -- and that is no improvement. The difficulty is not that componential analysis "is only applicable to restricted series of terms which have certain shared and contrastic features" (Nida (1964: 87)). For all semantic analysis is based on the relations between members of relatively small lexical sets, and we can always extract 'components' from such relations.

2.2.2 Possible new ways

Optimal methods for representing semantic facts on a wide range will doubtless combine available techniques. We are not in a position to advance theory on this matter; our aim is simply to equip ourselves for particular limited investigations. However, it is wise to consider the overall requirements of a total semantic description of a language (whatever that would be) so that we may have the benefit of the most powerful descriptive

machinery available.

(a) A particularly attractive suggestion for the 'optimal semantic description' has been offered by Dixon.* It combines

*These suggestions were presented in a course at Harvard during the Fall of 1968. I have not consulted any published material on Prof. Dixon's ideas; therefore I am responsible for any deficiencies or misrepresentation of his point of view.

formal componential analysis with verbal definition based on the full syntax of the language. In particular, the method isolates a subset of the vocabulary, the nuclear words, which lend themselves to componential analysis. The rest of the vocabulary is then defined in terms of the nuclear words (which themselves are not 'defined' except by their componential relations to other nuclear words), given full syntactic resources of the language. Dixon has advanced this sort of analysis for verbs in Dyirbal, an Australian language; it seems possible that for some sub-sets of the vocabulary such complicated machinery will be unnecessary. (For example, many nouns seem adequately described by a referentially based componential analysis.)

If we are to apply this method, for even some small set of words, we encounter several obvious difficulties. First, we must isolate the nuclear words.* We are not without intuitions

*In Dyirbal unique evidence is provided by the fact that the ordinary language is mapped onto a vastly reduced (and quite different) avoidance-language, such that several ordinary language words may be associated with just one avoidance-language word (which may be further qualified to make explicit the meaning). The avoidance-language word may in turn be ordinarily associated with just one ordinary-language word: one might call it the 'central' meaning of the avoidance-language word. Dixon takes the central word (i.e., the word which has a more or less direct avoidance language equivalent) and treats it as the nuclear word in the set of all words which are mapped onto the single avoidance-language word. In English we must resort to other tests, some of which may be pragmatic.

on the matter. Given the set of words 'laugh', 'giggle', 'chuckle', 'guffaw', 'chortle', 'titter', 'snicker', and so on, we can easily identify 'laugh' as the central nuclear word. Our basic criterion is that definitions of each of the other words are most naturally given in terms of 'laugh'; but that it would be hard to define 'laugh' in terms of any of the others. The method we are considering for semantic description would treat 'laugh' as nuclear (capable of entering into semantic relations with other nuclear words -- as, perhaps, 'smile'), in terms of which the other non-nuclear words associated with 'laugh' may be defined. (Hence, assuming that we had elsewhere reason to find the word 'loud' nuclear, we <u>might</u> define 'guffaw' as 'laugh loudly': a phrase whose syntactic deep structure would contain only grammatical relations and the nuclear lexical items 'laugh' and 'loud.')

Such a method certainly requires sharpening and justification; we mention it here because it clearly represents a sophistication of descriptive techniques (though the underlying idea of different strata of words, some defined, and others undefinable, is an old one.) We can, as it were, <u>deepen</u> this sort of analysis. For example, it may be possible, given an account of the derivational processes of a language, to carry out this combined sort of analysis on hypothetical roots alone, rather than on 'words' realized in surface structure.

(b) Nida proposes several methods for representing the <u>range</u> of referential meaning of single words. (1964: 88-99) In particular he notes that it is often possible to distinguish several "widely different types of contexts" in which a single

word may appear. Some semantic theorists have urged that different senses of a 'word' (i.e., sharing one phonological shape) be treated as quite separate. However, Nida suggests that distinct senses can often be reconciled by reducing them to components and observing the cohesivemess of the different senses with respect to them. (Cf. particularly his treatment of the Anuak word jwok, (1964: 91-92).) The fact that words have often wide ranges of meaning can be handled only with difficulty by traditional techniques of ethnographic semantics.* The problem relates

*Keesing (1969) claims that Lounsbury's kinship studies represent a theory of the semantics of labels for "things" which allows for the typical polysemy of such words."

to more than homonymy. We can identify homonyms (usually with only distributional facts, about collocations of different senses of a word with different other key words.)* And we may use

*The General Inquirer system for content analysis makes use of a simple procedure for identifying one of several homographs by machine. It stores a description of the common collocations for each homo-graph and checks each questionable occurrence against that description. Usually collocations of only three or four words are sufficient to disambiguate homographs. (Stone (n.d.))

componential analysis, as Nida suggests, to distinguish (and, importantly, link together by common componential dimensions) the different senses.

If, moreover, we couple this basic componential analysis with extension rules of some sort, our description may be able to encompass metaphorical or mythological extensions of meaning. Such a technique may succeed with grammatical categories as well. Dixon (1968) employs such rules in the description of Dyirbal noun classes ('genders') which have specifically syntactic

function, but which are not without semantic content. He identifies a few basic 'concepts' which govern the assignment of nouns into classes. In addition he proposes a set of transfer rules, which transfer a noun which seems to belong in one class into another; the rules are based on a limited number of principles (as, for example, ritual or mythological association between objects of different classes.) Though particular applications may appear ad hoc (as mythology can be, apparently, ad hoc), the rules of transfer embody the conceptual linkages which govern noun-class membership; anomalies which, by a simple componential system, seem to be just exceptions, are made to accord with a few productive rules.*

*We have here a device formally similar to Lounsbury's reduction rules for kinship analysis, or to a reversal of the rules to form 'expansion rules.' From a finite core meaning for a given term (or grammatical category), finite rules extend or change the meaning. The lexical rule suggested by McCawley (1968) which we mention above in §2.1.2, may be taken as a kind of "extension rule" -- extending the core meaning of a subset of lexical items in specific contexts.

Note that such extension rules are heavily bound to context. We could say that "This coat is warm" is two-ways ambiguous; but "This drainpipe is warm" can only be taken to be ambiguous in the same two ways if we understand the speaker to be wearing a drainpipe.

Such a device will figure in ethnographically interesting semantic descriptions by relating, say, modes of expression in one domain (e.g., space) by extension to other domains (e.g., time). In considering exotic languages we commonly find it difficult to understand the multiple, seemingly unrelated uses of single words or grammatical categories. We may be unable to recast our thought so as to make unlikes seem like; in such cases, devising extension rules may provide a homogeneity to

to native expression which we could otherwise never fathom, let alone appreciate.

2.2.3 Looking back

We have come to a stage in anthropology in which even the best works of ethnographic semantics (e.g., Berlin (1968)) complain of the lack of new ideas, powerful descriptive techniques etc. Indeed, our results in the field of ethnographically oriented semantic description are paltry, indeed, when viewed from the perspective of the possibilities. In this essay we have tried to revive some older goals for a particular sub-branch of linguistic anthropology. For want of a better name, we have called the sorts of semantic inquiry inspired by Boas, Whorf, Sapir, Malinowski, etc. "ethnographic semantics"; for we feel that the sorts of endeavor we saw proposed in Part I would be more deserving of that name than are the rather pale studies which exist. In Part II, we have tried to suggest procedures and techniques for a particular sort of ethnographic semantics, -- not new but often unnoticed or untried procedures and techniques. The general aim has been (i) to widen our notions of 'ethnographic semantics' to include studies of truly ethnographic interest, and (ii) to show that a reasonable place to look for new ideas for an 'ethnographic semantics' is to current linguistic semantics. We can perhaps let the old methods die without, at the same time, killing off our original inspirations.

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