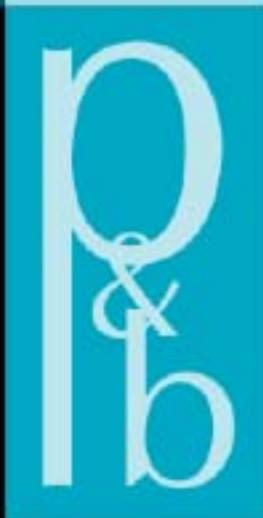


Talk and Taxonomy

A methodological comparison
of ethnosemantics and
ethnomethodology with reference
to terms for Canadian doctors



Peter Eglin

John Benjamins Publishing Company

TALK AND TAXONOMY

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No. 8

Peter Eglin

*Talk and Taxonomy:
A Methodological Comparison of
Ethnosemantics and Ethnomethodology
with Reference to Terms for Canadian Doctors*

TALK AND TAXONOMY

A Methodological Comparison of
Ethnosemantics and Ethnomethodology
with Reference to Terms for Canadian Doctors

PETER EGLIN
Wilfrid Laurier University

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1980

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PREFACE

The thesis of this work is that social or cultural competence consists more of an interpretive or methodological ability to use language in the service of interaction than of a substantive knowledge of collections of cultural categories and of the semantic relations between the terms naming those categories. Ethnomethodology, that is, better accounts for cultural competence than does ethnosemantics. Moreover, the attempt in ethnosemantics to describe competence in terms of a rule-like semantics is vitiated by its tacit reliance on the very interpretive ability which ethnomethodology successfully explicates.

Both logic and evidence are advanced in support of the argument, the first mainly in Part One which develops the case for interpretive sociology (Chapter Two) and applies it programmatically to ethnosemantics (Chapters Three and Four), the second in Part Two which provides the empirical material. By constructing from formal eliciting in interviews a taxonomy of terms for Canadian doctors (Chapter Seven), and then, after Wieder, turning attention to the production of that ethnography as an interactional accomplishment (Chapter Eight), two ends are met. Doubt is cast on the claim that semantic arrangements model competence, while light is thrown on the interactional organization of talk, talk out of which, it needs to be said, eliciting, interviews, taxonomies and ethnosemantic ethnographies are built.

The crucial questions of reflexivity and interpretive method are posed in Part One but not taken up until the Epilogue when the evidence

of Part Two is in. The Introduction to Part One stages the argument by introducing terms, starting from the broadest sociological context and homing in on the particular matters in question.

For intellectual excitement I am delighted to acknowledge the works of Garfinkel, Sacks and Wittgenstein. Through his teaching and example Roy Turner showed us graduate students how to attend small data with respect. It is an invaluable, if costly, lesson. Canada Council supported three-quarters of the doctoral research behind this book. Useful criticism on various parts of the manuscript came from the late Yehoshua Bar-Hillel, Mary Black, Derek Edwards, the late Michael Egan, Tony Giddens, Terence Hays, Hal Scheffler, Stephen Tyler and Larry Wieder. I wish to thank Werner Cohn and former fellow students for their support, and Audrey Leeman for typing the manuscript.

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To Pam and Chris

PART ONE. PROGRAMMATICS: THE LOGICAL AND METHODOLOGICAL ADEQUACY OF ETHNOSEMANTICS AND ETHNOMETHODOLOGY AS SOCIOLOGIES ATTEMPTING TO ACCOUNT FOR CULTURAL COMPETENCE.

1. INTRODUCTION

Of attempts to describe sociology's conflicting self-conceptions and varying practice the 'three-paradigm view'¹ - 'positivistic', 'interpretive', 'radical' - has perhaps most to offer (Habermas 1972a [1965]:308; Radnitzsky 1970; Giddens 1971:VII-VIII; Sherman 1974; Fletcher 1974; Fay 1975; Smart 1976; Benton 1977; Boughey 1978; cf. Ritzer 1975). The labels identify three sociological traditions varying on the two crucial criteria of meaning and values. Positivistic or natural-scientific sociology from Comte to Durkheim to 'grand theory' and from Comte to Durkheim to Mach to the Vienna Circle to Lazarsfeld and 'abstracted empiricism' treats meaning as methodologically insignificant and values as best kept out of it (Giddens 1974:3-4 and 1977:29-57). The interpretive tradition from Kant to Weber through Schutz to ethnomethodology², and from Kant to Mead to Chicago symbolic interactionism (Deutscher 1973:326), and from Kant to Wittgenstein (Janik and Toulmin 1973, especially Chapter 7) to Winch (Giddens, 1976:44-51) is equally value-free by profession but finds in meaning grounds for rejecting natural-scientific method as a model for sociological method. Radical sociology from Marx down is assuredly value-committed but divides on the meaning criterion into a more positivistic branch and a more interpretive one (Bottomore 1975); in the latter case I am think-

ing of the 'critical theory' of the Frankfurt School and Dorothy Smith's work (Smith 1974, 1975). Since the questions of topic and method involved in the issue of language as they have arisen in the theoretical traditions dealt with here - ethnosemantics and ethnomet hodology - have been discussed without reference to values, I do so too.³

My attention, then, is confined to positivistic and interpretive sociologies.⁴ More narrowly, the focus is on the way each conceives both its datum and the relations between data. In these terms a third non-radical sociology is usefully introduced - what I shall call 'semantic sociology'. Though largely fictitious, it permits the argument to proceed. All three sociologies are described in Chapter Two. Of the three the semantic and interpretive varieties are the ones in chief contention. By 'the way each conceives of its datum' is meant whether social facts are thought of as 'brute' or 'institutional', and by 'relations between data' whether those relations take the form of 'regularities', 'rules' or 'instructions'.

First, the facts. Whether labelled 'verhalten-handeln' (Weber 1968 [1925]:57, fn.3), 'behaviour-action' (Wilson 1970a:698; 1970b:58), 'behaviour-conduct' (Merton 1936:895), 'molecular-molar' (from Barker; see Turner 1966:266), 'etic-emic' (Pike 1967:37-72), 'observer-actor' (Cohn 1969), 'appresenting term - appresented term' (from Husserl; see Schutz, 1967c:294-97), or 'actual-observed-appearances-of-an-object - object-that-is-intended-by-the-particular-actual-appearances' (Garfinkel 1963:194), the distinction between non-meaningful and meaningful categories has long been with us (Winch 1958:45-51, 116-20). Searle's discussion of 'brute' and 'institutional' facts in *Speech Acts* (1969), following Anscombe (1958), is most useful since he ties it to the important concept of 'constitutive rule'. The latter is a major building block in the argument (see also Taylor 1971).

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)

To understand these statements requires 'knowing' the institutions in terms of which these statements are facts. And to know these institutions is, to anticipate Chapter Two, to know the constitutive rules which underlie them.

The distinction is useful insofar as it allows me to argue for the importance of institutional facts in sociological study and thereby to move a step towards making the case for interpretive sociology. Beyond that I do not wish to defend it. As the late Michael Egan reminded me, Wittgenstein destroyed forever the possibility of easily assimilating pain language to brute facts. The hegemony of physicalism in the philosophy of science has obscured the extent to which being able to traffic in purportedly brute facts is a methodological accomplishment of the sciences, and not simply given in the nature of things (Schutz 1967a:5). Henry Elliot helped me see that.

Second, the relations. These derive from the conception both of a datum and of what it means to give a sociological account of such a datum - whether deductive explanation, grammatical description (in the linguistic sense), or procedural explication. Deferring discussion of accounts to the Epilogue it can be said now that for each of the three sociologies datum and accounting come together in a particular kind of formal object that specifies the relations between data and becomes the object to be accounted for. Positivistic sociology deals in regularities, semantic sociology in constitutive rules, and interpretive soci-

ology in interpretational instructions or, simply, instructions (cf. Eglin 1975). These objects can be represented as sentences containing brute and institutional facts, as follows. Individual lower-case letters stand for brute facts, individual upper-case for institutional facts:

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

While these distinctions are sufficient to launch the argument of Chapter Two - that an adequate sociology needs to be interpretive - one further set of terms is worth introducing at this point to frame the link between Chapter Two and the other chapters of Part One. As interpreted in American philosophy of science, largely through Morris' influence on Carnap (Bar-Hillel 1964), the Peircean trio - syntax, semantics, pragmatics - has come to characterize the different parts of scientific method. 'Syntax' refers to the purely formal relations among axioms and theorems - a matter of logical analysis; 'semantics' subsumes the correspondence rules that provide observational content for those theoretical concepts involved in testing; 'pragmatics' deals with the mechanics and procedures of actual experimentation (Carnap 1942:10). As a first approximation, it would be true to say that positivistic sociology, as champion of deductive explanation, engages in all three areas of activity, but sees semantics and pragmatics as subservient to syntax (Schrag 1967; Popper 1968:61): "Grand theory is drunk on syntax, blind to semantics" (Mills 1959:34); the propositions under which empirical regularities are subsumed are the output of the syntactic component of the theory; semantic sociology is just that, an exercise in semantics; and interpretive sociology is the inchoate discipline of pragmatics - the study of "the judgemental work of the user" (Garfinkel 1967:71, cited on p. 45 herein: cf. Bar-Hillel 1970b:74ff; Weinreich 1966:150; Helmer 1970; Sayward 1975), or of members' methods, interpretive procedures and instructions for making sense (to define ethno-

methodology [Sharrock 1977:552] in anticipation of Chapter Three) - where that is conceived as basic to any work of a semantic or syntactic kind (Carnap 1939:166).

Admittedly, I am stretching terms here. By an interpretive pragmatics I mean more, clearly, than is contained in Carnap's definition (cf. Sayward 1975). The latter could well apply to the sociological twin of positivistic 'grand theory', namely Mills' 'abstracted empiricism' (though Mills [1959, 1953] prefers to call it 'semantic'), and I do not intend that. Further clarification must await Chapter Two. Also it should be noted that I intend no comparison with the 'interpretive semantics' of transformational-generative-grammar fame. I now prefer to call my second sociology 'semantic' and not 'grammatical' (cf. Eglin 1975), reserving the term 'grammar' for Wittgenstein's special usage. However, there are occasions when the linguists' usage must be respected, and I so indicate in the text.

In Chapter Three ethnosemantics is described as a form of (constitutive) semantics, and ethnomethodology as a form of (interpretive) pragmatics. In arguing in Chapter Two for the primacy of interpretive over semantic sociology (and, by extension, over positivistic sociology), I am arguing for pragmatics over semantics and, in Chapter Four, for ethnomethodology over ethnosemantics - that is, that the ethnosemantic attempt to explain competence in terms of a rule-like semantics is vitiated by its tacit dependence on that very interpretive ability explicated by ethnomethodology.

2. REGULARITIES, RULES AND INSTRUCTIONS: THREE SOCIOLOGIES

The claim that an adequate sociology must be interpretive comprises both a particular view of the sociologist's subject matter, and a corresponding method. As method arises out of practice a discussion of it is best left until last (Epilogue). My thesis in this chapter is primarily that sociological data are best conceived as instructions or, better, instructings (see Garfinkel and Sacks 1970:345, fn.15 on the importance of the continuous aspect). I proceed by describing the methodological features of positivistic, semantic and interpretive sociology in turn, showing that the use of regularities depends upon the use of constitutive rules which themselves depend upon the use of instructions.

Positivistic sociology - regularities

I should preface this section by warning that my use of such terms as 'sentence', 'statement' and 'proposition' may not always be rigorous. The reader is best referred to Bar-Hillel (1970a:165, 195-97, 213, 217, 280-85, 364-69) who has devoted considerable attention to the matter.

Recall the form of a regularity:

(1) If X then Y. [or] If x then y.

The sentences in (1) are models of the conditional statements typically found in deductive theorizing. The form containing the capital letters represents the high-level, abstract, theoretical, 'universal' proposition. The small-letter form depicts the low-level, concrete, empirical, 'singular' hypothesis derived from such theorizing (Popper 1968:59-77).

In sociology these matters are the abiding concern mainly 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. I shall take them as read. A universal statement of (1) would be, for example,

- (4) When task groups are differentiated with respect to status characteristics 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 regularities at different levels of abstraction. Neither is in conditional form as given though both could be rendered so without loss of meaning. A successful explanation would subsume many of the singular type under few of the universal type. What are called correspondence rules provide the link between the abstract concepts of (4) and the concrete concepts of (5). An example of a correspondence rule is:

- (6) Air force rank is a status characteristic. (Cohen 1966:6)

That is, a concrete observational term, 'air force rank', is posited as an indicator of the abstract theoretical concept, 'status characteristic'. But (5) is not yet a statement in brute fact terms (Frank 1964: 341). Testing requires an operational version (Schrag 1967:363). For 'air force rank' that version would be, say, the verbal response to the interviewer's question, 'What is your rank ?'. While this is deliberately a simple case social scientists are, of course, familiar with the remarkable lengths to which we or our 'methodologists' go to produce 'brute data' through artful indices and scales. Contrary, perhaps, to our common sense of the matter it would seem that arriving at a testable

statement in terms of purportedly brute facts is a considerable methodological accomplishment.

I have been using 'brute fact' and 'institutional fact' rather loosely to refer both to concepts and, more properly, to relations between concepts. Hopefully little of importance hangs on the difference. I want to continue the license in order to state my main point of this section. This is that, as indicated in the first of the sentences in (1), the theoretical concepts in regularities are institutional facts (Y, Q), be the institution conceived as 'science', a 'critical tradition' (Popper 1976:95), the particular research programme or, perhaps best, the relevant 'disciplinary matrix' (Kuhn 1974):

A regularity or uniformity is the constant recurrence of the same kind of event on the same kind of occasion; hence statements of uniformities presuppose judgements of identity Such judgements are intelligible only relatively to a given mode of human behaviour, governed by its own rules. In a physical science the relevant rules are those governing the procedures of investigation in the science in question.... So to understand the activities of an individual scientific investigator we must take account of two sets of relations: first, his relation to the phenomena which he investigates; second, his relation to his fellow-scientists. (Winch 1958:83-84, emphasis added; see Schutz 1964:69)

That is, while the theoretical terms of science and positivistic sociology have no necessary relationship to categories that are meaningful to the population being studied, they are nevertheless meaningful, their meaning frequently being only partially determined (Bar-Hillel 1970a:200, cf. Lyons 1968:412-27), to the community of scientists using them. Such 'primitive' terms (Garfinkel 1961:61) are institutional facts in this sense. Searle (1969:51) gives the inverse square law as an example of a paradigm of knowledge consisting only of brute facts. In my analysis the relations, to speak properly, between such concepts in physics as 'attraction' and 'bodies' or 'force' and 'mass' are, rather, institutional facts of science. They are reducible, in principle, to "physical or psychological properties or states of affairs" only through the battery of correspondence rules and operational definitions exemplified above - or, as I would rather speak now, so reducible by virtue

of 'constitutive rules'. That is, the paradigmatic (Kuhn) facts of science are institutional facts and, like the facts of other institutions, are underlain by constitutive rules.

Insofar as constitutive rules say what some *activity* counts as in some context (see next section) they typically embody more than is contained in the positivist's correspondence rules and operational definitions. In a previous version (Eglin 1975:379) I offered the following as an example of a constitutive rule on the model of sentence (2), invented to fit the small groups material already cited:

- (7) 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).

It seems to me now that this is no more than a correspondence rule and operational definition rolled into one, and would not satisfy the two conditions for constitutive rules given by Searle - these being the possibility of deviance and the extension to new cases (1969:33-42). Since the small groups researchers, like other scientists, do not formulate their own activities in terms of constitutive rules, and since there is no suitable ethnography of a sociological laboratory setting (Turner 1967, and Crowle 1971 notwithstanding)⁵, I cannot readily supply a telling example of a constitutive rule for this field. One must make do with self-descriptions of the following sort:

The rank order of actual participation in the current interaction of the group stands in potential contrast to any rank order of status expectations. Who speaks how much to whom in the group is a 'brute fact' characterizing the actual present situation. Speaking takes up time. When one member speaks it takes time and attention from all other members of the group, some of whom may want to speak themselves. *To take up time speaking in a small group is to exercise power over the other members* for at least the duration of the time taken, regardless of the content. (Bales 1970:76, emphasis added; quoted in Sacks, Schegloff and Jefferson 1974:71!, fn.24)

In the face of such glosses I can only reassert my thesis which is that positivistic sociology's object of explanation, the regularity, is un-

derlain by one or more constitutive rules.

It might be objected at this point that the adequacy of the positivistic theorist's rules and definitions is judged by, among other criteria, how well the theory stands up to tests; so that constitutive rules would play no part in the success or otherwise of the scientific game. However, it can be shown that constitutive rules are required here to translate the array of experimental *results* (purportedly 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)

For a sociological example:

[the] relationship between [high school students'] educational aspirations and expectations and their social class position ... is such a well established finding that...if a survey did not show such a relationship the methodology of the survey would be suspect. (Porter et al. 1973:42).

That is, what will count as 'falsification' or 'corroboration' will depend on more than formal criteria (Kuhn 1970a). It will depend on, say, accepted practice among a group of fellow practitioners, practice formulatable as constitutive rules, so that not only the theoretical concepts of science but the data themselves are institutional facts; the second sentence in (1) is really a methodological fraud, in both the defamatory and creative senses of that term. The point is no longer new (Winch 1958; Garfinkel 1961:60; 1967:95-96, 100-103; Kuhn 1970b: 13-16; 1970c:238-39; McCarthy 1973:370; Elliot 1974).

Semantic sociology - constitutive rules

Recall

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

Constitutive rules (Searle 1969:33-42) tell what it is bits of the brute world count as in terms of some human institution, given some

context. (In view of his expressed intention (1969:15) to follow Chomsky (1957, 1965) by taking his (Searle's) 'intuitions' as the data, it is not clear whether Searle views the x-term in the constitutive rule as a brute fact or not (cf. 56). Since the issue is complicated and goes beyond the confines of this already wide discussion, suffice it to say that my conception may depart from Searle's on this point.) Constitutive rules are to be distinguished from 'regulative rules', 'instructions', 'precept rules' and the like (Rawls 1955; Black 1962:109-15; Hayek 1963: 334-35; Ganz 1971; Runciman 1972:21-22, fn.31 for Kant and Weber; Weber 1975-76; cf. Popper 1975:237).

Following sentence (2), an example of a constitutive rule is:

- (8) In the game of cricket (Z), hitting-the-ball-full-pitch-a-cross-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 it can be said 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 sociological accounting. Just as regularities partake of the function of explaining by prescribing a relationship between two or more variables, so rules are themselves part-accounting-of-the-data as well

as part-formulation-of-the-data. That is, they are both accounting tool and object to be accounted for. 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, of the linguistic rather than Wittgensteinian sort, is analogous to logical analysis within a theory (Chomsky 1970a, 1970b; cf. Black 1970).

In contrast to Eglin (1975:380) I do not now claim that the constitutive-rule-and-grammar conception fits any substantial part of extant sociology. To a certain extent the ethnographies of the Chicago School and Goffman's work, that is, parts of symbolic interactionism, will stand being formulated in these terms (Lemert 1970:492 ff.; cf. Goffman 1974:5-6). Chapters Five and Six of Wieder's (1974) study of the convict code in a halfway house do provide a good example of such an analysis, though his use of 'constitutive' (for example, 73) owes more to phenomenology than to linguistic philosophy. A fuller account of his important work is given in Chapter Six.

The extent to which my semantic sociology approximates what some sociologists do is not at issue here. Its virtue lies in allowing me to clarify the nature of instructions, to set these off from rules and regularities, and to pursue an effective critique both of positivistic and semantic sociology in this chapter and, more importantly, of ethno-semantics in Chapter Four. For now, it will be recalled, I am arguing that regularities are underlain by constitutive rules which themselves depend upon instructions. To develop the latter half of this claim I proceed by attacking the phrase 'in context Z' that forms the first part of a constitutive rule.

The problem arises with constitutive rules of how one is to take the phrase 'in context Z'. Specifically, how does this come to be a capital-letter, institutional fact in the first place? I want to argue that the problem of context is crucial; that a specification of context is not available simply by inspection (Garfinkel 1967:10; cited

on p.41 herein); 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 I shall call 'instructing'. I shall proceed by way of Searle's account of promises in *Speech Acts*.

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; cf. Hempel 1952; Nagel 1952; Schutz 1967b; Garfinkel and Sacks 1970:339)

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 constructive 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. While Searle acknowledges this -

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)

- it in no way absolves his analysis from the sort of critique being developed here.

To advance constitutive rules as descriptive explication 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, are 'indexical' (Garfinkel and Sacks 1970:348-50), and require interpreting.⁶

Interpretive sociology – instructions

To 'see what one means' is to do understanding. Anticipating the section on ethnomethodology in Chapter Three, it can be said that

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

I propose to characterize the operational structure of interpretive work, after Wieder (1974:224), as the reading (giving and receiving) of instructional interpretations or, simply, of instructions (Cicourel 1970:152). Sentence (3) is the model of an instruction:

(3) Find any lower-case letter to be Z, and see that in context Z, x counts as Y.

An example would be, loosely put,

(9) 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 two initial 'unknowns' (c and x), and the incorporation of a constitutive rule. Another example, this from Roy Turner but formulated by me, would be

- (10) 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.⁷

More generally, for the purpose of making sense (Kjolseth 1972), what social actors provide for themselves and for each other in their utterances and other actions are *displays* of meaningful items and not merely the items themselves (Garfinkel and Sacks 1970:344). By 'display', however, I do not mean to adopt wholesale Goffman's machiavellian thesis of impression management (Goffman 1959, 1976). As was revealed in the case of Agnes (Garfinkel 1967: 164-85),

The whole point of Garfinkel's incongruity procedures is to show that the sense of possibility and its technique of impression management is false to the naive intersubjectivity which is the unarticulated structure of our everyday trust in and competence with social reality. (O'Neill 1972:217)

Rather I wish to characterize displays here as instructions or as containing instructions for 'going beyond the information given' (Bruner 1957) to interpret the items.

The task of interpretive sociology is, then, to explicate in some fashion - rather than explain, or merely describe - how societal members instruct each other how their actions are to be taken and are taken (including what those actions are). That this instructing is done in the very course of action and not as a running commentary on it; that is, that it is built out of the very materials it itself explicates; this is what is meant by *reflexivity* (Garfinkel 1967:7-9; Heap and Roth

1973). To say it again, somehow members (speaker-hearers) instruct each other how particular behaviors (utterances) are to be taken (heard) *in the very course of enacting (uttering) the particular, that is, without coming out and saying so in so many words* (Garfinkel and Sacks 1970).

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 1972b:168)

Talk, including the interview talk that forms the material of Part Two, is an obvious candidate for explication in these terms (Lyons 1968: 419; Wieder 1970:133), as I tried to indicate by the words in parenthesis in the previous paragraph. It is also the stock-in-trade of ethnomethodology. In the next chapter, after assimilating ethnosemantics to semantic sociology, I shall elaborate this discussion of interpretive sociology with the ethnomethodology of Garfinkel and the conversational analysis of Sacks.

In this chapter I have outlined the methodological features of three more or less idealized sociologies in order (a) to argue that attempts to base sociology on regularities and rules are inadequate, (b) to argue that instructions are sociology's fundamental subject matter, and (c) thereby to prepare the way for showing the relationship between ethnosemantics - a rule-based semantic sociology - and ethnomethodology - a form of interpretive sociology - in Chapter Three.

3. ETHNOSEMANTICS AS SEMANTIC SOCIOLOGY AND ETHNOMETHODOLOGY AS INTERPRETIVE SOCIOLOGY

Ethnosemantics as semantic sociology

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 ethnosemantic description is a set of semantic rules. Such a rule states the conditions under which a given term names a given object. This brief description of ethnosemantics will stand until the fuller treatment of Chapter Four is given. The aim in this section is to posit ethnosemantics as a kind of semantic sociology by showing that semantic rules are constitutive rules. This stratagem will permit the criticism of ethnosemantics developed in the following chapters to apply to a kind of sociology. Games provide a way of starting.

When it comes to providing a model of their approach a wide range of modern theorists turns to games (that is to say actual games, not the entities of mathematical game theory). This is true of Rawls (1955), Wittgenstein (1958), Moore and Anderson (1960), Goffman (for example, 1961:34; Garfinkel 1967:164-85), Garfinkel (1963), Hockett (1968; see Fillmore 1969), Searle (1969), Goodenough (1969, 1970), and Grathoff (1970). While Garfinkel's use will be considered in the next section, here the intention is to note the parallel treatment of games by Searle (following Rawls 1955:25-29), the philosopher of constitutive rules, and Goodenough, a father of ethnosemantics.

Each distinguishes his intended object of description, rules,

from mere statistical regularities in game play. For Goodenough, following Leach's somewhat controversial distinction, 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 (see Eglin 1976:360, fn.15), 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 periodic 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 are all those concepts which are backed by constitutive rules, concepts such as touch-down, 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).⁸

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 semantic 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' (from Wallace and Atkins 1960:61-62; cf. Goodenough 1965; Wordick 1973). 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 category conventionally labelled as 'mother'.

One can abstract from this to

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

By using Pike's (1967) terminology, which Goodenough adopts (1970:108 ff.), as does ethnosemantics in general, (14) can be reduced 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 semantic rules (15) are constitutive rules in the field of ethnosemantics. 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. (Searle 1967:125)

But since ethnosemantics attempts to restrict itself to locutionary meaning (Austin 1962; see Lyons 1963:4; Hymes 1970b:111) or propositional acts (Searle 1969:24ff.; Rosaldo 1974:155), it is necessary 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.

If constitutive rules are the hallmark of semantic sociology then ethnosemantics is such a sociology. If this is so, then ethnosemantics is both subject to the critique of semantic sociology already offered in Chapter Two, and a vehicle for further criticism of the latter through criticism done on *it*. Setting up ethnosemantics in this way has been the point of this section.

Ethnomethodology as interpretive sociology

The next task is to show that ethnomethodology is a form of interpretive sociology as defined already, so that in using ethnomethodology to criticize ethnosemantics in the following chapters I am at the same time arguing the merits of two kinds of sociology. As before games provide a point of entry, Searle's account again being the foil. Whereas Searle and Goodenough had football in common, Searle and Garfinkel play chess.

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 1967:140ff.)¹⁰ 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)¹¹

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 whomsoever 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 1972a: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. This consideration led to my formulating interpretational instructions 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.

I want to put this last point more precisely and in so doing relate the notions previously introduced of interpretive procedure, instruction and talk.

I have said that interpretive sociology studies members' interpretive procedures. Members are conceived as providing each other instructions for interpreting their actions (in the very course of performing the behaviours which are to be counted as doing those actions). When it comes to talk the tools for building those instructions are the sequential properties of that talk. That is to say talkers have at their disposal sequential positions or slots, such as 'current', 'prior', 'next', 'first', 'second', 'opening' and 'closing', with which to locate their utterances so as to have them heard to be doing the intended actions (Sacks et al, 1974). Further, many interactions carried out in speech have a conventional adjacency-pair structure. For example, as in the case analyzed in Chapter Eight, an utterance that does an invitation sets up a slot for it being accepted or declined. 'Accept/decline' is then the preferred hearing for whatever utterance occupies the turn following the invitation. Not that such has to occur, but that whatever does occur (whether acceptance, declining, 'no answer', snub...) will be hearable or recognizable as such by virtue of the sequential properties of that pair (Eglin 1980).

This applies to instructions on the model of sentence (3) as follows. For talk, a particular utterance (x) counts as doing some action (Y) in the context of some sequential property (Z). By locating an utterance sequentially its owner is instructing the hearer, as it were, to find that in that sequential context the utterance counts as a cer-

tain action. This is possible by virtue of the sequential properties or 'preference rules' (Sacks et al. 1974:704; Sacks 1973:139; Sacks and Schegloff 1979; Turner 1976:237; Coulter 1979:184-185,fn.19) which organize talk.

I have said that the instructional formula is open-ended. The reader will have noted that in order to recognize an action the hearer-member is required to effect a correspondence between two unknowns (p. 16). namely the brute particulars of the utterance, and the brute circumstances of its setting. The operation needed to do this is circular, or reflexive. In a truly profound statement Sacks has said

What you get is, there are actions which for them to be effective need to be formulated via some particular Device. And then those actions invoke that Device. That is, the utterances which might do those actions invoke that device. (Sacks 1976:G6)

This speaks about ethnmethodology's central concept of reflexivity. Because this is basic to a discussion of what interpretive *method* looks like, and because, following Durkheim and Weber, method is best considered in connection with analytic practice, I save its treatment to the final chapter. The question of the correspondence of particular and context is, in other terms, one of figure and ground, or of document and pattern. The 'documentary method of interpretation' (Mannheim 1952)

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 1967:78; see also 1956:192-95; 1961:57-59; 1967:39-40; Sharrock 1977:560-61)

Because of the centrality of talk among social phenomena, because of the centrality of conversation among the 'speech exchange systems' (Sacks et al. 1974:730), and because of the fruitfulness of conversational analysis as a research programme within ethnmethodology (see *Sociology*, January, 1978; Schenkein 1978; Psathas 1979), I have tried to show the applicability of my conception of interpretive sociology specifically to conversational analysis as a way of bringing ethnmeth-

odology under the rubric of interpretive sociology. From Garfinkel's definition one can infer that the interpretive devices of conversation are only one species of the documentary method of interpretation. Indeed, understanding "is not one thing; it is as various as the language games themselves are". So says Wittgenstein in the Brown Book (Rhees 1969:VII), and echoing him,

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)].(C. Garfinkel 1967:31, emphasis added within the bracket; cf. Wittgenstein 1958: paragraph 133)

The 'way of questioning or answering', the language game of interviewing (Sacks et al. 1974:710), is the favoured method of ethnosemantics. It is the major critical topic in the chapters to come.

4. 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 1967:35).

The common behaviour of mankind is the system of reference by means of which we interpret an unknown language. (Wittgenstein 1958: paragraph 206; cf. Cicourel 1967:119 fn.20).

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).

In trying to discover the nature of cultural competence ethnosemantics leaves out of account the judgmental or interpretive work of a society's members, and that neglect is fatal to its programme. This is the critical nub of the book and of this chapter. It derives from Garfinkel and, more implicitly, from the later Wittgenstein. The chapter is organized as follows.¹²

Ethnosemantics (Sturtevant 1964; Colby 1966; Tyler 1969a; Conklin 1972) is characterized by specifying its goals in terms of its theory of culture. Its borrowings from semiotics 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, followed by a critique from ethnomethodology (Garfinkel 1967). Two tacks are taken throughout. The one centers on data gathering, the other on the semantic arrangements that form the results. Their respective internal

problems - the problem of abstracting from pragmatics, and the problem of context - reduce, under the gaze of ethnomethodology, to instances of indexicality. The ability of ethnosemantics to produce orderly results is re-reviewed by ethnomethodology as a case of the accomplishment of social order.

Ethnosemantics

GOALS AND THEORY OF CULTURE

The long-term goal of ethnosemantics is to explicate an intuition - the intuition that some things are appropriate, some things not. It is implicitly 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 the 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).¹³

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 lan-

guage (Frake 1962:75).

The world-view of ethnosemantics 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, ethnosemantics 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)

My 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" ?¹⁴

SEMIOTIC BACKGROUND

Conceptually and methodologically, ethnosemantics has drawn on semiotics (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). I shall concentrate on semiotics because to a large extent the relevant concepts in structural linguistics and

cognitive psychology are subsumable under semiotic concepts.

Thus, in their founding empirical papers (Goodenough 1956; Lounsbury 1956), and accompanying programmatic statements (Lounsbury 1954, 1955; Goodenough 1957 [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 continued 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). I 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 ethnosemantics, signification is given a strictly referential interpretation by way of the sign 'lexeme'. Moreover, reference itself is restricted to denotation (Lyons 1963:4; 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 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 semiotics is the trio introduced in Chapter One of *syntactics* (or syntax), *semantics* and *pragmatics* (for example, Werner 1966, and Black 1969:187 fn.7). According to Goodenough's early statement, one modified only slightly in later work (Goodenough 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 (including '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).¹⁵ This postulated order for their study - syntax first, semantics second, pragmatics third - is something I shall want to question (in the spirit, if not the letter, of Hymes' crusade [for example, Hymes 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). Goodenough, in the 1957 paper, ignores indexical signs, throws iconic signs to structural linguistics (syntax, cf. Jakobson 1971b: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 (1971b:357; see also 1956, 1971a, and Lounsbury 1960), "Relatively little attention has been paid to what Peirce called 'iconic and indexical signs'" (1971: 170). That is understated. However, while ethnosemantics has busied itself with the semantic structure of lexemic symbols, ethnosemantics, quite separately, has developed a sophisticated discussion of indexical signs and of pragmatics, to both of which I shall return.

The fourth borrowing from semiotics 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, that is, the judgemental work of the user (see p. 4) - was reified into a classification of types of discourse.¹⁶ That idea carried over into ethnosemantics 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. 1.¹⁷

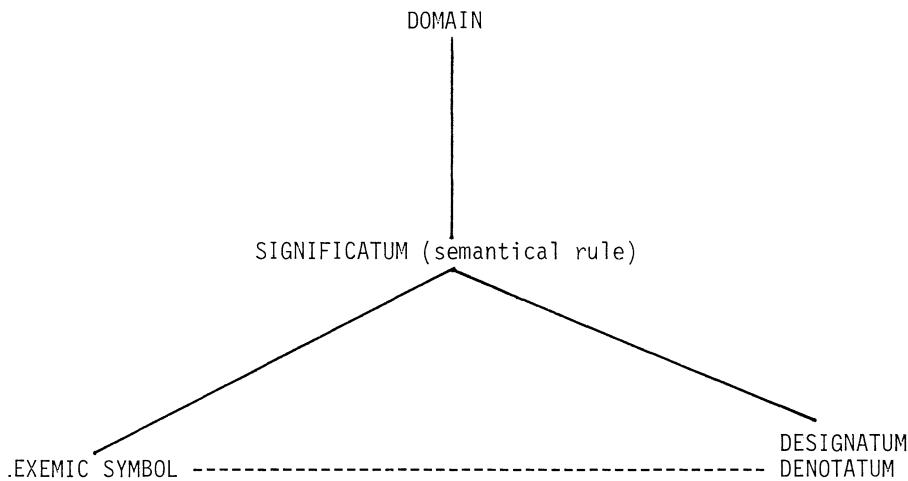


FIGURE 1: THE SEMIOTIC TRIANGLE IN ETHNOSEMANTICS

Sources: Peirce 1932; Morris 1938; Lyons 1968:404; Friedrich 1971; Scheffler and Lounsbury 1971:3-12.

Internal critique of ethnosemantics

THE DATA-GATHERING OPERATION AND THE QUESTION OF 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 1965b:335; cf. "It is impossible to cross the gap between language and things without really crossing it" [Pears 1965a: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' (1966/67) little-noted report 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)

On this, the following criticisms may be made:

- 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 1972:33).

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'.¹⁸ 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 - 'agreement' - is translated by the investigator into a 'conditioned response'. Q-R's are modelled on S-R's (Black and Metzger 1965:142; cf. Hymes 1966:26; Moerman 1968:164; Epling 1967:261; but see - with reference to Epling - Scheffler and Lounsbury 1971:142).

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 add-

ed). 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 constraint 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 one 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)¹⁹ - 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) one 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). This last point is discussed further in Chapter Six.

More is made of 'international accomplishment' and 'translation work' in the section on ethnomet hodology, where they are seen to be of critical interest. For now, it is hoped that this internal critique of the data-gathering operation provides ground 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 1970b:70; Helmer 1970:733).

SEMANTIC ARRANGEMENT AND 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 my 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 ethnosemantics has seen itself as moving. If it be granted that some not-so-recent writings of Wallace and Tyler are representative of mainstream ethnosemantic analyses, Wallace's being directed more at cognitive matters, Tyler's more at social ones, then one may ask of those writings what they imply for the ethnosemantic 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 con-

strains 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 1960:397; Hymes 1964b:26; 1964c:97-98)" (1966a:694).²⁰ Therefore, "I will attempt to relate terminological variation to the contexts in which terms of reference are used" (694; cf. 1966b:515; Goodenough 1965:287 fn.12; Pelto 1966:201; Berreman 1972). 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. Fillmore 1966:220; Rosaldo 1972:84; Sankoff 1972:563). The article was reprinted in Tyler (1969a) and again, with slight revisions, in Gumperz and Hymes (1972a). In the revised version is added,

The important point is that this chapter demonstrates the possibility 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 [1971:391] proposes a quantitative approach to handling variability.)

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. Good-enough 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, while in the light of my formula for instructions it is suggestive to say that form (lexeme) and context mutually determine meaning (Hymes 1962:19; 1964c:97-98), or that act and situation do so (Frake 1964a:133), such formulations remain inadequate as long as 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 the argument from context made against constitutive-rule accounts in Chapter Two.

It is at this point that the ethnomethodological 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 characterizing the "utter pervasiveness of indexicality" (Peirce 1932:172; Wells 1967:104; Luckmann 1972:31). And, secondly, that ethnosemantics nevertheless achieves 'rational' results raises in a new way the concomitant 'problem of social order': how societal members (such as semantic ethno-

graphers) establish 'equivalencies among non-identical contexts'. It is time to shift perspective.

Ethnomethodological Critique of Ethnosemantics

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

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 ethnomethodology is essentially the same* (cf. Garfinkel's definition of competence [1967:57 fn.8; see Moerman 1969: 465; Cicourel 1970:147; Turner 1970a:5; Phillipson and Roche 1976:78; Mehan 1972:1; Gumperz and Hymes 1972b]). I am saying that ethnosemantics cannot reach that goal by pursuing semantic ethnographies, but that ethnomethodology is better equipped for success. A clue to the difference is that both the obstacles in the way of ethnosemantics and its 'accomplishments' are instances of the very phenomena which constitute ethnomethodology's topic and domain of inquiry. This part of the chapter will recast those problems as that topic, and discuss the latter in terms of indexicality and accomplished social order.

ETHNOSEMANTICS REFORMULATED IN TERMS OF ETHNOMETHODOLOGY

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

- (A) impossibility of obtaining results;
- (B) results obtained.

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

- (A¹) logical impossibility of obtaining results;
- (B¹) results obtained for-all-practical-purposes.

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

- (A¹)
 - (i) The problem of abstracting from pragmatics;
 - (ii) The problem of context.
- (B¹)
 - (i) 'Agreements' between ethnographer and informant (Siverts);
 - (ii) People's ability to establish 'equivalencies among non-identical contexts' (Tyler).

For ethnosemantics (Tyler [1969d] excepted) (A¹) (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). (B¹) (i) and (ii) are merely taken for granted.

For ethnomethodology, however, (A¹) and (B¹) are the two sides of the indexical coin, the currency of which is universal. The observations gathered under (A¹) are instances of *indexicality*. The observations gathered under (B¹) are instances of *accomplished social order*. For ethnomethodology the problem which provides it with a programme is: given indexicality, how is social order possible?²¹ Said about talk 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) ?

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

INDEXICALITY²² AND 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 1967:40; also 1961: 60; 1967:4-7; Garfinkel and Sacks 1970:348-350)

Indexical expressions are to be contrasted with so-called objective or context-free expressions. Any investigative inquiry - science, ethno- semantics... - 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 ethnomethodology for ethno- semantics 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 1967:10, emphasis added).

This has the consequence, for instance, that all rules - semantical rules, componential definitions, contextual rules, constitutive rules - are inadequate in isolation to subsume specifiable sets of objects or actions. They need to rely on something external to them for their sense - some language-game, some form of life, some 'what anyone knows' (Garfinkel 1967:275). 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 ethnosemantics (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 me cast this in terms of the semiotic triangle introduced earlier. According to ethnosemantics 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 1967:6). The ethnomethodological 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 situations. 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 AND 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, practitioners of ethnosemantics routinely discover an orderly world, which provides orderly results - results, moreover, which are purportedly 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 this sample, is [sic] *interpretation free*... data of this kind has 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 ? After outlining in a paragraph how ethnomet hodology would formulate the order question and what answer it would give, I shall list ten ways whereby ethnosemantics achieves order using its theoretical and methodological apparatus to gloss over the underlying interpretive work (cf. Stoddart 1974; Katz 1975).

Practitioners of ethnosemantics find social order *in* the world, independently of their accounts of it. In contrast, ethnomet hodology treats social order as an accomplishment of societal members, such as professional ethnographers, and sees that order as a feature of the accounting by which it is told. Where the one sees data regularities or investigator/informant correspondence as 'interpretation free' or, more riskily, as 'agreement', the other sees such expressions as glosses for interpretive work which remains to be explicated. The latter's 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 its topic of investigation. Its claim is that the finding and the telling are the same (Garfinkel 1967: 1; 1974:17; cf. Attewell 1974). It provides a general formulation of such interpretive work in terms of the documentary method of interpretation. As was seen in Chapter Three, since the documentary method is not one but many methods, ethnomet hodology's field of inquiry lies open.

Here, then, the question of social order becomes a matter of the *work* by which semantic ethnographers produce order in their data and in their results. What is the extent of that work in theory and practice ?

(a) Let me 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 himself/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 1967:77-78)

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 problem of formal eliciting. It is the same argument as that about the relationship between interview situations and non-interview situations at the end of the last section on indexicality.

(b) 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 1967:70; cf. Hale 1966:808)

Motivated by a desire for rigour this practice 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' (1967:66-71; cf. Cicourel's 'dummy' [1970:160]). The cultural dope is made out in these further features of ethnosemantics:

- (c) the treating (hearing) of informants' responses as non-problematic answers-to-questions (Garfinkel 1967:266-267, 92), no notice being taken of the work required to do that hearing;
- (d) treating such answers as conditioned responses (as discussed in the section on the data-gathering operation);
- (e) portraying agreements as the "demonstrable matching of substantive matters" (Garfinkel 1967:30);
- (f) "portraying the usages of the member of a language community as ... culture bound [and this includes the situational variant]" (Garfinkel 1967:71);
- (g) "construing the pairing of appearances and intended object - the pairing of 'sign' and 'referent' [sic] - as an association [see Goodenough 1956:195]" (Garfinkel 1967:71);
- (h) assuming "that an invoked shared agreement on substantive matters explains a usage" (Garfinkel 1967:28);
- (i) 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 situation upon the occasion that the necessity for a decision arose" (Garfinkel 1967:41).

In all these cases "a procedural description of such symbolic usages is precluded by NEGLECTING THE JUDGEMENTAL WORK OF THE USER" (Garfinkel 1967:71, emphasis and upper-case added), where 'user' refers not only to the subject or informant, but to the anthropologist as well.

- (j) Stability of response across informants is a favorable indicator in the eyes of the ethnographer. Recall Siverts' (1966/67) assertion 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 ...?" and replies "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 1967:70; Wittgenstein 1958) ? These questions are not addressed by ethnosemantics but its eliciting and analysis are predicated on answers to them. Answers *are* made, but tacitly, common-sensically. Common-sense work is at the heart of the ethnosemantic enterprise. Note has already been taken of the unexplicated grounds by which eliciting is said to elucidate unsystematic questioning, by which non-interview events are said to elucidate interview events. Here again one must point to the commonsense 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 ?

If Chapters Two and Three provided a general, formulaic answer grounded in sociology, and if this last section has given an answer in terms of the methodological practices of ethnosemantics, then to answer the question in finer detail one must turn to empirical materials. Such are the substance of Part Two. (See also Eglin, forthcoming b.)

5. CONCLUSION

Using the distinction between brute and institutional facts I distinguished three kinds of sociology by the objects they seek to account for. Positivistic sociology seeks to explain regularities by theories. Semantic sociology seeks to explicate constitutive rules in grammars. Interpretive sociology seeks to account for interpretational instructions by a method yet to be determined (Epilogue; cf. Coulter 1974:104). The first enterprise depends upon the second enterprise which depends upon the third. Into this scheme were introduced two approaches the goal of which is an adequate description of cultural competence - ethnosemantics and ethnmethodology. By identifying semantical and constitutive rules ethnosemantics was cast as a kind of semantic sociology. Because of the documentary method and the *et cetera* clause I proposed that ethnmethodology is a kind of interpretive sociology. There followed a detailed methodological critique of ethnosemantics from ethnmethodology. Ethnosemantics fails in relation to ethnmethodology just as semantic sociology fails in relation to interpretive sociology - they neglect the interpretational foundations of their semantic rules. A number of conclusions can be drawn - (a) about scientific sociology, (b) about sociolinguistic (semantic) theory, and (c) about both in terms of the immediate subject of this work, ethnosemantics.

(a) 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 common-sense explanation and description. The model of an instruction is a formula

for the operation of commonsense. By showing that instructions underlie constitutive rules which underlie regularities, I hope to have shown, admittedly indirectly, that commonsense work is foundational for the practice of science, specifically scientific sociology.

(b) 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, I 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), and mean by it that such selection is domain-governed. As seen in Chapter Four domains do not solve the problem of context. Furthermore, it is no improvement simply 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 argued throughout, an adequate sociological pragmatics needs be interpretive.

(c) Both these points are present in the critique of ethnosemantics in Chapter Four. The nub of this critique is that ethnosemantics leaves out of account the interpretive work of societal members, including that of its own practitioners. In the light of ethnomethodology, ethnosemantics 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 socio-logical 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 a further elaboration of that world, a production rooted in that world. As such, ethnosemantics becomes another case for ethnomet hodological 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

6. INTRODUCTION

To give body to the programmatic and critical argument of Part One, two empirical studies are presented. The first study is a conventional ethnosemantic investigation, the second an ethnomethodological inquiry into the first. The study by Black and Metzger (1965) of American lawyer terms provides the model for the ethnosemantic study - on terms for Canadian doctors. The strategy of turning a conventional study on its head has been used before in many dissertations by students of ethnomethodology. Mention of two such studies, Wieder (1974) and Crowle (1971), 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" (1974:43). 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, and the study an example of semantic sociology.

However, by taking a mental step 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 1974:223-24)

More simply, "Instead of 'predicting' behaviour, [a code] rule is actually employed as an interpretive device ... but is *experienced* as predictive" (Wieder 1974:197). Finally, the convict code is "much more a *method* of moral persuasion and justification than it is a substantive account of an organized way of life" (Wieder 1974:175).

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 positivistic sociology.

However, he then re-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, un-

explicated 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)

My work resembles Wieder's study in being an ethnography, Crowle's study by focusing on interviews, and both studies by having two parts, where the second part is a re-analysis of the first part. Chapter Seven describes the ethnosemantic 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.

It needs emphasizing that neither part is an investigation of the *use* of doctor terminology. One does not learn how to use a tool or word by interviewing their users *about* that use, but by coming to master the use of that instrument oneself and then explicating that competence, or, at least, by closely observing the use and then explicating its happiness conditions. Turner (1970b) has done that for a small segment of English disease terminology in direct contrast to the method of Frake's (1961) ethnosemantic analysis of Subanun disease terminology (cf. Wootton 1975). Cicourel (1976), in a now classic study, has revealed some of the interactional uses by police and probation officers of the categories 'criminal' and 'sick'. Insofar as ethnosemantics fails to discover the use of terms through formal eliciting (Siverts 1966/67:332), then any investigation into eliciting itself will similarly fail. What is revealed, however, by the latter study is something of the nature of doing ethnographic semantics. This is seen to depend on how to do interviews (Cicourel 1974; Turner 1976; Smith 1978), and this in turn on how to talk. Such is the critical and positive topic of Chapter Eight.

7. TERMS FOR CANADIAN DOCTORS - ETHNOSEMANTICS AND TAXONOMY

After a brief description of the Black and Metzger study of American lawyer terms (the Lawyers Study), I shall present the methods and results of the 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 1 reproduces one line of Table One of the Lawyers Study - the chart of lawyer terms.

TERM	DIMENSIONS									
	<i>Kind of term</i>		<i>Settings</i>		<i>Kinds of practice</i>					
	1.1		2.1	2.2	3.1	3.2	3.3	3.4	3.5	
Defense Lawyer	B		BC	AB	AcB	Ab	A	Aa	C	

Source: Black, Mary, and Duane Metzger, "Ethnographic description and the study of law", p.157.

TABLE 1: ONE LINE FROM THE CHART OF REFERENCE TERMS FOR LAWYERS

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 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 and was carried to a similar stage of analysis.

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 2 gives the principal (ethnosemantic) results of the Doctors Study, and is modelled on Table 1 of the Lawyers Study. Table 3 is the key to the symbols in Table 2, the Lawyers Study again providing the model.

	1 KIND OF TERM	2 KIND OF WORK	3 BASIS OF SPECIALTY
physician 1	A	0	0
doctor	A	0	0
physician 2	Bc	B	0
surgeon	Bc	A	0
general practitioner	Bb	B or 0	Ga
family physician	Bb	B or 0	Ga
public health doctor	C	B	Gb
psychiatrist	C	B	D
neonatologist	C	B	Bb
perinatologist	C	B	Ba
paediatrician	C	B	Bc
internist	C	B	Bd
geriatrician	C	B	Be
cardiologist	C	B	Aa
dermatologist	C	B	Ah
gastroenterologist	C	B	Ai
endocrinologist	C	B	Aj
nephrologist	C	B	Ak
renologist	C	B	Ak
neurologist	C	B	Ac
physiatrist	C	B	Ae
rheumatologist	C	B	?
respirologist	C	B	Ab
haematologist	C	B	Am
anaesthetist	C	B	Ea
radiologist	C	B	Eb
pathologist	C	B	F
obstetrician	C	A	Ba
gynaecologist	C	A	C

ophthalmologist	C	A	Ad
urologist	C	A	Ag
otolaryngologist	C	A	Af
cardiovascular surgeon	C	A	Aa
thoracic surgeon	C	A	Ab
neurosurgeon	C	A	Ac
orthopaedic surgeon	C	A	Ae
plastic surgeon	C	A	Ah
interne	Ba	0	0
resident	Ba	0	0
fellow	Ba	0	0
certificate	Ba	0	0

"0" Not relevant

TABLE 2: CHART OF REFERENCE TERMS FOR CANADIAN DOCTORS

With two sets of exceptions the dimensions and values given in Table 3 are sufficient to uniquely discriminate all the terms in Table 2. '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, interne. 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'.

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
-

TABLE 3: CODE OF SEMANTIC DIMENSIONS

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 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. I have not drawn on this distinction in specifying the dimensions of Tables 2 and 3; the goal there was merely to discriminate all the terms (while preserving the *emic* distinctions). However, it is

included 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. (a) 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 the specialty names displayed in Figure One stand as lexemes.

(b) 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 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.

(c) 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. (d) Not all taxa are uniquely labelled. The domain of medical doctors has the familiar problem of the same term occurring at differ-

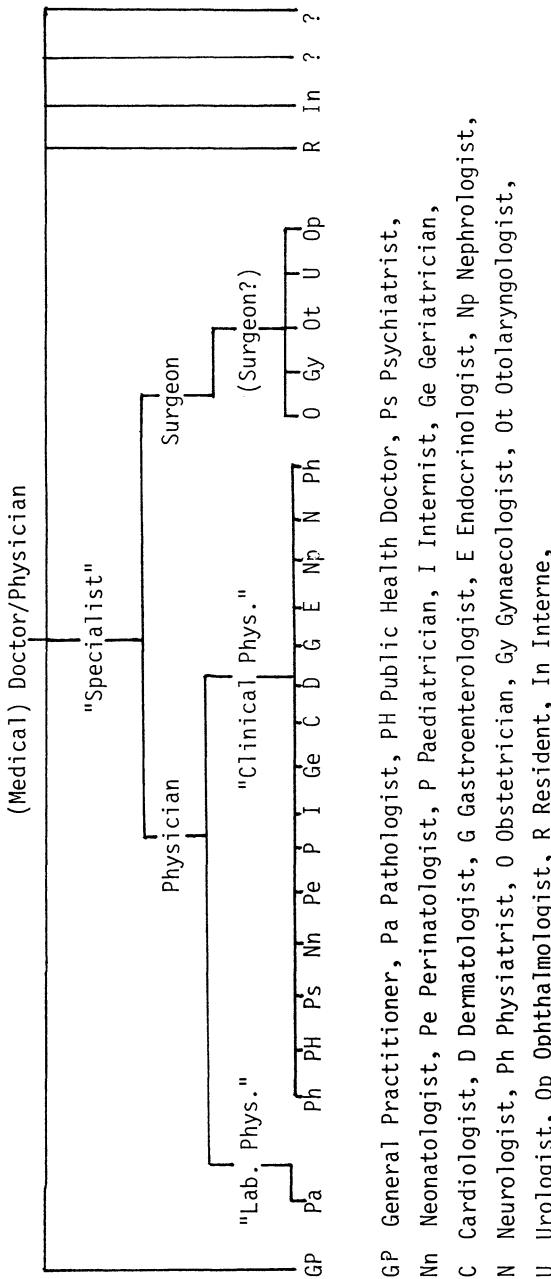


FIGURE 2: PARTIAL TAXONOMY OF TERMS FOR CANADIAN DOCTORS

ent 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'. (e) 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, 1975). (f) While the discriminations portrayed in this semantic arrangement are ones attested to by informants' responses, this particular formulation 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; I take it up in Chapter Nine.

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 himself /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

practise in most or all systems of the body.

This division may attach only to hospital settings and to those who practise there. The g.p. who served as informant did not reproduce this classification, either spontaneously or when asked. In addition, there is the specialty, recognized 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 3 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. I 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 this case adequate for the purpose. 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, objections in the chapter following that.

8. TERMS FOR CANADIAN DOCTORS - ETHNOMETHODOLOGY AND TALK

The great bulk of sociological and anthropological data is gained through *talk*, yet little is known about this principal research method of social science. This is true of the standard form of the talking method, the interview. This is not to say that social scientists do not know how to interview. On the contrary, they are (often) experts. It is that knowledgeable practice 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; see Becker 1956 for the medical students case). But what it is that is tacitly known in knowing how to interview is explicitly unknown. For knowing how to interview derives from knowing how to talk. Only in the last fifteen years or so, with the pioneering work of Harvey Sacks (Coulter 1976), has talk been studied systematically.

Ethnosemantics is known for that interview method called formal eliciting. Though not its only talking method it is the intendedly most 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. (Frake 1964a:133)

- it must be said that it is not clear how anything more would improve on the interview product, nor how that 'anything more' is itself done. In contrast to the critique of formal eliciting in Chapter Four, this chapter develops that critique in terms of empirical data. Whereas in Chapter Seven the interview talk was seen and used as a semantic data-

generating device, here it is treated as a topic in its own right, namely, a form of sociolinguistic interaction closely akin to conversation. Given certain characteristics of that interaction serious questions arise about the ethnosemantic results.

At least two kinds of critical material can be found in the interview transcripts. By reading on the 'semantic surface' one can find numerous substantive variations, inconsistencies and doubtful data; that is, informants appear to differ about the meaning of particular words. Such items can be treated in two ways: in terms of them one can engage the semantic ethnographer in a debate directed at the repair of the original analysis; or one can see those 'problems' as pointing to the essentially ad hoc nature of that analysis. I shall try to illustrate this later in the chapter. By reading below the surface can be discovered another order of data to do with the structure of talk itself. At this level what appears on the surface as mere variation acquires some motivation. Here talk is not 'mere talk', that which in popular thought (cf. Turner 1972:388) is the opposite of 'action', but is indeed a rich species of linguistic, social action (Austin 1962). Then, not just action but interaction, the phenomenon brought off by actors working in concert, the interaction in question being the ethnosemantic interview. An interactional analysis of such a socio-linguistic phenomenon may be thought of for heuristic purposes as having three components: a transcript of the interview organized into turns at talk, a description of those turns as actions such as complaint, excuse, hint, insult, invitation, etc., and an analysis of how, for the actors, the utterances in those turns are recognizable as performing those actions. In conversational-interactional analysis to date (see Schenkein 1978, Psathas 1979) that has involved displaying the sequential properties of those utterances-as-turns.

In Chapter Three I showed how actors use the conventional sequential structures of conversation as a means of building their talk as interpretational instructions telling each other what actions those utterances are hearably performing. While interviews are a species of

talk they are not simply conversations, in that

The turns which an 'interview system' organizes alternately are 'questions' and 'answers'... [whereas] the turn-taking organization for conversation makes no provision for the content of any turn, nor does it constrain what is (to be) done in any turn.
(Sacks et al. 1974:710)

It is not to be expected then that the devices which organize the three levels of conversational structure - turn-by-turn structure, topic structure and overall conversation structure - should automatically be replicated for interviews. Consequently, while the analysis to follow will illustrate the use of the three-component model it does so less systematically than might be hoped for. It is more a series of remarks than a rigorous analysis. In only partial defence I would still hold that "our work is now both too empirical to 'follow from' a theory of society and too young to propose one" (Moerman 1972:198).

The 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 transcript 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 discussions, 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 without saying.²³ 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, notably the paramount significance of work/not work as a scheme of interpretation. 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 (see Turner 1976:242). This reappears below.

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 I 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 1967:70). 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 heard 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 ethnosemanticist 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 know-

ing quite what I want, the informant exploits his list-making capacity to produce a candidate answer-to-my-question (Turner, 1976:242). 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, than 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 semantic ethnographer) 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 'pre-coded entries on a memory drum' but as elements in a *formulation* (Schegloff 1972b:80) 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 sub-routines 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 1974:53; emphasis added; cf. Perchonock and Werner 1969:238)

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:

- (54) I: Oh really ?
(55) E: Yeah.
(56) I: Oh my lord.²⁴

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.

Formulating-and-waiting may be a general feature of interviews, especially where the interviewer stands to the interviewee in the relation of professional to layman. These two categories, Sacks proposes (1972:39, 61-63), comprise a membership categorization device called 'K'. Examples are SPC staff member - suicidal caller (Sacks 1972),

police-caller (Schegloff 1972a), therapist-patient (Turner 1972; 1976), doctor-patient (Balint 1957; Cicourel 1974) and teacher-student (Eglin 1979b). In studies of interviews or quasi-interviews between these parties consisting predominantly of Q-A sequences, it is frequently reported that the interviewee (layman) bids or offers illnesses or troubles as candidate accounts for the interviewer (professional) to inspect for their propriety to his business. For example in one case of a first interview between a therapist (T) and would-be patient (P), Turner claims that the interview is controlled by the opening diagnostic question "What brings you here?". Given that the occasion is a psychiatric interview the question asks for an account which shall warrant P's acceptance or rejection as a patient.

Though P may not know what will constitute the right kind of 'problems', or a problem of sufficient magnitude, she is nevertheless obliged to offer some characterization of those problems for T's diagnostic scrutiny. (Turner 1976:242; cf. 1972:385)

In this case P has two unsuccessful go's at formulating her troubles before T picks up on her third bid (Eglin 1979a).

Because of the way K distributes authority between its categories in favour of the professional, authority for the results obtained in interviews rests with the interviewer (though, as will be seen in Chapter Nine, there is a further twist in the ethnographic case). And this would seem to be true of inquiry in general, whether the interrogated be client, patient, student, subject or respondent (the case of the informant being, again, special). Authority for answers rests with the questioner.

Ethnographers, like other scientists, generally recognize this, turning to tests to secure their claims against the charge of arbitrariness. It is not clear, however, that their attempts in practice to validate findings work any better than the argument in principle I examined in Chapters Two (Positivistic sociology) and Four (Indexicality).

Validation and variation

In what might be called 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.²⁵ 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, ethnosemantics must presuppose shared knowledge in order to find it.

One tack that can be taken in a critique of ethnosemantics 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 neonatologists perform 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; Schegloff 1972b:432-33, fns.15 and 16). Consider the following two cases consisting of a two-utterance exchange and a sign in a bookstore window advertising a sale.

- (a) A: Do you want a coffee ?
B: No, I've just eaten.
- (b) BOOKS AND PAPERBACKS

These naturally occurring events were noticed by me under a viewing rule derived from ethnosemantics. 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).

Let me explicate this claim by analyzing, albeit briefly, the exchange in (a) (Eglin 1980). In so doing I can exemplify the three-component model of conversational analysis. The material ((i) the transcript) is an imperfect fragment since, though naturally-occurring, it was noted only in memory and without the surrounding particulars. However, it is by its nature self-contained so permits some analysis (cf. Schegloff 1972b:80).

On the face of it, and in terms of substantive cultural theories, (a) contains a 'mistake'. Since coffee is not eaten but drunk B should have said something to the effect of having already had a drink. He evidently has his semantic domains crossed. The following problems of this brief analysis may be noted: it fails to account for the 'acceptability' of the exchange, that is, that it was brought off without remark, challenge, bemused looks or request for clarification; it constitutes the speech of B as mistaken, and runs the risk of standing in a corrective position to members' talk; and it overlooks some interesting structures of the exchange when conceived as interaction (Turner 1970b; 1977). Two of these devices are worth attention - the adjacency pairs, question-answer and invitation-declining ((ii) the actions). In fact in this case the invitation is made in the form of a question. That B construes A1 as an invitation is evident in his offering an excuse or justification in addition to his rejection; for in declining an invitation one may wish to avoid being rude; one may wish to be invited again on another occasion.

I have been saying A1 is an invitation, and not simply an offer (though only some matters hang on the difference), because that is the way I remember it - here, clearly, the data are weak. Allowing that it is an invitation, an invitation to what? The answer is something of the order 'taking a break'. That is, it is at least an invitation to that sort of small social encounter signalled formally by drinking coffee or the like. In this context "... I have just *eaten*" is hearable as saying that B has had his break, perhaps even a meal, which may or may not have included a drink. In this way he provides accept-

able grounds for turning down the invitation((iii) the analysis).

If this is an interactional analysis, a piece of pragmatics, then it organizes how the two lexical items in apparent conflict, 'a coffee' and 'eaten', are to be heard. It relates both to the *social occasion* rather than physical process of eating and drinking. And this is true not simply for the observer but for the co-participants themselves. That is, by virtue of the conversational slots or places made available by the invitation and question adjacency pairs they can bring off a *hearably* acceptable exchange. This last point is crucial, as it points to the phenomenon of reflexivity and the question of the very possibility of a principled analysis of conversational interaction. This I will take up in discussing interpretive method in the Epilogue.

While, under some supposedly objective standard, I could cull from my interview data similarly 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 semantic 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,²⁶ for these purposes and with these intentions. (Austin 1962:144)

For many practical purposes correctness is not only not important, but to insist on it is to be seen as incompetent (Garfinkel 1967:36-75). 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 1967:10) for some practical purpose. For the intern, the Royal College and Revenue Canada, who gets to be called²⁷ 'doctor' is at some times, 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* (Garfinkel 1967:24). They are, to cite informants' remarks, "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. The latter's business can and does go on irrespective, in an important sense, of terminology (cf. Garfinkel 1967:186-207).

9. CONCLUSION

It may be objected that to mount a critique of ethnosemantics based on the ethnography of Chapter Seven is to cut down a straw man. Surely, the objection might go, the lexemic description of some of the composite terms needs improving; inadequate account is taken of polysemy; formal eliciting is, after all, only a discovery procedure and the good semantic ethnographer pays as much attention to the context of justification; the good ethnoscienctist does not rely on interviews alone but supplements them with observational data; the good cognitive anthropologist would not be content with a few hours of recorded talk but would remain substantially longer in the field (see Stoddart 1975). These are fair objections given that while the emphasis has been on eliciting I have written throughout of ethnosemantics as a whole; I have been addressing all of cognitive anthropology, not just data acquisition (Tyler 1969a:Section 2).

Rather than inviting the charge of partial treatment my focussing on eliciting is a way to encounter ethnosemantics at its strongest point. For here inference, rule and arrangement are tied to data in an intendedly rigorous way. If the validity of systematic eliciting can be undermined little hope resides in unsystematic questioning and observation. But, the objection continues, is not to argue so to fall into the same inductive trap laid beneath the feet of ethnosemantics in Chapter Four ? Is not the manner of discovery fundamentally irrational (Burling 1969) ? Does not confidence in findings rest on the severity of tests ? While few ethnographers are explicitly Popperian, none would

likely deny the importance of justification. But, as I argued in Chapters Two, Four and Eight, tests are social occasions in their own right, and are thereby no more immune from relying on interpretive procedures than are the acquiring and analyzing of data the validity of which they purportedly evaluate. Tests will not rescue the enterprise.

But, even so, is not the analysis of Chapter Seven too cursory, if not slipshod, to sustain a meaningful critique? Can it really stand for the work of Goodenough, Frake, Berlin and contributors to the American Ethnologist?

To object thus is partly to miss the point. Let us suppose that the lexical descriptions were more precise, that polysemy was precisely shown, that the distinctive features of the lexicon were fully and clearly defined, that some attempt was made to depict individual variation. To attempt this the ethnographer would spend more time in the field. In search of precision he or she would sooner or later turn to the medical dictionaries and the handbooks of the Royal College. From these key informants a relatively clean taxonomy could be assembled, and individual variability could be measured from it. But what would these results be? Firstly, they would picture a knowledge probably no single person possesses, and so would be irrelevant to what persons do need to know to act appropriately. Secondly, they would fail to show that members can be held accountable for what they (should) know (Turner 1976; Sharrock 1974); that, knowledge being socially distributed (Schutz), some members will accountably and authoritatively (cf. Hays 1976:506, fn.6) know things others can rely on them to know; that others need not know those things but only how to get to know them when necessary. Thirdly, to conceive individual variability as 'error' from the master scheme misconceives the relationship of authority: one does not need to know today what can be looked up in a dictionary or the headman's head tomorrow. This is the burden of the discussion of Sacks' K in Chapter Eight. K holds normatively for matters medical (Garfinkel 1967:186-207) including knowledge of the terms for the different kinds

of doctors. Not only does K organize who may properly know those terms but who may properly not know them, or know them only partially, in a here-today-gone-tomorrow sort of way. It is not clear that ethnosemantics would produce such a finding other than by accident.

What then 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; recall the quote from Searle on p. 14).

What is to be made of terms such as neurologist, paediatrician, etc., that are elicited in interviews depends upon the interactional structure of interview talk and the membership categories of the parties in relation to the topic of inquiry. While for social-scientific knowledge the academic interviewer plays the professional to the interviewee's layman, yet for the ethnographic interview it is usually the interviewee-informant who is the expert in the domain of knowledge being investigated. One gets, then, the novel situation where each party to the interview must rely on the other to decide the adequacy of the elicited information. Put a little differently, in abstracting terms from the informant's utterances the naive ethnographer-questioner is deciding what is a good answer and what can be taken from such an answer, while at the same time he depends on the informant's understanding of what he is about so as to be able to treat the informant's answers as naive 'conditioned responses'.

This would seem to be a special case of Schutz's point about the interlocking of motives in ideally rational discourse:

if I project a rational action which requires an interlocking of my and the Other's motives of action to be carried out ..., I must, by a curious mirror-effect, have sufficient knowledge of what he, the Other, knows..., and this knowledge of his is supposed to include sufficient acquaintance with what I know. This is a condition of *ideally rational interaction*...

It seems that under these circumstances rational social inter-

action becomes impracticable even among consociates. And yet we receive reasonable answers to reasonable questions... (Schutz 1967 a:31-32).

To the extent that the ethnographic interview approximates ideally rational discourse, to that extent it becomes senseless. To the extent that it produces scientifically rational results, to that extent it has relied on its participants' use of the commonsense concept of rationality (Eglin 1979a), a competence ethnosemantics specifically fails to discover.

Part of what one has to know or believe in order to operate in a manner acceptable to a society's members are just such features of commonsense knowledge as how to answer a series of questions by formulating-and waiting, how to use *K*, how to list, idealize, abstract, validate and talk against an indefinite background of assumed common knowledge. 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 selection of one term over another is not done from some definite semantic domain but from collections organized as the occasion demands in pragmatically well-formed ways (Sacks 1972; Schegloff 1972b; Woottton 1975:37-42). The invoking and use of such collections and their arrangements is the accomplished production of skilled ethnographers such as medical doctors and other anthropologists.

EPILOGUE - THE QUESTION OF INTERPRETIVE METHOD

The 'machinery', because it is members' 'machinery', in the way it is specifically used to do [accountably rational activities] is thereby part of the phenomenon as its production and recognition apparatus. (Garfinkel and Sacks 1970:358)

Since Weber launched interpretive sociology in 'The Fundamental Concepts of Sociology' (Weber 1968 [1925]) there has been, Schutz and Giddens (1976) notwithstanding, no fully satisfactory treatment of interpretive method. I would argue that this need cause no great concern since reflexivity ties analysis to the phenomenon in a unique way: however it is that phenomena are analyzable by members, so are they available for analysis by observers. This ties analysans to analysandum in a tight and special way whereby the former is also included in the latter. There need be no general method, nor puzzlement over first-order and second-order constructs.

To make the case consider again the two-utterance exchange from Chapter Eight:

A1: Do you want a coffee ?

B1: No, I've just eaten.

I said in that chapter that by virtue of the conversational slots made available by the question and invitation adjacency pairs A and B can bring off a *hearably* acceptable exchange. 'Hearably' introduced reflexivity, described as follows in Chapter Two: somehow members (speaker-hearers) instruct each other how particular occurrences (utterances) are to be taken (heard) *in the very course* of enacting (uttering) the

particular, that is, without coming out and saying so in so many words (Garfinkel and Sacks 1970:344). Now, the use of conversational slots (or positions or locations) is reflexive. For example, how is 'No, I've just eaten' to be heard as 'answer/declining invitation'? Notice the speaker does not supply an accompanying description to label it so (Sacks 1963; Wieder 1974:164-66). It is hearable so by virtue of its being sequentially positioned 'after' an utterance which is thereby constituted as a question/invitation. This is not trivial, as the case of insertion sequences indicates (Schegloff 1972a:363-70; 1972b:76-79, 106-114). By being a candidate answer and locating itself in the position that an answer should be in if it wants to be heard as such, it refers, reflexively, to the device that makes it so hearable. To cite Sacks again,

What you get is, there are actions which for them to be effective need to be formulated via some particular Device. And then those actions invoke that Device. That is, the utterances which might do those actions invoke that device. (Sacks 1976:G6)

This is of the most profound importance. It is so in at least the two following ways. First, it is in this way that social interaction (conversation) is analyzable, and thereby producable (and reproducible),²⁸ *in its course by the members doing it*. It is in this way (better, these ways) that members construct themselves and each other, own and others' actions, and the rest of the world and its furniture as the particular persons, actions and world they recognize them and it to be. The second important feature of reflexivity, and the main point here, is this:

The analyzability of conversation by *professional students of language use* depends upon the analyzability of conversation in its production, and over its course, by *participant conversationists*. (Turner 1976:233, emphasis added; see Schegloff and Sacks 1974:234; Garfinkel and Sacks 1970:358).

Because members analyze their concerted interaction in its course as a condition of its very production, because in so doing they *exhibit* that analysis for their own and others' use, because that reflexivity

is procedurally *visible*, because it entitles, obliges, demands and requires that it be attended to and respected as a condition of the orderly pursuit of everyday life, that achievement is analyzable by members who are linguists, sociologists, philosophers and the like (Garfinkel 1967:vii; Eglin 1980). The sole difference between members' analysis and professionals' analysis is that the one is tacit, the other explicit. Because the professional account has otherwise the same character as members' instructions for producing orderly interaction it achieves no more certainty than members' methods do. In the end it can do no more than show (Wittgenstein) or display (Mehan and Wood 1975) the phenomenon it is an account and further instance of.

FOOTNOTES

1. Quotation marks appear as " " if direct quotes or data are placed within the text, and ' ' if terms are mentioned rather than used, or used in a technical or otherwise unusual sense. Square brackets enclose my insertions in otherwise quoted texts.
2. By locating ethnomethodology within 'interpretive sociology' I do not, of course, mean to suggest that it subscribes to the notion that some special method of the observer called empathy or introspection distinguishes interpretive sociology (Garfinkel 1967:94-95, fn.6). Rather is ethnomethodology indebted here to Schutz who corrected Weber on this, saying that 'interpretation' was something done by *all* social actors, not simply by scientific observers, and that precisely such interpretive work was a necessary *topic* of study (Eglin 1979a:12; forthcoming a).
3. A host of sins is covered by my blanket use of the term 'values', I realize. But they are both incidental to the main argument and do not, I believe, make useless this simplified picture of sociology. Runciman (1972) is useful on Weber who started it all, Fay (1975:15) sorts out some of the various questions subsumed by the issue of values, while Louch (1966:50-60) is essential beginning reading for anyone who would seriously tackle the fact-value dichotomy.
4. Proponents of interpretive sociology and ethnomethodology have sometimes collected non-interpretive sociologies under the rubric 'normative' (Wilson 1970a, 1970b; MacKay 1974), partly thereby to show that those sociologies are continuous with the 'practical sociological reasoning' (Garfinkel) of lay persons.
5. But see now Latour and Woolgar (1979) which appeared after the manuscript was completed.
6. I must put aside for another time and place discussing the difficult issue of whether interpretation is involved in following a rule (Wittgenstein 1958: paragraphs 198,201; Kenny 1975:173; Giddens 1976: 74; Sharrock 1978). See footnote 21.
7. "The object itself [tables, chairs...], the cultural object so defined and constituted, may thus also be understood as yielding sets of instructions for how to act towards it, how it may be inserted into

human programmes of action" (Smith 1978:46).

8. 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 ethnosemantics see Wieder (1970: 118, 120).

9. Searle's semantic theory goes well beyond that of ethnosemantics at this point (1969:25), and in a laudable direction (Turner, 1970a; 1970b). But both remain linked in terms of constitutive rules, and these are the focus of my critique. Footnote one on page 36 of *Speech Acts* suggests that Searle might object to my equating of semantical and constitutive rules, but he does not develop the point.

10. 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 (1973) and Heap (1975), to mention just two items in a growing literature.

11. My 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 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) and Schwyzer's (1969), is more radical - as I try to show further on in the text. For a related critique of constitutive-rules accounts see also Coulter (1979:184-185, fn.19).

12. A version of this chapter appeared in *Semiotica* (Eglin 1976). It has benefitted immensely from two papers: first, of course, Wieder's critique of the sign theories in ethnosemantics (Wieder 1970; see also Turner 1970b); and, second, Helmer's programmatic paper on a pragmatically-oriented, non-Humesian sociology of language (Helmer 1970; see also Kjolseth 1972:53). Both Wieder's critique and mine 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, I focus on the methodological assumptions and practices of ethnosemantics 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.

13. Frake's sentence appears to be the only attempt to provide a (relatively) explicit formula for the dependent variable in ethnosemantics. In sharp contrast to Frake (1964a:133), Kay, alone, claims the possibility of predicting actual behaviour as opposed to verbal judgments (Kay 1970:28). By the mid-sixties Chomskyan rhetoric - 'structural description', 'reading', 'competence', grammar as predictive theory

- was well in evidence in ethnosemantics (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 difference between ethnosemantics and transformational generative grammar (cf. Hymes 1964a, 1970a).

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

15. 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).

16. The course of this reification is traced through Morris's work in the longer manuscript from which this chapter is drawn (Eglin 1972).

17. The technical problems of the concept of domain 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). The latter informs my critique below.

18. 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).

Other workers in ethnosemantics have been less clear than the formal elicitors about method. While not *in fact* proceeding inductively, they have nevertheless (!) written 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 1972:32); 'The features were thus derived inductively, based on a detailed scrutiny of the data and on intuitions gained from field research" (Seitel 1974:52) - but (2) not examined their methods or

shown 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.

19. Contrast the calls for (1) '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 (1975), Katz (1975), Wieder (1974), (2) Crowle (1971), Cicourel (1974), Turner (1976) and (3) Garfinkel (1967) and Turner (1974) respectively. See Chapters Six and Eight.

20. I 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.

21. 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 1967:74, fn.13). Also, the problem of social order in question is not the Hobbesian or *social* problem of (moral, or normative) order, at least not in the first place, but the *cultural* problem of "order in the symbolic systems which make communication possible" (Parsons 1951:36, quoted in Giddens 1976:172, fn.10; see Eglin 1970:10-13; cf. Sharrock 1977:553; Zimmerman 1974). Furthermore, the 'problem' is to be understood as an analytic rather than a concrete one. I do not mean to suggest that, for members in their unreflective everyday activity, indexicality presents troublesome problems (Schegloff and Sacks 1974:234; Schegloff 1972b:432, fn.4). On the contrary, as Sacks (1976) has shown, the indexical indicators (pronouns, demonstratives, etc.) are an interactional resource rather than a source of problems. Only for the 'overhearer' (Schegloff 1976), including the armchair analyst, does indexicality-as-ambiguity arise (Eglin 1977).

22. Other than in the writings of ethnomethodologists, the notion of 'index' can be found, persistent but largely untreated, and under various names, in semiotics, philosophy and linguistics (Garfinkel and Sacks 1970; Bar-Hillel 1970a:198-199). Here is just a sample of references, including a couple from sociology and anthropology: Peirce (1932:143, 170-172), Morris (1938:17-25), Dewey (1946), Reichenbach (1947:4-6), Burks (1949), Kecskemeti (1952:75-78), Bar-Hillel (1970a), Jakobson (1956:61, 66; 1971a:131; 1971b:346-347, 357-358), Lounsbury (1960:123), Szasz (1961:115-116), Weinreich (1966:154-158; 1968:166), Fillmore (1966:220), Wells (1967:104), Lyons (1968:275-281), Friedrich (1971:170), Bauman (1973:5).

Garfinkel and Sacks (1970) detail indexicality's intellectual

history in philosophy and logic, including the discussions by Husserl, Russell and Goodman. Its treatment in semiotics 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 ethnosemantics 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): "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 1967:11; cf. 1972b:309). See footnote 21.

23. Though not *literally* without saying. See Turner (1972) for an interactional analysis of 'prefatory work' and 'starting' in therapy sessions. Turner takes off from Pike's (1967) unexplicated noticing of the 'assembling' done prior to a church service.

24. 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.

25. Or perhaps an 'interruption', 'delay', 'interesting interlude' or whatever.

26. In contrast, compare Black (1969:187, fn.7).

27. By 'called' I mean 'referred to as', not (necessarily) 'addressed as'.

28. That reflexivity is tied to the reproducibility of social interaction is clearly a claim that needs some explication - more than I can offer here. On reproducibility the interested reader should consult Sacks (1972:31, 36), Turner (1970a:177), the critique of the latter by McHugh et al (1974:121), and Turner's reply (forthcoming). Reflexivity and reproduction are central concepts in the recent tour de force by Giddens (1976). His definition of reflexivity as 'self-reflection' (8), 'self-awareness' (17) and 'self-monitoring' (114) seems uninteresting, and is certainly different from the concept in use here and in ethnomethodology generally. Closer to the latter is his notion of the 'duality of structure' by which he means "that social structures are both constituted by human agency, and yet at the same time are the

very medium of this constitution" (121). Structures come into being by 'structuration', that is "the reproduction of practices" (121), so that "to enquire into the process of reproduction is to specify the connections between 'structuration' and 'structure'" (120, emphasis in original). If Giddens will allow that 'social structures' embrace the interactional structures found in conversation by Sacks and co. then the latter's studies over the last fifteen years (Coulter 1976) provide the sort of vigorous, 'empirical' analyses of the reflexivity and the reproduceability of social interaction that his (Giddens') phrases (for example, Rule B TWO, p. 161) appear to call for.

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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
<u>word</u>	Stress
<u>word</u> -	Said deliberately, almost staccato-like
—	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.
- (7) E: O.K. Well could we start there ?
- (8) I: Sure.

- (9) E: O.K.
- (10) I: Uhm (Pause) /
- (11) E: I'm not going to write just
- (12) I: uhmm 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, uhmm 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.
- (13) E: Uhuh well uhmm where do you fit into that yourself ?
- (14) I: Well as you mean as resident ?
- (15) E: Yeah o.k.
- (16) I: Uhmm 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 uhmm
- (17) E: Uhuh
- (18) I: 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 uhmm what would be the other kinds of things you could have done had you if you hadn't chosen what you what you will do ?
- (20) I: Oh I see well like uhmm 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: uhuh
- (24) I: becoming a paediatrician
- (25) E: uhuh
- (26) I: or becoming a uhmm what they call an internist or you know uh connected going through a specialty connected with in-

- ternal medicine.
- (27) E: uhuh
- (28) I: There's so many fine grades like you know the like uh uh paediatrician is a type of physician
- (29) E: uhuh
- (30) I: uh an internist is a type of physician uh and then 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 ?
- (34) I: Yeah.
- (35) E: O.k. uh this within uh what category was ?
- (36) I: Paediatrics
- (37) E: Within paediatrics // you have what
- (38) I: which is the one I chose
- (39) E: yeah
- (40) I: uh you'd have uh cardiologist
- (41) E: uhuh
- (42) I: probably (slurred) just at the beginning your whole list you'd have what you'd call your general paediatrician
- (43) E: uhuh
- (44) I: whooo it is does sort of uh consultant work and also does primary care medicine
- (45) E: uhuh
- (46) I: uhmm
- (47) E: and after that
- (48) I: and then you have your paediatric specialities (sic) cardiology uh
- (49) E: (whispered) specialist
- (50) I: uh cardiology, nephrology uh hematology uh gastroenterology uhm (aside) now we're getting down where I have to, uh neonatology
- (51) E: ha
- (52) I: uh and there uh there there's more uhm let me think

- (53) E: Try and be as exhaustive as you can
- (54) I: Oh really/
- (55) E: 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: uhuh
- (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: uhuh
- (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: uhuhuh uhuh
- (62) I: uhm and I don't know of any paediatric ear nose and throat specialists
- (63) E: uhuh 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 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
(85) I: haha
(86) E: or uh people who deal with specifically respiratory diseases
(87) E: uhuh
(88) I: there is uhm cardiologists as I mentioned deal with uh diseases of the cardiovascular system
(89) E: uhuh
(90) I: there is uhm gastroenterologists
(91) E: uhuh
(92) I: deal with uh disorders of the intestines and bo uh laver-num and stuff there is nephrologists deal with the uh kidney
(93) E: uhuh
(94) I: there's urologists that deal with the lower parts of the urinary tract uhm they might not like that they deal with the whole urinary tract but uh uhm there are uhm uhm as I mentioned orthopods *orthopaedics* deals with the skeletal and musculature abnormalities uh there is dermatologists deal with the skin
(95) E: uhuh

- (96) I: uhm oh my goodness (Pause) I wou I uh think I've run out
uhm
- (97) E: o.k.// if you think of any
- (98) I: I would say that
- (99) E: if you think of anything else
- (100) I: yeah
- (101) 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 paediat-
rics you could speciali 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 paediat-
rician 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 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 internal medi-
cine 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