TERMS FOR CANADIAN DOCTORS: LANGUAGE AND SOCIOLOGY,
ETHNOSEMANTICS AND ETHNOMETHODOLOGY

by

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ABSTRACT

In trying to discover the nature of cultural competence, ethno¬semantics leaves out of account the judgemental or interpretive work of a society's members, and that neglect is fatal to its programme. This critical thesis is the nub of the dissertation. The latter is constructed as an argument, and is organized in two parts as follows.

The first part is programmatic. After an introduction (Chapter One), three kinds of sociology are introduced and formally described (Chapter Two). Ethnosemantics and ethnomethodology are cast as "grammatical" and "interpretive" sociology respectively (Chapter Three). This enables us, in pursuing a methodological critique of ethnosemantics from a position based in ethnomethodology (Chapter Four), to draw conclusions about sociology (Chapter Five): insofar as "positivistic" sociology presupposes "grammatical" sociology which presupposes "interpretive" sociology, then (1) "positivistic" explanation is not, in principle, superior to commonsense explanation, (2) an adequate sociology needs be interpretive, and (3) ethnosemantics, in engaging in what ethnomethodology calls "constructive analysis", fails to be an adequate sociology.

The second part is empirical. The argument is now pursued in terms of data from a study of terms for Canadian doctors. After an introduction (Chapter Six), the methods and results of the ethnosemantic part of the study are presented (Chapter Seven). These results are then critically examined in the light of an ethnomethodological analysis of the
interviews which generated them (Chapter Eight). It is concluded (Chapter Nine) that ethnomethodology provides both the missing analysis and an account of ethnomethodology's failure.
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I cheerfully bear final responsibility for this piece.
TO MY WIFE AND SON

"Whereof one cannot speak, ...."
PART ONE

PROGRAMMATICS:

THE ADEQUACY OF ETHNOSEMANDICS AND ETHNO-
METHODOLOGY AS THEORETICAL SOCIOLOGIES

ATTEMPTING TO ACCOUNT FOR CULTURAL

COMPETENCE
CHAPTER ONE
INTRODUCTION TO PART ONE

Broadly conceived, this work is about the best way to do sociology. Narrowly conceived, its subject matter is classification. We require of a certain kind of product - a kind of classification - that it address the process of its production - the activity of classifying. Since, in Chapter Two, we propose some classifications of our own, here, for clarity's sake, we suggest a classification of classifications - (1) theoretical classifications, (2) folk classifications, and (3) heuristic classifications.

(1) In science, classifications are held to arise properly only from theories - as the types into which some concept divides or, if the concept enters into a hypothesis and is interpreted, as values of a variable. The properties of such classifications - exhaustiveness, exclusiveness... - are contrasted specifically with those of (2) folk classifications, the properties of which are looser and less well-defined. However, these latter may be and have been studied in their own right - Conklin's massive bibliography, *Folk Classification* (1972), lists over five thousand items. The folk category can be said to subsume the theoretical category, the latter being the "property" of one kind of folk - scientists. Taxonomies, paradigms and trees are names of some of the kinds of classification studied by cognitive anthropologists - such semantic arrangements are the formal subject matter of ETHNOSEMANTICS. This essay is about the short-
comings of ethnosemantics as a kind of sociology. (3) Heuristic classifications are pragmatic devices serving some practical purpose at hand. They hold no significance beyond that purpose. It may well be acknowledged that all classifications, of whatever kind, have this feature. It is part of the argument of this essay that this observation is crucial rather than trivial.

As pegs on which to hang the argument we propose two heuristic classifications: of facts—"brute" and "institutional"; of kinds of sociology—"1000", "2000", and "3000". These are used, like the formal devices of Wittgenstein's Tractatus, as steps on the ladder of an argument—as we ascend we pull up the ladder behind us. "Brute" and "institutional" are defined in Chapter Two. By "1000" we intend sociology modelled on the natural sciences, and known by such labels as "positivistic", "logical empiricist", "hypothetico-deductive", "deductive-nomological".... By "2000" we intend those sociologies which are taken up with actors' meanings conceived as some kind of map, and which are known by such labels as "grammatical", "symbolic (interactionist)", "cultural code".... By "3000" we intend "interpretive sociology", distinguished by such terms as "pragmatic", "members' methods", "procedures", "phenomenological".... It can be seen how these kinds of sociology parallel our kinds of classification. Sociology 1000 is included in this work principally because it's difficult to leave it out. Our main interest is in Sociology 2000 and Sociology 3000—specifically those versions of each known as ETHNO-SEMANTICS and ETHNOMETHODOLOGY.
Using the "facts" dichotomy Chapter Two characterizes the three kinds of sociology in terms of the different formal objects that each seeks to explain, and in terms of the different modes of explanation employed by each. This is done within the context of an argument intended to demonstrate the superiority of interpretive sociology over the other kinds. Ethsem and ethmeth are then introduced in Chapter Three as kinds of grammatical and interpretive sociology respectively. Chapter Four elaborates the argument of Chapters Two and Three in the form of a methodological critique of ethsem from the point of view of ethmeth. In its own terms ethsem is seen to generate successfully both data and results, despite the acknowledged problem of context or of "abstracting from pragmatics". But in terms of ethmeth that success is seen to depend on a course of interpretive work that remains unexplicated and inexplicable within the ethsem "paradigm". The conclusion is drawn, in Chapter Five, that the only adequate sociology is that which takes account of such interpretive work.
1. Being unwieldy, both terms are abbreviated frequently throughout the dissertation. Ethnosemantics becomes ethsem, and ethnomethodology becomes ethmeth.
CHAPTER TWO

HOW SOCIOLOGY 1000 PRESUPPOSES SOCIOLOGY 2000

WHICH, IN TURN, PRESUPPOSES SOCIOLOGY 3000

Introduction

In *Speech Acts* (1969) Searle, following Anscombe (1958), uses the terms "brute" and "institutional" to distinguish two kinds of facts. Examples of brute facts are recorded in the statements, "This stone is next to that stone", and "I have a pain". At first blush these are records of simple sense experiences, requiring no "social" knowledge for their understanding:

One might say they share the feature that the concepts which make up the knowledge are essentially physical, or, in its dualistic version, either physical or mental (Searle, 1969: 50).

Examples of institutional facts are available in "Ms. Jones married Mr. Smith", "Liverpool beat Leeds 5-2", and "Montgomery saluted". In these cases more is involved than simple sense experiences. Here, knowledge of a different kind is required for an adequate understanding:

There is no simple set of statements about physical or psychological properties or states of affairs to which the statements of facts such as these are reducible (Searle, 1969: 51). ¹

Sociology 1000, Sociology 2000 and Sociology 3000 differ according to what it is they conceive requires accounting for, and how it is that that accounting should be done. For each, datum and methodology come together in a particular kind of abstract object that becomes the topic of
explanation. For Sociology 1000 the abstract object is the REGULARITY, for Sociology 2000 it is the (CONSTITUTIVE) RULE, and for Sociology 3000 it is the INTERPRETATION. These objects can be represented as sentences containing brute and institutional facts, as follows (where individual lower-case letters stand for brute facts, individual upper-case for institutional facts):

(1) [REGULARITY] If Y then Q.
(2) [CONSTITUTIVE RULE] In context Z, x counts as Y.
(3) [INTERPRETATION] Find any lower-case letter to be Z, and see that in context Z, x counts as Y.

Our thesis is that interpretations are the proper object of sociological explanation. Our procedure is to give an example of each sentence (in the context of the kind of sociology which employs each), and to show that the use of regularities depends upon the use of rules which themselves depend upon the use of interpretations.

Sociology 1000 - Regularities and Theories

Sentence (1) above stands for synthetic, conditional statements typically found as hypotheses derived from deductive theories. In sociology this is chiefly the province of the experimental study of small groups. The characteristics of sociology-on-the-natural-science-model and of the structure of a scientific explanation are well-known from innumerable introductory textbooks. We will take them as read.

A "universal" statement of (1) would be, for example,
(4) "When task groups are differentiated with respect to some status characteristic external to the task situation, this differentiation determines the observable power and prestige order within the group, whether or not the external characteristics are related to the group task" (Berger, Cohen and Zelditch, 1966: 31).

A "singular" statement of this kind would be, for example,

(5) "In 3-man Air Force crews, pilots were more influential than gunners in arriving at a group projective story" (Cohen, 1966: 5).

Such statements express empirical regularities. The process of explanation consists of logically deriving them from a set of axioms and definitions, and subjecting them to empirical test. They are made testable by being put through a filter of correspondence rules and operational definitions (Schrag, 1967: 363). The output is a sentence like

(6) If y then q.

Continuing with our example, an instance of a correspondence rule would be

(7) "Air force rank is a status characteristic" (Cohen, 1966: 6).

That is, an observational term, "air force rank", is posited as an indicator of the theoretical concept, "status characteristic". The operational version of air force rank (say, the verbal response to the interviewer's question, "What is your rank?") would then be the brute fact y.

Our argument is that the theoretical concepts in regularities are institutional facts \((Y,Q)\), and that the apparatus of correspondence rules and operational definitions used to reduce these to brute facts \((y,q)\) can be represented as a set of constitutive rules on the model of sentence (2) - for example,
(8) In context Z, y counts as Y.

In terms of our example,

(9) In the context of the status-differentiation-and-power theory or research programme (Z), the answer to the researcher's question "What is your rank?" (y) counts as (the respondent's) status characteristic (Y).

By formulating it this way we can say that "positivistic" sociology's object of explanation - the regularity - is underlain by (one or more) constitutive rules. This is the point of this section. The point of saying it becomes clearer in the following sections, and is stated in Chapter Five, the conclusion to Part One.

Sociology 2000 - Constitutive Rules and Grammars

Constitutive rules (Searle, 1969: 33-42) tell what it is bits of the brute world will count as in terms of some human institution, given some context. They are to be distinguished from "regulative rules", "instructions", "precept rules" and the like (Black, 1962 [1958]: 109-115; Hayek, 1963: 334-335; Ganz, 1971). Following sentence (2), an example of a constitutive rule is

(10) In the game of cricket (Z), hitting-the-ball-full-pitch-across-the-boundary-line (x) counts as "a six" or "six runs" (Y).

Such rules turn the brute world into the social world, mere behaviour into meaningful action, nature into culture. In this fashion we can say that

...'institutions' are systems of constitutive rules. Every institutional fact is underlain by a (system of) rule(s) of the form 'x counts as Y in context C' (Searle, 1969: 51-52).
To give an account of some feature of the social world is, by this approach, to state the rules which provide for the orderliness of the phenomenon:

The rules account for the regularities in exactly the same way that the rules of football account for the regularities in a game of football, and without the rules there seems no accounting for the regularities" (Searle, 1969: 53).

Some of these rules will be constitutive rules and, in general, the constitutive rules determine the other rules (Searle, 1969: 69).

It is slightly misleading to say that rules are simply the "object" of explanation. Just as regularities partake of the function of explaining by prescribing a relationship between two or more variables, so rules are themselves part-explanation-of-the-data as well as part-formulation-of-the-data. That is, they are both explanatory tool and explanatory object. Nevertheless, there is more to the analysis of institutional facts than the mere provision of one or two rules. As Searle's account of promises shows (1969: 63), a number of rules are required, so raising the question of the relation between them. The ordering of rules in a grammar is analogous to logical analysis within a theory. Grammar stands to rule, then, as theory stands to regularity.

The constitutive-rule-and-grammar conception fits well certain kinds of sociology; they are being glossed here as Sociology 2000. Ethnographies of occupations, professions and institutions such as emanated from Chicago after 1945, Goffman's work, and symbolic interactionism will stand being formulated in terms of a constitutive-rules account. Chapters Five and Six of Wieder's (1975) study of the convict code in a halfway
house provide a particularly fine example of such an analysis, though he
does not make explicit use of these terms.

The body of work at issue in this dissertation is ethnomemetics.
In Chapter Three we argue that this rigorous form of semantic ethnography
is a kind of Sociology 2000. Its semantical rules will be recast as con-
stitutive rules. Before that the case must be made for claiming that con-
stitutive rules are underlain by interpretations. We proceed by attacking
the phrase "in context Z" that forms the first part of a constitutive rule.

The Problem of Context (In Searle's Account of Promises)

There arises the problem with constitutive rules of how we are to
take the phrase "in context Z". Specifically, how does this come to be a
capital-letter, institutional fact in the first place? We want to argue that
the problem of context is crucial; that a specification of context is not
available simply by inspection; that its formulation out of brute facts is
not different from that of other institutional facts; that this being so,
constitutive rules depend themselves on an underlying operation which we
shall call "interpretation". We shall proceed by way of Searle's account
of promises.

In order to delimit the object to be explained Searle finds it
necessary to "[ignore] marginal, fringe, and partially defective promises";
to "confine [his] discussion to full blown explicit promises and ignore
promises made by elliptical turns of phrase, hints, metaphors"; to "ignore
promises made in the course of uttering sentences which contain elements
irrelevant to the making of the promise"; to "deal...only with categorical promises and ignor[e] hypothetical promises"; to "simply assume the existence of grammatically well-formed sentences"; to have it as a condition that "Normal input and output conditions obtain" where that includes "such things as that the speaker and hearer both know how to speak the language; both are conscious of what they are doing; they have no physical impediments to communication, such as deafness, aphasia, or laryngitis", and where communication is serious and literal ("I contrast 'serious' utterances with play acting, teaching a language, reciting poems, practicing pronunciation, etc., and I contrast 'literal' with metaphorical, sarcastic, etc.") (Searle 1969: 55, 56, 57). In short, Searle is

...going to deal only with a simple and idealized case. This method, one of constructing idealized models, is analogous to the sort of theory construction that goes on in most sciences ....Without abstraction and idealization there is no systematization (1969: 56). 9

In other words, in order to say what will count (x) as the object of analysis, a "promise" (Y), Searle has to do an enormous amount of WORK to specify the context (Z). That is, in order to carry out his analysis in terms of constitutive rules and the like he has to employ a constitutive rule to define his object; and in employing the rule he cannot take the context as given but must formulate it in such "institutional fact" terms as "serious", "literal".... In this way he shows that there is no escape from the recourse to institutional facts, facts which themselves rely on further constitutive rules.
To advance constitutive rules as explanation is then to trade on one's readers' ability to "see what one means" given that there is an irreducible, unspecifiable element in the rules themselves. The rules, that is, require interpreting.

Sociology 3000 – Interpretations and Interpretive Accounts

To "see what one means" is to do understanding. Anticipating the section on ethnomethodology in Chapter Three, we can say that

...a common understanding, entailing as it does an 'inner' temporal course of interpretive work, necessarily has an operational structure (Garfinkel, 1967b: 31).

We propose to characterize the operational structure of interpretive work as the reading (giving and receiving) of instructional interpretations or, simply, of instructions. Sentence (3) is our model of an instruction. An example would be, loosely put,

(11) See that what's-going-on-here (c) is a quarrel (Z), and hear, in the context of the quarrel (Z), utterance (x) as an insult (Y).

Note the imperative form, the frank inclusion of an unspecified move (from c to Z), and the incorporation of a constitutive rule. Another example would be

(12) Find this (g) to be an alphabet-learning book (Z), and see that in the context of an alphabet-learning book (Z), the display-of-a-capital-letter-B-on-one-page-and-the-picture-of-a-'bear'-on-the-facing-page (x) counts as (something like) "B for BEAR" (Y).

That is, the notational displays on the pages are instructions for the reading, and not simply the reading itself. The same "picture-of-a-bear" in
another kind of book could be read as "M for MAMMAL" or as "H for HUNTER", and so on.

Such instructional interpretations (or interpretings) are the stuff of Sociology 3000 or, as it may be called, interpretive sociology. They are, on this view, what is to be accounted for in the social world. They are also a model of the (explanatory) account. Datum and account have the same structure. Put differently, what is being said is that (1) the stuff of the social world - what fills up social space - is instructions, and (2) any account of such (a set of) instructions is itself an instruction. Or, another way, the phenomena to be accounted for are accounts (because that is what is "out there" socially speaking), and any account of such accounts is itself, of course, an account. In this way any account (in the form of sentence (3)) also accounts for its own possibility. It is in this sense a further instance of the same phenomenon for which it is an account.

Something of this sort is characteristic of so-called "reflexive sociologies" (Heap and Roth, 1973). It has been put this way:

The specific character of ordinary language is this reflexivity. From the viewpoint of formal language we can also say that ordinary language is its own metalanguage (Habermas, 1972: 168).

We may put it yet another way. For the purpose of making sense (Kjolseth, 1972), what social actors provide for themselves and for each other in their utterances and actions are displays of meaningful items and not merely the items themselves (Garfinkel and Sacks, 1970: 344; Goffman, 1959: 65). The displays are characterized here as "instructions" or as "containing" instructions for interpreting the items. Finding the instruc-
tion in the display requires using some other instruction from some other display in some other place at some other time. But the form of the instructions remains the same - "Find any small letter to be Z, and see that in context Z, x counts as Y". The process is continuous, recursive, and as such conforms, we believe, to the views of the later Wittgenstein (1958). As one Wittgenstein interpreter puts it,

> For we never reach a point where an exit must be made from the maze of words. Admittedly, if a verbal explanation is given at one point, it is only successful if at some other point a connection with things is already understood; and at some points it is more natural not to offer more words. But at no point is an exit obligatory (Pears, 1965 [1951]: 280).

The process depends on displayed items (or uttered particulars) being themselves meaningful, while possessing also an "open horizon" or "surplus of meaning" (Garfinkel, 1961: 61). Since any use will have some meaning (attributed to it), the device of displaying instructions which point to the required meaning allows the actor to go "beyond the information given" (Bruner, 1957) to the proposed sense. "Pointing" is all that is possible, however; and pointing is context-dependent, which means that what is being pointed at is never ultimately decidable. Therefore, whatever sense is made is good enough only until further interpretation through further instructions renders it obsolete, or puts it in question, or whatever.

Conversations, including interviews, are obvious candidates for description of this kind (Wieder, 1970: 133). They are also the stock-in-trade of ethmeth. In Chapter Three, after presenting ethsem as a kind of Sociology 2000, we shall show how ethmeth is a case of Sociology 3000. This will provide a context for the critique of ethsem in Chapter Four.
FOOTNOTES

1. Similar sorts of distinctions are being made in the "molecular – molar" (from Barker; see Turner, 1966: 266) and "behaviour – action" (from Weber; see Wilson, 1970a: 698; 1970b: 58) pairs of sociology; the "observer – actor" (see, for example, Cohn, 1962, 1964, 1967, 1969) and "etic – emic" (Pike, 1967: 37-72) pairs of anthropology and linguistics; and the "appresenting term – appresented term" (from Husserl; see Schutz, 1962 [1955]: 294-297) and its derived "actual-observed-appearances-of-an-object – object-that-is-intended-by-the-particular-actual-appearances" (Garfinkel, 1963: 194) pairs of phenomenology and ethnomethodology.

The "brute-institutional" distinction is introduced in order to argue for the existence and importance of institutional facts in sociological explanation. That, in the end, there may be no such things as brute facts, that brute facts are assimilable to institutional facts, is an opinion we share. Thus we are aware of what Wittgenstein might say about the purported brute-fact status of "I have a pain". Another example would have made the point, however. As stated in Chapter One, we agree with Wittgenstein about the use of ladders.

2. One outcome of the abortive Encyclopaedia of Unified Science project was the infusion of semiotical concepts into American philosophy of science. Morris introduced Carnap (Bar-Hillel, 1964) to the Peircean trio – syntax, semantics, pragmatics – and these came to characterize different segments of a deductive explanation. "Syntax" referred to the purely formal relations among axioms and theorems; "semantics" subsumed the correspondence rules that provided observational content for those theoretical concepts that were to be tested; "pragmatics" dealt with the mechanics and procedures of actual experimentation (Carnap, 1942: 10). As a first approximation it would be true to say that (1) Sociology 1000, as champion of deductive explanation, engages in all three areas of activity, but sees semantics and pragmatics as subservient to syntaxics (Schrag, 1967; Popper, 1968: 61) – regularities are the output of the syntactic component of the theory; (2) Sociology 2000 is basically an exercise in semantics; and (3) Sociology 3000 is the inchoate discipline of pragmatics (Weinreich, 1966: 50; Helmer, 1970), where that is conceived as basic to any work of a semantic or syntactic kind (Carnap, 1939: 166).

3. Though neither (4) nor (5) is in conditional form as given, it is fairly obvious that they could be rendered so without loss of meaning. For "universal" and "singular", see Popper (1968: 59-77). "Sentence" and "statement" are being used rather loosely in this discussion – pace Bar-Hillel (1970: 165, 195±197, 213, 217, 280-285, 364-369). The same is true of "use", "sense" and "meaning" later in the chapter.
4. In this case the "institution" is "science" or "experimental sociology" or the particular research programme or theory - or, better still, the relevant "disciplinary matrix" (Kuhn, 1974). That is, while the theoretical terms of "positivistic" science and sociology have no necessary relationship to categories that are meaningful to the population being studied, they are nevertheless "meaningful" (only partially determined [Bar-Hillel, 1970 (1969): 200]) to the community of scientists using them. They are institutional facts in this sense. They are reducible (in principle) to "physical or psychological properties or states of affairs" only through the elaborate battery of rules and definitions which we formulate as constitutive rules. Searle (1969: 51) gives the inverse square law as an example of a paradigm of knowledge consisting only of brute facts. In terms of our analysis, such concepts in physics as "force" and "mass" are, rather, institutional facts of science. We appreciate that this is an area of controversy in the philosophy of science. See, for example, the papers in Suppe (1974), and the paper by Elliot (1974).

5. It might be objected at this point that the adequacy of the "positivistic" theorist's rules and definitions is judged by, among other criteria, the results of the empirical test. However, it can be shown that constitutive rules are required here to translate the array of experimental results (brute facts) into the research findings (institutional facts); for "it is always possible to say that the experimental results are not reliable, or that the discrepancies between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding" (Popper, 1968: 50, cf. 107 fn*3). One must, however, turn to Garfinkel (1967a [1962]: 95-96, 100-103), Kuhn (1970a: 13-16; 1970b: 238-239) and Elliot (1974) for an appreciation of what is implied by the condition Popper describes. See also Bar-Hillel (1970 [1969]: 200) and McCarthy (1973: 370).

6. In view of his expressed intention (1969: 15) to follow Chomsky (1957, 1965) by taking his (Searle's) "intuitions" as the basic data, it is not clear whether Searle views the x-term in the constitutive rule (sentence (2)) as a brute fact; (compare page 56 for example). Since the issue is complicated and takes us beyond the confines of this already wide discussion, suffice it to say that our conception may depart from Searle's on this point.

7. Chomsky has made this claim throughout his work, though not without retort (Chomsky, 1970a; 1970b; Black, 1970). The relation is analogy only.

8. A fuller account of Wieder's study is given in (our) Chapter Six.
9. Compare Hempel (1952), Nagel (1952), and Schutz (1962 [1954]). On idealization it has been remarked, "In a search for rigor the ingenious practice is followed whereby utterances are first transformed into ideal expressions. Structures are then analyzed as properties of the ideals, and the results are assigned to actual expressions as their properties" (Garfinkel and Sacks, 1970: 339).

10. Searle is not disturbed by this state of affairs: "certain institutional concepts...will appear in the analysans as well as in the analysandum; I am not attempting to reduce institutional facts to brute facts; and thus there is no reductionist motivation in the analysis" (1969: 56). He is content to rest on his linguistic intuitions. While in one sense we can agree with that, the question arises, which Searle does not address, of the source of his ability to sort out the "simple and idealized case" from the welter of real-world "complications". Such ability cannot be simply linguistic. We come to this in Chapter Eight.

11. We owe the bear example to Roy Turner, though its formulaic version in terms of sentence (12) is our own. This section on instructions is heavily indebted also to chapters seven and eight of Wieder (1975). These chapters are partially reprinted in Wieder (1974: 159-172).
CHAPTER THREE

ETHNOSEMAN TICS AS A KIND OF SOCIOLOGY 2000,

AND ETHNOMETHODOLOGY AS A KIND OF SOCIOLOGY 3000

Ethnosemantics as a Kind of Sociology 2000

Ethnosemantics deals with collections of terms sharing a common feature of meaning: examples are kinship terms, colour terms, plant terms and animal terms. Such collections form semantic domains. The core of an ethnosemantical description of a domain is a set of semantical rules. Such a rule states the conditions under which a given term names a given object. This brief description of ethsem will stand until the fuller treatment of Chapter Four is given. Our aim in this section is to posit ethsem as a kind of Sociology 2000, by showing that semantical rules are constitutive rules. This stratagem will enable us, in criticizing ethsem in the rest of the dissertation, to be thereby offering a critique of a kind of sociology at the same time. We lead into this section by way of games.

When it comes to providing an illustrative example of their approach, a wide range of modern theorists turn to games. This is true of Rawls (1955), Wittgenstein (1958), Moore and Anderson (1960), Garfinkel (1963), Hockett (1968), Searle (1969) and Goodenough (1969, 1970). In the next section we shall consider Garfinkel's treatment. Here our intention is to note the parallel treatment of games by the author of "constitutive rules" (Searle, following Rawls [1955: 25-29]) and by a father of ethsem (Goodenough).
Each distinguishes his intended object of description (rules) from mere statistical regularities in game play. For Goodenough, following Leach, the latter are the province of the social anthropologist, the former that of the cultural anthropologist:

Suppose we had the Philadelphia Eagles as an object of inquiry. A social anthropologist would concentrate on the different offensive and defensive formations he sees the Eagles employ in actual play and would assess the way their use apparently functions with respect to their ability to win football games.... A cultural anthropologist, on the other hand, would concentrate on the things one has to know in order to be able to play football or understand it as a spectator (1969: 330; emphasis added).

Goodenough refers to Atkins and Curtis (1968), who write

By 'game rules' we mean here game-defining rules, in the sense of those sets of relatively fixed conventions by which particular games are given their basic structure or constitution (213; emphasis added).

and concludes himself

...a game is nothing but a miniature and highly formalized culture (Goodenough, 1970: 105).

In almost identical fashion (though neither refers to the other) Searle contrasts a brute-fact description of a game of football with one directed at the institutional facts which constitute the game. This is worth quoting in full.

Let us imagine a group of highly trained observers describing an American football game in statements only of brute facts. What could they say by way of description? Well, within certain areas a good deal could be said, and using statistical techniques certain 'laws' could even be formulated. For example, we can imagine that after a time our observer would discover the law of periodical clustering: at statistically regular intervals organisms in like colored shirts cluster together in a roughly circular fashion (the huddle). Furthermore, at equally regular
intervals, circular clustering is followed by linear clustering (the teams line up for the play), and linear clustering is followed by linear interpenetration. Such laws would be statistical in character, and none the worse for that. But no matter how much data of this sort we imagine our observers to collect and no matter how many inductive generalizations we imagine them to make from the data, they still have not described American football. What is missing from their description? What is missing are all those concepts which are backed by constitutive rules, concepts such as touchdown, offside, game, points, first down, time out, etc., and consequently what is missing are all the true statements one can make about a football game using those concepts. The missing statements are precisely what describes the phenomenon on the field as a game of football. The other descriptions, the descriptions of the brute facts, can be explained in terms of the institutional facts. But the institutional facts can only be explained in terms of the constitutive rules which underlie them (Searle, 1969: 52).

Both Searle and Goodenough go on to say that in these respects languages and cultures are like games. Adequate description of languages and cultures must take account of the institutional facts which constitute them, for

...speaking a language is performing acts according to constitutive rules (Searle, 1969: 52; see also 12 and 37),

and

...what is a language if not a set of standards for human conduct of a particular kind? (Goodenough, 1970: 108).5

Recall the form of a constitutive rule (where individual lower-case letters stand for brute facts, upper-case for institutional facts):

(2) In context Z, x counts as Y.

A semantical rule states, for example, that "mother", as an American kinship term, denotes that class of objects having the simultaneous features, "first generation above ego", "female", "lineal".
This can be rewritten as

(13) In the context of the semantic domain of American kinship terms, the collection of feature components, "first generation above ego", etc., counts as the taxonomic concept conventionally labelled as "mother".

We can abstract from this to

(14) In domain (K), collection of feature components (m) counts as taxon (M).

Using Pike's (1967) terminology, which Goodenough adopts (1970: 108ff.), as does ethsem in general, we can reduce (14) to

(15) In domain (K), etic fact(s) (m) count as emic fact (M).

The parallel of (15) with (2) should now be clear. If (2) is the general form of a constitutive rule, then semantical rules (15) are constitutive rules in the field of ethnosemantics. If constitutive rules are the hallmark of "grammatical sociology", that is Sociology 2000, then ethsem is a kind of Sociology 2000. If this is so, then ethsem is both subject to the critique of Sociology 2000 already offered (in Chapter Two), and a vehicle for further criticism of the latter through criticism done on it. Setting up ethsem in this way has been the point of this section.

Ethnomethodology as a Kind of Sociology 3000

Our purpose is to reproduce the relation of ethsem to Sociology 2000 in the relation of ethmeth to Sociology 3000, so that in using ethmeth to criticize ethsem (in the rest of the dissertation) we are at the same time arguing about the merits of two kinds of sociology. As in the previous section, we enter the discussion via games. Searle's account again provides
the foil. Whereas Searle and Goodenough had football in common, Searle and Garfinkel have chess in common.

Like Searle (1969: 33-42), Garfinkel (1963) develops his notion of "constitutive" (qualifying "order" and "expectancies") in relation to games like chess, and then extends the analysis to social action in general. Given the disjunction between brute and institutional facts or, as he puts it, between the "actual-observed-appearances-of-an-object and the-object-that-is-intended-by-the-particular-actual-appearances" (Garfinkel, 1963: 194), then it is the function of constitutive or "basic" rules to "frame the set of possible events of play that observed behaviors can signify" (195). More generally,

A sign correctly corresponds to a referent in terms of the assumed constitutive order that itself defines 'correct correspondence' (195).

So far this is little different from either Searle's or Goodenough's account (cf. also Garfinkel, 1967c: 140 ff.). But Garfinkel continues that he has been "unable to find any game whose acknowledged rules are sufficient to cover all the problematical possibilities that may arise" (199):

I suggest that one is in the area here of the game's version of the 'unstated terms of contract', consisting perhaps of one more rule that completes every enumeration of basic rules by bringing them under the status of an agreement among persons to play in accordance with them, a rule which formulates the list as an agreement by the final 'finely printed' acknowledgement, 'et cetera' (199).

The "et cetera" clause is one of a family of considerations which Garfinkel calls "ad hoc" considerations; the other members of the family are "unless", "let it pass" and "factum valet".
These considerations are quite generally found when professionals—sociologists, anthropologists, linguists, whosoever—make use of instructions, formulas, rules, and the like. There is always an implicit additional section to such statements, one that might be headed: 'practical advice to whosoever might seek to insure the usefulness of the instructions (formulas, etc.) to analyze the situations'... 

'Et cetera' refers to the piece of implicit practical advice that runs: 'Read it like this, "..." and so forth', i.e., to see the rule, if you understand the rule, you presumably can recognize other circumstances and cases of its application without all of them being stated here (Garfinkel, 1972 [1966]: 312).

The et cetera clause corresponds to the "irreducible, unspecifiable element" in rules, as noted in Chapter Two. It is a way of saying that there is a gap between any rule and (a specification of) behaviour which is in accordance with that rule. Recognition of this led to our formulating of instructional interpretations on the model of sentence (3) in the previous chapter. Those parts of sentence (3) consisting of the words "Find" and "see that" are the specific counterpart of the et cetera clause; the imperatives of the instruction are the answer to the openendedness of the clause.

So far the ethmeth literature contains no instructions of this sort. Approximations are to be found in the work of conversational analysts. Sacks offers the following "members' maxim" at one point:

Select that [membership categorization] device that exclusively describes the set of persons at hand, and use that device on them (Sacks, 1966, quoted in Speier, 1970: 205).

Turner has "if an utterance can be read as an instance of an utterance-type, then so hear it" (forthcoming: [ms.] 7). Note the imperative form and the unspecified move, but note also the absence of explicitly incor-
Conversational "rules" such as these are posited as resources on which talkers draw, and as norms to which they orient, in the conduct of talk. They are not fully-fledged instructions of the kind we propose.

Nevertheless, from these incipient instructions, from the et cetera clause, and from certain general statements to follow, it is clear that ethmeth is part of the same enterprise we have glossed as interpretive sociology. At its very heart is the notion, derived from Mannheim (1952), of the "documentary method of interpretation":

The method consists of treating an actual appearance as 'the document of', as 'pointing to', as 'standing on behalf of' a presupposed underlying pattern. Not only is the underlying pattern derived from its individual documentary evidences, but the individual documentary evidences, in their turn, are interpreted on the basis of 'what is known' about the underlying pattern. Each is used to elaborate the other (Garfinkel, 1967a [1962]: 78; see also 1956: 192-195; 1961: 57-59; 1967a [1964]: 39-40).

Whereas we mean this quote to point in a general way to our model of an interpretation, the following quotation speaks to the notion of display:

In the particulars of his speech a speaker, in concert with others, is able to gloss those particulars and is thereby meaning something different than he can say in so many words ....It is not so much 'differently than what he says' as that whatever he says provides the very materials to be used in making out what he says (Garfinkel and Sacks, 1970: 344).

"Making out" equals "making sense" equals "interpreting" equals "doing understanding". Ethnomethodology's particular focus on understanding or interpreting has settled on speaking as its prime subject:

Not a method of understanding, but immensely various methods of understanding are the professional sociologist's proper and hitherto unstudied and critical phenomena. Their multitude is
indicated in the endless list of ways that persons speak
["ironically,...metaphorically,...cryptically,...narratively,...
in a questioning or answering way,...and the rest" (29)] (Gar­
finkel, 1967b: 31, emphasis added within the bracket; cf. Witt­

The "way of questions and answers" is the favoured way of ethnosemantics.
In the next chapter the ethmeth position on rules, interpretation, ques­
tions and answers is brought to bear critically on ethsem's use of those
very same ideas.
FOOTNOTES

1. We mean here actual games like chess and football, and not the entities of mathematical game theory.

2. Fillmore's review (1969) of Hockett's (1968) critique of Chomsky discusses their differences in terms of the properties of games. The issue is quite closely related to that being treated here.

3. We appreciate that many anthropologists would find this a controversial way to distinguish social and cultural anthropology.

4. In the recent past the different disciplines investigating language have been remarkably immune from each other's influence. Goodenough said, "I have sought to avoid entanglement in general semantic theory" (1956: 216; cf. Lounsbury, 1968: 221). The separate development of ethsem and Chomskyan linguistics (traced in Eglin, 1972; see also Hymes, 1964a; Keesing, 1972; Black, 1974: 555) is well-known. While Hymes, from the anthropological side, has addressed himself to a sympathetic critique of Chomsky and to the incorporation of the latter's work in an enlarged sociolinguistics (Hymes, 1972, 1973, 1974), work in linguistic semantics has tended to neglect anthropological semantics (Nida's and Lyons' texts being notable exceptions), until very recently (see now Leech, 1974).

With those philosophers of language who have drawn from Chomsky the situation is worse. Thus Katz can say in 1974 (personal communication) that the reason "I don't discuss anthropological semantics, ethnoscience, ethnosemantics, etc. [in his 1972], is that I don't know very much about them." Similarly, Bar-Hillel writes (personal communication, 1972), "I was not, to my shame, aware of the large ethnological and anthropological literature dealing with topics I had known mostly from a philosophical point of view." This may be over-modest since he himself berates Katz for not showing "any awareness of the importance of semantic fields" (1970 [1969]: 186).

About relevant work in philosophy ethsem, apart from a few book reviews, has been equally lackadaisical (though Tyler [1969d] is an exception). Thus kinship, biotaxonomy and especially colour terms (see, for example, Pears, 1965 [1953]; Wittgenstein, 1958; Harrison, 1972) have been extensively discussed by philosophers of language, in addition to general issues in semantic theory. But one looks in vain for signs of them in ethsem. Wittgenstein's name, for example, does not occur in Tyler's anthology (1969a). In Nida's comprehensive text (1964) he is mentioned peremptorily in an introductory note on the contribution of symbolic logicians. Frake (1961) quotes two words from the Blue And Brown Books, this title being one of two Wittgenstein listings in Conklin's massive bibliography (1972). He is not to be found in
D'Andrade's review (1972b), nor in Black's compendious review (1974). (Indeed the latter, under "Philosophical Approaches" [536-541], gives a nod to Collingwood, Quine, Carnap and Reichenbach, but no "linguistic naturalists" [Bar-Hillel, 1970 (1969): 192] are mentioned.) Colby, alone, it seems, does not fail to include Wittgenstein. His admirable survey (1966) mentions "family resemblance" (7; cf. Weinreich, 1966: 206), "language game" (12 fn. 21), and even manages a quote (16). But when he says of Levi-Strauss' transformational models and Chomsky's generative grammar that "Basic to these new developments [the paper was completed in 1964] is the idea of rules, in many respects similar to Wittgenstein's treatment (1953 [1958])" (10), one begins to wonder.

None of the foregoing should be read as high-handed denunciation of particular authors or of whole fields. One can only be immensely grateful for, for example, Bar-Hillel's "valiant and by now successful effort to raise the level of discussion of language" (Harman, 1973: 150). But, in calling attention to this otherwise parlous state of affairs, we are thereby calling for an end to it. It should be noted that Tyler (1973) has deplored this situation in sociolinguistics in general.

5. Compare the following - "The study of culture is thus the study of normative categories and the relations among them just as the study of language is" (Kay, 1966a: 106; emphasis added). For further incisive remarks on the "normative" feature of ethsem see Wieder (1970: 118, 120).

6. The empirical adequacy of this definition (from Wallace and Atkins, 1960: 61-62) is not at issue here. For a review of the varying semantic analyses of American kinship terms, including that of Goodenough himself (1965), see Wordick (1973).

7. The "emic-etic" distinction was introduced in footnote one of Chapter Two.

8. Indeed, Searle says "the semantic structure of a language may be regarded as a conventional realization of a series of sets of underlying constitutive rules" (1969: 37), and "The rules of semantics are...constitutive, for acting in accordance with them constitutes performing such illocutionary acts as promising, making statements, giving orders and so on" (1967: 125). Since ethsem attempts to restrict itself to locutionary meaning (Austin, 1962) or propositional acts (Searle, 1969: 24ff.; Rosaldo, 1974: 155), we need to rewrite Searle's statement as "the rules of (ethno-)semantics are...constitutive, for acting in accordance with them constitutes performing minimally adequate referential (or propositional) acts". Searle's semantic theory goes well beyond that of ethsem at this point (1969: 25), and in a direction which we
applaud (Turner, 1970a; 1970b). But both remain linked in terms of constitutive rules, and these are the focus of our critique. Footnote one on page 36 of Speech Acts suggests that Searle might object to our equating of semantical and constitutive rules, but he does not develop the point.

9. Clearly there are important differences between phenomenology, represented here by Garfinkel, and linguistic philosophy, represented by Searle. The reader is invited, however, to entertain the particular similarity suggested here. Beyond that, see Roche's (1973) important contribution on this matter, Heap's remarkable doctoral dissertation (1975) and, again, footnote one of Chapter Two.

10. Our claim here - that what Garfinkel is saying is that something more than a constitutive-rules account is necessary for an adequate account of a game like chess - is not countered, we think, by Searle's footnote one on page 34 of Speech Acts; that is, that included in "the rules of the game" are such rules as that each side is committed to trying to win. Garfinkel's notion, like Wittgenstein's (1958), is more radical - as we try to show further on in the text.

11. For some useful critical remarks on conversational analysis as ethnomethodology, see Coulter (1973) and Blum and McHugh (1971: 98-99). For further examples of "members' maxims", see Kuhn (1970b: 239).
CHAPTER FOUR

LEAVING OUT THE INTERPRETER’S WORK: A METHODOLOGICAL CRITIQUE OF ETHNOSEMANTICS BASED ON ETHNOMETHODOLOGY

The member of the society uses background expectancies as a scheme of interpretation (Garfinkel, 1967a [1964]: 35).


In some respects, [ethnomethodology] is the counterpart within sociology of ethnographic semantics and ethnoscience....Ethnography and ethnomethodology share a methodological stance in that both give primacy to explicating the competence or knowledge of members of a culture, the unstated assumptions which determine their interpretations of experience (Gumperz and Hymes, 1972b: 301).

Introduction

In trying to discover the nature of cultural competence ethnosemantics leaves out of account the judgemental or interpretive work of a society's members, and that neglect is fatal to its programme. This critical thesis is the nub of the work and of this chapter. It derives from Garfinkel and, more implicitly, from the later Wittgenstein. The chapter is organized as follows.

Ethnosemantics (Sturtevant, 1964; Colby, 1966; Tyler, 1969a; Conklin, 1972) is characterised by specifying its goals in terms of its theory of culture. Its borrowings from semiotic are made explicit in order to provide a point of departure for the critique. The latter has
two parts - an internal critique drawing on work within the field, fol-
lowed by a critique from ethnomethodology (Garfinkel, 1967a). Two tacks
are taken throughout. The one centers on data gathering, the other on
the semantic arrangements that form the results. Their respective inter-
nal problems - the problem of abstracting from pragmatics, and the problem
of context - reduce, under the gaze of ethmeth, to instances of indexical-
ity. Ethsem's ability to produce orderly results is reviewed by ethmeth
as a case of the accomplishment of social order.

Ethnosemantics

Goals and theory of culture

The long-term goal of ethsem is to explicate an intuition - the
intuition that some things are appropriate, some things not. It is im-
PLICITLY assumed that all people have such an intuition, but that the
"things" vary culturally. It is assumed that people can and do, in a
routine, everyday way, make judgements as to appropriateness of things.
Such ability to make judgements constitutes "competence". The judgement
is the observable evidence of the unobservable intuition.

The model of the judging "is not: 'if a person is confronted
with stimulus X, he will do Y,' but: 'if a person is in situation X,
performance Y will be judged appropriate by native actors'" (Frake, 1964a:
133).

An ethnosemantic explanation would provide a theory that predicts
judgements given situation and event. Put differently, the theory supplies
the "appropriate" reading to an event given the situation. "The 'theory' here is not so much a theory of culture as it is theories of cultures, or a theory of descriptions" (Tyler, 1969c: 5, emphasis added to "theory of descriptions"; cf. Werner, 1969: 336, and Kay, 1966a: 112-113).

The proximate goal of ethnosemantics is to provide what is seen as a vital input to that theory - an account of the taxonomic semantics of the language of the culture in question. It is assumed that members of a culture share classifications of the world; that such classifications are a prerequisite for communication, for meaningful behaviour, for competent judging of appropriateness (Black, 1969); and that these classifications are largely encoded in the semantic system of the language (Frake, 1962: 75).

The world-view of ethsem can be described, then, as follows. By investigating the semantics of a language, a culture's cognitive categories will be revealed. Cognitive categories, in systematic form, make that culture's cultural code (Kay, 1966a; 1970). Each competent member knows the code. Knowledge of the code is a prerequisite for appropriately interpreting (Frake, 1964a: 133; Conklin, 1968: 174) what is going on in the society. Interpretation is the basis for action and interaction.

In summary, and in the words of Goodenough's classic paper, ethsem in the long run seeks, for any society, to explicate its culture where

...a society's culture consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members, and do so in any role that they accept for any one of themselves (Goodenough, 1957: 167, emphasis added; cf. 1963: 284 fn. 5, 257-265).
so that the

...test of such a model as would result from an ethnosemantic investigation would require one to answer the question: 'How would the people of some other culture expect me to behave if I were a member of their culture; and what are the rules of appropriate behaviour in their culture?' [Tyler, 1969c: 5] (Turner, 1970b: 5; cf. Wallace, 1962: 351).

Our question is: can the proximate endeavour of semantic description accomplish the long-term goal of culture explication, of discovering "whatever it is one has to know or believe in order to operate in a manner acceptable to [a society's] members"?

Semiotic background

Conceptually and methodologically, ethsem has drawn on semiotic (Morris, 1938, 1946; see Wieder, 1970), structural linguistics (including Whorf, 1956, and Pike, 1967; see Hymes, 1970a, and Keesing, 1972), and cognitive psychology (Bruner, Goodnow and Austin, 1956; Bruner, 1957; see Wieder, 1970). We shall concentrate on semiotic because to a large extent the relevant concepts in structural linguistics and cognitive psychology are subsumable under semiotical concepts.

Thus, in their founding empirical papers (Goodenough, 1956; Lounsbury, 1956), and accompanying programmatic statements (Lounsbury, 1954, 1955; Goodenough, 1957 [written 1954]), Goodenough and Lounsbury discuss the trio of significatum, designatum and denotatum, pointing out the parallels in structural linguistics: distinctive feature, phoneme, allophone respectively. This is all well-known (Wallace and Atkins, 1960: 67; Lounsbury, 1968: 223-224; Keesing, 1972), and continues to be central in their work (Goodenough, 1965, 1967, 1968, 1970; Lounsbury, 1964; Scheffler and Lounsbury, 1971; but cf. Huddleston, 1974).
The parallel trio from cognitive psychology - criterial attribute, category, and infinite-array-of-discriminable-stimuli - is referred to in the work of Frake and Conklin (for example, Frake, 1962, and Conklin, 1962), and has been criticized by Wieder (1970). We shall not elaborate on their or his remarks.

"Significatum", "designatum" and "denotatum" are elements in the semiotic triangle (Fig. 1), bound together in the relation of signification. In ethsem, signification is given a strictly referential interpretation by way of the sign "lexeme". Moreover, reference itself is restricted to denotation; (in addition to previous references, see Lyons [1963: 4], and Hymes [1970b: 111]). Ethnosemantic results consist of the mapping of lexemes on significata. The mappings take the form of semantical rules (such as the one we proposed for "mother" in Chapter Three). Extrapolations from these results are made, on the one hand to statements about cognitive structure, and on the other hand to statements about social structure (Tyler, 1969b: X; Colby, 1966: 8).

A second set of terms borrowed from semiotic is the trio we introduced in footnote two of Chapter Two - syntactics (or syntax), semantics and pragmatics (for example, Werner, 1966, and Black, 1969: 187 fn. 7). According to Goodenough's early statement (modified only slightly in later work [1970: 111-112]), "Much of descriptive ethnography is inevitably an exercise in descriptive semantics" (1957: 173; cf. Lounsbury, 1955: 159; 1956: 158-159; Morris, 1964: 60-62). Following Morris (1938: 35) there has been deliberate abstracting from pragmatics, the pragmatic (in-
FIGURE 1

THE SEMIOTIC TRIANGLE IN ETHNOSEMANTICS

DOMAIN

SIGNIFICATUM (semantical rule)

LEXEMIC SYMBOL

DESIGNATUM

DENOTATUM

cluding "sociological" [Morris, 1938: 30]) factors to be brought in later under the assumption that the "analysis of the semantic structure of a system enables us to form hypotheses regarding social behaviour" (Lounsbury, 1956: 184; cf. Conklin, 1964: 47; Black and Metzger, 1965: 163-164 fn. 5; Kronenfeld, 1973). This postulated order for their study — syntactics first, semantics second, pragmatics third — is something we shall want to question (in the spirit if not the letter of Hymes' crusade [for example, 1964b: 6, 9-10]).

A third set of terms, less explicitly acknowledged but also Peirce's (1932), is the trio, icon, index, symbol (Burks, 1949). Good-enough, in the 1957 paper, ignores indexical signs, throws iconic signs to structural linguistics (syntactics, cf. Jakobson, 1971 [1965]: 350), and takes non-iconic signs for the ethnosemantic programme. But he clearly means, by "non-iconic", "symbolic" where symbolic signs refer by convention. As Friedrich notes in his 1971 review, echoing Jakobson (1971 [1965]: 357; see also 1956, 1971 [1957], and Lounsbury, 1960),

Relatively little attention has been paid to what Peirce called 'iconic and indexical signs' (1971: 170; also, forthcoming: [ms.] fn. 7).

That is understated. However, while ethsem has busied itself with the semantic structure of lexemic symbols, ethmeth, quite separately, has developed a sophisticated discussion of indexical signs and of pragmatics, to both of which we shall return.

The fourth borrowing from semiotic is the notion of "type of discourse". In the hands of Charles Morris, the sphere of pragmatics -
that is, the relation between the sign-user or interpreter and the signs he uses, or, in more dynamic terms, that field comprising the judging acts of interpreters (cf. Weinreich, 1966: 150; Werner, 1966: 44; Bar-Hillel, 1970 [1954]) - was reified into a classification of "types of discourse". That idea carried over into ethsem as the notion of "domain" (Conklin, 1962: 130; Lyons, 1963: 84; Wieder, 1970: 113 fn. 6), that bounding context from which any lexeme in the domain drew its sense (Lyons, 1968: 427) by contrast with the other members of the domain (Lounsbury, 1956: 161-162; Conklin, 1962: 124; Kay, 1966b: 20; Tyler, 1969c: 8; Scheffler and Lounsbury, 1971: 11). See Fig. 1.

The critique will not address explicitly the notion of domain. The latter's technical problems have been noticed by those within the field (especially the biotaxonomists), and by those marginal to it (for example, Schneider, 1969; McClaran, 1971: 6). More importantly, an ethnomethodological critique has been done by Wieder (1970: 113-114, 120, 129-131; but cf. also Frake, 1964a: 140-141). However, criticism of it will be implicit in the section on ethmeth.

Internal Critique of Ethnosemantics

The data-gathering operation - abstracting from pragmatics

If their programmatics are to be believed ethnosemanticists proceed in general inductively (Goodenough, 1957: 168; 1965: 287 fn. 3; Sturtevant, 1964: 100). To the extent that the inductivist programme is followed, however, it is logically bound to fail. "This crucial point is
the tautology that we cannot name a class without naming it" (Pears, 1965 [1953]: 335; cf. "It is impossible to cross the gap between language and things without really crossing it" [Pears, 1965 (1951): 271]; cf. also Kaplan and Manners, 1972: 182-184). Nevertheless, ethnographers such as Metzger, Williams, Black (1963) and Frake have devised impeccable discovery procedures for doing the impossible. To illustrate and elaborate the argument let us look closely at the "Tenejapa" half of Black and Metzger (1965), and compare it with Siverts' little-noted report (1966/67) on the same project.

According to Black and Metzger (1965),

The eliciting heuristic starts of necessity with Western categories, but the ethnographer can discard this position once he has an initial set of responses, and from then on everything he does depends on the last thing he did. The boundaries of the system he explores are revealed as he proceeds (141-142; see also Williams, 1966: 14).

(a) The cost of trying to be presuppositionless is the necessity of starting with one's own categories. Thus even at their most inductive and open-ended they must fill the slot in the question "'what is an interesting question about ________?'" (146).

(b) In the sample of eliciting given in the body of the paper it is not at all clear that "everything [the ethnographer] does depends on the last thing he did". For example, in a sequence of questions concerning the events following a murder the ethnographer "leads" the whole time. This is most blatant at the point, following questions on the disposal of the body, at which he goes on to ask, quite suddenly, "'What does the killer do if he is smart?'" (153).
(c) In fact it is only by introducing these unsolicited questions that boundaries are established at all. The Tenejapa data are sufficient to show that, to the extent that the inductive procedure is rigorously followed, to that extent the task is endless. That is, there is no sign of system closure or domain-boundedness in these data except at those points where the ethnographer "steps in". As noted in another study, "simply requesting informants to be more and more specific results in greater and greater informant variability" (D'Andrade, 1972a: 33).

Unfortunately, the impression conveyed in the paper of inductive method and 'clean eliciting is misleading:

However, what I have outlined here is merely an ideal picture of the eliciting situation exposing in a somewhat abbreviated fashion the main features of an interview routine and the basic operations involved. Departures from this model are certainly countless. Steps are sometimes taken in a different order. This is to say, that while the eliciting process in itself is highly informal and rather casual at times, involving all kinds of stimuli, circumlocutions and prodding, the basic check regarding FTR [Frame-Term-Response]-stability is always adhered to (Siverts, 1966/67: 329, emphasis added; cf. Williams, 1966: 16; Keesing, 1967: 11; Manning, 1973).

So much for "formal eliciting". Siverts reveals other interesting features of the investigation including the methodological notions of "context-free units" and "conditioned response". The latter is "the result of an agreement between anthropologist and informants upon a native-language sequence" (327, emphasis added). That is, what the investigator and informant bring off as an interactional accomplishment - "agreements" - is translated by the investigator into a "conditioned response".

The same translation work is necessary when the investigator has to accommodate the informant's "tendency to respond not only to the question at hand but to anticipated questions, indeed not unlike exchanges in ordinary conversations" (Siverts, 1966/67: 330, emphasis added). Investigators who have used the interview method cannot have failed to make the previous observation; yet it is rarely acknowledged - and when acknowledged, rarely seen as theoretically important. Thus,

The constraints of this method, particularly on highly articulate informants, is considerable. Almost every question we asked was answered by a textlet instead of a list. The most interesting information was often in the parts of the answer which was least expected (Perchonock and Werner, 1969: 238; cf. Berreman, 1972: 580).

While we must be grateful to Siverts for perhaps the only account that approximates what actually occurs in ethnosemantic interviewing - an ethnography of the ethnographer (Berreman, 1966: 350) - that account invites the three following conclusions: (1) the method of formal eliciting is not imbued with the systematic rigour it is elsewhere claimed to have; (2) such light thrown on actual interviewing practices reveals the problems and practices of trying to overcome the contextedness of the enterprise, that is, the problem of abstracting from pragmatics; (3) we may seriously question the value of the resulting ethnography - "a voluminous log-book of FTR-sequences" (Siverts, 1966/67: 329) - especially since how "these categories are actually manipulated in social life is beyond the scope of the procedure itself" (332).
More is made of "interactional accomplishment" and "translation work" in the section on ethmeth, where they are seen to be of critical interest. For now, it is hoped that this internal critique of the data-gathering operation provides grounds for agreeing with Carnap that

If we are concerned with a historically given language [a natural language], then pragmatical description comes first and then we may go to semantics (Carnap, 1939: 166; 1942: 13; cf. Kecskemeti, 1952: 73; Spang-Hanssen, 1954: 26; Bar-Hillel, 1970 [1954]: 70; Helmer, 1970: 733).

The semantic arrangement - the problem of context

Having gathered the data, the ethnosemanticist analyzes them. The end-product of the analysis is a semantic arrangement - taxonomy, paradigm, tree, etc. - which, according to various criteria, gives an adequate semantic description of the data. In the famous Burling debate of the mid-sixties (Burling, 1964a, 1964b; Frake, 1964b; Hymes, 1964d; Hammel, 1964; Wallace, 1965), it was pointed out that there are logically many correct semantic descriptions of any given lexical set. If it is not assumed that there has to be one "correct" solution only (Hymes, 1967: 633), but that, in principle, two or more or all solutions may be "correct", then what is the status of these variants? Is it that there is a common core with subcultural modifications of the boundaries, or a central model with fully-fledged, alternative subcultural systems, or a system where all variants are equally "correct" (cf. Goodenough, 1963: 262; 1965: 259; Tyler, 1969c: 5; Wallace, 1970a: 23-36; Sankoff, 1971)? For our purposes this is the most important issue arising from the Burling debate.
Recall that descriptions of cognitive representations on the one hand, and social representations on the other, are the two poles of cultural competence towards which ethsem has seen itself as moving. Let us take some not-so-recent writings of Wallace and Tyler as representative of mainstream ethsem analyses, Wallace's being directed more at cognitive matters, Tyler's more at social ones. With the emphasis on Tyler's progress, let us assess those writings' implications for the endeavour to discover "whatever it is one has to know or believe in order to operate in a manner acceptable to [a society's] members".

From his relational analysis of American kinship terminology, which analysis he claims has cognitive-psychological reality, Wallace concludes:

> Kin terminologies may be reckoning devices, like systems of weights and measures, whose utility depends more on internal coherence and convenience of calculation than on their fit with the social system (Wallace, 1970b: 152, emphasis added).

That is, social-structural relevance of terminological analysis is quite limited. His single, powerful, elegant and psychologically real solution is, in Wallace's view, a reckoning device such as a person might employ in social interaction, rather than a model of what constrains his social interaction. Despondent conclusions of a related sort are recorded by D'Andrade et al. (1972).

In contrast to Wallace, Tyler has been trying to pin down his structures to the social world, sacrificing a single model for empirical sensitivity. He declares in a 1966 paper that, because of its inadequate
treatment of variation, that branch of ethnosemantics called "formal
analysis"

...does not provide the minimum information for deciding who
will be called what in any kinship system (cf. also Swartz 1960:

Therefore,

I will attempt to relate terminological variation to the contexts
in which terms of reference are used (694; cf. 1966b: 515; Good-

What Tyler finds is that there

...are many contextual factors to be taken into consideration.
Among these are: social setting, audience composition, sex
and age of speaker/hearer, and - most difficult of all - something
that might be called the speaker's intention (704-705; cf. Fill-

The article was reprinted in Tyler (1969a) and again, with slight revi-
sions, in Gumperz and Hymes (1972a). In the revised version is added,

The important point is that this chapter demonstrates the possi-
bility of extending formal rules to these contextual factors.
It is not an argument against the validity of formal analysis;
rather, it is an argument for the extension of formal analysis
to include extra-genealogical factors ([1966a] 1972: 268; cf.
Basso, 1972).

Tyler is advocating the importance of variation by context but proposes
an extension of the existing method to deal with it. (In contrast, but
following from the same sort of observations, Sankoff proposes a quantita-
tive approach to handling variability [1971: 391].)

However, while that paper was going through reprintings, Tyler
himself was moving to a more radical position. In his 1969 paper, "A
formal science", he has the following.
The slogan that meaning varies with context is a form of holistic argument. Like Hegelian holism it is workable only if it can be demonstrated that contexts are finite. Note also that if rules of use are to incorporate contextual features, it is not even possible to formulate rules unless contexts are finite. It does not need demonstration to prove that the total physical surroundings or context of any utterance are never exactly the same on two different occasions. Thus, contexts cannot be finite. This is the paradox of the contextual theory. Since the notion of context violates the idea of rule, we cannot properly speak of meaning as a rule of use. Yet, since humans do seem to take contextual features into account, they must have some means of establishing equivalencies among non-identical contexts (1969d: 75; cf. Goodenough, 1956: 197 fn. 5).

If what Tyler says is true, then it undermines his own method (1966a) of dealing with contextual variation. If rules are to build in contextual factors, but context cannot be specified, then the programme cannot be carried on. Moreover, it is no use saying, with Hymes, that form (lexeme) and context mutually determine meaning (1962: 19; 1964c: 97-98), or, in Frake's terms, act and situation (1964a: 133), for the problematic terms - "context", "situation" - are left unexplicated (Wieder, 1970: 119-120).

Tyler's point undermines the whole ethnosemantic enterprise.

This discussion of Wallace and Tyler has been at pains to show that: (1) insofar as a unitary, cognitively valid model is achieved, social-structural significance is lost; and (2) insofar as context is allowed to operate, to that extent the enterprise loses itself in the attempt to pin down elusive context. Social structure as semantic arrangement disappears in an infinity of contexts. (The reader will recall Chapter Two. We have here recapitulated for ethnosemantics the argument about context made against constitutive-rule accounts in the third section of that chapter.)
It is at this point that the ethmeth treatment of "indexicality" must be introduced in support of Peirce's insight into that idea's indispensability and utter pervasiveness. For the "irremediable elusiveness of context" is one way of characterising the "utter pervasiveness of indexicality" (Peirce, 1932: 172; Wells, 1967: 104; Luckmann, 1972: 31). And, secondly, that ethsem nevertheless achieves "rational" results raises in a new way the concomitant "problem of social order": how societal members (such as semantic ethnographers) establish "equivalencies among non-identical contexts": It is time to shift perspective.

Critique From Ethnomethodology

The demonstrably rational properties of indexical expressions and indexical actions is an ongoing achievement of the organized activities of everyday life (Garfinkel, 1967b: 34).

Introduction

Recall that the goal of the ethnosemantic programme is to explicate culture where culture is knowledge - "whatever it is one has to know or believe in order to operate in a manner acceptable to [a society's] members". The goal of ethmeth is essentially the same (cf. Garfinkel's definition of "competence" [1967a (1964): 57 fn. 8; see Cicourel, 1970: 147; Phillipson and Roche, 1971: 34; Mehan, 1972: 1; Moerman, 1969:465; Gumperz and Hymes, 1972b]). We are saying that ethsem cannot reach that goal by pursuing semantic ethnographies. We are now proposing that ethmeth is better equipped for success. A clue to the difference is that both the
obstacles in the way of ethsem and its "accomplishments" are instances of the very phenomena which constitute ethmeth's topic and domain of inquiry. This part of the chapter will recast those problems as that topic, and discuss the topic in terms of the ethmeth ideas of "indexicality" and "accomplished social order".

**Ethsem reformulated in terms of ethmeth**

Accounts of data-eliciting and of semantic analysis such as the ones already discussed disclose, when adequately reported, two pervasive characteristics which we shall present as a contradiction:

1. impossibility of obtaining results;
2. results obtained.

The contradiction is removed by adding "logical" to (1), and "for-all-practical-purposes" to (2). The characteristics are now

1a. logical impossibility of obtaining results;
2a. results obtained for-all-practical-purposes.

These descriptions are meant as summary glosses of the critical points made, and other features noticed about ethnosemantics in the previous sections:

1a. (i) The problem of abstracting from pragmatics;
   (ii) The problem of context.

2a. (i) "Agreements" between ethnographer and informant (Siverts);
   (ii) People's ability to establish "equivalencies among non-identical contexts" (Tyler).
For ethnosemanticists (Tyler [1969d] excepted) (1a)(i) and (ii) are not, of course, seen as logical problems but as methodological ones. They are problems for which the solution is methodological innovation and/or the subsuming of more pragmatic information under semantic description (Berreman, 1972: 584 fn. 5). (2a)(i) and (ii) are merely taken for granted.

For ethmeth, however, (1a) and (2a) are the two sides of the indexical coin, the currency of which is universal. The observations gathered under (1a) are instances of "indexicality". The observations gathered under (2a) are instances of "accomplished social order". For ethmeth the problem which provides it with a programme is: given indexicality, how is social order possible? Said about language this becomes: how is it that coherent conversations are produced despite

(1) the non-grammaticality of utterances,
(2) the absence of shared meanings,
(3) the non-literalness of meanings, and
(4) the indexicality of utterances?
(Crowle, 1971: IV).

We shall now elaborate on indexicality and accomplished social order, tying in aspects of ethnosemantics on the way.

11

Indexicality – the logical impossibility of results

Indexical or occasional expressions are those whose ...

...sense cannot be decided by an auditor unless he knows or assumes something about the biography and the purposes of the speaker, the circumstances of the utterance, the previous course of the conversation, or the particular relationship of actual or potential interaction that exists between user and auditor.

The expressions do not have a sense that remains identical through the changing occasions of their use (Garfinkel, 1967a [1964]: 40; also 1961: 60; 1967b: 4-7; 1967d: 179-180; Garfinkel and Sacks, 1970: 348-350).
(It should be clear how this description parallels, yet goes well beyond, Tyler's list of "contextual factors" quoted in the previous section.)

Indexical expressions are to be contrasted with so-called objective (context-free) expressions. Any investigative inquiry - science, ethsem, ordinary conversation - which is "directed at achieving...agreement among 'cultural colleagues'" (Garfinkel and Sacks, 1970: 349) exhibits profound concern for the "nuisances of indexicals", seeking to remedy them by substituting objective expressions for them.

Such 'methodological' concerns are accompanied by a prevalent recommendation that terms, utterances, and discourse may be clarified, and other shortcomings that consist in the properties of indexical expressions may be remedied by referring them to 'their setting' (i.e., the familiar recommendations about the 'decisive relevance of context') (Garfinkel and Sacks, 1970: 349-350).

The radical, not to say dramatic, point of ethmeth for ethsem is the one that follows:

...not only does no concept of context-in-general exist, but every use of 'context' without exception is essentially indexical (Garfinkel, 1967b: 10, emphasis added).

This has the consequence, for instance, that all rules - semantical rules, componential definitions, contextual rules (Tyler), constitutive rules (Searle, 1969) - are inadequate in isolation to subsume specifiable sets of objects or actions. They needs rely on something external to them for their sense - some language-game, some form of life, some "what anyone knows" (Garfinkel, 1967a [1960]: 275). But, more than that, any defining description of the language-game has the same problem itself - every use of "context" is essentially indexical. Domains, therefore, as the would-be
language-game of ethsem (Colby, 1966: cf. the second paragraph on p. 7 with p. 12 fn. 21), will not mechanically prescribe the right meaning of their member terms.

Let us cast this in terms of the semiotic triangle introduced earlier. According to ethsem a word (lexeme) refers to an object (denotatum) or class of objects (designatum) in terms of a set of necessary and sufficient conditions (significata); where a term is polysemous the association of word-conditions-objects is relative to some domain (Scheffler and Lounsbury, 1971: 11). The question arises however, of how members decide which domain is relevant on any occasion of the use of some term (Wieder, 1970: 120). Domains do not solve the problem of context. They merely push it one step back.

But, as is being argued, one step back is no step anywhere. There is no solution in this direction. Recommendations to secure more information on the situation or setting are beset with the same problem (Handel, 1969: 10; Garfinkel and Sacks, 1970: 338). In this light, then,

...trying out the knowledge [that is, the ethnosemantic results]...in 'real', non-interview situations...[on the assumption that]...when some responses (events) do not occur as predicted from elicited information the ethnographer can discover the additional determinants of response-variation which had not been encountered in interview (Black and Metzger, 1965: 164 fn. 5; cf. Black, 1969: 169, 186-187 fn. 3).

will only raise the question of the contextualization of those (non-interview) events (Garfinkel, 1967b: 6). The ethmeth critique asks for the nature of the grounds by which it is supposed that events in non-interview situations bear some elucidating relationship to events in interview situa-
tions. As will become clear below those grounds reside in common-sense methods of reasoning, not scientific ones.

For now it is sufficient to note that the problem of abstracting from pragmatics and the problem of context reduce to the problem of how to remedy indexical expressions.

**Accomplished social order – results for-all-practical-purposes**

The question is: how do members produce social order given the problem of indexicality. There can be no doubt that, despite indexicality being an inescapable feature of ethnographic inquiries, ethsemists routinely discover an orderly world, which provides orderly results – results, moreover, which are independent of investigator, method and informant:

...the data offered reflect regularities which must be taken account of. These are regularities among conditions which produce regularities in informants' responses. Data of this nature, while requiring some ordering such as we have provided in the sample, is interpretation free...data of this kind has [sic] a structure of its own, about which investigators may agree regardless of their theoretical interests and without regard either to other kinds of material they may wish to use to expand the data or to further analytic operations they may wish to perform upon it (Metzger and Williams, 1966: 390, emphasis added).

How is such social order possible? We shall outline in a paragraph how ethmeth would formulate the order question and what answer it would give; then we shall list ten places where ethsem achieves order using its theoretical and methodological apparatus to gloss over the underlying interpretive work.
Ethsemists find social order in the world, independent of their accounts of it. In contrast, ethmeth treats social order as an accomplishment of societal members, such as ethsemists, and sees that order as a feature of the accounting by which it is told. Where ethsem sees data regularities or investigator/informant correspondence as "interpretation free" or, more riskily, as "agreement", ethmeth sees such expressions as glosses for interpretive work which remains to be explicated. Its question would be "what is the work for which [[interpretation free]] is that work's accountable gloss?" (Garfinkel and Sacks, 1970: 352). The "how" of such findings of order is ethmeth's topic of investigation. Its claim is that the "finding" and the "telling" are the same (Garfinkel, 1967b: 1; 1974 [1968]: 17; cf. Attewell, 1974). It provides a general formulation of such interpretive work in terms of the "documentary method of interpretation". Since the documentary method is not one but many methods, ethmeth's field of inquiry lies open. (This point was made, without specific reference to ethsem, in Chapter Three.)

The question of social order becomes, then, a matter of the work by which, in our case, ethsemists produce order in their data and in their results. What is the extent of that work in ethsem theory and practice?

(1) Let us take notice first of an absence – an absence in published accounts of ethnosemantic studies of what takes place between the research's conception and the start of eliciting. Intended to discover "whatever it is one has to know or believe..." such accounts leave out
how the ethnographer him/herself learns to ask questions, hear answers, what questions to ask, and what answers to ignore. It therefore occurs that the investigator frequently must elect among alternative courses of interpretation and inquiry to the end of deciding matters of fact, hypothesis, conjecture, fancy, and the rest despite the fact that in the calculable sense of the term 'know' he does not and even cannot 'know' what he is doing prior to or while he is doing it. Field workers, most particularly those doing ethnographic and linguistic studies in settings where they cannot presuppose a knowledge of social structures, are perhaps best acquainted with such situations....Nevertheless, a body of knowledge of social structures is somehow assembled....How...? (Garfinkel 1967a [1962]: 77-78).

It is true that the eliciting routines and strategies of Metzger and Co. were designed out of some appreciation of this problem. But this common-sense solution - engaging in more research (formal eliciting) to decide what had been learned previously (unsystematically) - only raises the problems of formal eliciting. We made the same argument about the relationship between interview situations and non-interview situations at the end of the last section on indexicality.

(2) Desirous of "discerning how people construe their world of experience from the way they talk about it" (Frake, 1962: 74), the formal elicitors (for example, Black, 1969: 172-174) find it

...necessary to instruct the construing member to act in accordance with the investigator's instructions in order to guarantee that the investigator will be able to study their usages as instances of the usages the investigator has in mind (Garfinkel, 1967a [1964]: 70).

Such instruction of informants is our second to-be-noted feature of ethsem (cf. Hale, 1966: 808). Motivated by a desire for rigour it can lead to
such strange notions as requiring informants "to ignore any possible scene of the questioning" (Black, 1969: 173). This is part of the work of making out the members of the studied society as what Garfinkel calls "judgemental" or "cultural dopes" (1967a [1964]: 66-71; cf. Cicourel's "dummy" [1970: 160]). The "cultural dope" is made out in these further features of ethsem:

(3) the treating (hearing) of informants' responses as non-problematic answers-to-questions (Garfinkel, 1967a [1960]: 266-267; 1967a [1962]: 92), no notice being taken of the work required to do that "hearing";

(4) treating such "answers" as "conditioned responses" (as discussed in the section on the data-gathering operation);

(5) portraying "agreements" as the "demonstrable matching of substantive matters" (Garfinkel, 1967b: 30);

(6) "portraying the usages of the member of a language community as...culture bound [and this includes the situational variant]" (Garfinkel, 1967a [1964]: 71);

(7) "construing the pairing of appearances and intended object - the pairing of 'sign' and 'referrent' [sic] - as an association [see Goodenough, 1956: 195]" (Garfinkel, 1967a [1964]: 71);

(8) assuming "that an invoked shared agreement on substantive matters explains a usage" (Garfinkel, 1967b: 28);

(9) treating the properties of common understandings or common culture
...as precoded entries on a memory drum, to be consulted as a
definite set of alternative meanings from among which one was
to select, under predecided conditions that specified in which
of some set of alternative ways one was to understand the situ­
ation upon the occasion that the necessity for a decision arose
(Garfinkel, 1967a [1964]: 41).

In all these cases "a procedural description of such symbolic
usages is precluded by NEGLECTING THE JUDGEMENTAL WORK OF THE USER"
(Garfinkel, 1967a [1964]: 71, emphasis and upper-case added). "User"
refers not only to the subject or informant, but to the anthropologist as
well. If the previous points have emphasized the informant's (neglected)
judgemental work, the following feature highlights that of the investiga­
tor.

(10) Stability of response across informants is a favourable
indicator in the eyes of the ethsemist. Recall Siverts' (1966/67) asser­
tion that "the basic check regarding FTR [Frame-Term-Response]-stability
is always adhered to" (329). On noting informants' tendency "to respond
not only to the question at hand but to anticipated questions, indeed not
unlike exchanges in ordinary conversations" (330), he asks, "Is such a
reaction ruining the whole argument about stability...."

Not quite. Repeating the interview at some later date with
another informant would produce a similar situation, we hold,
since the 2nd. ethnographer and informant are supposed to
follow the rules of the game (331).

But, we may ask, what game is it that has as a feature tendency-to-respond-
not-only-to-the-question-at-hand-but-to-anticipated-questions? What are
the rules of that game (Garfinkel, 1967a [1964]: 70; Wittgenstein, 1958)?
These questions are not addressed by ethsem, but its eliciting and analysis
are predicated on answers to them. Answers are made, but tacitly, commonsensically. Common-sense work is at the heart of the ethsem enterprise. We have already taken note of the unexplicated grounds by which formal eliciting is said to elucidate unsystematic questioning, by which non-interview events are said to elucidate interview events. Here again we must point to the common-sense work which provides for seeing "second" interviews with "second" informants as "checks" on "firsts" (cf. Bricker, 1974). These are all forms of Tyler's more general noticing of people's ability to establish "equivalencies among non-identical contexts" (where "people", of course, includes ethnographers). How is it done? "How are events being analyzed so that they appear as connected?" (Zimmerman and Wieder, 1970: 290). What is the judgemental work of the user?

We gave general answers to this question in the sections on Sociology 3000 and ethmeth in Chapters Two and Three respectively. In the latter, for example, we described the documentary method of interpretation. Beyond this we must turn to empirical materials to see interpretive work in action. This we do in Part Two.
FOOTNOTES

1. A version of this chapter will appear in *Semiotica* (forthcoming). The chapter has benefitted immensely from two papers: first, of course, Wieder's critique of the sign theories in ethnomethodology (1970; see also Turner, 1970b); and, second, Helmer's programmatic paper on a pragmatically-oriented, non-Hymesian sociology of language (1970; see also Kjolseth, 1972: 53). Both Wieder's critique and our's are ethnomethodological (as is Cicourel's [1967]). Whereas Wieder draws out the absurd models of man and society implicit in the sign theories of Goodenough and Lounsbury and of Frake and Conklin, we focus on the methodological assumptions and practices of ethsem in transforming "brute" events into "data" and "data" into "results" (cf. Sankoff, 1971: 405). Consequently, more attention is given here to the work of Metzger, Williams and Black. Black and Metzger's (1965) study of Tenejapa and American "law" terms is of particular importance, since it provides the model for the ethnosemantic analysis reported in Chapter Seven.

2. Frake's sentence appears to be the only attempt to provide a (relatively) explicit formula for the dependent variable in ethsem. In sharp contrast to Frake (1964a: 133), Kay, alone, claims the possibility of predicting actual behaviour as opposed to verbal judgements (1970: 28). By the mid-sixties Chomskyan rhetoric - "structural description", "reading", "competence", grammar as predictive theory - was well in evidence in ethsem (for example, Durbin, 1966; Kay, 1966a; Werner, 1966). The notion of a "cultural grammar" is widespread (for example, Conklin, 1968: 174; Colby, 1975). Keesing (1972) is a useful reminder of the differences between ethsem and transformational generative grammar (cf. Hymes, 1970a). See footnote four of Chapter Three for further references on this.

3. For linguistic semantics of the Katz-Fodor variety three "no's" have already been recorded (Helmer, 1970; Kjolseth, 1972; Coulter, 1973). Our critique of ethsem is of a piece with these papers.

4. In practice, that variation of result persisting after the completion of semantic analysis has typically been cast into the "garbage bucket of pragmatics" (Bentley, 1945: 40) or, de gustibus, into the "pragmatic wastebasket" (Bar-Hillel, 1971; see also Lyons, 1968: 420; Helmer, 1970: 733-734, 743; Garfinkel and Sacks, 1970: 350, quoted below in the section on "indexicality").

5. We have traced the course of this reification through Morris' work in the longer manuscript from which this chapter is drawn (Eglin, 1972).
6. Lest the intent of the critique be mistaken let it be said that it is to the credit of Metzger and Co. that they attempted to formulate explicit discovery procedures in the interests of rigour, publicness and replicability. This way problems are more easily seen, their sources more exactly located (cf. Chomsky's view of the value of pre-Chomskyan structural linguistics [for example, 1968: 19-20]). Indeed Black has said, in response to an earlier version of this chapter, that she "couldn't agree more that (some of) the particular data presented [in the 1965 paper] were inconsistent with the procedure" (personal communication, 1974).

Those others, who, like Metzger and Co., have not in fact proceeded inductively, have done a double disservice in (1) writing in an obfuscating inductive format for heuristic purposes (Lounsbury, 1956: 171; Wallace, 1961: 459; Tyler, 1969a; see Keesing, 1967: 11), at the same time as acknowledging the muddiness of the waters - "aided by some advance knowledge of what to look for" (Lounsbury, 1956: 168; cf. 1953: 406); "The discovery of culturally relevant components requires some advance knowledge of what to look for" (Colby, 1966: 9); "Fieldwork and analysis should be carried out simultaneously" (Tyler, 1969b: X); "a great deal depends on the interviewer's familiarity with the culture and willingness to reorganize earlier formalizations in the light of later inconsistencies" (D'Andrade, 1972a: 32); "The features were thus derived inductively, based on a detailed scrutiny of the data and on intuitions gained from field research" (Seitel, 1974: 52) - while (2) not examining their methods or showing how they did it (cf. Berreman, 1966: 351).

Remarks such as these (even when elaborated [Paul, 1953]) show only that they, like their subjects, rely on the "documentary method of interpretation" and, like their subjects, take that method for granted (cf. Berreman, 1966: 352). In contrast, the work of the formal elicitors allows us to begin at least to examine that method.

7. Contrast the calls for (1) an "ethnography of ethnography" (Berreman, 1966: 350; Conklin, 1968: 175), (2) "ethnographies of interrogation" (Grimshaw, 1969: 21), and (3) a "sociological pragmatics" (Morris, 1938: 30; Carnap, 1942: 10) with the actual work that has been done along these lines by ethnomethodologists - for example, (1) Stoddart (1974), Katz (1975), Wieder (1975), (2) Crowle (1971), Cicourel (forthcoming) and (3) Garfinkel (1967a) and Elliot (1974) respectively. See footnote five of Chapter Two.

8. We have corrected the references to Hymes in the Tyler quote, and re-lettered them according to the ordering adopted here. Tyler argues the same point, in opposition to Buchler (1964: 781), in his 1966b, and again in his 1969f.
9. In the next section we give ethmeth's account of the point Tyler is making. It is interesting that Tyler himself has been moving towards ethmeth (personal communication, 1972; 1973).

10. It will become clear that this is not being asked in the sense in which Denzin asks it, the sense which is criticized by Zimmerman and Wieder (1970: 294; cf. Garfinkel, 1967a [1964]: 74 fn. 13).


Garfinkel and Sacks (1970) detail indexicality's intellectual history in philosophy and logic, including the discussions by Husserl, Russell and Goodman. Its treatment in semiotic since Peirce has been given elsewhere (Eglin, 1972). Briefly, Morris, who found it in Peirce, lost it between Foundations Of The Theory of Signs (1938) and Signs, Language and Behavior (1946), its absence continuing into ethsem through Goodenough's discussion of signs in his foundational 1957 paper (as already stated). This was written in the same year (1954) that Bar-Hillel published "Indexical expressions", and at about the same time that Garfinkel coined the term "ethnomethodology" (Garfinkel, 1974 [1968]). "I use the term 'ethnomethodology' to refer to the investigation of the rational properties of indexical expressions and other practical actions as contingent ongoing accomplishments of organized artful practices of everyday life" (Garfinkel, 1967b: 11; cf. 1972: 309).

12. We put it this way, understanding the "problem" to be an analytic (as opposed to a concrete) one. We do not mean to suggest that, for members in their unreflective everyday activity, indexicality presents troublesome problems. ("Member" includes ethnosemanticists (ethsemists) as practicing professionals.) However, for this member, the resolution proposed here remains an unhappy one.

13. Ethnographers' methods for "telling" their work as adequate ethnography have been documented at length in a recent doctoral dissertation by Stoddart (1975). Katz has treated similar issues in his recent doctoral dissertation (Katz, 1975). We will not discuss these matters here.
CHAPTER FIVE
CONCLUSION TO PART ONE

Using the distinction between brute and institutional facts we distinguished three kinds of sociology by their objects of explanation. "Positivistic" sociology (Sociology 1000) seeks to explain regularities by theories. "Grammatical" sociology (Sociology 2000) seeks to explicate constitutive rules in grammars. Interpretive sociology (Sociology 3000) seeks to account for instructional interpretations through interpretive accounts (cf. Coulter, 1974: 104). The first enterprise depends upon the second enterprise which depends upon the third. Into this scheme we introduced two approaches the goal of which is an adequate description of cultural competence - ethnosemantics and ethnomethodology. By identifying semantical and constitutive rules we cast ethsem as a kind of "grammatical" sociology. Because of the documentary method and the et cetera clause we proposed that ethmeth is a kind of interpretive sociology. We presented a detailed methodological critique of ethsem from ethmeth. Ethsem fails in relation to ethmeth just as "grammatical" sociology fails in relation to interpretive sociology - they neglect the interpretational foundations of their grammatical rules. A number of conclusions can be drawn - (1) about scientific sociology, (2) about sociolinguistic (semantic) theory, and (3) about both in terms of the immediate subject of this work, ethnosemantics.
(1) As has been said before (Sacks, 1963; Zimmerman and Pollner, 1970; Wilson, 1972; Elliot, 1974), scientific explanation and description has no "logical" or in-principle superiority over commonsense explanation and description. Our model of an instruction is a formula for the operation of commonsense. By showing that instructions underlie constitutive rules which underlie regularities, we hope to have shown, admittedly indirectly, that commonsense work is foundational for the practice of science, specifically scientific sociology. This is the "point" referred to at the end of the section on Sociology 1000 in Chapter Two.

(2) While it is true that "Linguistic forms, whether morphemes or larger constructions, are not tied to unique chunks of semantic reference like baggage tags" (Frake, 1962: 77), it will not do, we contend, to say that

...it is the use of speech, the selection of one statement over another in a particular sociolinguistic context, that points to the category boundaries on a culture's cognitive map (Frake, 1962: 77).

That is, it will not do to say that and mean by it that such selection is domain-governed. As we saw in Chapter Four domains do not solve the problem of context. Furthermore, it is no improvement to partition use according to sociolinguistic context (situation, speech community, class, dialect, style, register, code, channel, etc.). The same problem which bedevils domain dogs all such "pragmatic" factors. As we have argued throughout, an adequate sociological pragmatics needs be interpretive.

(3) Both these points are present in the critique of ethsem in Chapter Four. The nub of this critique is that ethsem leaves out of
account the interpretive work of societal members, including that of its own practitioners. In the light of ethmeth, ethsem is another case of "constructive analysis" (Garfinkel and Sacks, 1970: 340). Its internal problems of abstracting from pragmatics and of context (except in Tyler's radical sense) reduce to one of substituting objective for indexical expressions. In Garfinkel's terms, seeking such substitutability provides constructive analysis with its infinite task (Garfinkel and Sacks, 1970: 339, 349). Its achievement of orderly results is shot through with "practical sociological reasoning"; that is, in common with all conventional social-science data extraction from speech (Cicourel, 1967: 119), it relies on common-sense methods of making sense for accomplishing itself as rational. It relies on the very competence which it is seeking to discover and describe, but, unlike ethnomethodology, does not make that resource a topic (Turner, 1970a: 117; Zimmerman and Pollner, 1970). Rather than standing over against the world for which they are said to account, its results are further elaboration of that world, a production rooted in that world. As such ethnosemantics becomes another case for ethnomethodological investigation - investigation aimed at "discovering whatever it is one has to know...."

Such an investigation is the subject of Part Two.
PART TWO

DATA:

USING THE SAME MATERIALS, AN ETHNOSEMANTIC
STUDY, AND AN ETHNOMETHODOLOGICAL STUDY,
OF CULTURAL COMPETENCE
CHAPTER SIX
INTRODUCTION TO PART TWO

To give body to the argument of Part One, two empirical studies done by the author are presented. The first study is a conventional eth­sem investigation, the second an ethmeth inquiry into the first. The study by Black and Metzger (1965) of American lawyer terms provided the model for our ethsem study - on terms for Canadian doctors. The ethmeth literature provided many examples of the analysis of conversational materials (Douglas, 1970; Sudnow, 1972; Turner, 1974) - we rely mostly on Cicourel's (forthcoming) study of the taking of medical histories by doctors. The strategy of turning a conventional study on its head has been used before - in dissertations by Wieder (1975 [1969]) and Crowle (1971). Mention of them will help place this part of the present work in the context of a relevant literature.

Wieder conducted a "participant-observation" study of a halfway house. He formulated it as

...embarking on a traditional ethnography of a normative culture and then turning...attention to the production of that ethnography as an accomplishment (1975: [ms.] 20).

On the basis of lengthy observation and formal and informal interviews with staff and residents, he "discovered" a set of maxims - the "convict code". At the level of a traditional ethnography the code was his results. It consisted of rules that any member to that setting would need to know
to act appropriately in the setting. That is, the code was a grammar.

At this level the study is an example of Sociology 2000.

However, by "stepping back" and looking now at his own and others' formulating of, invoking of, and appeal to, the code as an interpretive device, he came to see that

What sociologists describe as the convict code in their writings is one further instance of the product which results from the practices of 'telling the code'. Such accounts have the same logical status that 'telling the code' has in the very settings in which the code is told....

Thus, 'telling the code', and any particular instance of formulating the code, exhibits, rather than describes or explains, the order that members achieve through their practices of showing and telling each other that particular encountered features are typical, regular, orderly, coherent, motivated out of considerations of normative constraint, and the like (Wieder, 1975: [ms.] 235-236, emphasis added).

More simply,

Instead of 'predicting' behaviour, [a code] rule is actually employed as an interpretive device...[but is] experienced as predictive (Wieder, 1975: [ms.] 202-203, emphasis added).

This looks back to the conclusion of Part One and forward to the conclusion of Part Two.

In contrast to Wieder's use of the ethnographic method, Crowle's work focused on the experimental method - specifically, the post-experimental interview. He conducted a series of conventional experiments and tape-recorded the post-experimental interviews. At one level these procedures gave conventional results on the social-psychological topic in question - the effects of evaluation apprehension and commitment on confession of prior information by fully informed subjects. In these terms the study is a case of Sociology 1000.
However, he then reviewed the (transcribed) interviews as social interactions in their own right. He found that, in order to maintain the sense of the questions, interviewers routinely deviated from the "standardized" interview script.

It seems safe to conclude that standardization...of the interview could only be achieved by violations of some of the basic rules of social interaction [for example, cutting off an 'interested speaker'] (Crowle, 1971: 40).

He concludes further,

Thus in doing our experiment we relied on various implicit, unexplicated and intuitive abilities of the participants - they were resources of the experiment, in the same way as the laboratory and the stimulus materials were resources - the experiment would not have worked without them (Crowle, 1971: 57).

Our study resembles Wieder's study in being an ethnography. It resembles Crowle's study by focusing on interviews. It resembles both by having two parts, where the second part is a re-analysis of the first part. Chapter Seven describes the ethsem study and offers its conventional results - an incipient grammar of the domain of doctors' terms. Chapter Eight then details the interpretive work by which those results were achieved in the course of the interviews and analysis which generated them. Chapter Nine concludes.
CHAPTER SEVEN

TERMS FOR CANADIAN DOCTORS - ETHNOSEMANTICS

After a brief description of the Black and Metzger study of American lawyer terms (the Lawyers Study), we shall present the methods and results of our study of doctors' terms (the Doctors Study).

The Lawyers Study

Six hours of interviews with one informant, a law student, produced a chart of thirty-six lawyer terms each defined by a series of values on three major dimensions. For example, Table I reproduces one line of Table I of the Lawyers Study - the chart of lawyer terms.

TABLE I

ONE LINE FROM THE CHART OF REFERENCE TERMS FOR LAWYERS

<table>
<thead>
<tr>
<th>TERM</th>
<th>KIND OF TERM</th>
<th>SETTINGS</th>
<th>KINDS OF PRACTICE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Defense Lawyer</td>
<td>1.1</td>
<td>2.1 2.2</td>
<td>3.1 3.2 3.3 3.4 3.5</td>
</tr>
<tr>
<td></td>
<td>B</td>
<td>BC AB</td>
<td>AcB Ab A Aa C</td>
</tr>
</tbody>
</table>

When translated from the code of dimensions and their values which accompanies the table, the letters in the table mean that

When a lawyer (law student) calls a person a 'defense lawyer', he is using a term
- that is descriptive of the man's practice (B on 1.1);
- that is not used on court proceedings, but may be said about a lawyer on a case, and also where no particular case is involved (B and C, on 2.1);
- that is used when the speaker is talking to other lawyers; also when he is talking to nonlawyers (A and B, on 2.2);

The referent of the term:
- (3.1) may be specializing in criminal work (B), or else in the area of civil practice called 'insurance law' (Ac);
- (3.2) does a lot of trial work, representing defendants (Ab);
- (3.3) has clients who retain his services individually (A);
- (3.4) appears in trial court to do his work (Aa);
- (3.5) has an independent practice (C) (Black and Metzger, 1965: 161).

Such an account was not, however, to be taken as final results.

The chart represents a stage in the eliciting-analysis-validation process. It is neither the complete corpus of frames, terms, and responses by which the information was obtained, nor a final elegant analysis of minimal differences in criteria governing selection of lawyer reference terms. It is simply a working device constructed by the ethnographer in the field at a point in the eliciting where a systematic validation of data was desired (Black and Metzger, 1965: 156).

(The Doctors Study was carried to a similar stage.)

The process of eliciting-analysis-validation consisted of (1) learning appropriate native-language questions (for example, "what kinds of legal practice are there?"); (2) presenting these question frames to the informant, systematically substituting the lawyer terms in the frame (for example, "Does the Attorney General press litigation?", "Does the defense lawyer press litigation?" "Does the [X]...?"), and (3) determining the minimal set of questions that will discriminate all the terms.
The Doctors Study was essentially a replication of the Lawyers Study as described here.

The Doctors Study

Eliciting

Over a period of a year and a half the author made several visits to the Paediatrics Department of the Faculty of Medicine of a large Western Canadian university. The department is situated in a large city hospital. There he conducted formal interviews with three informants, and informal interviews with two informants, one informant being in both groups. The seven or eight hours of talk were tape-recorded, and the bulk of them transcribed. The informants were doctors. All but one were paediatric specialists, the one being a resident. As a partial test a further interview was recorded much later in the home of a general practitioner.

The first informal interview was directed at discovering relevant questions and ascertaining the rough boundaries of the domain. A second, formal interview with the same informant furnished a fairly definite collection of terms and some possible dimensions on which they varied. Further formal interviews with different informants were done to check stability of responses, to encounter possible variation, and to enlarge the corpus.

The main eliciting device was the question-frame, "What kinds of [X] are there?". Responses became the terms in new frames. In this way,
following the formal eliciting method, an attempt was made to exhaust the taxonomic inclusion relations among the collection. "What is this a division according to?" was one frame used to elicit dimensions of difference. As with the Lawyers Study, the eliciting, validation and analysis occurred as a continuous process. The results obtained are displayed in the following section.

Results

Table II gives the principal (ethnosemantic) results of the Doctors Study, and is modelled on Table I of the Lawyers Study. Table III is the key to the symbols in Table II, the Lawyers Study again providing the model.

With two sets of exceptions the dimensions and values given in Table III are sufficient to uniquely discriminate all the terms in Table II. "Gastroenterologist", for example, is distinguished from "haematologist" by a different value on the dimension "system of the body"; the first of the two terms indicates a specialty in the intestinal tract, the second in blood. The first set of exceptions is the group of "training" terms - resident, intern, etc. Details are not given in the tables but these terms vary according to the stage reached in training towards a specialty; they are marked by years spent and exams passed. Synonyms form the second set of exceptions. "Doctor" and "physician 1" appear to be synonymous within the medical domain, though certain practitioners on the margins of the medical profession - optometrists, chiropractors, osteopaths - seem to warrant that label by some. "Nephrologist" and "renologist" are synonymous, as are "general practitioner" and "family physician".
<table>
<thead>
<tr>
<th>1</th>
<th>KIND OF TERM</th>
<th>2</th>
<th>KIND OF WORK</th>
<th>3</th>
<th>BASIS OF SPECIALTY</th>
</tr>
</thead>
<tbody>
<tr>
<td>physician 1.</td>
<td>A</td>
<td>O</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>doctor</td>
<td>A</td>
<td>O</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>physician 2.</td>
<td>Bc</td>
<td>B</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>surgeon</td>
<td>Bc</td>
<td>A</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>general practitioner</td>
<td>Bb</td>
<td>B or O</td>
<td>Ga</td>
<td></td>
<td></td>
</tr>
<tr>
<td>family physician</td>
<td>Bb</td>
<td>B or O</td>
<td>Ga</td>
<td></td>
<td></td>
</tr>
<tr>
<td>public health doctor</td>
<td>C</td>
<td>B</td>
<td>Gb</td>
<td></td>
<td></td>
</tr>
<tr>
<td>psychiatrist</td>
<td>C</td>
<td>B</td>
<td>D</td>
<td></td>
<td></td>
</tr>
<tr>
<td>neonatologist</td>
<td>C</td>
<td>B</td>
<td>Bb</td>
<td></td>
<td></td>
</tr>
<tr>
<td>perinatologist</td>
<td>C</td>
<td>B</td>
<td>Ba</td>
<td></td>
<td></td>
</tr>
<tr>
<td>paediatrician</td>
<td>C</td>
<td>B</td>
<td>Bc</td>
<td></td>
<td></td>
</tr>
<tr>
<td>internist</td>
<td>C</td>
<td>B</td>
<td>Bd</td>
<td></td>
<td></td>
</tr>
<tr>
<td>geriatrician</td>
<td>C</td>
<td>B</td>
<td>Be</td>
<td></td>
<td></td>
</tr>
<tr>
<td>cardiologist</td>
<td>C</td>
<td>B</td>
<td>Aa</td>
<td></td>
<td></td>
</tr>
<tr>
<td>dermatologist</td>
<td>C</td>
<td>B</td>
<td>Ah</td>
<td></td>
<td></td>
</tr>
<tr>
<td>gastroenterologist</td>
<td>C</td>
<td>B</td>
<td>Ai</td>
<td></td>
<td></td>
</tr>
<tr>
<td>endocrinologist</td>
<td>C</td>
<td>B</td>
<td>Aj</td>
<td></td>
<td></td>
</tr>
<tr>
<td>nephrologist</td>
<td>C</td>
<td>B</td>
<td>Ak</td>
<td></td>
<td></td>
</tr>
<tr>
<td>renologist</td>
<td>C</td>
<td>B</td>
<td>Ak</td>
<td></td>
<td></td>
</tr>
<tr>
<td>neurologist</td>
<td>C</td>
<td>B</td>
<td>Ac</td>
<td></td>
<td></td>
</tr>
<tr>
<td>physiatrist</td>
<td>C</td>
<td>B</td>
<td>Ae</td>
<td></td>
<td></td>
</tr>
<tr>
<td>rheumatologist</td>
<td>C</td>
<td>B</td>
<td>?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>respirologist</td>
<td>C</td>
<td>B</td>
<td>Ab</td>
<td></td>
<td></td>
</tr>
<tr>
<td>haematologist</td>
<td>C</td>
<td>B</td>
<td>Am</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Continued . . . .
### TABLE II (Continued)

**CHART OF REFERENCE TERMS FOR CANADIAN DOCTORS**

<table>
<thead>
<tr>
<th>KIND OF TERM</th>
<th>KIND OF WORK</th>
<th>BASIS OF SPECIALTY</th>
</tr>
</thead>
<tbody>
<tr>
<td>anaesthetist</td>
<td>C</td>
<td>B</td>
</tr>
<tr>
<td>radiologist</td>
<td>C</td>
<td>B</td>
</tr>
<tr>
<td>pathologist</td>
<td>C</td>
<td>B</td>
</tr>
<tr>
<td>obstetrician</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>gynaecologist</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>ophthalmologist</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>urologist</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>otolaryngologist</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>cardiovascular surgeon</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>thoracic surgeon</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>neurosurgeon</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>orthopaedic surgeon</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>plastic surgeon</td>
<td>C</td>
<td>A</td>
</tr>
<tr>
<td>intern</td>
<td>Ba</td>
<td>0</td>
</tr>
<tr>
<td>resident</td>
<td>Ba</td>
<td>0</td>
</tr>
<tr>
<td>fellow</td>
<td>Ba</td>
<td>0</td>
</tr>
<tr>
<td>certificate</td>
<td>Ba</td>
<td>0</td>
</tr>
</tbody>
</table>

"0" Not relevant
TABLE III

CODE OF SEMANTIC DIMENSIONS

1. KIND OF TERM

A general term descriptive of all persons who have passed their medical exams

B descriptive of "position in the profession" or "level of practice"
   a in training
   b in general practice
   c specialist

C descriptive of specialty

2. KIND OF WORK

A "operates"

B does not "operate"

3. BASIS OF SPECIALTY

A system of the body
   a heart    b chest    c brain    d eyes
   e bones    f ear, nose and throat   g urinary tract
   h external parts   i intestinal tract
   j hormone system   k kidneys   m blood

B age
   a foetus and new borns   b new borns
   c children   d adults   e the old

C sex (women)

D mental/physical dualism (mental)

E technology
   a anaesthetizing   b X-rays

F live/dead tissue (dead)

G private/public health
   a private   b public
The last two provide an interesting case of the "creativity" or "productivity" (Frake, 1962: 78) within this system of terms. "Physician 2" is a cover term for all those doctors in medical rather than surgical specialties. The movement within the profession to make general or family practice into a medical specialty is marked by the coinage of the new term, "family physician", "physician" being the term indicating the desired status. Why, when public health became a specialty, the term "public health physician" did not arise requires a different explanation. That public health does not enjoy the social status of the other medical specialties is no doubt related to its practitioners being known as "public health doctors".

More generally, further terms can be created in at least three ways. (1) Combining specialty names produces such forms as "paediatric cardiologist" and "haematological pathologist". (2) A specialty name can be combined with one of a number of more general terms:

- general: general paediatrician, general surgeon, general pathologist (cf. general hospital)
- primary care: primary care paediatrician
- anatomical: anatomical pathologist
- ambulatory: ambulatory paediatrician
- adolescent: adolescent paediatrician
- diagnostic: diagnostic radiologist
- therapeutic: therapeutic radiologist

(3) Particularly in the field of research (laboratory medicine) specialties are spawned by combining "medical" with the name of the relevant science -
"medical biochemist", "medical microbiologist", "medical geneticist"....

Though immunology is a science, "clinical immunology" is yet a clinical medical specialty rather than a laboratory medical specialty. We have not drawn on this distinction in specifying the dimensions of Tables II and III; our goal there was merely to discriminate all the terms (while preserving the emic [Pike, 1967] distinctions). However, we have included it in the taxonomy shown as Figure 2.

Many of the terms have a hierarchical relationship that is usefully presented in the form of a taxonomic diagram. Several points about Figure 2 require comment. (1) There is the problem of establishing "lexemes". Taken strictly, Conklin's criterion for identifying a lexeme—that "its meaning cannot be deduced from its grammatical structure" (Conklin, 1962: 121) — would mean that only "doctor", "physician", "surgeon" and the training terms are true lexemes (suggestive though that may be). The meaning of terms like "cardiologist", for example, is predictable from a knowledge of (the meaning of?) the morphemes "cardio", "logo", and "ist". However, as Frake points out, a form such as [cardiologist] "is a standard segregate label whose function in naming cannot be distinguished from that of forms like [surgeon]" (1962: 78). For this reason we let the specialty names displayed in Figure 2 stand as lexemes.

(2) However, not all taxa, or slots in the taxonomy, are labelled with a lexeme (even so broadly defined). Non-lexemic terms are identified on the diagram by quotation marks. For example, though its labels are non-lexemic, or semantically endocentric (Conklin, 1962: 121, 132), the
FIGURE 2
PARTIAL TAXONOMY OF TERMS FOR CANADIAN DOCTORS

(Medical) Doctor/Physician

"Specialist"

Physician

"Lab. Phys."

"Clinical Phys."

Surgeon

(Surgeon?)

GP General Practitioner  Pa Pathologist  PH Public Health Doctor  Ps Psychiatrist
Nn Neonatologist  Pe Perinatologist  P Paediatrician  I Internist  Ge Geriatrician
C Cardiologist  D Dermatologist  G Gastroenterologist  E Endocrinologist  Np Nephrologist
N Neurologist  Ph Physiatrist  O Obstetrician  Gy Gynaecologist  Ot Otolaryngologist
U Urologist  Op Ophthalmologist  R Resident  In Interne
distinction between "laboratory physician" and "clinical physician" is important in that it is drawn by the Royal College of Physicians and Surgeons of Canada with respect to the examination of candidates; it also reflects a difference of emphasis as between "the care of patients" (clinical specialties) and the "study of disease per se" (laboratory specialties). Whether "specialist" is lexemic or not is both moot and of little import for this work.

(3) In common with taxonomies from other domains (Conklin, 1962: 132) the medical taxonomy has terminal taxa (that is, those at the lowest level of the taxonomy) which are lexemic, plus a host of non-lexemically labelled categories below the level of the terminal taxa. The names for these are formed in the combinatorial ways already described.

(4) Not all taxa are uniquely labelled. The domain of medical doctors has the familiar problem of the same term occurring at different levels of the taxonomic hierarchy. "Physician" is both a permissible cover term for the domain, contrasting with the names of other professional workers (lawyer, teacher...), and a class of specialist, contrasting with "surgeon".

(5) Some terms contrast at different levels. "Surgeon", for example, contrasts with "physician" at its own level, and with "resident" at the terminal level. The different kinds of contrast within a taxonomic hierarchy have been ably discussed by Kay (1966b; 1971).

(6) While the discriminations portrayed in this semantic arrangement are ones attested to by informants' responses, this particular formula-
tion is the author's. It is not clear to what extent any single informant "knows" the domain quite in this way. The question is thus raised of the psychological reality or cognitive validity of such an analysis. This is an abiding concern both of (ethno-)semantics (for example, Wallace, 1965) and of linguistics (for example, on Chomsky, Hockett, 1968: 42, and Lyons, 1970: 24-25). It is an issue of importance to the argument of this work, but not in the form addressed by these writers; we take it up in the next chapter.

One seemingly important division is not shown on the taxonomy. It is the tripartite classification - internist/paediatrician/general surgeon. This was put forward by two informants. In an ethnomodel, or informant diagram (Conklin, 1964) drawn by one of them, these three categories occupied a "central position between" general practitioners and more specialized specialties. By "between" is meant that in the informant's scheme of things the domain of medicine was organized in the form of a "treatment route" - from g.p.s (supported by para-medics) to specialists (supported by laboratory and sub-specialties). The "general specialties" - internal medicine, paediatrics, general surgery - occupy the middle region, supposedly receiving patients from g.p.s and dispatching them, where necessary, to more specialized doctors. Any clinical-medical specialist could, without contradiction, also call him/herself an internist (if dealing mostly with adults) or a paediatric [X] (if dealing mostly with children). Unlike the "true" or "sub-" specialists, the internist, general paediatrician and general surgeon practice in most or all systems of the body.
This division may attach only to hospital settings and to those who practice there. The g.p. who served as informant did not reproduce this classification, either spontaneously or when asked. In addition, there is the specialty, recognised by the Royal College (1973) but not offered by informants, of general pathology. It includes all the divisions of laboratory medicine, and thereby would seem to share the status of the three terms already discussed.

It should be stressed again that, as with the Lawyers Study, the results presented are not the "final results". Not all terms in the corpus are presented (for example, "syphilologist", "oncologist"). The set of dimensions and values is not the most parsimonious that could be devised, nor the most elaborate - the system-of-the-body components listed in Table III are merely glossed rather than given full extensions. No attempt has been made to specify "core" terms and generative rules for predicting the other terms. We would concur with Scheffler and Lounsbury that analyses such as those given here are "little more than simple ethnographic statements" (1971: 143).

Nevertheless, as with the Lawyers Study, the results are a "version" of final results, being in our case adequate for our purposes. The question remains, of course, whether or not results of any kind, final or otherwise, ever avoid being always and only "adequate for some purpose". This issue will be treated in the next chapter.
CHAPTER EIGHT

TERMS FOR CANADIAN DOCTORS - ETHNOMETHODOLOGY

The great bulk of sociological and anthropological data is gained through talk. These disciplines may be unique in knowing next to nothing about this their principal research method. The interview is the standard form of the talking method. Ethnosemantics is known for that interview method called formal eliciting. Chapter Four contained a critique of formal eliciting, a critique done at one remove from data. The present chapter revisits that critique, only now in terms of data.

The previous chapter presented results based on data gained from interviews in which an attempt was made to use formal eliciting. The interview talk was seen and used as a data-generating device. In contrast the present chapter considers that talk as a conversation. Given certain characteristics of conversation we raise questions about the status of the ethnosemantic results of the previous chapter.

Given the mind-bending nature of conversational and "indexical" analysis, what we offer here is a series of remarks rather than a finished product: "our work is now both too empirical to 'follow from' a theory of society and too young to propose one" (Moerman, 1972: 198).

At least two kinds of critical material can be found in the interview transcripts. (1) Reading "on the surface", as an ethnosemanticist might do, we can find numerous substantive "variations", "inconsistencies" and "doubtful data". These items can be treated in two ways: (a) in terms
of them we could engage the ethsemist in a debate directed at the repair of the original analysis; (b) we could, however, see those "problems" as pointing to the impossibility of (final) repair of that analysis. We shall try to illustrate this distinction in what follows.

(2) Reading "below the surface" we can discover another order of data to do with the structure of talk itself. At this level what appears on the surface as mere variation receives some motivation. We shall try to illustrate this also.

Some Data

Following ethnosemantic precedent I sought first to establish the existence of a domain, to discover a meaningful and productive question about the domain, and to use such a question to generate a list of terms with which to begin eliciting proper. The Appendix contains a transcript of the first part of the first interview. In discussing the transcripts I shall be making implicit comparison with the eliciting displayed in Black and Metzger (1965: 147-153), and with the hypothetical exchanges given in Frake (1962) and Tyler (1969c: 12).

"Before We Start"

Notice that, in keeping with all ethnosemantic discussion, the beginning of the interview is absent. The work of introduction, arranging of seating, plugging-in of recorder, mutual sizing-up of ethnographer and informant — all this goes, literally, without saying. The same is true of
the work that produced the occasion prior to its happening - the phone-calls, explanations, date-settings. Here is part of my first field note.

We walked over to King's office where I was introduced. Boxer left. "Explained" my status and the project to Tom King. He wanted to know what I would do with the information. I talked about "semantic space" and assured him that no confidential information was involved. In using the term "names" I was heard as meaning personal names. Corrected that. He phoned Doug Race in the I.C.N., told him I was a graduate student doing a paper or something, in "language-semantics", and he used the term "statistics" to explain the nature of the results. Then he told me about Doug's rounds and said I could see Doug at 2:00 p.m. that afternoon.... Got to I.C.N. at 2:10 p.m.... Race doing rounds. By 2:25 p.m. he had finished, we had found a room, plugged in tape recorder, switched on light, found out how to make recorder work, and were ready to go. I explained who I was (grad. student, dissertation) and what I wanted: that I was interested in the "world of medicine" and wanted to get into that by way of the names for the different kinds of what Tom King calls "specialties".

The point of this is that the interview between ethnographer and informant does not take place in a social vacuum. Interactional work by the ethnographer is necessary to gain access to the interview setting. Such work throws into relief the already organized scenes of the lives of the informants. The ethnographer is constrained to provide an account of his presence, identity and proposed activity. Not only is the ethnographic exercise conducted from within the society (Turner, 1970a: 177), but the society limits what can be discovered about it.

In the case of a (social-scientific) interview, the ethnographer's account of himself to the informant must include some version of what the interview is to be about. One inevitably, as an interactional necessity, structures the field prior to investigating it. The ethnographer's desire to avoid imposing an alien structure on the native domain remains an ideal
only. Not that it is ever finally clear what the interview is or was "all about". Rather ethnographer and informant rely on each other to talk against a background of some version of "what it's all about". "What it's all about" is an unspoken resource of the encounter. This reappears below.

4

Lists

There is a tension in the ethnosemantic field situation between avoiding predetermining the domain, and instructing the informant how to answer. Both are recommended (Black, 1969). In the last section of Chapter Four we drew on Garfinkel's remarks about how it is

...necessary to instruct the construing member to act in accordance with the investigator's instructions in order to guarantee that the investigator will be able to study their usages as instances of the usages the investigator has in mind (Garfinkel, 1967a [1964]: 70).

I encountered this phenomenon in trying to cope with the informant's initial response to my opening question (Appendix, (1)):

(2) I: Yeah, I guess so.

I read this as an invitation for a clarification. Now how is clarifying to be done other than by further specifying what I want to find out? As ethnosemantist I know that, operationally, I have found a domain if the informant can produce a list in response to a "What kinds of [x] are there?" question. And, presumably, that is an issue that one should be able to settle independently of mentioning that a list is what is wanted; for, after all, the informant can always fail to produce a list.
But what does the production of a list tell? That the informant is unpacking a cognitive arrangement in his head which requires only the right question for its elicitation? Or is it rather that the informant is exploiting his own "methodological" ability to "do" a list, since that is what I said I wanted?

Compare a shopping list. Would it be correct to infer that, because a member can construct a shopping list, the items on the list are mentally stored in some fashion corresponding to the structure of the list?

I propose, rather, that lists are always and only produced for some purpose, their structure speaking to the occasion for which they are produced, at the same time as constituting that occasion. Not knowing quite what I want, the informant exploits his list-making capacity to produce a candidate answer-to-my-question. In so doing he provides for himself a device for generating more listables - "doctors connected with" (12). He shows he can do a list:

...subjects, in complying with the investigator's demands and answering his questions, may be doing no more, in effect, then demonstrating their agreement (with the investigator) that such questions and operations are answerable and/or permissible, as the case may be (Epling, 1967: 261).

Formulating and Emergent Meaning

In abstracting from utterance (12) such labels as "public health", "psychiatry" and "surgery", I (as ethsemist) am not only ignoring the "control" of those items by the phrase "doctors connected with", but am failing to appreciate their status as not simply "precoded entries on a memory drum" but as elements in a formulation of the field - a formulation rounded off
by the concluding words, "primarily when you say the different kinds of physicians, that's primarily what it means". What one finds in the transcripts - both in this case and in others to be presented - is that both parties are engaged in what we shall call "formulating-and-waiting". This is in contrast to what is presupposed of respondents by both traditional-sociological and ethnosemantic interviewing practices:

The traditional view of interviewing provides for a logic of questions and answers that standardizes the output....The format is seen as an obvious way to elicit stored information. How stored information is organized and how access is to be made is not defined as a serious problem. The researcher assumes the respondent will be presented with 'normal' speaking intonation, standardized syntactic structures, and standardized topics as indexed by the same lexical items....

The organization of stored experiences, however, may require different formats and subroutines for their elicitation. The respondent's monitoring of his or her own output and the interviewer's reactions, provides a feedback that can trigger off other items of stored information that a standardized question can block. Participants usually begin an interview with vague conceptions of what is going to happen. They begin to assume common meanings that emerge implicitly and explicitly over the course of the interview. These emergent meanings provide an implicit working background that can help clarify the participants' questions and answers. This negotiated clarification process occurs in all interviewing....But these negotiated exchanges do not become part of the data base used for making inferences reflected in the findings (Cicourel, forthcoming: [ms.] 3-4, emphasis added).

What the list will come to include develops over the course of the interview. After the first formulation (utterance (12)) comes a second based on paediatrician/surgeon/internist ((20)-(26)). What can be made of these is subject to reinterpretation in the light of the surprise elicited by my suggesting he be as exhaustive as he can:
At this point he introduces the term "specialist" together with a systems-of-the-body division as a device to organize the field:

(66) I: ...so that every system of the body you can think of uh you could find a specialist....

Later still,

(120) I: ...I I guess really you can't exclude I don't know how vast you want to go uh there's uh many doctors who many M.D.s who deal exclusively with research. I guess you'd have to put them in there I'm sure there's more....

Throughout I am responding with "uhuh", and in that powerful way helping to produce what I came to formulate as a list—a list that became my "data", data for which the semantic arrangements proposed in Chapter Seven are the analysis.

Validation and Variation

In what we might call the "ethnosemantic field practice ideology", a first interview is properly seen as an exploratory tapping of the rough outlines of the domain. Controlled eliciting is a feature of subsequent interviews in which the ethnographer probes deeper. Interviews with different informants then provide validation and variation.

At best this account obscures at least the following features of such interview occasions. Only for the ethnographer is any subsequent interview a "second" to a previous "first", or a "third" to a previous "second", and so
on. For each different informant the encounter with the ethnographer is for him or her a "new" occasion. It may turn out to be a "sole" encounter - not a "first" since there is no "second" for it to be a "first" to. Only for the ethnographer is a difference a "variation" from the norm, or a term heard twice a "confirming" instance. Rather than being a method for discovering shared knowledge, ethsem must presuppose shared knowledge in order to find it.

One tack that can be taken in a critique of ethsem is to cite cases which do not fit the proposed semantic arrangement. So, for example, I can produce from my data the label "ophthalmologist" and the following utterances:

E: Would you say an ophthalmologist was a kind of physician?
I: (4) An ophthalmologist might be more of a physician or more of a surgeon but basically he would be more of a physician.

According to the Royal College, however, ophthalmology is a surgical specialty. (Notice how I cite the Royal College as an authority - just as a member would! This is taken up below.) Also, I can cite the "diagnostic procedures" that a neonatologist performs that can be glossed as "minor surgery" - though being physicians they are not supposed to operate. Similarly, general practitioners will perform caesarian sections if no obstetrician is available. A public health doctor may be a (licensed) general practitioner; a hospital resident may be a g.p., though it is not allowed; and so on.

To argue thus, however, is to engage the semantic ethnographer on his or her own ground. The ideological response is to improve the analysis by bringing the "variation" into the model. This can be done under the auspices of such statements as
Variants are not mere deviations from some assumed basic organization; with their rules of occurrence they are the organization (Tyler, 1969c: 5).

Variation that cannot be accommodated in this way can be accounted for in terms of a domain's "fuzzy boundaries" or in terms of "probabilistic considerations", and the like. The ultimate weapon is to invoke the competence/performance distinction and treat "performance" as a residual category or wastebasket for unexplained variation.

Analysis carried out under these assumptions assimilates "appropriate" use of a term to (semantically) "correct" use. What the work of conversational analysts has shown, and which is in itself a commonplace observation, is that the "correctness" of a use is not a necessary, or for many purposes a sanctionable, criterion of appropriate use (Moerman, 1972: 199).

(1) A: Do you want a coffee?
   B: No, I've just eaten.

(2) (Sign advertising book sale)

| BOOKS AND PAPERBACKS |

These naturally occurring events were "noticed" by me under a viewing rule derived from ethsem. They clearly contradict simple taxonomic relations that one might propose for the domains of "eating (?)" and "books". Yet no sooner is one confronted with them than one is elaborating the sense of the items and the occasions in which they (might have) occurred in order to render those uses plausible (cf. Fillmore, 1973: 285 fn. 3). This activity is members' work. Members rely on each other to find a rule (instruction -
Chapter Two) with which to "see" the items as "rule-governed", "orderly", "regular", and thereby ordinary and unnoticeable (Wieder, 1970: 134).

While, under some supposed "objective" standard, I could cull from my interview data contradictory uses of terms, in doing so I would be failing to see that for both informant and ethnographer such uses raised no problems of interpretation. It is not that "correctness" is never important, but that its being important for some purposes and not for others is precisely what is true about it. "Correctness" is always "correctness-for-all-practical-purposes".

It is essential to realize that 'true' and 'false', like 'free' and 'unfree', do not stand for anything simple at all; but only for a general dimension of being a right or proper thing to say as opposed to a wrong thing, in these circumstances, to this audience, for these purposes and with these intentions (Austin, 1962: 144).8

For many practical purposes correctness is not only not important, but to insist on it is to be seen as incompetent. For other purposes the issue matters. Thus,

The American Medical Association recently has been engaged in a running argument with the Department of Commerce to decide whether the practice of medicine is a trade or a profession (Bram, 1955: 46).

In a letter to a newspaper concerning labour relations and withdrawal of services by housemen (residents and interns) in the hospitals of the province, an intern writes,

'Interns are not doctors but students working to become doctors'. This statement is a misrepresentation of the facts. Mr. Brown has misled the media and the public by failing to discriminate between the terms doctor (MD) and licensed physician. As the
registrar of the College of Physicians of [Province X] can confirm, all interns in [Province X] are doctors having graduated from approved universities.

Is an osteopath a doctor? "Yes", says one informant. "No", says the Royal College of Physicians and Surgeons of Canada. Compare the 1974 Canada Income Tax Filing Guide, paragraph 49, Medical Expenses:

The following are the types of expenses you may claim: (a) payments to a hospital or qualified medical practitioner, dentist or nurse (the expression 'medical practitioner' includes a qualified chiropractor, Christian Science practitioner, naturopath, optometrist, osteopath, podiatrist or therapist); (b).... (19).

The point of these examples is not that any position is (going to be in the end) the one-and-only right one. The point is that here we have members defining terms and appealing to reasonable grounds to support their definitions because it matters. It is not that another member is not entitled to formulate another definition and invoke reasonable grounds for its adoption. Rather, we are saying that what one can find in the world are members doing ethnographies for each other (Garfinkel, 1967b: 10) for some practical purpose. For the intern, the Royal College and Revenue Canada, who gets to be called "doctor" is at some time, on some occasions, and for some purposes an important and consequential matter.

What medical informants supply for semantic ethnographers, and with the production of which the ethnographer collaborates, are ethnographies for the disinterested inquirer. They are "off-the-top-of-my-head", "offhand", "I've-never-really-done-this-before", "we-don't-know-often-among-ourselves-what-we-mean-by-the-designations-of-such-and-such", "that's-really-of-value-to-you-is-it?" ethnographies. It is clear that what is
good enough for a social-scientific interview is not necessarily adequate for deciding employment status and wage scales. What will pass muster in a half-hour (interview-) conversation between rounds will not satisfy the Royal College. Indeed it is the latter's specific business to stipulate what counts as a kind of (medical)-X. And because it is known to be its business it is not necessarily the (important) business of anybody else. Their business can and does go on irrespective, in an important sense, of terminology.
1. This is not to say that social scientists do not know how to interview. On the contrary, they are (often) experts. It is that very knowledge which has provided for the large sociological literature on "the interview" - a literature bent largely on the improvement of the interview as a research tool (cf. Manning, 1967; for the "medical student" case see Becker, 1956). But what it is we know in knowing how to interview is largely unknown. For knowing how to interview derives from knowing how to talk. Only in the last decade or so has talk been studied systematically - by Sacks, Schegloff, Turner, Jefferson and others.

2. Though not the only talking method in ethsem, it is the most intentionally rigorous one. While notice must be taken of Frake's disclaimer - "Let me emphasize....that I do not believe an adequate ethnography can be produced from a record only of what people say, most especially it cannot be produced from a record only of what people say in artificial interviewing contexts removed from the scene of their ordinary cultural performances" (1964a: 133) - it must be said that it is not clear (1) how "anything more" would improve on the interview product, nor (2) how that "anything more" is itself done. See the section on indexicality in Chapter Four.

3. In talking about myself as interviewer it seems more natural to use "I" than the "we" used heretofore.

4. This section on lists has been richly informed by a conversation with Dr. W.W. Sharrock.

5. Here is a similar case from another interview.

(In a sequence of questions aimed at finding out the meaning of the specialty terms)

E: Obstetrician?

I: (5) uhm (4) women's diseases and the delivery of children mothers

E: Gynaecologist?

I: UhnI suppose (draws in breath quickly) you're going to split it like that then you'd call an obstetrician uhm one who would deliver babies and you'd call the gynaecologist a specialist in women's diseases.
6. Or perhaps an "interruption", "delay", "interesting interlude" or whatever.

7. Notice, further, that in abstracting categories from his utterances I, the naive ethnographer, am the one who is deciding what is a good answer and what can be taken from such an answer. Somewhat in reverse I depend, at the same time, on his understanding what I am about so that I can then treat his answers as naive, as "conditioned responses".


9. See the studies reported in Garfinkel (1967a [1964]).

10. By "called" we mean "referred to as", not (necessarily) "addressed as".
CHAPTER NINE

CONCLUSION TO PART TWO

To give substance to the argument of Part One we have presented in Part Two an ethnosemantic study and an ethnomethodological critique of that study. The critique was of ethsem's method of deriving data from talk. We suggested that terms functioned in talk in ways that make it impermissible to abstract them as lists under the assumption that the list, once ordered, represents a cognitive map of some sort. Rather it is more nearly true that that abstracting depends itself on the very cultural competence it is attempting to explicate. This leaves the question of what to make of the semantic arrangements, such as the chart and taxonomy in Chapter Seven, that form ethnosemantic results.

Simply put,

From the standpoint of ethnomethodology these apparently definitive descriptions appear as idealizations of what members are doing when they employ categories and criteria (Wieder, 1970: 134).

What one has to know or believe in order to operate in a manner acceptable to a society's members are just such methods as "idealizing", "abstracting", "formulating" and the like. This is not to say that terms do not have "stock uses" (from Ryle, quoted in Turner [1970a: 186]). Rather, it is to say that the choice of one term over another is not done from some definite collection, or from a collection organized in a semantically well-formed way. Such collections and their arrangements are, instead, the accomplished productions of skilled ethnographers such as medical doctors and other anthropologists.
While the specific topic of this work has been the practice, results, and assumptions about language characterizing one approach in anthropology known as ethnosemantics, we have hoped to speak also to sociology generally. Rather than attempt to summarize the whole dissertation let me conclude with a recommendation. It is this. Sociology must re-discover philosophy - not the crude positivism of certain brands of the philosophy of science, but the philosophy of language, and perhaps phenomenology. The reason is simply this: both the data (in huge part) and the theories of sociology are linguistic entities. We shall return once more to ethsem to elaborate the point, then leave the last word to Wittgenstein.

As a footnote to her uncompromising ethnosemantic analysis of the Ojibwa taxonomy of "living things", Black says she prefers

...to leave the arguments as to whether 'the world' exists in the data, in the ethnographer's descriptive analysis, or in the language used by one or the other, to the philosophers' offices and behavioural science graduate seminars, where such matters are of consequence. While these decisions may be basic to what an ethnographer does, the experience of confronting the 'raw' material and learning from it something you had not known before need not wait upon their resolution (1969: 187 fn. 4).

The last sentence quoted is unexceptionable. But given ethsem's theory of language and perception (Chapter Four), one may ask how it could provide for "learning...something you had not known before" (where now Black is the native). A conclusion of this work is that an adequate account of that process would needs invoke something like the documentary method of interpretation. This, as can be seen, reintroduces "philosophical" ques-
tions into the conduct of empirical work. Such questions cannot be left to the philosophers' offices and graduate seminars. Conversely, the later Wittgenstein's philosophy of language and the writings of Garfinkel may be characterized as contributions to an "ethnography of thinking", an enterprise that is both philosophical and empirical. This kind of philosophy is surely not something a mature science or scientist needs to be separated from. Rather the opposite.

Throughout I have been addressing an issue that is caught well in these words:

It is Wittgenstein's later doctrine that outside human thought and speech there are no independent, objective points of support, and meaning and necessity are preserved only in the linguistic practices which embody them. They are safe only because the practices gain a certain stability from rules. But even the rules do not provide a fixed point of reference, because they always allow divergent interpretations. What really gives the practices their stability is that we agree in our interpretations of the rules. We could say that this is fortunate, except that this would be like saying that it is fortunate that life on earth tolerates the earth's natural atmosphere. What we ought to say is that there is as much stability as there is.

This extreme anthropocentrism produces a strange effect on people. They feel that it goes too far, and that it ought to be possible to stop at some earlier point, as Wittgenstein himself had done in the Tractatus. But where? (Pears, 1971: 168).
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APPENDIX

TRANSCRIPT OF FIRST PART OF FIRST INTERVIEW

Compared with the standard of transcribing that is characteristic of published pieces of conversational analysis, the following is a relatively rough transcription. These are the transcribing conventions used:

E Ethnographer
I Informant
/
// Point at which interruption occurs
[ ] Point at which overlapping of current speaker's utterance by next speaker's occurs
[ ] Speakers talking simultaneously
[ ][uhuh] Interjection by other speaker
(Pause) Metaconversational instruction
(( )) Talk unintelligible to transcriber
_stress_ Stress
_gap_ Said deliberately, almost staccato-like

gap_ Gap between words indicates small pauses
(5) Number of seconds pause

(1) E: I'm not sure what you uhm call all the different kinds of what I call physicians. I don't know if like if I asked you uhm what are kinds of physicians would that be a sensible question?

(2) I: Yeah I guess so.

(3) E: I mean you could you produce a uh a list of a of answers to that?
(Pause) Or would/

(4) I: Yeah no I/

(5) E: would woulda nother term be you know

(6) I: yeah that's right that's a good question. [uhuh] I could propose an answer.
O.K. Well could we start there?

Sure.

O.K.

Uhm (Pause)/

I'm not going to write just

uhm uh now I say there's an answer and it's hard to say but I think there are different kinds of physicians uh physicians to me mean anybody who has gone through medical school [uhuh] and has got a degree in medicine [uhuh] and uh uh just applies to different fields in that certainly some connected with public health [uhuh] doctors connected with public health, doctors connected with psychiatry, doctors connected with surgery, uhm doctors connected with general practice, and uh specialists in the field uh who called consultants [uhuh] primarily when you say the different kinds of physicians that's primarily what it means.

Uhuh well uhm where do you fit into that yourself?

Well as you mean as resident?

Yeah o.k.

Uhm hahaha as a resident I don't know really where I fit into that in that uh it's part of a training program to become a certain kind of physician uh one that I you know choose to be uh

Uhuh

I don't know I couldn't I I myself don't classify myself yet as a physician in that I don't do anything uh I don't do primary care I just uh
(19) E: Uhuh well when you do do something uhm what would be the other kinds of things you could have done had you if you hadn't chosen what you will do?

(20) I: Oh I see well like uhm I don't know when I originally finished medical school I had my choice of doing various things [uhuh] uh becoming various what I say various types of physicians

(21) E: yeah

(22) I: and uh uh the choice which I picked uh uh was between uh surgeon becoming a surgeon

(23) E: uhh

(24) I: becoming a paediatrician

(25) E: uhh

(26) I: or becoming a uhm what they call an internist or you know uh connected going through a specialty connected with internal medicine.

(27) E: uhh

(28) I: There's so many fine grades like you know the like uh uh paediatrician is a type of physician

(29) E: uhh

(30) I: uh an internist is a type of physician uh and then within within these two categories you can also split them up I would think uhm you have paediatric cardiologist and paediatric hematologist/

(31) E: [o.k.

(32) I: [and paediatric nephrologist

(33) E: can I just write those down?
Yeah.
O.k. uh this within uh what category was?
Paediatrics
Within paediatrics // you have what
which is the one I chose
yeah
uh you'd have uh cardiologist
uhuh
probably (slurred) just at the beginning your whole list you'd have what you'd call your general paediatrician
uhuh
whooo it is does sort of uh consultant work and also does primary care medicine
uhuh
uhm
and after that
and then you have your paediatric specialies (sic) cardiology uh
(whispered) specialist
uh cardiology, nephrology uh hematology uh gastroenterology uhm (aside) now we're getting down where I have to, uh neonatology
ha
uh and there uh there there's more uhm let me think
Try and be as exhaustive as you can
Oh really/
Yeah.
(56) I: Oh my lord uhm well let me see uhm if if you're gonna just stick to ex it it's hard to be exhaustive in that you have various fields like you can cover every system of the body

(57) E: uuhuh

(58) I: in which there are specialists in that field some of them are specialists only in paediatrics and some are specialists uh in just that field and cover both paediatrics and adult medicine so like an ear nose and throat specialist

(59) E: uuhuh

(60) I: uhm you would get to see a child but he'd be an ear nose and throat specialist who'd cover both child ren (sic) and adults

(61) E: uuhuh uuhuh

(62) I: uhm and I don't know of any paediatric ear nose and throat specialists

(63) E: uuhuh uh

(64) I: uhm uh you have ophthalmologists some uh deal primarily with children

(65) E: [uhuh

(66) I: [in ophthalmology some and but most of them cover both children and uh adults and just about every ruddy system of the body you can think of there are paediatric surgeons also which uh is a sort of is a special group of paediatrics [uhuh] uhm uh connected with the field of surgeon and they deal only surgery and an paediatrics so that every system of the body you can think of uh
you could find a specialist uh uh whether it's in that whether he deals with paediatrics only or whether he has both paediatrics and uh adult medicine.

(67) E: [uhuh]

(68) I: [uhm there are uhm uh orthopaedics paediatric orthopaedics uh uh and we do have specialists just connected with paediatrics in that field uhm there are uh urologists paediatric urologists

(69) E: now I I get you now (( )) but uhm yeah uhm

(70) I: see what I was roughly dividing it up into is systems of the body.

(71) E: Yes well o.k. do you want to give me uhm those?

(72) I: systems of the body?/ roughly divided up into?/

(73) E: yeah

(74) I: O.k. if you if you sort of divide it into what specialties uhm uh there are for paediatrics or for anybody

(75) E: [ yeah yeah

(76) I: [ really there are ear nose and throat

(77) E: right

(78) I: ear nose and throat eyes ophthalmologists

(79) E: uhuh

(80) I: there's uh if you just going from the head down

(81) E: uhuh

(82) I: there's uhm uh neurosurgeons and neurologists which are two different groups

(83) E: uhuh

(84) I: neurosurgeons and neurologists uhm there's uhm I don't know how you'd call them uhm uh respirologists
I: haha

E: or uh people who deal with specifically respiratory diseases

E: uuhh

I: there is uhm cardiologists as I mentioned deal with uh diseases of the cardiovascular system

E: uuhh

I: there is uhm gastroenterologists

E: uuhh

I: deal with uh disorders of the intestines and bo uh lavernum and stuff there is nephrologists deal with the uh kidney

E: uuhh

I: there's urologists that deal with the lower parts of the urinary tract uhm they might not like that they they deal with the whole urinary tract but uh uhm there are uhm uhm as I mentioned ortho-pods orthopaedics deals with the skeletal and musculature abnormalities uh there is dermatologists deal with the skin

E: uuhh

I: uhm oh my goodness uhm (Pause) I wou I uh think I've run out uhm

E: o.k.// if you think of any

I: I would say that

E: if you think of anything else

I: yeah

E: I'll just put it on o.k. Right now uh as you were sss

I infer from what you were saying that uhm in paediatrics you could specialize in any of these? or any of these could specialize in // paediatrics?
(102) I: any of those could specialize in paediatrics

(103) E: uhuh

(104) I: right

(105) E: right

(106) I: as a paediatrician uh/

(107) E: it's a different set of it's a different classification then than this one that you you've just given me

(108) I: it's it's a little different uh but really as a paediatrician if I chose I could specialize in any of those

(109) E: uhuh

(110) I: but the the only problem is is if I say wanted to become uh pae­diatric urologist

(111) E: uhuh

(112) I: the amount of training which I'd have to go through to reach that would be unrealistic.

(113) E: uhuh uhuh uhm uhm if I wasn't uh a pae paediatrician uhm what mi what might I be? wi not not without reference to this section

(114) I: Ok. o.k. wi without reference to to that uh oh you might add to that also uhm an obvious one endocrinologist uh uhm uhm well if if you forget about paediatrics you could go uhm uh into in ((() my lord oh no that's o.k. uhm uh you could go into internal medicine

(115) E: o.k.

(116) I: uhm you could go into surgery uhm uh you could go into psychia­try and you know eh again within within internal medicine you
can [yeah] roughly divide yourself up like that also uhm not
to that extent but just about uhm

(117) E: and you said public health before

(118) I: public health right public health uhm let me see uhm you
can go into uh uh there are various fields of laboratory medicine
uh you know connected with uh various doctors who deal with the
uh both the investigative laboratory bacteriologist an uhm
virologist and and various things which roughly categorized
into laboratory medicine

(119) E: uhuh

(120) I: uhm there's pathology uhm and I I guess really you can't
exclude I don't know how vast you want to go uh there's uh many
doctors who many M.D.s who deal exclusively with research I guess
you'd have to put them in there I'm sure there's more uhm

(121) E: Is general practice a cat category? (( ))

(122) I: Yeah general practice yeah is certainly uhm gee

(123) E: o.k.

(124) I: I I can't really offhand think of any more