THE PRODUCTION OF AN ETHNOGRAPHY:
SOME METHODOLOGICAL AND SUBSTANTIVE
ISSUES FOR ANALYZING SOCIAL SETTINGS

by

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ABSTRACT

This study seeks to provide an analysis of some of the features which underly any ethnographic description. First, it focuses on the daily routine of a community medical clinic in a large city in Western Canada, then it "looks back" on the methodological and theoretical issues inherent in the production of any ethnography. A daily routine known as "chart rounds" (a review of patients' medical histories) is examined in detail. That description itself then becomes a topic of inquiry in its own right.

The analysis rests on field observations conducted over a year and a half within the research setting. During this period the researcher was privy to medical examinations, to chart rounds, and to much of the ongoing routine of the Clinic. I was also able to tape-record various aspects of its organization. Most of the material which I have analyzed consists of transcriptions taken from tape recordings of doctor-patient interviews and of chart rounds.

Some of the issues which will be given special attention are (1) the beginning of the ethnographic report and the relationship of this section to the subsequent sections of an ethnography; (2) how it is that ethnographic descriptions are necessarily based in a set of common sense relevancies; (3) the use of 'talk' in interaction and as a source of data for "discovering" the self-organizing features of the settings and occasions from which this talk is collected; and (4) the relationship between ethnographic description and the
researcher in the research setting.

The research reported here is to be seen as exploratory and tentative. It is not intended as a manual for ethnographic researchers, but as an attempt to explicate some of the organizational features in the construction of an ethnographic description. No doubt it raises many more questions than it answers, but its purpose will be satisfied if it is able to generate some debate about the organization of ethnographies.
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INTRODUCTION

The data and observations which constitute the ethnographic section of this dissertation represent more than one and a half years of field work in a community medical clinic (hereafter called the Clinic) located in greater metropolitan Vancouver. During this period, I observed the ongoing activities of the Clinic, made notes, conducted and tape recorded interviews, etc. That is, I did those things which might be expected of "any" researcher who intended to produce an ethnography. However, this was not my primary intention and the findings which constitute the crux of this paper diverge widely from those of standard ethnographic practice for this paper is directed towards an examination of the problems inherent in the production of an ethnography rather than merely an elaboration of the features of the setting. Further, my findings are exploratory and tentative. This introduction is provided as an explication of some of the intellectual currents that run throughout this paper.

To begin with, I would like to make it clear that, when I began my research at the Clinic, I had no final product in mind. That is, although I initially intended to do an ethnography I did not intend to focus upon the problems involved therein. Further, I had no formal research proposal guiding my activities at the Clinic, and no specific hypothesis about the parameters that this dissertation would encompass. Instead, after spending considerable time at the Clinic, I found myself in possession of a large collection of field notes and audio tapes of various medical interactions, and was faced with the problem of how I
could best make use of them. These are but some of the ways in which my work differs from conventional ethnographic reports.

Before starting the present dissertation, I had constructed a detailed analysis of a daily clinical routine known as chart rounds. At this time, I assumed that this analysis would occupy a section of the final research report. (Although I did not have any conception of what the final ethnographic description would look like.) When it came time to address myself to the pragmatic task of writing a dissertation, I saw, for instance, that it was necessary to provide an initial description of the Community Clinic so that I would be able to place my description of the chart rounds in its proper ethnographic context. It was a concern with matters such as this, i.e., a concern with the features which make for an adequate ethnography that eventually shaped this present dissertation. The point to note, however, is that I had no interest in these matters prior to starting field work in the Community Clinic. It was only after I began the ethnography that I became interested in certain features of ethnographic description as features which were in themselves worthy of investigation. To this end, I decided to make an ethnography into a topic in its own right, and to utilize my own description of the Clinic as data in order to illustrate some of the pervasive features of ethnographic description.

Thereby, while the description of the Clinic presented in this report is intended as a bona fide ethnographic description, that description will later be treated as a resource for discovering (or better, "rediscovering") organizational features of an ethnography.
It is an attempt, necessarily an exploratory and tentative one, to move towards what Berreman has called a "sociology of ethnographic knowledge; an ethnography of ethnography."

To embark upon an analysis of my own description of the Clinic and to then turn that description into a topic for investigation is not equivalent to saying that such an analysis constitutes a special or privileged case because of my self-conscious awareness of the research experiences under discussion. Such an awareness plays no special part in my subsequent analysis of the organizational properties of ethnographic description. It is, however, a topic which occupies a prominent position in recent work dealing with ethnographies. Let us consider some of the recommendations that attention to this issue has generated.

In their article "The Emergence of Self-Consciousness in Ethnography" Dennison Nash and Ronald Wintrob conclude that:

(The) rise in interest in the subjective or personal factor in anthropological research appears to have been accompanied by a de-objectivation of the anthropological world view. By this we mean that field observations are less likely to be thought of as having the quality of objective reality and more likely to be conceived as relative to the observers personality, his experiences and his actions in the field. They point out that the ethnographer is inherently biased. Many social scientists maintain that such inherent biases should be brought into conscious awareness so that some remedy can be found for the errors that they cause. There is some dismay about the paucity of knowledge
concerning the ethnographer's actual field experiences. Consequently, it has been suggested that incorporating one's personal field experiences into the ethnographic report might make for a higher degree of clarity and objectivity. The relationship between the ethnographer and the subjects of his inquiry has also been cast into question. Particular attention has been paid to the type of knowledge the ethnographer can obtain vis-à-vis that relationship. For example, Robert Jay contends that:

The main point I am trying to make in this paper is that the relationships we form with the subjects of our work — for whatever reasons we settle upon those relationships — control the kind of knowledge that the material we gain will yield and also control how we exercise whatever responsibility we may feel to our subjects and to ourselves as persons. I have further been trying to show that there is a certain knowledge relevant for understanding one another in our own personal lives, in which we look upon one another as autonomous, mutually responsible selves, and that there is another kind of knowledge relevant to understanding our subjects as shaped and moved by extrapersonal forces allowing at most very limited autonomy or responsibility. The knowledge involved in each kind of understanding is not directly or even readily translatable into that of the other. That disconformity between our direct, personal sight of the universe and the sight that an objective science gives us has been at the root of much intellectual discontent in Western civilization for some time.

The ethnographer, then, is no mere reporter of socially observable phenomena, but an integral part of the description he produces. Jay seems to be suggesting that the ethnographer can make his report a more valid document of the setting by considering the types of personal relationships that he develops with his informants, and incorporating
his feelings about these relationships into his report. Jay goes on
to outline some of the possible advantages of this approach.

It is precisely here that I see the difference
between my position in anthropology and that of
many, perhaps most, of my colleagues. I want to
choose, with full awareness, to relate to my sub-
jects fully as persons, as I would to any other --
friend, colleague, student, chance acquaintance.
That choice determines the realm of knowledge I
shall be able to explore, and it is a realm for
which the concept of culture, and for that matter
of social structure, ecology, and the like (extra-
personal bases for explaining behavior) are of
only peripheral value.®

Another procedure which takes the ethnographer's bias into account
and attempts to bring that bias to the level of consciousness is the
analysis of anthropologists via the use of psychological experiments
and Rorschach tests. Dennison Nash, for one, feels that Ann Roe's⁹
psychological studies of anthropologists are a useful method for as-
sessing the individual anthropologists overall adaptability to what
he calls the role of "ethnologist-stranger":

In her project involving a number of kinds of
research scientists, Roe interviewed and tested
8 eminent anthropologists and gave group
Rorschach to 25 anthropologists on five faculties.
Her claim that "the research scientists studied
individually...show precisely the differences (on
the Rorschach) between fields that the subjects
studies in groups do" appears valid. In their
Rorschach responses, anthropologists are most
like psychologists and least like biologists
and physical scientists. However, it was not
possible to specify differences between dif-
erent fields of anthropology, e.g., between
archaeology, physical anthropology, and social
or cultural anthropology....(Therefore) if the
general characteristics of Roe's sample suggest
some adaptive potential for the role of ethnologist-
stranger, we may be encouraged to undertake further
studies with more refined samples to reach more firm conclusions.\textsuperscript{10} (emphasis mine)

From the perspective of this research report the criticisms I have regarding this movement of self-awareness, and the incorporation of that awareness into the body of the research report, is that it tells us little about the formal properties of ethnographies and it is in no way clear that such concerns tells us much about the social background and psychological characteristics of ethnographers, matters which are in themselves of enormous complexity. To regard the ethnographer as inherently biased is to give up the enterprise of ethnography. If the ethnographer is unintentionally but nevertheless biased, it follows that accounts of his field experiences and the relationships he develops with his subjects in the field will also be biased. Does the mere recounting of one's personal feelings towards one's informants make the ethnographic description more objective? Further, are we to exempt psychologists from this claim to bias? Can it be said that biases of any kind are peculiar to those anthropologists whose province is ethnography? And are the psychologists who contrive these elaborate testing procedures necessarily exempt from this ailment such that they occupy a privileged position from which they may cast judgment upon ethnographers? I think not. Indeed, there are ways in which focussing attention on ethnographic bias and on the ethnographer's self-conscious awareness is one way of not looking at the formal properties of ethnography.

Since this dissertation is directed towards an examination of the organizational features of an ethnographic description, I have provided the reader with little information about my relationships with members
of the Clinic, about my musings or reflections or about "how it is that I felt" at this or that time during the course of my research. This does not mean, however, that I do not intend to address the issue of the relationship which obtains between the ethnographer, the research setting, and the members who are under study. Indeed, chapter four: "The Researcher and the Research Setting," is concerned specifically with these issues. However, the material which is examined in that chapter consists of field notes and audio tapes of interactions between myself and members of the research setting. Issues dealt within that chapter are: (1) participant and non-participant observation; (2) the relationship of the ethnographer's situated ethics in the doing of field work; and (3) how the role of the researcher can be taken as a resource for the discovery of the features of a setting. In contrast, the issues dealt within chapter four are similar to those mentioned by Nash and Wintrob, although the analysis of the data presented does not involve any attempt on my part to speculate about my feelings at the time of the production of the data, or about the personal relationships between myself and members of the research setting. Although recollections and personal feelings are often incorporated in my description of the Clinic (since such feelings and personal recollections are a constituent feature of many ethnographic descriptions) they are not a part of the analysis of that description. I wish to point out that I am engaged in the study of the organizational features of the construction of an ethnographic description and not with what any individual ethnographer will
This dissertation is concerned with the production of ethnographies, yet, up to this point, there has been no attempt to specify what an ethnography consists of. Then, too, while I have said that this report is something other than a conventional ethnographic description, to properly make this claim is to entitle the reader to an explanation of the difference between the two, and this undertaking necessarily involves a definition of what is meant by the term ethnography. However, to provide a complete answer to this basic question is itself a task of much more than a dissertation magnitude, for no complete definition is possible. In a few brief pages, then, I would like to try to explain some of the differences between this research report and more conventional ethnographic descriptions. It is hoped that the following explication will satisfy some of the questions about the basic characteristics of an ethnography, although I realize that it will not provide the reader with a definitive or final answer.

To begin with, The Dictionary of Anthropology affords us with the following definition of 'ethnography':

Ethnography. The study of individual cultures. It is primarily a descriptive and non-interpreive study.

Compare this with Charles Frake's more elaborate formulation:

Ethnography...is a discipline which seeks to account for the behavior of a people by describing the socially acquired and shared knowledge, or culture, that enables members of the society to behave in ways deemed appropriate by their fellows. The discipline is akin to linguistics; indeed, descriptive linguistics is but a special case of ethnography since its domain of study,
speech messages, is an integral part of a larger domain of socially interpretable acts and artifacts. It is this total domain of "messages" (including speech) that is the concern of the ethnographer. The ethnographer, like the linguist, seeks to describe an infinite sets of variable messages as manifestations of a finite shared code, the code being a set of rules for the socially appropriate construction and interpretation of messages.12

Does Frake's formulation resolve our questions about the basic character of ethnographies? Is it in any way clearer or better than the definition given by the dictionary? When would you use one and not the other? It is apparent that for some persons and in some situations the first definition might well be more appropriate; if, for example, someone who had little interest in the subject were to require a definition of the word. On the other hand, the second definition would be seen as more appropriate in a discussion between fellow ethnographers or sociologists. While both equate ethnography with cultural description, the difference between them is that Frake's formulation tells us something about how such a description is to be accomplished. It is this feature of ethnographic description -- the manner in which that description is to be produced -- that occupies a focal point of debate within anthropology.13

The precursors for modern ethnographies were the letters, diaries, travel documents, etc. by laymen who had some contact with exotic societies. Today bona fide ethnographic descriptions are confined almost exclusively to the professional community. Thus, our standards for deciding what it is that constitute an ethnography changes over time.
Whereas the aim of ethnography is the production of a recognizable cultural description, the manner in which this aim is achieved, and the specific character of the description produced, have been controversial issues for social scientists, and to say that an ethnography is concerned with describing a culture and to leave it at that, tells us next to nothing since its goals and the methods or fundamental "how" of its production are themselves problematic features of that description.

While acknowledging that the scope and definition of ethnography has changed in the past, is undergoing further re-definition in the present (and will undoubtedly change in the future), Harold Conklin has proposed the following formulation of ethnographic concerns:

An ethnographer is an anthropologist who attempts — at least in part of his professional work — to record and describe the culturally significant behaviors of a particular society. Ideally this description, an ethnography, requires a long period of intimate study and residence in a small well-defined community, knowledge of the spoken language, and the employment of a wide range of observational techniques including prolonged face-to-face contacts with members of the local group, direct participation in some of that group’s activities, and a greater emphasis on intensive work with informants than on the use of documentary or survey data.14

This characterization seems to provide us with a standard from which we can point out certain similarities and differences between the present research report and conventional, i.e., current or standard ethnographic descriptions. Therefore, let us take Conklin's statement as a working definition.

Since ethnography is concerned with producing cultural descriptions, it should be incumbent on any ethnographer to give his readers
some idea about the relationship between the data he collects and
the character of the description that he produces with and from that
data. This is not usually the case.

However, my second chapter, "Issues in Producing an Analytical
Description," takes this relationship between research data and the
production of an analytical ethnographic description as its central
concern. That is, it branches off from chapter one which contains a
standard piece of ethnographic material describing a community clinic.
This description would, in standard ethnographies, stand as a preface
to some subsequent, more detailed or particular analysis of some aspect
or aspects of the clinic's organization. Chapter two considers some
of the issues involved in the production of such a description. It
looks at some of the ways in which a social researcher might approach
the opportunities which access to the Clinic would afford him, and
also examines the primary source of data for his analysis and the
relationship that adheres between the data and the setting from which
it is collected. Thus, it begins with an analysis of how a traditional
medical sociologist might approach the setting if given the opportunity
to do so. It is suggested that a medical sociological perspective would
not be concerned with producing a description of clinic organization
alone, but would also address itself to some preconceived research hypo­
theses concerning topics and issues within the area of medical sociology.
The Clinic would constitute the locus from which to gather data about
such things as status relationships between physicians and nurses, be­
tween nurses and orderlies, or staff and patients, etc. While this
approach is not to be criticized as worthless, I feel that it can be faulted for failing to respect what I have called "the integrity of the research setting." That is, medical and other branches of sociology tend to approach settings with a butterfly net of a priori categories facts, figures and hypotheses in which they will catch the setting itself. On the other hand, ethnographers who have been concerned with describing the strange, and often exotic features, seem to be more concerned with preserving the culture's "integrity." Thus, as Tyler tells us, "In a very real sense, the anthropologist's problem is to discover how other people create order out of what appears to him to be utter chaos," and the solution to this problem can be either "impose a pre-existing order on it, or discover the order underlying it". Many ethnographers believe that ethnographic descriptions are fundamentally concerned with the latter task and must therefore treat the cultural scenes observed as having, in and of themselves, a socially organized character available to empirical investigation and description. The cultural world of the ethnographer is socially constructed and maintained by and for its members. Ethnographic description recognizes and respects this feature and attempts to explicate the order underlying the doings of the culture under study. This is what I mean when I speak of ethnographies as something which respect the "integrity of the research setting." That is, unlike the medical sociologist, the ethnographer is concerned with the members' explanations of the ways in which cultural scenes are constructed and maintained.

The discipline of ethnography has been subjected to much criticism
from concerned social scientists and chapter two addresses some of the pitfalls of what may be regarded as the earlier or more traditional methods of analysis. The thrust of this criticism is that, while purporting to describe aspects of the organization of other cultures, traditional ethnographic analysis has approached these cultural scenes with pre-established sets of theoretical interests that, as Tyler has shown us, impose a pre-existing order on them.

Among anthropologists, a new style of ethnography (i.e., cognitive anthropology) has attained currency. Briefly stated, whereas the old ethnography was concerned with discovering anthropological universals and a general theory of culture, cognitive anthropology addresses itself to the ways in which:

...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 behavior in their culture? Answers to these questions are provided by an adequate description of the rules used by the people in that culture. Consequently, this description itself constitutes the "theory" for that culture, for it represents the conceptual model of organization used by its members. Such a theory is validated by our ability to predict how these people would expect us to behave if we were members of their culture.17

Basic to this ethnographic model is a change in the anthropologists' traditional concept of 'culture'. The earlier anthropological position has been stated succinctly by Tyler:

Previous theoretical orientations in anthropology can in a very general way be classed into two types -- those concerned primarily with change and development and those concerned with static descriptions. Thus, the evolutionists and the diffusionists concentrated on patterns of change,
while the functionalists eschewed this work as mere "speculative history," and focused on the internal organization and comparison of systems, hoping thereby to discover general laws of society....

These formulations were attempts to construct universal organizational types which were linked either by similar processes of change or by similarities of internal structure. In order to achieve this goal, only certain kinds of information were accepted as relevant, and concrete ethnographic data had to be elevated to more abstract forms such as index variables and typological constructs.  

The new ethnography, on the other hand, operates on the assumption that to describe another culture is to describe what it is that one would need to know in order to be regarded as a competent cultural member of that culture. The parameters of this task have been formulated by Charles Frake as follows:

First, it is not, I think, the ethnographers' task to predict behavior per se, but rather to state rules of culturally appropriate behavior. In this respect the ethnographer is again akin to the linguist who does not attempt to predict what people will say but to state rules for constructing utterances which native speakers will judge as grammatically appropriate. The model of an ethnographic statement 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." The second difference is that the ethnographer seeks to discover, not prescribe, the significant stimuli in the subjects' world. He attempts to describe each act in terms of the cultural situations which appropriately evoke it and each situation in terms of the acts it appropriately evokes.

Thus, this perspective conceives of a culture as a set of rules for appropriate behaviour which bona fide members of that culture utilize in order to conduct (and to be seen as conducting) themselves in recognizably
appropriate ways. These rules have a demonstrably consequential character, for it is by attending to them that members are able to produce the organized character of their world. \(^{21}\) In this sense, the new notion of culture is related to Garfinkel's concept of competence. He states that:

\[
\text{I use the term "competence" to mean the claim that a collectivity member is entitled to exercise that he is capable of managing his everyday affairs without interference.} \(^{22}\)
\]

The new ethnography is an eminently practical enterprise. It attempts to provide a description of a culture that could be used by a cultural stranger in such a way that the stranger could, if he so wished, conduct himself in a manner which members of the culture described would deem appropriate for that culture. That is, the reader would be able to act appropriately and, through his actual, on-going performance, display and maintain what Garfinkel has called his cultural competence.

The material discussed in chapter two of this report provides certain criticisms of this new approach to ethnographic description, particularly to the programme that it seeks to accomplish. At the same time, in attempting to provide an analytical description of some aspect of the social organization of the Clinic, this dissertation adopts the position that such a description should be responsive to those self-organizing features of the research setting which it is studying. Therefore, an ethnographer should not attempt to impose some pre-existing framework on that which he describes.

These issues are discussed in chapter two, then, a more detailed
description of a clinical routine known as chart rounds (i.e., a daily review of patients medical records) is presented. This constitutes an appropriate topic fulfilling the requirements that the preliminary description of the Clinic in chapter one provides for. I decided to describe chart rounds since it is a normal, routine feature of the organization of the Clinic; a feature which members of the setting take for granted as a constituent feature of their daily roles as physicians, nurses, medical students, patients and the rest. Pragmatically, the occasion of chart rounds was selected because my research at the Clinic made it possible for me to acquire an extensive corpus of notes and tapes of the interaction occurring in these medical sessions. Thus, I was able to make transcribed records of actual chart rounds. Aside from the availability of audio recordings, it was selected because it is an occasion which has as its underlying rationale, the generation of talk about particular patients and related medical matters.

Then, in my third chapter "Charts Rounds: An Interactional Analysis," I shall attempt to describe this occasion by attending to the talk which occurs between participants as data from which to discover the self-organizing features of chart rounds. Here, I shall adopt an ethnomethodological orientation. While it is not my intention to examine the major tenets of ethnomethodology, I would like to offer some comments concerning the general characteristics of the analysis that will be presented in this chapter.

As I have already noted, I had no specific topic of inquiry,
research proposal or hypothesis when I first began this dissertation. Instead, the material contained in this report, that is, the direction that it takes and the topics that it addresses were "discovered" or flow from the production of the paper itself. Hence, its "non-standard" format. While the accepted ways for doing an ethnography may have many advantages, they may also have inherent disadvantages. Consider the fact that ethnographic researchers often provide research proposals regarding the scope, character, and objective of their research. If the main purpose of ethnographic descriptions is to discover how some "other" culture is organized, it seems odd to speak of objectives other than describing the underlying organizational rules of that culture. I would like to suggest that to have any other objective, e.g., to have predetermined ideas about what you will find and how you will find it is to lose sight of what may actually be there awaiting discovery. 24

While Conklin describes an ethnographer as "an anthropologist working in a foreign culture", as a sociologist attempting to describe his own culture, I do not encounter some of the obstacles that are characteristically faced by anthropologists (such as learning a foreign language). My problems are of a different nature. For instance, I am confronted with the fact that my research subjects often know that I am a sociologist and "know" what it is that sociologists do. Thus, I must deal with the relationship between my own aims and those imputed to me by members of the research setting. (This feature was operative throughout my stay with the Clinic. It is given special attention in
Thus, being a member of the society which I am interested in describing is both an asset and a liability. Turner has stated the relationship between sociological topics and one's own cultural knowledge of those topics in the following manner:

It is increasingly recognized as an issue for sociology that the equipment that enables the "ordinary" member of society to make his daily way through the world is the equipment available for those who would wish to do a "science" of that world. This might be formulated as the sociologists "dilemma," but only so long as a notion of science is employed that fails to recognize the socially organized character of any enterprise, including the enterprise of doing science.25

This dissertation, then, is an attempt to utilize my own cultural equipment to provide an ethnographic description of a routine occasion within the Clinic. Throughout this report I intend to demonstrate the relationship between the production of an ethnographic description and that description's dependence on common-sense cultural knowledge for it to obtain an accredited status as an "adequate" ethnographic description. I find it difficult to state this point more succinctly and, therefore, wish to inform the reader that it constitutes a major point of research interest in this document.

Like anthropological ethnographers, I am concerned with linguistic events that are a part of those cultural scenes that I am attempting to describe. Unlike such ethnographers, however, I am neither conducting formal interviews with my subjects, nor engaged in formal eliciting techniques aimed at taxonomic construction.26 Instead, my chief source of data is those naturally occurring instances of interaction themselves. Much of the integrity of the interaction is
preserved in audio tapes. The forthcoming analysis of chart rounds in chapter three is an attempt to describe them through an examination of transcribed tape recordings. By attending to talk as a naturally occurring phenomenon, I am suggesting that it is possible to attend to the self-organizing features of cultural organization. To quote Turner again:

A science of society that fails to treat speech as both topic and resource is doomed to failure. And yet, although speech informs the daily world and is the sociologist's basic resource, its properties continue to go almost unexamined...If we take sociology to be, in effect, "a natural history of the social world," then sociologists are committed to a study of the activities such a world provides for and of the methodical achievement of those activities by socialized members.27

While the findings offered in this paper do not purport to describe the occasion of chart rounds in ways that will allow the reader to act as a competent member of the Clinic (and thereby differ from the descriptive goal of the cognitive anthropologist), the description offered does try to (1) respect the integrity of the setting by attending to actual linguistic events, and (2) be responsive to the self-organization of the linguistic data under consideration.

Admittedly, the findings presented in this paper raise complicated issues for the study of social settings. These will be addressed in forthcoming sections of this report. At this time, I wish only to give the reader some indication of what I meant when I referred to my findings as exploratory and tentative.

The analysis of chart rounds presented in chapter three is an
analysis conducted under the auspices of an ethnomethodological orientation. It is not my concern to address the question "What is ethnomethodology?" I would, however, like to offer some comments about what it is that I mean when I refer to an "ethnomethodological orientation." Then, I would like to discuss certain methodological considerations about the primary source of data for this analysis of chart rounds, i.e., conversational transcripts taken from tape recordings done during chart rounds.

An ethnomethodological orientation does not mean that the researcher operates with some prefabricated theory of society. Rather, all social organization is seen as members' accomplishments. And all "investigations" are directed towards discovering how it is that social order is possible in the first place. More precisely, findings about members' methods of social organization can be achieved only by confronting data itself, and can not be generated from any substantive body of theory, hypotheses, or research design which simply assumes this order-as-given and summarily dismisses it. Such procedures do not lend themselves to the learning of a set of technical skills such as survey or interview techniques, or experimental designs; that is, specific skills wherein the researcher acquires the "how to do it" of the enterprise. Members' methods are available for investigation, but such investigation is not dependent on the acquisition of a body of methodological techniques. Thus it is apparent that ethnomethodology is not a method.²⁸

To present an analysis of chart rounds under the auspices of an ethnomethodological orientation as it is, is to adhere to Harold Garfinkel's recommendation that:

In exactly the ways that a setting is organized, it
consists of members' methods for making evident that setting's ways as clear, coherent, planful, consistent, chosen, knowable, uniform, reproduceable connections, i.e., rational connections. In exactly the way that persons are members to organized affairs, they are engaged in serious and practical work of detecting, demonstrating, and persuading through displays in the ordinary occasions of their interactions the appearance of consistent, coherent, clear, chosen, planful arrangement. In exactly the ways in which a setting is organized, it consists of methods whereby its members are provided with accounts of the setting as countable, storyable, proverbial, comparable, picturable, representable — i.e., accountable events?

Any data utilized for such a study must be a constituent feature of some organization. The data must be produced and used by the setting's members of that setting during the course of their everyday affairs. Furthermore, if we are to study members' everyday affairs and activities, then we must also examine data that are produced as part of and reflect back upon these affairs and activities. If we are to properly study and describe social life, we must be able to attend to the details of events that have actually happened. We should be able to do this in an abstract way, but, nevertheless, such a description should be responsive to the actual details of the events that they purport to describe. Thus, some social scientists have elected to focus their attention upon members' talk as a decidedly natural occurrence in sequences of interaction, developing in and over the course of various settings and occasions. Chapter three is one such instance of such an analysis. Specifically, then, I will be treating productions of talk as one of the fundamental ways in which members achieve and
display in, and through the occasion of their interactions, the consistent, coherent, clear, chosen, and planned arrangements, i.e., that fundamental foundation for any type of social order.

That members' talk can be subjected to rigorous sociological analysis is demonstrated in the recent works of Harvey Sacks\(^\text{30}\) and other "conversational analysts".\(^\text{31}\) A clarification and justification of the methodological foundations for conversational analysis goes well beyond the scope and intent of this research report. My reason for not addressing these issues is not simply that to do so would require more space and time than current concerns warrant. Rather, it is based on a feature of the present literature concerned with conversational analysis. This literature does not provide the reader with methodological statements about its foundations, and, since such statements have not been made, I am unable to present a summary of them. However, since I intend to present an analysis of conversational material in chapter three, I feel that it is uncumbent on me to present an analysis of the methodological issues raised in that chapter.

Thereby, I could attempt to relate the work of conversational analysts to various developments in linguistics and linguistic philosophy. It is sometimes proposed, for example, that work on theories of meaning must be absolutely fundamental to work in conversational analysis. This would seem to suggest that an appropriate procedure would be to attempt to explicate the connections between the philosophical literature and theories of meaning\(^\text{32}\) and
conversational analysis. The point, however, is that these connections would be artificial, and might have no necessary connection to the type of analysis presented in chapter three. Let me give an example.

In philosophy there is debate about "meaning" and the manner in which the concept operates in language use. The philosopher H.P. Grice has proposed a formulation of "meaning" in terms of the subjective intentions of individual speakers, while John Searle has offered us an opposing view in which meaning is seen as dependent on speakers attending to the proper conventions of a language rather than to subjective intentions of individual speakers. It would be possible to engage in a lengthy and elaborate discussion of these philosophical "theories of meaning" since, commonsensically, it would appear that such a discussion is relevant to the present analysis of conversational materials. However, this is not the case. While I could compare and contrast these arguments, I know of no way of tying them to the analysis of the conversational materials presented in my own account of chart rounds.

Furthermore, my analysis of chart rounds was generated independently of these "theories of meaning". It does not seem to be the case that, in daily life, we need to refer to theories of meaning in order to understand each other's utterances nor does it appear useful for me at this point to provide the reader with an explication such as Grice's theory of meaning for I see no way of tying that theory to the analyses in chapter three. What, then,
is the alternative?

The analysis of conversation presented in chapter three is my attempt to treat an instance of chart rounds as a source of data from which to explicate some of the organizational features of this occasion. Thereby, I attend to the talk that occurs during the occasion of chart rounds. In connection with this, the reader is directed to the detailed utterance-by-utterance analysis in chapter three and is invited to offer alternative analyses if those which are offered there seem incorrect. While this may seem totally ascien
tific, it is the nature of the data that makes the analysis take this character. Roy Turner has made this point when discussing how the analysis of conversational materials is responsive only to the materials upon which that analysis is conducted:

I stress not only that it is to the participants that my analysis must be tied, but also the fact that we are dealing with a possible interpretive schema. The significance of this is two-fold. In the first place, the analysis I am offering is not intended to provide a definitive, once-and-for-always reading of the utterance sequence under examination. Nevertheless, I don't mean to suggest, by offering this observation, that the analysis is tentative in the sense that with the application of more time and energy it might become definitive. What is intended, is that for the participants themselves determinate readings depend upon the application of interpretive schemas. Such schemas warrant the intelligibility and propriety of the hearing of an utterance: there is, of course, no further warrant. Thus, the conversational analyst may argue well or badly, may be right or wrong in finding one piece of analysis to be entailed by another; but he cannot provide a "scientific" warrant for his claim which is stronger than the warrant members employ as conversationalists. And this is not an admission of the primitive
state of the art, but a characterization of the enterprise and of the criteria of adequacy which control it, given the materials to which it is ultimately responsive. 35

And now I shall turn to my initial characterization of the setting.
Footnotes: Introduction

1. This does not mean that I was unable to produce a formal research proposal. Indeed, during the course of my field work at the Community Clinic the director of the clinic required that I produce such a document. The point is, however, that that research document was itself produced for practical purposes and should not be regarded as being indicative of "guiding" the character of this work. Rather it was produced to satisfy the director's request for a research proposal and thereby satisfy one of the organizational constraints placed upon me by the director. A copy of this research proposal is presented later.


5. Nash and Wintrob, op. cit.

6. Ibid.


8. Ibid., p. 375.


16. Ibid., p. 11.

17. Tyler, "Introduction", *Cognitive Anthropology*, p. 5.

18. Ibid., p. 2.

19. The methods used in order to obtain this goal include the construction of taxonomies, lexicographical systems, formal interview and eliciting techniques, discourse analysis, and ethnosemantics to name but a few. See Tyler, *Cognitive Anthropology* for a detailed representation of these methodological positions.


23. Harold Garfinkel's *Studies in Ethnomethodology*, (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1967) can be regarded as the primary source for those who are doing or who wish to know about ethnomethodology. Other suggested references are Thomas P. Wilson's "Normative and Interpretive Paradigms in Sociology" in Jack. D.
Douglas, Ed., Understanding Everyday Life (Chicago: Aldine Publishing Co., 1970), pp. 80-103. These illustrate some of the differences between what I have called an "ethnomethodological orientation" and standard ways of approaching social phenomena. Roy Turner's, Ed., Ethnomethodology, (London: Penguin Books, 1974) furnishes us with an example of the wide variety of research that is now being conducted under the name of ethnomethodology. I would like to point out, here, that ethnomethodological studies are, at present, viewed with some disdain by many of the "standard brand" methodologists. And, whereas stigmatized disciplines are often called upon to defend their position, it may well be the case that their status reflects fashions and trends in universities, in publishing companies, in public taste, etc., rather than any actual qualities they may have.

24. Even Berreman, for example, adopts such a research perspective when he states in his article "Social Categories and Social Interaction in Urban India", American Anthropologist, (Vol. 74, 1972), p. 567 that:
   The research was undertaken from a "symbolic interactionist" perspective, using detailed observation and inquiry regarding what people do in face-to-face interaction, to discover how they choose among alternative behaviours in terms of the meanings of specific attributes, actions, and social situations have for them and for those with whom they interact.


26. See footnote 19.

27. Turner, op. cit.

28. Richard J. Hill and Kathleen Stones Crittenden, Eds., Proceedings of the Purdue Symposium on Ethnomethodology, (Department of Sociology, Purdue University: Institute for the Study of Social Change, 1968). This monograph contains a series of edited transcripts between Harold Garfinkel and other sociologists in which the issue of whether ethnomethodology is a "method" or a major topic of discussion.

29. Harold Garfinkel, Studies in Ethnomethodology, p. 34.

30. Throughout this report I have been influenced by the writings of Harvey Sacks and I would like to take this opportunity to acknowledge the insight I have obtained through encountering his work.


CHAPTER I

BEGINNING AN ETHNOGRAPHIC REPORT

The Initial Description

The Setting

The initial overtures for this paper came from my thesis supervisor. A colleague had informed him of an attempt to establish a series of community medical clinics in Vancouver. He suggested that it might be possible for me to obtain a position in one of these clinics since many of the doctors attached to the project seemed amiable to the goals and values of sociology and might allow me to study the project.

While I had had no previous training in medical sociology, the opportunity to do an ethnography in such a setting seemed too good to miss. Before discussing the procedures I used to gain access to the setting, I would like to provide a description of the Clinic and its surroundings.

It is located in a predominantly lower class area in the eastern end of the city. The neighbourhood is predominantly Italian but has a large Chinese population and a number of other, smaller, ethnic groups. The Clinic is located on a busy street lined with clothing shops, record shops, pool halls and an array of ethnic restaurants and groceterias.

The Clinic itself is housed in what was once a business office, but has been renovated to meet the needs of medical practitioners. What were initially secretarial offices were converted into examination
rooms. A small laboratory, a reception area and three small offices were constructed in the rear of the building. During the course of this research, however, the Clinic expanded its facilities by acquiring an adjacent building which has been renovated to accommodate a dental clinic.

Yet, from the outside, it is an inconspicuous building; one without many of the characteristics typically associated with medical buildings. Although the name of the Clinic appears on the door and on the window of the waiting room as well, and a closer inspection will show a list of those physicians who operate the Clinic, it is quite possible to walk or drive past the building without realizing that it is a medical clinic.

Although it is a community medical facility, the Clinic does not provide in-patients care, i.e., it has neither a resident patient population nor a specialist facility such as an eye, ear, nose and throat clinic. Therefore, it does not have restrictive medical practices but provides the type of medical care one would normally associate with a private medical practice. However, this similarity to ordinary medical practice requires some essential clarification.

While the patients at the Clinic are, as a matter of routine, assigned to one, and only one physician, they are also thought of as patients of the Clinics as a whole and may thus, as a matter of course, be treated by a physician other than their "own" (regular) physician. While continuity of patient care is viewed (here, as elsewhere,) as desirable, contingencies peculiar to the Centre may mean
that a patient's regular physician can not attend to him. When this happens the patient is seen by another Clinic physician. Note however, that patients are informed of this practice.

The original staff consisted of four full-time physicians, one public health nurse, a laboratory technician, a volunteer community worker, and three secretaries. Later, related medical professionals such as a nutritionist and dental hygienists were added to the staff. In addition, since the Clinic has some support from the Faculty of Medicine at the University of British Columbia, fourth-year medical clerks (students) may elect to spend two weeks training at the Clinic in order to gain experience in the "real world" of the medical office. In addition to a full-time staff and medical students who work on a part-time basis, the Clinic has a group of family physicians who donate their services on a part-time basis. Originally, physicians who maintained their own practice and worked only part-time had a roster of patients at the Clinic who they saw on a scheduled basis. However, this procedure was changed to allow them more time for discussion with the medical students.

While the staff does not view the organization as "welfare" or charity clinic, there is a substantial proportion of its patient population who is, for economic or social reasons, unable to receive conventional private medical care. One physician characterized the patient population as "...the type of persons that a regular doctor would not want to drop into his office. They scare other patients, cause trouble, and require bureaucratic red tape." Thus, the
clientele of the Clinic is labelled as somehow different from that of "conventional" office practice. The Clinic sees itself as an alternative to the traditional mode of treatment offered in the out-patient centres of the local hospitals. Those patients who could be seen by the staff as financially and socially acceptable to conventional physicians, yet preferred to use the facilities of the Clinic, were seen as expressing a "preference" for the type of care offered at the Clinic.

In addition to its regular operations, the Clinic also runs a Night Drop in Clinic for youths. This Night Clinic is typical of the pattern followed by many of the youth-oriented Clinics that have recently sprung up in many metropolitan areas. The Youth Night Clinic advertises in one of the local underground newspapers and, as is expectable, much of the patient population on such nights consists of youths from the community regularly serviced by the Clinic and of transients who have come to Vancouver and are in need of medical attention or advice. All share some common and recurrent problems. Many youths want general information on contraception or request birth control pills or abortions. Other common problems include amenorrhea (overdue menstrual periods), vaginal discharge, venereal disease, skin problems, and nutritional problems arising from a vegetarian diet or improper nutritional habits. Absent from this list are drug-related complaints for the Vancouver area has many neighbourhood clinics that specialize in such problems, and the majority of patients suffering from such problems utilize those
facilities rather than the services of the Clinic. Indeed, the staff of the Clinic does not want the place to be characterized as a "drug" clinic, and those who telephone the Clinic about drug-related problems are referred to another treatment facility. This does not mean that a patient who comes to the Youth Night Clinic with a drug problem will be refused treatment, but that the clinic will not go out of its way to acquire a drug-using patient population.

Here is a quote from their "Student Handbook, Community Youth Night Program, September 1971". It furnishes us with an interesting self-description:

I PHILOSOPHY AND HISTORY

An often unrecognized medically under-privileged, neglected and alienated group is the "youth" of our society. Medical groups have been organized in the past to provide temporary emergency medical care during rock festivals and student demonstrations, but many of these groups were as transient as their patients. In September of 1970 the Community Clinic and the Interprofessional Education of _______ developed an "adolescent clinic" to reach and provide medical care to the youth community of Vancouver. The Community Clinic, under the direction of Dr. BobkAbeñal Jones, donated the physical, pharmaceutical and laboratory facilities and encouraged the volunteer help of physicians and dentists, while the PPE group, under Dr. L. Pitkin's auspices, arranged the volunteer multidisciplinary help of medical, nursing and social work students.

The objectives of the "adolescent clinic" were to deliver high quality care to a young population between the ages of 13 to 25 years, to educate the adolescent in preventive (contraceptive) medicine, and to provide a learning situation in which students could experience inter-professional cooperation.
II INTERPROFESSIONAL EDUCATION

1. It is our belief that only an interprofessional team can provide comprehensive care.

2. The Thursday night program offers the student an opportunity to observe the roles of different professionals and utilize their specialized skills when appropriate. Nursing, nutrition, rehabilitation, dental and medical students will staff the clinic each week.

The Youth Night Clinic is much different from day-time office practice. A typical evening clinic begins around 7:30, and at or around that time one may often find a group of both patients and medical students congregating in front of the main entrance -- all awaiting one of the physicians who has a key. Indeed, I often found it difficult to distinguish medical and nursing students from the actual patient population since both groups were of the same age. Thus I had to rely heavily on cues such as manner of dress and grooming to provide an indication of their "real" social identities. It is interesting that some of the medical students were also confronted with this problem and its possible consequences and brought their stethoscopes so that it was sometimes possible to locate "students" by searching for the stethoscope hanging out of their pocket. Eventually, I was able to identify many of the students on sight. However, since the students on duty differed from week to week, identification was always a potential problem.

My first encounter with the research setting was with the Youth Night Clinic. The following field note recalls my first impressions of the Clinic and illustrates the problem of staff identification.
October 13, 1971 — accompanied by ________ and Dr. Turner. Relatively free and easy atmosphere with a noticeable absence of what I'll call "recognizable apparel", that is, doctor's uniforms. Staff wear name tags. Staff composed not only of doctors, but also nurses, nutritionists, rehab, medicine, etc. Name tags consist of name + medical category. One person was wearing a tag that said "Isabel Nurse" where I take it that her last name was not 'Nurse'.

To say that staff recognition is problematic point to joke that occurred. Since staff is voluntary and often changes from week to week there is/can always be a new set of personnel. Consequently determining who these new people are can be interesting for the staff already present. Dr. Turner and ________ were asked the question "If anybody here was a doctor". They were seen as potential candidates for that slot given their age and the fact that we were on the inside (in the lunch room/lounge area) rather than in the waiting room. I was treated as a student.3

As I was to find out later, the organization of the staff and provisions for the care of patients is different on youth night than during day-time practice. For instance, the regular secretarial staff is replaced by volunteer help from the community. The two following transcriptions may give the sense of what is meant by "a relatively free and easy atmosphere." The first involves a medical student and the Clinic's receptionist, and the second a patient and the receptionist:

#1. CLINIC RECEPTIONIST AND MEDICAL STUDENT:

1. Receptionist: Hi

2. Medical student: Hi, I am a second-year medical student thought I'd come by tonight spend some time here if I can

3. Receptionist: Yes
4. Researcher: Sure (Note: I was helping at the reception desk on this occasion and my tape recorder was placed in view near the top of the receptionist's desk.)

5. Medical student: Look around for awhile, some of my classmates have been by and said it was worth coming down

6. Receptionist: Good, we've been very busy but we're just sort of (sitting) getting our second wind.

7. Medical student: Get your second wind eh - so so should I just float around or

8. Receptionist: Yes

9. Researcher: Might put you to work

10. Receptionist: Yes might put you to work

11. Medical student: Can't stay too long have to go home and do some work (laughs)

12. Receptionist: Good ( ) and you know I'm sure you'll find your way around it isn't too complicated that you'll get lost.

#2. CLINIC RECEPTIONIST AND PATIENT

1. Receptionist: Have you been here before?

2. Patient: (Note: There is much noise around the reception desk due to patients queueing to see the receptionist)

   I was suppose to have an appointment on Monday but I'm going away so I called in today and they said to come in tonight.

3. Receptionist: That's fine, ah but the doctor won't see you till I've made this out (a patient's intake form). You've never been before
4. Patient: Yes I've been here before, my doctor was Doctor Miller but he's on holiday so they said I'll be seeing Doctor Jones

5. Receptionist: I see, so what's your name?

6. Patient: Joan Smith

7. Receptionist: Smithnt (the receptionist looks at the files to find the patient's name and corresponding chart number) two seven two nine (pause) You ah ah are you a vegetarian?

8. Patient: Umm?

9. Receptionist: Ah I just wondered because ah we have someone here who is interested in ah taking statistics umm

10. Patient: Just about though

11. Receptionist: Umm?

12. Patient: Just about though

13. Receptionist: Umm, just about though ah are you still at two three four six Howard Avenue?

14. Patient: Yes

15. Receptionist: and you still have the same B.C.M.P. (British Columbia Medical Plan) number?

16. Patient: Yes

17. Receptionist: and ah what did you come in to see the doctor tonight about?

18. Patient: ah he's going to ah I.U.D. he's going to give me one of those
19. Receptionist: Well (  )
20. Patient: O.K.
21. Receptionist: Fine -- have a seat

This relaxed, less formal attitude is intended as something which encourages participation in the youth night program.

Once the receptionist has obtained the necessary information and collected the minimum ($2.00) fee, the patient is asked to take a seat in the waiting room. It is not uncommon on youth night to see an odd assortment of patients. On one particular night the waiting room was filled with members of the Hare Krishna order accompanying a fellow devotee who had broken his ankle while chanting on the street. There were also some very nervous young women, [And, according to the staff, such nervousness usually indicates that they have problems related to birth control or abortion.] transient youth, young transient parents with their children, and some adults who, for various personal and situational reasons, prefer to attend the Youth Night Clinic.

The students who call the patients from the waiting room address them by first name or, sometimes, first name plus last name, but terms such as Mr. or Miss are seldom used. The patient is conducted into offices which, during the day serve as regular physicians' offices, but are, for the present purposes, made available as interviewing rooms. Here, the students interview the patient and note his medical history. This procedure provides them with an opportunity to make a tentative diagnosis. After the interview the patient is taken to an examination room or is asked to return to the waiting room.
until an examination room becomes available. And since there are only three examination rooms and a short supply of doctors, it is the norm that patients are often returned to the waiting room. It is not at all uncommon to hear students asking, "Is there a doctor available?" or "Is there an examination room open?"

When an examination room becomes available and a physician is located, the patient is recalled from the waiting room. Frequently, one of the students will try to secure the examination room (i.e., to occupy it physically), while the other student tells the physician that they are going to get the patient. (Furthermore, it is not uncommon for additional students to be summoned to an examination room if, upon examination it is revealed that a patient has some ailment that the physician considers to be of practical interest to students.) After the examination and any necessary laboratory work, the patient's dealings with the Clinic may be terminated, or he may be instructed to return to the Clinic the next week for a follow-up.

That the Youth Night Clinic deals with a transient patient population is, for some members of the medical profession, a problematic phenomenon. Thus, one physician was concerned with the fact that the Youth Night Clinic is dealing only in "episodic" medical care and not able to do the necessary follow-up care and which is standard medical practice. The Clinic's staff sees this failure as one of the major differences between the day-time practice and the Youth Night Clinic.

Yet, it is notable that the Youth Night Clinic also differs
from the day-time Clinic in other essential ways. Its patients are observed as coming in "off the street", and, while a particular patient may be referred to as a "regular" Youth Night Clinic patient, he, like all other patients here, is not regarded as a regular in the sense of having made an appointment. It is also a fact that patients encounter student personnel before seeing a regular physician. And, as alluded to above, the staff views the youth night program as something which is essentially different from the day-time office practice. As the previous description of the Youth Night Clinic's "Philosophy and History" states, its purpose is "to provide a learning situation in which students could experience interprofessional cooperation". Yet, unlike the day-time practice which the staff sees as corresponding to everyday conceptions of standard medical practice, the very openness of the programme created essential problems. The director of the Clinic had apprehensions about youth night turning into what he called a "fish bowl", that is, that the Clinic might become so open that anyone could come down and view what was happening. And this would of course run contrary to the Clinic's conception of itself as a community medical practice rather than a neighbourhood medical drop-in centre. Yet, the centre seems to have avoided this problem for, as a nurse told me, "At first patients would just drop in but they have learned that it is necessary to make an appointment."

Since the Clinic operates on a group rather than strictly individual basis and it is possible for patients to be treated by physicians other than the one they normally visit, the Clinic has
developed a special routine procedure to monitor patient care. This procedure is called "chart rounds" and deserves special comment. It involves a review of patients' medical histories (their charts) by the medical staff of the clinic. Originally, this was done every morning, before any patients were attended to. The staff would review the charts of those patients scheduled to be seen that day. This allows the staff to keep a finger on the day-to-day situation of the Clinic. Later, this procedure was changed to allow rounds to be taken at the end rather than the beginning of the working day. This proved to be more efficient for two reasons. (a) Patients discussed at morning chart rounds might miss their appointments, and (b) unexpected (and unreviewed) patients might "drop in" to the clinic.

Gaining Entry

The above constitutes a general description of the Clinic. In this section of the paper I intend to provide an explication of the procedures which I followed in order to gain access to the clinic itself. While both my supervisor and his colleague thought there would be few problems involved in doing a research project at the Clinic, this did not turn out to be the case. Instead, my entrance into the Clinic as a sociological researcher proved to be immanently and diversely problematic.

My first encounter with the Clinic took place on October 13, 1971. A Youth Night Clinic was in progress as my thesis supervisor, his aforementioned colleague and I entered the Clinic. None of us
had visited the place before but my supervisor's colleague knew the
director of the Clinic and had arranged this visit for us. Our first
impressions of the Clinic are summed up in the following field note
which I made shortly after this "first contact":

Everyone commented that the Clinic did not appear
to be similar to any other clinic we had seen.
Thus, the problematic question: what is it about
the place that allows us to recognize it as being
a medical clinic? That is, without any of the
external appointments of medical settings what
allows this clinic to have its status as a clinic?
Doctors do not look like doctors, staff do not
look like typical medical staff, the place does
not appear to be a clinic.

We were all, no doubt, somewhat surprised by the absence of the
traditional "white" which is usually associated with hospital and
clinical settings. This being youth night, the place had that un-
expectedly informal character described above. And to add further
to the picture, there was a video camera team wandering throughout
the premises, filming doctor-patient encounters and the overall
Clinic. We were introduced to the director, Dr. Cough. Dr. Cough
said that he was busy at the time but would be able to talk with us
later in the evening and he suggested that we should "look around"
until then.

Dr. Turner and I saw the director later that evening. (Our
companion had left us to attend to some other business.) We pointed
out that we were both interested in studying doctor-patient communica-
tion and that it was often difficult for sociologists to gain access
to medical settings. We discussed the purpose of our research and
told how we hoped that the Clinic would prove an ideal research setting for this. Naturally, we also expressed our appreciation for being allowed access to the Clinic. It soon became evident that the director viewed the Clinic as something quite outside the traditional mode of treatment afforded by most hospitals. He appeared to be favorably inclined towards sociological research although he seemed to hold the view that most sociological research is or at least ought to be something which is socially relevant and/or problem-oriented. That is, he saw it as something akin to a remedy for a disease. Thus he hoped that our research might potentially contribute to the betterment of the Clinic. Several times, he expressed the hope that I would be able to give the Clinic some "feedback" on how well they were communicating with patients. We agreed that we too were interested in the communication obtaining between doctors and patients and suggested that the ideal procedure would be to tape-record physician-patients conversations. The director understood our concern for detail, but suggested that tape-recording conversations would be problematic because of the ethical issues involved. The first encounter ended satisfactorily and it was decided that I would return at a later date in order that we could discuss my research plans in greater detail.

I can recall a sense of excitement about this first encounter with the Clinic. At that time, an acquaintance of mine who was also a graduate student was engaged in a study of the maternity ward at one of the local hospitals. Unfortunately, he was encountering considerable resistance from hospital staff. I was enthusiastic
about the Clinic and its apparent openness, I entertained visions of being allowed relatively unhampered access to the setting and was excited by the prospect of viewing and tape-recording actual medical encounters.

My next visit to the Clinic was on November 4, 1971. (i.e., 22 days after the initial meeting) The purpose of the visit was to discuss with Dr. Cough what goals I would pursue in observing the Clinic and how I would conduct that observation. Prior to this, I had had discussions with my supervisor about what the "line" I should take when I saw the director. We decided that I should discuss my interest in communication, leaving the topic broad enough so that I would be permitted access to the workings of the Clinic while at the same time giving Dr. Cough some idea of what I would actually be doing. (At this time, it was my intention to tape some pieces of interaction occurring between doctors and patients.) After the second meeting at the Clinic I went home and made the following notes which, although lengthy, I shall now present in their entirety as they show clearly how I felt about the research setting at this time.6

Clinic, November 4, 1971 -- 1-3 p.m.

1. After telling Dr. Cough that I would like to observe the various aspects of the Clinic he posed the question: "With what purpose in mind?" My answer contained general comments concerning communication and how I would like to study doctor-patient interactions. I admit this is a gloss of the encounter but it will provide a "sense of this encounter.

2. Dr. Cough was concerned with the methodology of my research. He proposed an alternative
to observation interviews and surveys. Since I do not regard this alternative as adequate, I believe I was critical of the interview method. I explained that I was interested in observing the workings of the Clinic in some literal sense, rather than dealing with data removed from the daily routines of clinic life by various coding procedures. In discussing my role as observer certain factors became apparent. Dr. Gough wanted to know the intended length of my research study. I told him the study would take at least a year and that I intended to become involved with the Clinic. I offered my services as a volunteer worker and it appeared that this offer was well received. I thought that by volunteering I would not interfere with the workings of the Clinic and yet be able to gain a quick understanding of clinic procedure.

3. Dr. Gough seemed very much concerned with the utility of my research for the Clinic. He was not really interested in my own sociological position. No reason why he should be. He was very much Clinic-oriented. I did some interactional work stating that I am not engaged in pure theoretical research, hoping to demonstrate that I am not an "Ivory Tower" sociologist. I also said that I thought it was only fair that the Clinic should receive "feedback" from me concerning my research.

4. I feel as if I could have presented any research idea to the director and he would have transformed it into a research concern for the Clinic. Communication became the equivalent of problems in communicating. I felt that it was better to allow the director to adopt this view of my research rather than engage in some speech concerning "how difficult the concept of communication was" and so forth. I stated that I did think there would be some "spin off" of my research that would be relevant for the Clinic. Dr. Gough said that he wanted feedback from me. I said that was a good idea. Put bluntly, Dr. Tough has a definite idea of what he wants from me as a person about to come to the Clinic and I foresee that I may have to give it to him.
5. The end of our talk resulted in his willingness to allow my presence at the Clinic. He thought that the rest of the staff would agree but he wanted me to come back and explain to the staff my research goals. He thought that this would be a mere formality.6

Note that while the Clinic had initially held forth the promise of easy and unproblematic access, these expectations were not borne out by the actual course of events. The director of the Clinic demanded convincing answers to rigorous questions before entrance was allowed. His concerns with methodology, hypotheses, and the social utility of the research were much more rigid and demanding than those concerns which I encountered on the part of my own dissertation committee. Another appointment was made for me. I was to go to the Clinic in order to discuss my research with the staff. I remember feeling dismayed about the prospect of having to reiterate my "story" for their benefit. Concomitantly, I was becoming more and more pessimistic about the possibility of being permitted to undertake research in the Clinic.

My talk with the Clinic staff was similar to the interview with the director. I did not make any field notes at this time since I did not know whether or not I would ultimately be admitted as a researcher. Indeed, I feared that I would not be. Our meeting took place early one morning. The director introduced me to the staff and gave or a brief presentation of my research interests and mentioned that I would eventually like to tape-record medical interactions. The staff seemed upset by this. They made comments about my being able to evaluate their performance. I tried to impress upon them that I was
not interested or involved in such an evaluation. In addition to this, members of staff were very concerned about the use of the tape recorder and suggested that its, and/or my own presence in the examination room would raise a host of ethical and legal issues. The director of the Clinic was concerned about the possibility that I might publish anything about the Clinic in a sociology journal without first providing him with a copy. This meeting with the staff was terminated and I was told that I would be informed in a week or so about their decision. Three days later I received a phone call giving me permission to do research at the Clinic (during the regular day time and the special Youth Night Clinic), but that the use of a tape recorder was still a problematic issue.

Going to the Clinic for the first time as a research sociologist was a somewhat disorienting experience. I found myself attending to matters of dress and physical appearance that I had not considered prior to this event. During my past contacts with the Clinic it had become evident that dress was casual with blue jeans being permissible on youth night. By casual I mean that male physicians would often wear a sport coat and tie and female physicians and staff were often attired in dresses or pant suits. In contrast, I frequently wore a shirt and tie (never blue jeans) while doing research during daytime practice.

I was told that chart rounds began at 8:30 a.m. Other than that, I had no idea of what to expect in this situation or what would be expected of me. However, I hoped that doing these rounds would
allow me to learn the physical layout of the Clinic. I soon found out where it was not permissible or appropriate for me to go. Aside from the director's office, all the other rooms became open areas and as I soon learned through experience, I was permitted to use the phone or to make notes at the desk if it was not occupied. Since there was no organizational "slot" for me in the workings of the Clinic, a great deal of my time in the research setting was spent in purposeful wandering. Sometimes I would just sit in the lunch room, in the waiting room, or in an empty examination room. At this time, I want to emphasize that the staff made no organizational, i.e., functional use of my presence in the Clinic. Thus a very substantial portion of my research time at the Clinic was spent "standing around" and observing.

While it had formally been established that I would be able to observe doctor-patient encounters, it turned out to be over a month before I was permitted to enter an examination room. I would often stand in the area outside the examination rooms hoping that a physician would ask if I would like to observe a medical encounter. Sometimes I would ask to observe a medical interview and receive a reply such as "I think I'd better see him (or her) alone". It soon became clear that, while the Clinic presented a casual appearance, the ethic of privacy of the examination room was an ideal which was to be maintained. During this period of research, I regularly attended daily sessions of morning chart rounds and would often spend entire days at the Clinic. I arrived at the Clinic at 8:30 a.m. and usually left sometime after 5 p.m. when I would return home and apply myself to
the task of writing up notes from that day.

I soon learned that many of the staff bought their lunch at the local cafe. Often they would phone in their order and walk across the street to pick it up when it was ready. I started to order lunch from this cafe as well and to offer to pick up the lunch of other Clinic personnel as well as my own. I ate with the staff and participated in noontime lunch discussions.

My research at the Clinic was directed towards obtaining a corpus of audio tapes of various types of interactions that take place between staff and patients. Since this was not happening I often felt that the time which I spent there was being wasted. In retrospect, however, I can see many advantages accruing from this period. I became acquainted with the Clinic itself and learned something about the personalities of the physicians and nurses. Meanwhile, I became more and more a normal and unquestioned feature of the setting. The staff became accustomed to seeing me around, e.g., when I did not go to the Clinic, staff members would sometimes comment upon my next visit "where were you" or "we missed you." In many respects I began to feel obligated to go to the Clinic as much as possible, even though I was not securing the corpus of audio tapes that I had initially desired to obtain.

While the opportunity to observe chart rounds was providing me with certain details, it upset me that I was not permitted to tape-record even this aspect of Clinic life. I told the director that I was finding it difficult to remember all that was said during the
conference and that I felt that taking notes during chart rounds would be very disruptive. I asked him if it might be possible to tape record these sessions. He said that he would ask the staff and would let me know their decision. I was eventually allowed to tape-record morning chart rounds. I felt that this was a significant changing point in the conduct and progress of my research. And after spending two and one-half months in the field, I was finally permitted to bring a tape recorder into the Clinic.

Originally I used a Sony TC100 portable tape recorder but was eventually able to purchase a Sony TC40, a much smaller and more versatile machine. Clinic members soon became accustomed to the presence of the tape recorder. While up to this point I had only been permitted to observe a very small number of medical interactions between doctors and patients, once I began to tape-record chart rounds some of the physicians allowed me to observe, but not to record some of their interactions with patients. As time progressed, I realized that my chances of being permitted to observe doctor-patient interaction were better on certain days than on others, depending largely upon which members were working that day. For example, the director never asked me if I would like to observe him with a patient and, for obvious reasons, I felt that it was best not to make any attempt in this direction. While many other physicians were quite friendly otherwise, they were reluctant to allow me into the examination room. One of the younger physicians said that he was concerned with ethical considerations and, although he did allow me to observe some of his
encounters with patients, it was always on my own initiative; he never asked if I would "like to sit in on this one." He would allow me to observe one type of patient only, -- this usually was an older male rather than one of the many female patients who frequented the Clinic and who often required physical examinations. My two best informants were a female physician and an older physician who had recently moved to Vancouver and secured a position with the Clinic. Both informants, while not becoming close personal friends, expressed an interest in my research and allowed me access to their encounters with patients.

Some of the apprehension about my presence at the Clinic may be attributed to the fact that the Clinic was partially funded by the university and was conscious of its status as an experimental medical facility. The Clinic was being monitored by members of the medical profession to determine its effectiveness and worthwhileness. There was some conflict between those members of the medical profession who felt that the traditional "out patient" departments of hospitals were sufficient, and those physicians who advocated the establishment of community medical clinics. Because of this, the Clinic was more than ordinarily anxious about being able to present a favourable image.

Therefore, having a sociologist in their midst must have made for some uncertainties on the part of many staff members. I assume, too, that many of the staff did not believe me when I told them that I was not interested in the internal politics of the Clinic or the personal politics of individual staff members. When I first started the research I would often participate in the Wednesday business-lunch
meeting, but I soon refrained from this practice because some of the issues discussed at these meetings were "politically hot" and I wanted to make it clear that I was not concerned with the politics of the Clinic. Nevertheless, I believe that many of the staff still viewed my presence with considerable caution.

Until May 1972 my research activities at the Clinic consisted in observing and tape-recording sessions of chart rounds, and of observing the general workings of the Clinic and some, but not many, medical encounters. In addition I tried to attend the Youth Night Clinic on a regular basis. Some additional staff was hired during this period since additional funds had been acquired through various community and governmental agencies. Further, the Clinic had been able to expand to include not only general medical services but also dental and nutritional clinics. As was previously the case, there was still no defined organizational place for me in the structure of the Clinic, so that much of my time was spent standing and waiting for opportunities to view medical interactions.

In May 1972 I managed to secure a research stipend from the Department of Paediatrics at the University. The purpose of the stipend was to allow me to continue my research at the Clinic throughout the coming summer months. This was important because, aside from financial support alone, it meant that for the first time during my research at the Clinic, I was legitimately entitled to say that I was working for a medical department. It also brought me into closer contact with those physicians in the Faculty of Medicine who expected
to see some results at the end of the summer period. Hereafter my research position in the Clinic was one of a semi-credentialed research sociologist who was employed by a medical department to do research into doctor-patient communication. In this way clinic members knew that my research was being monitored by physicians outside the staff of the Clinic. I remember going to a cocktail party in which one medical Department Head asked me while the director of the Clinic was within hearing distance, if I thought that the Clinic was really worthwhile. I replied that I thought it was most innovative. Thus the director could not only credit me with a favourable response towards the Clinic but was also made aware of the fact that I was in contact with other medical professionals who were monitoring the Clinic. I confess that gaining this position of a credentialed appointment to do research at the Clinic made me feel more confident for I was now a bona-fide medical researcher receiving money from a medical department to secure tape recordings of doctor-patient interaction.

At this state I spoke to the director and informed him that I was hoping that I could begin to record medical interactions as soon as possible. He thought there would be no problem in doing so but asked that I construct a formal proposal of the type and quantity of medical interactions I wished to record. The rationale given for this was that the staff should have an idea of exactly what it was that I was interested in recording. I remember going home and considering the feasibility of complaining to one of my medical contacts that Dr. Tough
was not cooperating, but instead I decided to produce the requested proposal. It was subsequently distributed to the staff.

The use of the tape recorder posed certain practical and ethical problems. The Clinic staff decided that the physician should ask his patient if it was permissible to record the medical encounter. The ethical problems were further resolved when my research proposal was approved by the Faculty of Medical Ethics Committee. At last I was going to be allowed to tape-record medical interactions. The majority of the data in this report comes from transcripts of these tape recordings.

Thus far, this chapter has been oriented towards the production of an opening description for an ethnographic report. Its main concern has been to provide the reader with a summary and description of the Clinic, its setting and modus operandi. In contrast, I would now like to focus upon the ways in which the preceding description is, in itself, made available to us as a topic for investigation. In the remainder of this chapter I wish to examine the ways in which an initial description such as this is a constituent component of the "standard ethnographic format." I also intend to show how this initial description is related to other and subsequent sections of an ethnographic report. And in addition, I want to outline the relationship which obtains between the production of an ethnographic description and the recipients of that description.
Like any ethnographer, producing a description of a research setting, I have provided my reading audience with a general description of the character of the research setting.

The reader can presume that the materials presented thus far constitute what might be called a "background description" of the Clinic and that a more analytical description of the Clinic is to follow. He might expect further that in subsequent sections of this report I will address issues concerning various social organizational features of the Clinic. It would be reasonable to anticipate such subsequent topics as: the characteristics of community as compared to private medical practice, role-conflict between various clinic staff, drug problems amongst the young, middle class doctors and the treatment of lower class patients, the organizational structure of medical interviews, the search for an abortion, disease terms used by non-medical personnel, pregnancy and illegitimacy, medical diagnosis as a social achievement, and so forth. In short, after reading these initial materials, it would not be unreasonable for the reader to expect to find that the subsequent report was concerned with and organized around topics such as those suggested above. And to see such expectations as "normal" expectations would not be seen as inappropriate.

Indeed, the initial description of the Clinic thus far presented must be viewed as the background to some subsequent and more analytic section of the ethnography, for, were I to furnish only the previous
description of the Clinic and claim that such a description was an adequate ethnography, this claim would be rejected by anyone who has had some experience in ethnographic research. Although we are unable to specify exactly what it is that constitutes adequate ethnographic description we are able to see that these initial descriptive materials do not comprise an ethnography of the Clinic. At best, this initial description can be viewed as mere background material prefacing a subsequent analysis of some aspects of clinical organization.

In standard ethnographies, the fact that a description such as the one given at the start of this chapter is to be seen as "a preface or lead in" to some subsequent body of material, is not to be examined in those ethnographies themselves.

In contrast to standard ethnographies which take such features for granted as an obvious and unquestionable part of "the scheme of things," it is my intention to make a radical departure here. To wit, I want to ask how it is that such material can be seen as a resource to the reader for the understanding of subsequent sections of the ethnography, that is, useful in such a way that if this prefatory material was absent the ethnography could be viewed somehow deficient or defective.

Therefore, instead of proceeding with a description of the Clinic's analytic features, this chapter will now focus on how it is that such prefatory or background materials contribute to the social organization of the ethnography as a whole. I would like to add that it is not my primary intention to come up with a list of definitive answers for the
successful production of an ethnography. Yet, if no such lists are forthcoming then, hopefully, some thought-provoking questions will have been realized.

The description of the Clinic given at the start of the present chapter could have been, had I decided to follow it up as such, a certified and acceptable way of beginning an ethnographic monograph on the subject of the Clinic. It would have been seen and accepted as such by anthropologists and sociologists doing ethnographic research. It would not have been seen as some idiosyncratic format adopted for esoteric reasons.

It is a common practice for anthropological ethnographers to provide their readers with a preliminary description of "some other culture" as a preface for a more highly detailed and analytical description of that culture. Thus Raymond Firth in his work *We, The Tikopia* gives some preliminary information concerning the Tikopia:

Rarely visited by Europeans and with no white residents, Tikopia lies in the extreme east of the British Solomon Island Protectorate, and is inhabited by twelve hundred healthy and vigorous natives. Homogeneous in speech and culture, they are a unit of what may be termed the "Polynesian fringe" in Melanesia, their closest affinities being not with the people of the Solomons region but with those of Samoa, Tonga and even more distant groups to the east.

Almost untouched by the outside world the people of Tikopia manage their own affairs, are governed by the chiefs, and are proud of themselves and their culture. They are primitive in the sense that the level of their material technical achievement is not high and they have been affected in only a few externals by Western civilization; at the same time they have an elaborate code of etiquette, a clear-cut systematic social organization and they have developed very strongly the ceremonial side of their life. They still wear only their sim
simple bark-cloth, they live in plain sago-leaf thatch huts, they carry out the traditional forms of mourning, marriage, and initiation. *Mirabile dictu*, a large section of them still worship their ancient gods with full panoply of ritual, a condition almost unique in the Polynesia of to-day.

A brief reference to the religious condition of the people is necessary in order to give some idea of the setting in which my work was carried out....

In a similar fashion Evans-Pritchard makes some preliminary remarks about the Azande in his classic ethnography *Witchcraft Among the Azande*:

The Azande (singular, Zande) are a negroid people who live on the Nile-Congo divide. They are mesaticephalic, of medium stature, and of a skin colour varying from chocolate to light reddish brown. No further account of their physical characters is given here because the photographs of a number of Azande are sufficient to show the reader what they look like. Likewise no effort is made to assess scientifically their psychological characters, but it may be said that in the experience of the author, as well as in the experience of other Englishmen who have lived among them, the Azande are so used to Authority that they are docile; that it is usually easy for Europeans to establish contact with them; that they are hospitable, good natured, and almost always cheerful and sociable; that they adapt themselves without undue difficulty to new conditions of life and are always ready to copy the behaviour of those they regard as their superiors in culture and to borrow new modes of dress, new weapons, and utensils, new words, and even new ideas and habits; and that they are usually intelligent, sophisticated, and progressive, offering little opposition to foreign administration, and displaying little scorn for foreigners. The reader will be able to form his own judgement of their characters from the idea and actions recorded in this book.

The royal class are more proud and conservative; they are contemptuous of their subjects and detest their European conquerors. They are often handsome, frequently talented, and can be charming hosts and companions, but generally they mask behind a cold
politeness their dislike of the new order of things and of those who impose it, and I found that, with rare exceptions, they were useless as informants, since they firmly refused to discuss their customs and beliefs, always deflecting conversation into some other channel, and that they contrasted in this respect with their subjects, who seldom objected and were often keen, to furnish information....

It would appear that the accredited ethnographic constructions of an ethnographic report follow a recognizable structure which may be characterized as a "standard ethnographic format". When reading an ethnography, the first thing which a reader encounters is a body of materials designed to "set the scene", "to serve as an introduction to", or "to provide background material for" subsequent analytical sections of the ethnographic report. Firth's characterizations of Polynesian society and Evans-Pritchard's description of Azandeland occur in the introductory sections of their reports, that is, before their detailed main accounts of the society which they are studying in much the same way as my initial description of the Clinic stands as prefatory to what could well have been a more detailed and thoroughgoing report on the organization of the Clinic.

To speak of a "standard ethnographic format" is to do much more than to notice that ethnographic reports are recurrently constructed in this manner. In attending to this, one raises as an issue the relationship between these initial, introductory sections and those subsequent analytical sections of the ethnography that are presumed to be forthcoming. In this way, the format itself becomes available as a topic in its own right; one suitable for empirical investigation. It may well be the case that an investigation of the relationship
between the beginning sections of an ethnography and the more analytical sections of the ethnographic report will contribute to our understanding of the social organization of ethnographic description.

Let us begin our present inquiry by examining my own initial description of the Clinic and asking the following question: How does such prefatory descriptive material contribute to the reader's understanding of the forthcoming analytic sections of the ethnography?

One possible answer to this question is that such prefatory material provides the reader with a "sense of the setting". Unfortunately, this answer does not address the question, for the issue is how does this prefatory material relate to the subsequent analytic section of the ethnography rather than whether or not we are able to characterize its contents. In what sense is the material contained in the initial description of the Clinic relevant for an understanding of some subsequent organizational feature of the Clinic? It remains that while such material may be used by the reader to understand the subsequent analytical sections of an ethnographic report, exactly how the reader is to utilize this material remains something which is not customarily specified by the writer of an ethnography. An example may help to clarify this point.

Evans-Pritchard provides the reader with the following characterization of the Azande:  

The Azande of the Anglo-Egyptian Sudan live in Savannah forest. During the rainy season the grasses grow so high and so densely that they present a serious obstacle to any one who wishes to leave the paths. During the dry season, which commences in November and continues till
April, the whole hush is fired and the country is revealed as an undulating plain, intersected by innumerable small streams, and not so level as one might suppose when traversing it while the grasses are high. It is sparsely wooded and the trees grow to a great height at the sides of streams where they form fringing forests. Azande prefer to live along these streams rather than in the open plain, but are now forbidden to do so in the Anglo-Egyptian Sudan because species of tsetse (Glossina) which contain trypanosomes harmful to man breed near water. Here and there are outcrops of ironstone or granite, either bare or covered with low grasses.

Admittedly we recognize Evans-Pritchard's commentary about the Azande as being a recognizably standard way of beginning an ethnography and can rest assured that in the forthcoming sections of his work he will provide us with detailed accounts of Zande witchcraft. This commentary, however, gives absolutely no indication of the ways in which Evans-Pritchard's readers are to utilize this information vis-à-vis the subsequent materials on witchcraft. How is it that information about such things as the rainy season, the savannah forest, or the height of the trees at the side of the stream will be usable by the reader when reading subsequent parts of the ethnography?

The point I am making is that ethnographic descriptions do not, as a routine part of their procedure, recognize a need for outlining the relationship between prefatory sections of the ethnography and those sections that are typically to follow. By instructions I mean a common sense definition or set of principles telling the reader to use the materials in some specific way. Telling him, for instance, "to use the information about the rainy season when reading (and only when reading) chapter four of this report" or "to make special note of the fact that Azandé
territory is intersected by innumerable small streams," (or, perhaps, small streams).

My description of the Clinic included information about the "ethnicity" of the neighbourhood, the characteristics of the patient population, the dress and character of the staff, and so forth. These materials may give a "sense of the research setting," -- at no point in this description do I tell the reader why I have included this information or state how it is that this material is part of and relevant to subsequent sections of the ethnographic report.

Thus it appears that, although we can speak of a "standard ethnographic format" in which the ethnographer provides some initial and prefatory material in order to provide the reader with a sense of the setting, the reader is left on his own to make the necessary connections between these prefatory materials and the more analytical sections that follow. Let us continue our examination of the relationship between them.

There are some disciplines, for example, formal logic or chemistry, where the reader's ability to make sense out of what he is reading may be directly dependent upon the serial placement of the materials presented to him. That is, some of these presentations are unavoidably "cumulative" in nature so that the reader must understand theorem A before he can grasp theorem B, or he must appreciate chapter one before he will be able to read chapter two with the necessary comprehension, etc. This means that there will be recognizably correct ways of doing a report. Be that as it may, there seem to be no such constraints in the
ordering of the parts of an ethnographic report.

While ethnographies typically have beginning sections, and these sections constitute background material to the more analytical sections of the report, the reader is not required to read the beginning section first so that he will then have the requisite knowledge for understanding the subsequent sections of the ethnography. Indeed, the report could well be arranged in such a way that present lead-in material would then appear elsewhere in the monograph and this rearrangement would not interfere with the reader's ability to understand the material presented to him in the analytical sections of the report. For example, it would be possible for Evans-Pritchard to have informed his readers about Azande witchcraft, poison oracles, accusations of witchcraft, etc., without requiring that the reader first become familiar with his material containing information about the geography of the Egyptian-Sudan.

Our examination of the relationship between the initial and subsequent analytical sections of the ethnographic report suggests that its construction follows from the dictates of what is seen as an accepted format for ethnographies rather than from any necessary or logical ordering of material. It is also notable that introductory descriptions of the research setting, like my own description of the Clinic, are at the beginning of ethnographies merely to satisfy stylistic and presentational concerns, and not because it is in some strict sense necessary that the reader understands them before attempting to make good or comprehensive sense out of subsequent material. While one could perhaps claim that these initial materials are intended
to provide the reader with some sense of the setting or to get him in the proper mood, such claims have little theoretical import if these beginning sections make little contribution towards or are problematically related to the analytical materials that follow.

Another feature of ethnographic description which becomes apparent when we consider the initial sections of these ethnographies is that the descriptions which they give us could easily have been supplied by an educated layman. It is obvious, too, that in order to produce such a description, one does not have to acquire any special or previous training in the social sciences. Indeed, my description of the Clinic might well have been written had I no knowledge whatsoever about its possible or potential relationship to the subsequent sections of this report. All of these considerations point to the fact that this description is a product of "common sense" rather than rigorous "scientific" procedures. Therefore, the dominant facts about the materials under consideration are a) that their production rests largely upon mere common sense and is largely independent of any anthropological or sociological considerations; and b) that these materials are related to the subsequent sections of the ethnographic report in unspecified and problematic ways. This second feature of ethnographic description will be discussed in greater detail later in this chapter.

The preceding observations and comments are not meant as criticisms of the ethnographic enterprise, nor are they criticisms of the work of Firth or Evans-Pritchard. The observation that the initial prefatory
materials of ethnographic reports are not logically or theoretically connected to the subsequent analytical sections of an ethnography is not a proposal that such materials should be so connected.

It is observable too that the main part or body of an ethnography contains materials that are more analytical than those which occur in the introductory sections and that these more analytical materials generate various theoretical problems for the social scientist. Such sections typically concern such things as kinship and/or family organization, various views of religion or magic, or political organization and its relationship to economic organization.

Whereas members of the professional community frequently find that they are able to engage in conversations about, to agree with or argue about the main sections of ethnographic reports, these main features are, as has already been pointed out, generally prefaced by some descriptive body of material which has been designed to give the reader a sense for the setting of the major portion of the ethnography. And such prefaces, while omnipresent parts of standard ethnographic presentations are, in themselves, not seen as something which has any abiding theoretical interest. Thus, e.g., while there has been much controversy about Evans-Pritchard's rigid dichotomy of Azandé beliefs concerning that which just happens and that which is caused by magic, no investigator could be expected to argue with his statement that, "Azande prefer to live along these streams rather than in the open plain..." 11

Are we to regard these initial sections to ethnographic reports
as being merely "scene setting" materials? And, since their relationship to the more analytical sections of an ethnographic report is not grounded in any logical connection but in adherence to a presentational format alone, do they have no theoretical interest? I would like to suggest that instead of treating these initial materials as mere scene setting devices, they should be examined in their own right, i.e., in terms of their organizational relevance for the construction of ethnographic reports. Thus, having noted some of the features which obtain between the initial and subsequent sections of the ethnography, let us focus our attention upon the examination of these initial materials themselves, and attempt to explicate their organizational import for what may be seen as the standard ethnographic format.

The ethnographer enjoys a privileged position or perspective which the reader does not. For one thing, he has a greater knowledge of the culture under study than does the reader. Ethnographers often become specialists on some culture or society and can be said to "own" a certain expertise on that culture which, by and large, the reader does not and will never "own." The ethnographer has the job of mediating between the phenomena which comprise the research setting and the reader of his report. In many instances, all that the reader will come to know about the research setting is dependent on what it is that the ethnographer tells him. Ethnographic descriptions have been characterized by A.R. Lough as somewhat similar to "traveller's tales":

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Recall first that the anthropologist was at the beginning a traveller. By virtue of his acquaintance with parts of the world out of reach of most of us, he could tell stay-at-homes interesting stories about the diet and economy, the religious and sexual practices of alien and often exotic peoples. His talk looks to be of a piece with that of one's neighbour who has just come back (with slides) from Bermuda or a tour of the Western Parks.

So-called "arm-chair social scientists (that is, insofar as they are unable to participate in the actual experiences of the ethnographer they are necessarily finding out what he meant from a distance) must trust that the ethnographer has done his best to provide an accurate and comprehensive description of the culture being studied. The reader must assume that the ethnographer is not fattening up his descriptions or simply lying to him. For example, consider my description of the Clinic. It is evident that readers are not able to investigate my propositions about the Clinic, and to ascertain that it is in fact located in an Italian area of Vancouver, that its staff were casually dressed, that the Clinic had a free and easy atmosphere, etc. (In other cases, of course, where, say, the researcher is talking about some distant or extinct society, verification is an almost insurmountable problem.)

Further, this privileged position is a general feature of ethnographies and does not apply to each and every type of description. For example, people commonly read political documents or newspapers with the intent of finding not true accounts but propaganda in these. For they have assumed from the outset that the author has a biased viewpoint. In contrast, the reader of an ethnographic report is not supposed
to hold this type of attitude towards the ethnographer. While the reader may initially grant the ethnographer qua ethnographer, a high degree of unquestionability, this is to say that there is no guarantee that the reader will maintain this attitude throughout the reading of the ethnography. What is there about the character of ethnographic descriptions that allows the ethnographer to continue to have this privileged position? I suggest that those initial and prefatory sections of ethnographic reports, while not being logically connected to the more analytical sections that follow, help provide the ethnographer with this credentialed status.

Because he has a reading audience composed almost entirely of stay-at-homes, the ethnographer can rely on this fact to assume that his reading audience will have some curiosity about, but no actual experience in the research setting. For example, how members in the foreign culture dress, what women do in the society, various religious aspects of the culture, what type of personalities they have, and so forth are all matters that can be assumed to be of interest to the reader, and the ethnographer can presume that the readers of his ethnographic description can make use of the fact that his audience will have a set of common sense cultural relevancies concerning the culture being studied. The ethnographer can rely upon these cultural relevancies for the construction of his initial prefatory materials for he knows in advance that they will constitute items which can be appropriately included in the opening section of his ethnography.

While such materials may not be the central concern of the
ethnographic report, their placement in this initial or prefatory section satisfies much of the reader's curiosity about the research setting. These prefatory materials also serve to indicate that the ethnographer knows much more about the culture than he intends to put down in his report. And they further claim that he knows the setting in the ways that are typically expected of someone who has spent a considerable period of time in some culture. While the ethnographic report may be a specialized monograph on a subject such as kinship, the sum of materials presented in the opening section demonstrates that the ethnographer has considerably more knowledge about the culture than he has chosen to present in the ethnography. Thus, if he had wanted to he could have told the reader not only about kinship but about other aspects of social organization. Thereby, introductory materials often suggest that the author has much knowledge which is not spelled out in the main section of the paper. Yet, such materials do not indicate how it is that the ethnographer has chosen to include some things in the preface and quite different things in the body. Nor, of course, does the preface tell us exactly what it is that the ethnographer has failed to tell us in the major parts of his presentation. Thus, the relationship between these two parts remains unclear.

The ethnographer does not merely occupy a privileged position vis-à-vis his reading audience, but rather he displays that privileged position. The ethnographer uses the opening sections of his report to furnish his readers with a description of some phenomena relevant to the society which is being studied. He assumes that his audience would
normally be curious about these features and that they would expect him to mention them in his report.

It was pointed out earlier that these initial or prefatory materials do not come with an explicit set of instructions explaining their relationship to subsequent sections of the ethnography. However, these initial materials do provide the reader with a host of implicit resources which he can use in his subsequent reading of the ethnography. Thus, while the initial materials customarily included in such a report do not provide the reader with an explicit list stating how he should or must use them, they are still an important part of the ethnography and, as such, merit attention. Perhaps an example will help clarify what I mean by this.

Evans-Pritchard (see above) tells the reader that the Azande live in the Anglo-Egyptian Sudan (see above). Although this piece of geographical or locational information is not accompanied by a set of instructions telling the reader why it is relevant or important and how it is to be understood and used, it is, nevertheless, information that has consequentiality for the ethnography of the Azande. Had Evans-Pritchard left the reader to take a guess at the location of Azande territory, the reader could easily be misled. Were he to assume, for instance, that the Azande lived in South America or Polynesia, his entire understanding of the ethnographic report would be colored by this assumption since he would try to make what he read about the Azande fit in with his knowledge of these cultures. Or, to use an absurd example, were he to decide that the Azande were native to
Oxford rather than to Azandeland, it would be quite possible for him (the reader) to find Evans-Pritchard's materials on witchcraft preposterous since he knows about Oxford and what goes on there and realizes that practices such as those described by Evans-Pritchard could not possibly take place there. Then, too, the author is establishing the fact that the things described in the ethnographic report are things common to the Anglo-Egyptian Sudan (and, by implication, to Africa) and are not necessarily things bearing any resemblance to any other society with which the reader is himself familiar. In this way too, Evans-Pritchard is setting himself up as an expert, i.e., as one who, in contrast to the reader, is legitimately entitled to describe Azande culture.

Thus, materials that may initially appear to have the chatty character of a travelogue are usable by a reader as a resource for understanding the subsequent analytical sections of the ethnography. However, these materials are a resource made available to the reader which is to be used in any way he so chooses. For this reason, they are not subject to enumeration as a set of explicit instructions. For instance, my description of the Clinic stated that it was located in Vancouver. This description allows the reader to use his own stock of common sense knowledge about the organization of medical practice in North America and to come up with an approximation that is appropriate to Vancouver. And this characterization will be important to subsequent descriptions of the Clinic. Note, however, that the exact relationship between this geographical information and subsequent
analytical materials remains unspecifiable.

While such initial materials constitute resources which readers can utilize, the ordering of these materials is based on common sense rather than a set of scientific procedures. Earlier, I showed that the materials contained in my description of the Clinic was not the product of any special sociological training but were selected without reference to any scientific procedure. A few examples may help to clarify and expand upon this.

Let us assume that an ethnographer is writing a description of some feature in his own society. It may not be necessary for him to include a description of the climate, topography, or physiological characteristics of the people he is studying although such information is typically included in ethnographies of foreign cultures. The decision of what to include and what to omit is not, however, something which he decides upon after referring to some empirical standard for the construction of ethnographic reports. Instead, such decisions are made on the basis of what it is that he commonsensically knows about his intended audience and their relationship to the setting.

Thus, in my description of the Clinic, I felt no need to discuss the climate or geographical characteristics of Vancouver for I know that I can rely upon my reader's knowledge of Canada and Vancouver to supply the details for these features. Thus, were I to say for instance, that the number of patients attending the Youth Night Clinic is considerably higher in summer than in winter, I would not
have to go into an analysis for this but could rely on the reader's knowledge of Canada and Vancouver to account for this increase. That is, I would expect him to know that rainfall is heaviest here during the winter months, and that during the summer Vancouver attracts a considerable number of transients. And these should account for the different rates of attendance. If however, I expected my audience to be ignorant of the climate and geography of the city, then, and only then should such features be described for their benefit. Indeed, had I elected to include such information in my initial description of the Clinic, the reader may well have found the material boring or questioned my competence as an ethnographer.

Further it is apparent that a) different ethnographers may describe the same society in very different ways, and b) an ethnographer will attend to different features in different ethnographies. That is, he will not describe, say, the magical beliefs of every society for which he produces an ethnographic report. These features are a result of the fact that his descriptions are based upon the dictates of common sense rather than some empirical system.

A chief constraint here is the variable of the audience. This has pragmatic implications for the production of the report. Thus, one can imagine, say, Evans-Pritchard constructing a monograph which is intended for those familiar with the doing of ethnographies, and, on a later occasion, revising it in order to present it to a lay audience. It is apparent here that a lay audience would have certain problems appreciating a report addressed to professional ethnographers,
while a professional audience might well find any other presentation unsatisfactory.

Thus far I have shown how the ethnographer constructs the prefatory materials of the ethnographic report by using common-sense criteria to decide what is and what is not proper material in terms of his intended audience. I have also shown that readers are able to understand and to make good use of these materials in a way which is recognizably sensible. That is, whereas the ethnographer provides the reader with resource materials, he does this without knowing how it is that the reader will be able to make use of them in his reading of subsequent sections of the report. This allows for the possibility that the reader may utilize these initial resources in ways that are completely different than those intended by the ethnographer. Therefore, there does not have to be a one-to-one correspondence between the ethnographer's intentions and the reader's understanding in order for these materials to contribute to the understanding of subsequent sections of the ethnography.

Thus, it is to the ethnographer's advantage to provide the reader with as much information about the research setting as he thinks might contribute to an understanding of the subsequent sections of the ethnography. While these prefatory sections follow no empirically demonstrable order, they have the advantage of being open to an extremely wide range of interpretations. The ethnographer would usually prefer to furnish the reader with too much rather than too little information about the research setting in order to be on the "safe side". That is, he provides the reader with any and all information which might contribute to his understanding of the subsequent sections of the ethnography.
Here, remember that my description of the Clinic included materials designed to provide the reader with a sense of the research setting. However, I am unable to specify exactly how it is that these materials are relevant to the forthcoming sections of this paper. Nor am I able to formulate the ways in which the reader will utilize these materials when *vis-à-vis* subsequent parts of this presentation (such as my analysis of chart rounds presented in chapter three).

In this chapter I have examined some of the organizational features inherent in ethnographic reports. I have presented a description of a community medical clinic (the Clinic) and then examined that description itself in order to illustrate features common to the beginning sections of ethnographic reports. By treating this initial description as data, I have argued that an ethnography is ordered in terms of common-sense procedures and common sense relevancies rather than in terms of some specific empirical procedures. I have, to this end, focussed on the relationship between the ethnographer and his intended readers. In the following chapter, I shall consider some of the issues involved in producing the next section that is, the analytic section to an ethnography.
Footnotes: Chapter One

1. While having no previous training in medical sociology, I did have an interest in studying the general features of conversational structure. At the time this research was initiated I felt that the Clinic would provide an excellent opportunity for me to collect a corpus of conversational materials on which it would be possible to conduct a "conversational analysis". This goal became transformed over the course of the research.

2. Note: Respect for the confidentiality of the Clinic dictates that no source be quoted here:

   Community Clinic
   Address
   Telephone Number
   is open 9-5 Monday-Friday if you have B.C. insurance
   and make an appointment; or, if you don't have
   insurance, go to their Youth Clinic on Thursday 6-9
   p.m. Cost is $2.

3. Author's field note October 13, 1971. The reader should note that I constructed this note several hours after terminating my visit to the Clinic.

4. During youth night it is routine clinical practice to open a medical chart on each new patient. These charts are filled and are utilized on each subsequent visit to the Youth Night Clinic. These charts, however, are not usually reviewed during the occasion of chart rounds as the Youth Night Clinic patient population is regarded as strictly a transient patient population.

5. Author's field note October 13, 1971.


11. Ibid.


CHAPTER TWO

ISSUES IN PRODUCING AN ANALYTICAL DESCRIPTION

Introduction

In the preceding chapter, I looked at some of the features of the standard ethnographic format and examined some of the properties of those materials that may be said to occur routinely in the initial part of an ethnographic report. The next section of this report were it a "standard ethnography," would provide the reader with a detailed, analytical description of some noteworthy aspect of Clinic organization. This would necessitate that I select some feature which I considered to be particularly important or interesting and describe it in detail and in a way that is responsive to the culturally significant behaviour of Clinic members. Instead of presenting such a description, this chapter will attend primarily to the question: How is it that the ethnographer is able to do a description of a setting in the first place? Thereby, I will consider my relationship to the research setting and how it is that I am able to select some aspect of Clinic organization from which to produce a description. I will also examine my relationship to the type of data that I have collected and look at some of the ways in which I might utilize that data were it my intention to provide a "standard description of Clinic organization." As in the previous chapter, our focus will be on the production of an ethnographic description itself. Specifically, this chapter will discuss some of the issues involved in producing a description of "chart rounds." The core of this chapter will
be devoted to a look at the steps leading to the selection of this occasion as a topic in an ethnographic report and to an examination of the manner in which a description of its organizational features is produced.

I will begin this chapter with a review of some of the ways in which a social scientist could utilize the corpus of data which I collected during my research at the Clinic. This will force us to consider the relationship obtaining between the researcher and the research setting. I will examine the way in which a medical sociologist might utilize data collected at the Clinic and will demonstrate that such an approach fails to preserve the integrity of those activities and occasions from which the data originates. I will thereby show how it is that chart rounds constitute an appropriate possible topic for further analytical description. However, I will argue that a description of an occasion should be responsive to how the members of the setting organize that occasion, and point out some of the deficiencies inherent in traditional ethnographic analyses. Next, I will consider how a new approach to ethnography (i.e., cognitive anthropology) attends to the relationship between the researcher and the research setting. The research goal of this new approach to ethnography, although admirable, is criticized as an unattainable goal. After considering some of the various ways in which a researcher could relate to the data gathered of the Clinic, I will present the reader with what I term an "inadequate" description of chart rounds. In this way, I intend to demonstrate some of the advantages in using talk as a resource for describing the self-organizing
features of an occasion. At the same time, the issues discussed in this chapter are intended as an introduction leading up to the interactional analysis of chart rounds presented in the following chapter. This chapter, then, could be regarded as a map of the path that I ventured in producing the description of chart rounds which occurs in the following chapter.

The Research Setting: The Medical Sociology Approach

I found myself in the possession of many hours of audio tapes and many pages of field notes taken at the Clinic. Like any researcher, I now faced the task of organizing this data and presenting it to the reader in a competently organized format. Since I did not have a research hypothesis directing my research, I had to consider the various ways in which my data might best be utilized in a way that would reflect the research setting from which it was collected. Therefore, as a first step towards a solution to this problem, I thought about some of the ways in which a traditional medical sociologist might orient himself to research within the Clinic and how he might want to make use of the data which I had collected.

The traditional approach would regard the privileged position of the researcher being "in" the research setting as a way of obtaining information about how physicians treat lower class patients, how staff conflicts are resolved, patients' attitudes towards physicians, the "informal" organizational structure of the Clinic, etc. He would feel free to use data collected from the entire range of social interactions that occur within the Clinic, and would not be required to attend to the
social context from which his data was collected. An example may help to show what I mean by this.

Let us assume that the researcher is concerned with studying how physicians treat lower class patients. When I say that the researcher could utilize data obtained from within the Clinic and yet fail to attend to the social context in and by which that data was produced, I mean that he could use data obtained from such diverse pieces of interaction as medical examinations, coffee breaks, lunch-time conversations and so forth as indications of physicians' actual attitudes towards lower class patients. It would be appropriate for the researcher to extract from the stream of Clinic behaviour those instances of interaction that somehow manage to support his research interests. However, an approach such as this presupposes that the researcher knows beforehand -- in terms of hypotheses, methodology and research design -- the nature and scope of the questions that his research was directed towards answering in the first place. More importantly, it presupposes that the researcher knows what data would constitute sociologically relevant answers to such questions. It is apparent, however, that an orientation such as this is not attentive to occasions and activities as topics in their own right. That is, it is not concerned with preserving the integrity of the stream of Clinic behaviours but focusses upon the Clinic as a location for gaining information about some predecided research interest. That is, an approach such as this sees an event in a second-hand way. It sees what it sees and reaches the conclusions that it does only after filtering the events at hand through the prism of a prespecified package of
orientations and hypotheses. However, as I will show, there is an alter-
native to this approach. It is also possible for a social researcher to
attend to the occasions and activities that occur within the Clinic as
intrinsically interesting topics for investigation. That is, it is pos-
sible to treat the data which one obtains from the research setting as
something bounded primarily by its actual context (context-sensitive)
rather than something which will be defined primarily by the nature of our
research upon it. Thus, an occasion such as chart rounds can become a
proper sociological topic in its own right. It is my intention to attempt
such an analysis here.

To begin with, chart rounds occur on a regularly scheduled basis.
They form a recurrent and bounded occasion for members of the Clinic. That
is, they have prescribed beginning and ending times and constitute a
normal and natural part of the working day for Clinic staff. As such,
they constitute an interesting aspect of Clinic organization; an aspect
which an ethnographer might analyze in detail. This present description
is directed towards an analysis of chart rounds, however, it intends
above all else to respect the integrity of this occasion. It will do this
by describing how participants view and orient to it as a normal and
natural feature of their everyday lives. In this respect, such a des-
cription differs from the above mentioned medical sociology approach
since it is the occasion of chart rounds itself that is being examined
rather than factors derived from any preconceived, extraneous research
interest.
The Ethnographic Approach

Similarly, ethnographies are distinguished from most other researches by their abiding concern to protect the integrity of the setting they study. They try to produce a description of how members themselves orient to a setting rather than impose his own (the ethnographer's) extrinsic categories upon it. This means that the ethnographic researcher must have first hand knowledge of how the society under study is organized. Of course, this does not mean that the ethnographer has no theoretical interests, but rather that those interests which he does have must be subordinate to a description of how those cultural members he is studying actually view and organize their culture. Although we recognize that descriptions, such as Evans-Pritchard's presentation of Azande witchcraft, have relevance for broader anthropological issues (issues such as magic and religion), we can presume that these descriptions are intended primarily as descriptions of how the Azande organize their witchcraft practices. That is, they are not intended as elaborations or part of some preconceived hypothesis about how witchcraft is organized in general. (Of course, Evans-Pritchard or some other theorist might make use of his descriptions in this way in some subsequent analysis.)

Since I was concerned with the production of an analytical description of chart rounds, I was forced to examine the traditional ethnographic approach. I soon found that such an approach might be less responsive to the integrity of the research setting than it first seemed to be. At this point, I would like to introduce some materials that, at first glance,
might seem to bear little relation to the topic of ethnographic description. That is, I shall consider some findings gleaned from research into the social psychology of experiments. The relevance of these materials should become apparent over the course of the following discussion.

A vast amount of literature has recently been devoted to what has been termed the social psychology of the experiment. Researchers have attempted to abstract and examine some of the organizational features that researchers and subjects attend to when conducting or participating in an experiment. Thereby, much attention has been focussed upon the ongoing matrix of social life in which experiments necessarily occur. For example, those doing experimental social research often talk about "dependent" and "independent" variables. They try to inform their readers about those variables which were under their control and those which were not. The experimental researcher tells how his subjects were selected, what they knew or did not know about the experiment at hand, etc. Yet, research into the social psychology of experiments has shown how the experimental situation is eminently connected to the everyday real-life-world of the researcher and his research subjects. For instance, students may in fact know beforehand or be able to make accurate guesses about the design and intent of the research, and so on. All of these may occur outside of the experiment proper. Yet as current critics point out, it is only because the experimental situation is a part of our ongoing real life that it is made available to us as a topic for empirical investigation. Because this is so, it is futile to speak of
"independent" or "dependent" variables if we fail to attend to those background expectancies which allow for the organization of the experiment. The experimental researcher usually ignores, and feels justified in ignoring, factors such as these. It is apparent however that, were it not for features such as these, it would be impossible for him to conduct an experiment. In this way, experimental research abstracts observations about human behaviour out of the context of everyday life and ignores the fact that actions are irremediably a part of the context in which they occur. Needless to say, such an approach is of limited utility and, while it might purport to tell us something about pain, pleasure, the behaviour of crowds, etc., it will tell us nothing about how these come to be recognizable phenomena in the first place.

Although ethnographers do not speak of dependent or independent variables, the construction of an ethnographic report displays similar features. That is, the ethnographer assumes that he can simply and as a matter of routine, make observations related to various anthropological topics as if those topics were observable independent of the matrix of everyday life in which he found them. That is, although ethnographers have a more loosely defined research language and speak in terms of social organization, religion, magic, politics and power, the family, economic organization, and the like, it is often the case that these subjects are abstracted from the social context in which they were encountered in the course of field research. When the ethnographer makes a field note about, say, kinship organization, that note came from somewhere. It may have been generated from data which he picked up while
eating a meal, while walking with an informant, from overhearing a conversation, or from various situations which he may have encountered. To see each piece of data primarily as an instance of kinship organization is to deny the integrity of the social situations from which the information was collected. In some mundane sense, members of a culture do not just happen to exhibit kinship organization (or anything else for that matter). And to view some feature or features of a society as phenomena routinely displayed as a part of everyday life is to ignore the social situations from which ethnographers extract their data. Thus, traditional ethnographic description may inadvertently impose a pre-existing framework on a description of another culture by going to the field "armed" and "programmed" with a set of theoretical issues and topics that tend to make their ethnographic report less responsive to the self-organizing features of recurrent bounded occasions and activities. Thereby, too, they ignore the world of daily life as a primary topic for investigation.

Since my concern was with the production of an analytical description of chart rounds, I considered this traditional ethnographic approach and came to the conclusion that although such an approach was concerned with preserving the integrity of the research setting, it fell far short of its aim. Recently, however, traditional ethnographic descriptions have been reconsidered and their goals have been reformulated. The result of this reconsideration has been called the "new ethnography" or cognitive anthropology. I soon turned to this new approach to ethnographic description in the hope that it would help me to produce a
description which would respect the integrity of the Clinic.

Some of the prominent features of this new approach to ethnographic description have been outlined by Tyler in the introduction to his work, *Cognitive Anthropology*. He tells us that, in contrast to other, older ways of doing an ethnography,

...cognitive anthropology constitutes a new theoretical orientation. It focuses on discovering how different peoples organize and use their cultures. This is not so much a search for some generalized unit of behavioral analysis as it is an attempt to understand the organizing principles underlying behavior. It is assumed that each people has a unique system for perceiving and organizing material phenomena — things, events, behavior, and emotions. The object of study is not these material phenomena themselves, but the way they are organized in the minds of men. Cultures then are not material phenomena; they are cognitive organizations of material phenomena. Consequently, cultures are neither described by mere arbitrary lists of anatomical traits and institutions such as house type, family type, kinship type, economic type, and personality type, nor are they necessarily equated with some over-all integrative pattern of these phenomena. Such descriptions may tell us something about the way an anthropologist thinks about a culture, but there is little, if any, reason to believe that they tell us anything of how the people of some culture think about their culture.

In essence, cognitive anthropology seeks to answer two questions: What material phenomena are significant for the people of some culture; and, how do they organize these phenomena.3

To utilize this approach in attempting a description of chart rounds necessitates a concern with the ways in which the members who participate in chart rounds cognitively organize the occasion. By this I mean not only that my description must respect the integrity of this occasion, but also that it must discover and describe the way(s) in
which the staff produces the occasion as a regular and recurrent
feature of life at the Clinic. Naturally, to respond to the aims of
a discipline such as cognitive anthropology is to raise issues about
data collection and about what is to be done with the data after it
has been collected. These issues will be discussed later. For the
moment I would like to consider Tyler's formulation of the research goal
of cognitive anthropology:

The "theory" here is not so much a THEORY OF
CULTURE as it is theories of cultures, or a theory
of descriptions. The aim of such a theory is to
provide answers to the questions: 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 behavior in their culture.
Answers to these questions are provided by an
adequate description of the rules used by the people
in that culture. Consequently, this description
itself constitutes the "theory" for that culture,
for it represents the conceptual model of organi-
zation used by its members. Such a theory is
validated by our ability to predict how these
people would expect us to behave if we were mem-
ers of their culture.4

It is not my intention to engage in a criticism of cognitive
anthropology.5 I would, however, like to note two features derivable
from Tyler's preceding comments: First, and a positive feature, is
the way that this approach directs the ethnographer to discover how
social scenes and structures are organized from the standpoint of the
members who produce them. To this end, the very fact that some scene
can be recognized as an "X" in the first place is made a topic for in-
vestigation and is not simply incorporated into the ethnographer's
analysis. Second, and a negative feature, is that the research goal of
cognitive anthropology is not attainable. Some of the reasons why this
is so have been discussed in the preceding chapter, however, this will require further elaboration.

It seems as if, for the cognitive anthropologist, an adequate ethnographic description consists of an analysis that would allow the reader who had the opportunity and wished to do so, to behave in ways that would be deemed culturally appropriate by members of the society which he is describing. This is an utterly impossible goal for at least two reasons, the first reason is not nearly as interesting as the second. Let us consider the recommendation from the standpoint of an ethnographer who wishes to produce an adequate ethnographic description of chart rounds.

It should be obvious that no matter how detailed or responsive a description of members' cognitive organization or doing of chart rounds is made, it could never allow the reader to act as a fully competent participant to all aspects of this occasion. For instance, a necessary component of such competence is a certain degree of medical expertise and no matter how detailed and sophisticated my description became, the reader would not be able to act as a fully competent member of the Clinic such that he could fully participate in this occasion unless he were willing and able to acquire the appropriate medical training. The second and more interesting point is that of common-sense relevancies. That is, the construction of any description requires that the ethnographer utilize his own predetermined set of common-sense relevancies with reference to which items are to be included or omitted from his description. This point was discussed in detail in the preceding chapter. Further, the
ethnographer not only uses his own set of common-sense relevancies to produce his description, but also relies upon his reader to employ their own common-sense knowledge to inform or to fill in the ethnographic description. While Tyler's comments seem to suggest that the ethnographer would be able to describe everything one would need to know in order to act as a competent cultural member, as I have demonstrated here, to attempt any complete description is an impossible task to attempt. Tyler and others have, however, failed to take this into account.

After examining traditional ethnographic description and the recommendations promulgated by the exponents of cognitive anthropology, I found that I still faced the task of producing an analytical description of chart rounds. I felt that any description that I produced should respect the integrity of the occasion but I also realized that my description would not enable the reader to act in the same ways that a fully competent participant to this occasion would in fact act. Furthermore, unlike traditional ethnographers or those engaged in cognitive anthropology, I did not have some a priori theoretical or topical interest in the research setting before commencing my research. In many ways this proved to be an asset for I was trying to make the best possible sense of what I was observing at the Clinic. With no research proposal, design, or hypothesis to be empirically proved or disproved, I was often free to do so. Thus, while I propose to offer a more analytical description of some aspects of Clinic organization, namely chart rounds, the generation of this description was prompted by my examination of the
issues involved in the production of an ethnographic description, and chiefly my consideration of traditional and cognitive anthropologies.

The preceding discussion has dealt with the manner in which a researcher could relate to his description of some aspect of a setting's organization. It has not, however, provided the reader with any definitive recommendations for the construction of such a description. However, it is interesting that descriptions are something which I/we have been doing as a member of society long before gaining any professional sociological competence. For instance, it is by and through our everyday common-sense knowledge that we can see that the occasion of chart rounds could constitute a possible and appropriate subject for further investigation in a standard ethnography. This, like any of our ongoing affairs in the life-world is intimately connected to our ability to use a vast amount of common sense. What follows is a description of chart rounds. It is a description which I have labeled as "inadequate." The reasons for this will be examined after the reader has had a chance to examine its contents.

Chart Rounds: A Heuristically Inadequate Description

Chart rounds is a scheduled occasion for reviewing patients' medical histories. It is a review of patients' charts. During my research, there were two occasions where some of its characteristics were examined. At first, chart rounds consisted of a review of the charts of those patients scheduled to be seen on that day. This review took place in the morning, around 8:30 a.m., that is before seeing any patients. Later, this procedure was changed to a review of the charts
that belong to those patients who had been seen on the previous day.
When I questioned a nurse about the reason or reasons for this round-
about change, I received the following account:  

Researcher: And morning yeah, what was the reason for changing chart rounds?  
Nurse: Well we decided that we were missing a lot of people and a lot of the people we were discussing weren't coming in it would be better to discuss it after they came in and cover everybody.

This procedure was also changed. The following explanation of this new change in procedure was offered to me by the director of the Clinic: 

Director: ...We're just modifying our chart round procedure a little Bruce  
Researcher: Umm mm  
Director: in that ah ah we're we're going to change the system so that one of the part-time one of the full time family doctors is going to be responsible for a student each two weeks  
Researcher: I see  
Director: and part of their job have you met Sherri (Note: at this point an introduction occurs between a member of staff and a new medical student)...  
Ah the ah the ah idea will be that the family doctor will go through all of the charts from the preceding day and try to select out  
Researcher: Umm mm Umm mm  
Director: ah representative group ah which we'll sort of go over... 

Note the taken-for-granted character of chart rounds. This is attested to by the lack of concern which Clinic staff display in orienting
to their occurrence. Chart rounds take place every day at prescribed times. They constitute one of the many demand characteristics which Clinic staff orient to in the course of their daily occupational routines. Chart rounds are a part of the schedule of the Clinic. Thereby, such things as medical appointments are scheduled in a way that will not conflict with the time allotted for chart rounds. Indeed, I have seen physicians hurry through an examination in order to be on time for chart rounds. When staff arrive late for chart rounds, however, it is not normally necessary to offer an apology; they usually just enter the room and take a seat.

A minimal number of participants is required if chart rounds are to proceed as scheduled. I have observed that at least one physician, and at least one other medical person (usually a nurse or a medical student) is required. This necessary minimum complement of personnel is facilitated by having a scheduled time and place for reviewing charts. Thus, on any given day, members of the Clinic know who they may expect to see at chart rounds for they are familiar with each other's schedules. This does not mean that chart rounds do not take place if a usual or expected participant has not arrived, but his absence provides grounds for starting without him. Chart rounds are scheduled to last approximately forty-five minutes. That is, they have a scheduled beginning and a scheduled end, and are geared into some notion of time which participants orient to in their jobs at the Clinic.

While reviewing the charts requires a minimum complement of participants, it is typically the case that only one staff member presents
(that is, has control of) the charts. Physicians and nurses take turns presenting the charts to the rest of the group that has assembled for chart rounds. If, for some reason, the person scheduled to present the charts is absent, one of the other staff members will present the charts, and if the person who was initially supposed to present the charts should arrive while a review is in progress, the charts are not relinquished to him. Sometimes, the charts scheduled for review were assembled by the secretary on the previous evening; at other times they were selected by a physician just prior to chart rounds. Thus, they were made ready for the person presenting the charts during chart rounds. All the charts in the pile had to be reviewed, so that members were able to orient to the progress of any particular occasion by noting the number of charts left to be reviewed.

Chart rounds are intended primarily as a procedure which will facilitate and promote better patient care but they serve other functions as well. They provide an occasion for staff to exchange and to pool their knowledge about diagnoses, treatment, and so on. That is, they serve as a general forum where pertinent information is exchanged. Sometimes, a chart will generate discussions about current articles in medical journals, about research, about new techniques in surgery, etc. Organizationally, chart rounds are an occasion where physicians and nurses can and should take the opportunity to familiarize themselves with current advances in the medical profession. Of course, they also serve as a training session for medical students.

A patient's medical chart is always available as a document-in-use
for the benefit of medical personnel. It constitutes a patient's natural history and is referred to as a matter of routine in almost every interaction between physician and patient. A patient's chart consists of a standard 8 1/2 x 11 inch file folder which is divided into two parts; a "face sheet" is attached to the left hand portion of the folder. A face sheet is a medical form containing general information about the patient: name, address, age, occupation, and British Columbia Medical Insurance number. This form remains in the patient's chart for all subsequent encounters with the Clinic. In addition to the face sheet, the patient's chart contains another form. This is composed of detachable sections for laboratory work, a section for the writing of prescriptions, and a return appointment date for the patient. The physician consults this form during every encounter with a patient. It is constantly updated.

A completed medical form is left in the patient's chart and is later re-arranged in the following manner. A "visit record" form is attached to the face sheet. This form contains a brief summary of what occurred during a patient's visit. Each time the patient visits the Clinic, another "visit record" form is placed above the form which represents the preceding visit. A historical record of the patient's visits to the Clinic is thereby easily available. To the right hand portion of a patient's chart is attached all other medical documents and laboratory reports. Thus when a physician opens a patient's chart the information contained therein is usable for the purposes at hand -- a medical interview or chart rounds.
Chart rounds constitute for their participants an occasion characterizable by the necessity to "get through" them. "Getting through", however, is not subject to the capriciousness of the individual participants. The successful accomplishment of a review of the charts is tied to and interrelated to utilizing the patient's chart to satisfy the organizational features mentioned thus far. Whatever talk that occurs during chart rounds is oriented to these features. It is talk by medical personnel being medical personnel.

The above description of chart rounds is decidedly more analytical than the initial description which I presented in the previous chapter. Following the standard ethnographic format, I provided the reader with some prefatory, chatty comments about the Clinic in general and a subsequent detailed analysis. Its analytical status, however, is not a feature of its position in the standard ethnographic format, but derives from the fact that it is a detailed description of some specific aspect of clinical organization. The description of chart rounds presented above is supposedly informative about the ways in which participants accomplish its organization. The word 'supposedly' is used deliberately for I regard the above description as ana-an adequate one.

An alternative description will be offered in the next chapter. At this point, however, let us consider some of the factors which make for an inadequate description.

The above description of chart rounds is not an incorrect description. Yet, it could be criticized on the grounds that it does not provide enough information about the occasion. This criticism could be attended
to by expanding the descriptive material to include, for example, field notes or transcripts of things said during the occasion. The problem facing the ethnographer however, is not that of providing either, more or less data but deciding what the guidelines are for the construction of an ethnographic report. What is it that an analyst of some social scene must attend to in order to produce a description that is responsive to his observations and preserves the integrity of the setting under consideration? To provide a tentative answer to this question, one must consider the relationship between the researcher and the everyday world of the society he is investigating. Let me elaborate.

It is an inescapable fact that social scientists, like any other members of the world of daily life, see the world as a sensible and manageable phenomenon. Their everyday being in the world is not usually problematic for them. Note, for instance, how members of the Clinic found nothing strange, unusual, or problematic about the practical accomplishment of chart rounds. Instead, they were taken for granted as a routine feature of life around the clinic. Once I became acquainted with the daily routine of the Clinic, I was also able to relate to chart rounds in this way. That is, they soon became a routine and an expected part of my life as a researcher at the Clinic. And I found parts of my life oriented to chart rounds as a matter of course. To treat this commonplace order of the Clinic and particularly chart rounds as problematic is, however, an analyst's device to facilitate the production of an ethnographic description of this routine Clinic occasion. Members do not see the same problems as researchers do. Garfinkel explicates the analyst's
position when he states:

In exactly the ways that a setting is organized, it consists of members' methods for making evident that setting's ways as clear, coherent, planful, consistent, chosen, knowable, uniform, reproducible connections, -- i.e., rational connections. In exactly the ways that persons are members to organized affairs, they are engaged in serious and practical work of detecting, demonstrating, and persuading through displays in the ordinary occasions of their interactions the appearance of consistent, coherent, clear, chosen, planful arrangements. In exactly the ways in which a setting is organized, it consists of methods whereby its members are provided with accounts of the setting as countable, storyable, proverbial, comparable, picturable, representable -- i.e., accountable events.11

Thus the problem facing the ethnographer is to discover how the members he is observing utilize procedures and methods for producing recognizably, coherent social scenes. In attempting to provide an ethnographic description of chart rounds, I will take the very fact that they are events as problematic, and will attempt to explicate the procedures employed by the members of the Clinic in their production of chart rounds. In other words, an ethnographic description of chart rounds should attend to the self-organizing features that Clinic members utilize in producing this occasion. In contrast, the description of chart rounds that I presented earlier does not constitute a description of this type. Instead, it merely provides the reader with a description of what transpires. For this reason I have termed the previous description of chart rounds an inadequate description since it does not attend to providing a description of the self-organizing features of this occasion.

That the ethnographer should attend to the self-organizing features
of some social scene is itself a recommendation. However, there is still a problem about what the fundamental source of data for conducting such research will be. That is, what will constitute evidence of the social organization of some observable social scene, and how will this data be utilized?

Whatever the data or the intrinsic feature of the activity under study. That is, it must be produced and utilized by parties to the setting as an essential part of their everyday affairs. If we are to study members' everyday affairs and activities, we must utilize data that reflect these concerns. We should be able to attend to the actual ongoing details of an event when offering descriptions thereof. We can do this in an abstract way, but, nevertheless, such a description should be responsive to the actual details of those social scenes being described. A candidate piece of data for such a description is members' talk as it occurs in and over the course of sequences of interaction. Since the next chapter presents an analysis of conversational materials, (i.e., transcripts of recorded sessions of chart rounds), I would like to discuss the utility of these materials for study and description of social organization.

Talk and the Organization of Chart Rounds

In the course of my research I was able to observe and tape record various segments of interaction that occurred during chart rounds. Such recordings constitute a fairly detailed record of the occasions, for first, talk is a pervasive feature of social life and, of course, the
use of a tape recorder adds substantially to one's memory and to the reliability of one's field notes. But secondly, and most importantly, these materials are an exceptionally good record of chart rounds since these rounds are designed specifically for the purpose of discussing patients' medical histories. Talk does not facilitate some other objective, for example, giving a patient an injection or taking a blood sample, but rather talk is central to this occasion, that is, were no talk to take place here, chart rounds would be impossible in the first place. Chart rounds, then, is an interactional and conversational occasion such that the talk which transpires over the course of this occurrence is central to the self-organizing character of the occasion. A good deal of the integrity of the occasion is preservable by audio tapes. I propose that an ethnographic description of chart rounds should be responsive to the kinds of organizational features that are locatable within such a data record.  

I am not proposing that talk constitutes all that occurs during the occasion of chart rounds for it is easily observable that things other than talk routinely occur as an integral part of the occasion. Thus, people routinely stay seated, look at each other, leave the room to answer the phone, and so forth. The list is virtually endless and I have not attended to such occurrences aside from those instances incorporated into the subsequent analysis. Furthermore, while things other than talk occur, it is presumed and presumable that the reader will make use of his common-sense knowledge in order to incorporate these taken-for-granted features of the world into his analysis of these
occasions. As stated in the previous chapter, it would be impossible for the ethnographer to construct any report in the first place if he were not able to rely on his audience to use their common sense, that is, to fill out and inform the description which he offers them.

Specifically, then, transcripts made from tape recordings of chart rounds will constitute the data for my analysis of this routine Clinic occasion. Thereby I intend to examine a member's talk as constituting displays in and through which members accomplish and give to their actions the appearance of consistent, coherent, clear, chosen, planful arrangement. That is, my analysis will treat members' conversations as an index for determining how talk is a constituent feature of the organization of daily life.

As I said before, the format of chart rounds was changed twice during my research at the Clinic. These changes were instituted in order to increase the efficiency of patient care. While such changes occurred for organizational reasons, I am suggesting that there are certain invariant features that Clinic staff must necessarily orient towards in order to (1) allow changes in procedure to be recognized as such, and (2) provide for these changes to be easily implemented. Thus in my next chapter I will attempt to describe some of the features which provide for the interactional structure of chart rounds. I will not be concerned with predicting what will, as a certifiable matter of fact, be said at some subsequent chart rounds. While it is possible for Clinic staff to institute changes in its chart round procedure, it is the invariant features of the occasion that allow for changes in procedure.
occur in the first place. These are the things that I intend to study.

Our attention will now be directed towards a detailed examination of the routine features of chart rounds.
Footnotes: Chapter Two


2. By way of illustration consider the following excerpt from a doctoral candidate's research proposal:

The main topic of research is a detailed examination on the ideology and operation of leadership in a Melanesian society. A central aim of the study will be to depart from the common emphasis on personal characteristics as determinants of leadership in Melanesia. The strategic focus of the study will be the analysis of the relations between leaders and followers...QUESTIONS TO BE POSED IN THE FIELD:...1. Arenas of political competition: What are the arenas of political competition and how are these defined?...Are certain kinds of coalitions structurally enjoined, inhibited, or prohibited? What normative rules are appropriate in which arenas? How does all the above affect strategies of leadership?... 3. Political resources: How are political resources defined? Has the definition of political resources changed over time? How?...TECHNIQUES AND METHODS: 1. Formal Interviewing. Formal interviews will be conducted along the lines suggested by Black, Metzger, and others to elicit the conceptual framework of political activity....An important goal in this phase of work will be to discover the ways in which people conceive of and talk about politics and the determination of meaningful questions that can be asked about politics.... 5. Participant observation. Participant observation will carry much of the burden of data collection and will be particularly important in the analysis of disputing and political manoeuvring which may occur during the time spent in the field.

Thus, we have an instance of a field anthropologist going to the field programmed with a set of preconceived research interests and goals. It would appear that, in observing "politics," much of the matrix of everyday life from which such observations will be abandoned for some prior theoretical interest when it comes time to construct the final monograph.

4. Ibid., p. 5.


6. For a further discussion of this point see Harold Garfinkel, *(Ibid.)* pp. 24-31.

7. Transcribed from a tape recorded session of chart rounds.

8. Ibid.

9. I am using the term "demand characteristics" in the same sense as Roy Turner uses it in his paper "Occupational Routines: Some Demand Characteristics of Police Work", presented to the Canadian Sociology and Anthropology Association, Toronto, June 1969. He states: "By demand characteristics I mean to refer to those situational and contextual features which persons engaged in everyday routines orient to as governing and organizing their activities..."

10. This formulation is borrowed from a working paper by Melvin Pollner, Department of Sociology University of California at Los Angeles entitled "Working Notes on Ad-Hocing in a Self-Explicating Field." Pollner uses the term "getting through" in referring to, for instance, how traffic court is an occasion requiring courtroom personnel to "get through it".

11. Harold Garfinkel, op. cit., p. 34.

12. The use of transcribed material is often criticised on the ground that it does not constitute an adequate interactional record since transcripts do not attend the paralinguistic features of face-to-face interaction. Within our society, however, transcripts are often used in making serious and consequential decisions. For example, appellate courts often consult the transcripts of lower court proceedings in arriving at their verdict. Such transcripts are not regarded as inadequate because they do not contain paralinguistic features. That is, they are not regarded as deficient, unintelligible, open to a thousand and one interpretations, and so forth. It would appear that transcripts have a usable status despite the fact that they do not attend to paralinguistic features of interaction; this does not mean that there will be any necessary "fatal flaw" in any analysis that may be performed upon transcribed material. That
members of society can utilize transcripts and render practical and consequential decisions from them is indicative that they do not have the essential weaknesses that opponents to their use claim for them. An examination of transcripts of Clinic chart rounds should allow us to discover some of the self-organizing features of this occasion.
Introduction

It is the purpose of this chapter to do an examination of a piece of data obtained from a session of chart rounds. During the course of my research I was able to secure a number of audio tapes and, consequently, to produce numerous transcripts of what transpired on each occasion that was recorded. The instance to be examined was extracted from this corpus of materials. In the following, a nurse is presenting the charts of those patients to be seen by clinic staff on that day.

The Data

1. Doctor A. This is ah you know the ah upper (bar)
of the ah lower (bar) of the femeral gland
2. Doctor B. Um (4-sec pause)
3. Nurse John Doe
4. Doctor B. Who?
5. Nurse John Doe (spoken louder than in Utterance 3)
6. Doctor A. He's a routine baby who's a bit constipated
7. Doctor B. That's ah the
8. Nurse The commune
9.
10. Nurse ( )
11. Doctor A. They've put him a bit early for this afternoon.
I don't know whether he's coming to see me or
to see you but I shall be sort of squeezed I
think to get back in time you deal with it
12. Doctor B. Well ah Mama's patient
13. Doctor A. Umm?
14. Doctor B. Mothers a patient woman
15. Nurse He's probably here for
16. Doctor A. Oh yeah he's a month from his last shot so that's (mostly what he's here for
17. Doctor B. He had mumps?
18. Doctor A. Umm?
19. Doctor B. Did you say he had mumps?
20. Doctor A. Ho ah month
21. Doctor B. Oh ah month
22. Nurse He's here for his second shot
23. Doctor A. Judy was (pause) Judy was immunizing him I guess. Have we got him on fluoride?
24. Nurse Yeah
25. Doctor A. Yep ((Doctor has looked at chart)) I saw that the enlightened citizens of North City had voted for...

Before starting our analysis, the reader should note that I did not select this instance by referring to any preconceived method or methods for data selection. While an analysis has indeed been produced, it was not generated by a preconceived interest that "had in mind" prior to an examination of the corpus from which it was taken. Although it is true, this particular piece of data would not have been used were I unable to produce some analysis of it, it is important that the reader realize that it was selected in a relatively unmotivated manner. The data was not selected via a "coding procedure" where I had a set of analytical issues
and a set of categories that would allow me to select instances of data that would be appropriately responsive to such categories. The above data, then, is not to be regarded either as a sample of chart rounds or as a typical occurrence thereof. Rather, I selected this particular piece of conversation because it happened to strike my interest, and it is from this initial interest that the subsequent analysis took form.

A Characterization of the Data

As a beginning we might offer a single, simple characterization of the data, e.g., there appears to be a progression from talking about one patient to talking about another. While this observation may initially appear to be of little interest, it is nevertheless an observable feature and responsive to the data. Having noticed this, it might now be attended to in a more analytical fashion and come to reveal a more complicated structure than is first apparent. If we are to attend to the self-organizing features of this occasion, we should attend to the fact that progress from one chart to another is something which is oriented or attuned to by participants and thus a legitimate topic of investigation despite (and some would say because of) its mundane appearance.

The serial review of the charts is an artfully and intricately accomplished activity. Progression from one chart to another is problematic for the participants in many ways. For example, they face a problem with reference to the opening and closing of talk generated by the review of a specific chart or patient. The problem of how to terminate talk generated from a review of one chart and progressing to a review of the next is a constraint faced by the person presenting the charts. He
or she has some control over the proceedings and an obligation to "pace the charts" so as to manage the forty-five minute period allotted to the task at hand. Our concern here will be with the devices employed for terminating talk about one chart and opening talk about the next.

Throughout the analysis, reference will be made to the internal organization of conversational structure. The reason for doing so is that chart rounds is specifically an occasion wherein talk is produced by the participants and is, more importantly, a resource used by them. As such, it is available to the analyst as data from which to examine the self-organizing features of an occasion.

In utterance 2 (U2) we have an instance of a doctor completing talk generated from a discussion of a previous chart. U3 is an example of the person presenting the charts doing a progression to the next chart via the use of a patient's name. How is it that we are able to recognize completions and progressions in this context? What is it that provides for the accomplishment of termination of talk about one chart and the progression to the next? Obviously, it does not just happen, but is somehow made to happen. It is my intention to look at some of the ways in which this is a planned and motivated occurrence.

Let us begin by examining the more general phenomenon of how we recognize that a particular conversational participant has finished talking.

Harvey Sacks\(^1\) has noted two general features of conversation (1) at least no more than one party talks at a time in a single conversation and (2) speaker change recurs. With reference to the co-occurrence of these features he raises the following issues:
I want now to make a case for what may or may not be obvious to you on any sort of reflection; that achieving the co-occurrence of 1 and 2 takes work. And what we want initially to do, is to come up with some determination of the sorts of work it takes. We have an initial problem, how is it that while 2 occurs 1 is preserved, and what we want to do is to find out what the achievement of a solution to that problem involves. What sorts of coordinative work are involved.

First of all, there's that sort of work as between a current speaker and any others; which involves how is it that a current speaker is able to show other participants to the conversation that he isn't yet, that he's about to be, that he is now completing.

What does he do so as to indicate that he's still talking, or that now he's not talking. I take it that it's plain to you that it isn't obviously the case that you just have to keep spewing forth words; i.e., people are recognizably 'still talking' when they are e.g., paused.

Apparently in any event, speakers have ways of showing that they are still talking; and more importantly in its fashion, showing that they are now finished.

If the feature is that exactly one should be talking, then showing that you're finished when you're finished, is important so as to allow somebody to start talking directly upon your completion.

One sort of thing then is, how it is that a speaker goes about showing others that he's not finished, that he's about to be finished, that is he is finished, or whatever it is that those sorts of problems look like. Showing these things to all others. And it has a correlate, how is it that non-current speakers go about determining, from whatever it is that a speaker is doing, that he is or is not finishing, is or is not finished.

Another sort of problem concerns -- initially anyway -- the relationship between the various non-current speakers. How is it that the various current non-speakers coordinate their actions at the transition point so that at the transition point someone of them talks and only one of them talks?
The Progression Problem

The person presenting the charts must orient to what we may call "the progression problem", i.e., proceeding from one chart to the next. The person presenting the charts must monitor the other participants' talk not only for conversational appropriateness but also for indications as to when talk generated by one chart is "ended" or "possibly ended". Secondly, when such talk is over they present a next chart within some reasonable limit of time.

A possible solution would be for a termination to be explicitly called for -- either by the person presenting the charts or by one of the other participants. Someone could say, for example, "That is the end of discussion on this chart, let us proceed to the next." However, this could well prove more disruptive than beneficial.

Suppose speaker B proposes termination to the discussion of a chart but speaker(s) A, C, D,...N wish to contribute something to the conversation. A formulated ending by B would then constitute a premature ending and would require the other conversational participants to do something in order to forestall the proposed termination. Something akin to a vote might have to be taken to determine if indeed that was to be all the discussion about a particular chart. Such a formulated termination to the talk would also imply that the person doing the formulation has a degree of control over speaker selection and speaker activity that they do not in fact possess. Typically, formulated endings do not occur. How then is the progression from one chart to the next accomplished?

While the person presenting the charts must "place the charts", this
does not mean that he can proceed at any pace whatsoever. Rather, the progression of rounds is achieved by the consent of the conversational participants. A discussion can be monitored by the participants for its relatedness to the current chart. Pacing requires that the person presenting the chart, and other participants as well, orient to interactional devices for terminating the discussion and for opening talk about another chart. Given the sequencing rules of conversation discussed by Sacks, we may venture a solution to the progression problem.

Us's 1 and 2 deal with instances of talk generated from a previous chart. U3 is the name of the patient to whom the next chart belongs. Progression from one chart to the next has taken place. Can we formulate the notion of an end to the talk about a previous chart such that it warrants the Nurse in U3 progressing to the next chart. Our first concern, then, is with the notion of an end to the previous talk.

The four-second pause in U1 seems to indicate the end of the speaker's turn. That the pause comes at the end of what is recognizable as a complete utterance is a rather strong indication that this is a completed utterance. That is, if someone were to speak at this point, it would not be seen as an interruption. More generally, one of the ways speakers have of indicating that they are finished and a next speaker may speak (take a turn at talk) is to pause at the end of their utterance (as opposed to a pause in the middle of an utterance which might indicate that something more is to follow and that the present speaker has not yet finished). Sacks makes the following comments:

How is it that people go about producing recognizably complete utterances. And a basic thing,
at least generally, that seems to be involved is
that there's a generically available packaging
device for utterances; and that's the sentence.

And what we'll, at least for our purposes, be doing
with the sentence, is to be considering it as a
packaging device for utterances in conversation; to
be examined for those aspects of its structure which
are relevant to sequencing in conversation....

The sentence is a great packaging technique for a
series of reasons; only a few of which I'll mention.
It has a structure which can at all points be seen
as to whether it is possibly complete or not possibly
complete, and people are able to deal with it in such
a way as to see that e.g., it is now possibly com-
plete; i.e., to see on its occurrence that it's possi-
bly complete. And also, from its beginning it can
be looked at to see what it will take to complete it.
If somebody begins with "If", for example, then there's
already strongly usable information as to what it will
take to complete that sentence....

That suggests to us how people massively go about
producing utterances which are recognizably complete
or recognizably incomplete, and how then, if others
don't talk while they're talking, you get one-at-a-
time until the transition point. And it tells us
when it is that transition points will occur.3

Returning to our data, the pause following the Doctor's complete
utterance provides a slot whereby any of the other participants may take
a turn at talking. That is, a possible complete utterance followed by a
silence provides for a next speaker to take a turn at talking. Is there
some way we can assign ownership to this silence, that is, whose silence
is it?

One possibility is that the pause following the Doctor's utterance
may belong to the Doctor. That is a pause following an adequate complete
utterance does not "condemn" a speaker to the loss of his turn at talk.
It merely creates the possibility of such a loss. Given the pause in U1,
and given the fact that no one else has chosen to speak after the speaker has stopped, there might well exist some obligation for the speaker to continue. However, as the transcript shows, this does not happen.

The Doctor who spoke utterance U1 paused and provided a slot which anyone wishing to speak may fill, yet no one does. Therefore, we may regard the silence which occurs between Us 2 and 3 as a collective silence. By collective silence I mean that each of the participants, including the last speaker, are saying, in effect, "I have nothing more to say about this particular chart." Thus, a mere four-second pause generated out of the talk about one chart has signalled the closure of one chart and the opening of another.

This instance of a pause at the end of an utterance is one example of members' methods for producing and/or recognizing a completed utterance. The pause is heard as a possible end to the speaker's turn and a legitimate place for another speaker to begin. Given the fact that none of the other conversational participants took a turn at talking, is there a way of accounting for the Nurse, presenting the charts, speaking in U3? When everyone is silent, can we perhaps formulate whose turn it is to speak next given that the last speaker has produced a complete utterance and does not engage in a continuation? I suggest that it is not mere chance that accounts for the Nurse taking a turn at talk, but that there are good organizational reasons for her speaking next. These reasons trade upon the sequencing rules of conversation.

While the current speaker can select a next speaker, he need not
do so. Instead, he may, for example, select a next action. For instance, someone may ask a question, but not specify who should answer it. If a group of people had seen a movie, and you hadn't, you might say "Well, was it a good picture?" and then somebody will select themselves to speak, i.e., provide an answer to that question.

The fact that charts are presented by a particular individual is a resource which establishes a set of appropriate actions for the participants. It is possible to view settings and occasions as constraining topical talk. With reference to the organization of chart rounds, the "core activity" is the review of patients' charts. The person presenting the charts must accomplish a review by managing the talk that occurs during the occasion. That is, he or she has an obligation to listen to the talk which occurs, not only out of politeness, but to be able to see, e.g., when talk about a given chart is coming to an end, drifting, etc. In addition, since he must "pace the charts", he should proceed to the next chart when indications such as these constraints of time, etc. require that a progression be accomplished.

During the four-second pause following U2, the person presenting the charts can hear the ensuing silence as a consensual silence authorized by the participants. This provides good organizational reasons for her being the next speaker. She can say something pertaining to the current chart or to proceed to the next chart. This does not mean that the progression to a next chart prohibits further talk about a current chart. Hence, U3 is only a candidate for closure. Consider the following piece of data.
1. Researcher  What?

2. Doctor A.  The trial of labor. Letting the baby go and see if the baby comes out. If it doesn't come, move down then they'll ah do do ah Cesarean section

3. Researcher  Umm mm (7 seconds)

4. Nurse  Mrs. Smith

5. Doctor B.  Incidentally if I can go on for a moment there's a very recent paper in one of the journals...

Similarly, talk generated from one chart can be extended even though the progression to a next chart has been proposed. Such an extension, however, requires some remedial work. The 'Incidentally' in U5 ties U5 back to its relevant target, i.e., the discussion generated from the previous chart. What is of interest is that U5 does not propose that U4 constitutes an interruption. That is, the segment which reads 'Incidentally if I can go on for a moment' is not something that would justify our classifying Us 3 and 4 as interruptions to Doctor B's turn at talk. Since U5 does not address itself to an interruption, the referent of the utterance becomes the previous chart, i.e., in the preface 'Incidentally if I can go on for a moment' the hearers can orient to the utterance being tied back to the last chart (as opposed to, say, any other chart).

In "unpacking" the interactional structure of U5, I have attempted to show that, while talk about one chart can be extended even after talk about the next chart has been proposed, such an extension requires work, i.e., is an interactional accomplishment. Furthermore, by examining U5, we may be able to formulate when, in some temporal sense, what gets said,
is shaped interactionally by the structural features of the occasion. Let me elaborate.

U5 extends the talk about a previous chart. It occupies second position with reference to the first mention of a new chart in U4. If a participant wishes to say more about a particular chart, given the progression by the Nurse to the next chart, then there might be certain interactionally important reasons why U5 occurs in second position rather than say e.g., third, fourth, fifth, or nth position. I am suggesting that it becomes more problematic and requires more interactional work to say something about a previous chart after talk about a next chart has been allowed to develop. With each utterance about a new chart, it becomes harder to return to a previous chart. What gets said, with reference to any particular chart, is not merely an outcome of the fact that someone has something to say, but results from the ways in which participants must rely on the overall structure of conversation to find appropriate slots for their comments.

Thus far, it has been established that the four-second pause following U2 (in our original data) provides warrantable grounds for the person presenting the charts to proceed to the "next" chart. This is accomplished by using the patient's name as an interactional device for opening a review of his chart.

Having started with a characterization of our data, we have proceeded to examine that characterization in detail. Such a procedure tends to raise further issues to be examined that were not part of our initial characterization. One such issue is how the organization of the
occasion warrants the use of the patient's name to facilitate the progression of the charts.

It should be noted that the use of the patient's name is itself a methodical accomplishment. The person presenting the charts picks up the next chart and reads aloud from it. For example:

**Doctor:** ...Carol Bern (7 sec.) whose age (3 sec.) twenty-six (4 sec.) (Note: Doctor looking through chart) She seems to have a lot of stuff, abdominal pain, abdominal pain, urinary tract infection, cough and fever, pain in the chest and shooting pains in the legs, and a history of perhaps phebitis thinks this might be the same, cough and fever, agitated and can't sleep, worried. Rex: daughter's boyfriend won't make up his mind whether to marry her, agitated depression and is claiming pains in her legs again (3 sec.) (Note: The underlined portions of transcript indicate material being read literally from the patient's chart)

**Nurse:** She the one that John (another doctor) ( ) see her leg problems?

**Doctor:** Yeah, Umm of course....

The patient's name — last name (LN) + first name (FN), age and date of birth are written on his chart. However, the person presenting the charts gives a patient's name and age as it normally occurs in ordinary conversation. Name and age are not merely read aloud, but rather constructed into pieces of natural talk. Reading aloud is far more than just saying what is written. It is an accomplished activity, subject to the organizational constraints of settings and occasions.

The use of a patient's name serves as both a proposed terminator to the discussion of a chart and a device for opening discussion on
a next chart. The person presenting the charts can and should monitor the talk to determine if it is attentive to the constraints of the occasion, i.e., if it is contributing to the successful completion of chart rounds. When it becomes apparent that the talk generated by a chart is 'wandering' or 'closing down' the person controlling the charts can terminate the talk by introducing the next chart via the patient's name. This change in topic is usually accompanied by certain visual cues. The person presenting the charts might put down one chart and pick up the next while reading off the patient's name. This allocates the previous chart to the completed pile. The physical closing of a chart however, does not warrant the assumption that that chart has been fully reviewed. That is, talk generated by a chart may continue even though the person presenting the chart may elect to close it. His doing so, however, does indicate that he sees that chart as one which is possibly completed.

The Referential Adequacy of the Use of First Name plus Last Name

This section examines how the use of a patient's name does not provide adequate information about the patient. It must be remembered that the patient is not present at chart rounds. What we are dealing with is the use of names as adequate referential devices, that is, with third party references in conversation. First, let us consider the interactional work accomplished by the use of the patient's first name (FN) and last name (LN).

The social organization of chart rounds has as its fundamental concern the review of a corpus of patient's charts. One of the features
of chart rounds is that the patient may or may not be known to many of the physicians present. Further, the patient's physician may or may not be present. The use of the patient's FN plus LN provides sufficient information, for those physicians who know the patient, to locate the patient into some category vis-à-vis their encounters with that patient. It provides for a certain amount of memory work.

The previous section dealt with the use of the patient's name for progressing to the next chart. Such progression provided for appropriate speaker actions, i.e., talk about the chart. However, the patient's name can also provide for a next speaker for, if the patient's physician is present, then the introduction of a chart via the use of the patient's name provides grounds for that physician to speak next. If the patient's physician is not present but another physician who knows the patient is present, it is likely that he will make some comment at this time.

Although the use of the patient's name may provide those who know the patient with adequate referential information, a name alone does not provide much for those who are not acquainted with the patient. An examination of the question "Who?" of U4 should help to clarify this point.
Two different hearings of the question "Who?" of U4 are provided and acted upon. In U5 the Nurse hears the 'Who?' as a simple request for a repeat. One possible reason for a repeat is that Doctor B. did not hear her. Un U6, however, Doctor A. treats Doctor B's question as more than a simple request for a repeat. Instead he offers a referent other than the patient's name to locate the patient for him.

The question 'Who?' then becomes interesting when in fact the patient has already been named, i.e., one is already supposed to know who the patient is. The patient's name as an identifier is often not sufficient information to identify the patient to participating members. Similarly, a problem with third party references in the conversation between speaker A and speaker B above is when A makes reference to person C not known to speaker B. This requires that speaker A select a referentially adequate category device appropriate to the occasion.

It is apparent that FN + LN does not always provide enough information to successfully identify a patient. Doctor A in U6 produced a medically relevant characterization of the patient. This description allowed other participants to identify the patient, at least for all practical purposes. Consider the following data:

Doctor A. But I think that would be worthwhile one just to sort of get your signals coordinated between you and Karen on it. Ah Terry Martin

Doctor B. This is a girl with abdominal pains

Doctor A. Okay, Beverly Jones

Doctor B. This is a girl Judy has been seeing for anxiety
In addition to the name, some further characterization is usually called for. What will be used in the construction of such a characterization is interesting since the patient's history is immediately available. Such a characterization, I suggest, is constructed to provide participants with a grasp of the Clinic's current concerns with that patient. By proposing that a patient is "a girl with abdominal pains", or is "being seen for anxiety", the person is providing a characterization that is medically and organizationally relevant to the Clinic's transactions with the patient rather than just any description which happens to come to mind.

One of the interactional features of such medically relevant characterizations is that they can provide good organizational reasons for adducing a patient's reason for coming to the Clinic. That is, given the fact that the staff are reviewing the charts of those patients who are to be seen later that day, the characterizations used can be monitored by the participants to formulate a patient's reason(s) for coming to the Clinic on this day. Thus, in the above data, "has been seeing for anxiety" suggests that the patient is still coming to the Clinic because she has problems with anxiety. Similarly, staff can conclude that Terry Martín has "abdominal pains" and if he did not have, he would not be visiting the Clinic.

U6 constitutes a medically relevant characterization of the patient. Whether this characterization provides adequate grounds for establishing a patient's reason for coming to the Clinic will be examined in the next section. Thus far, however, it should be apparent that the examination
of small segments of conversational materials can be utilized to discover some of the ongoing properties of settings and occasions.

The Reason for a Visit: I

The reason for a visit constitutes an organizational problem for members of the staff. It is intended that the problems which a visit or series of visits entail will be, at least in part, resolved during chart rounds. U6 is a medically relevant characterization of the patient. The ways in which this characterization provides grounds for adducing a patient's reason for a visit requires that the ongoing interactional structure which produced it be examined in further detail. The concern is not simply whether or not U6 is a medically relevant characterization, but how such a characterization is achieved.

U6 seems to provide sufficient grounds for a visit by labelling the patient with the disease term "constipated". To focus only on this portion of the utterance, however, is to ignore the organizational import of the first portion, namely, "He's a routine baby". While apparently a non-medical characterization, I suggest that the segment "He's a routine baby," is the operative portion of the utterance in terms of providing the staff with a relevant reason for a visit. I shall elaborate.

Post-natal care at the Clinic involves a package of procedures which are conducted over a definitive period of time. Such care usually continues for about six and one-half months after birth. During this time, babies's growth, weight, height, neurological development and muscular coordination are checked thoroughly. In addition, urinalysis, blood tests, and vaccinations may be given. While all of these take place
during post-natal care, I should emphasize that the components of the package are arranged in a specific way so that the baby is not randomly checked each time he visits the Clinic. Instead, each visit constitutes a step in the developmental sequence of the programme of post-natal care. While the first visit may only require that his weight and height be measured, the fourth visit might require such things as the initiation of polio immunizations. The characterization "routine baby", then, is something that can provide members of the Clinic with a set of organizational parameters about why the patient is coming to the Clinic.

While the characterization "a routine baby" provides participants with some general parameters that could warrant a visit, a further reason remains to be discovered. Compare this characterization to one involving a relatively serious medical ailment, e.g., a baby with a congenital heart defect. Any serious condition would expectably be mentioned in the course of a review of the patient's chart and thus become available to participants as a possible reason for a visit to the Clinic. Unlike characterizations involving a serious medical ailment, "routine baby" does not provide the participants with sufficient information to adduce the patient's reason for coming to the Clinic. The first portion of U6 may narrow the parameters involved in accounting for the patient's coming to the Clinic but, this far into the utterance, no other reason for a visit has been formulated. What is the interactional import of the second part of U6 "who's a bit constipated"?

Given the first part of U6, whatever is appended to this must constitute a minor complaint. To append a serious medical problem would negate the initial characterization of the patient. One of the two, that
is, either "He's a routine baby," or ("who has a serious disease") would not rest on warrantable grounds. In contrast, the addition of "who's a bit constipated" to the part of the utterance reaffirms that the patient is a "routine baby" for the most serious problem that Doctor A can cite is that he is "a bit constipated". Being constipated is proposed as a standing condition and presumably the worst condition that can be said of this particular patient. It should also be noted that Doctor A does not propose that the patient is currently being treated for constipation whereas such treatment could well provide grounds for the patient's visit to the Clinic.

While U6 perhaps looks like a formulation of a patient's reason for coming to the Clinic, it only provides the possible parameters that could warrant such a visit. The operative part of the utterance is "He's a routine baby" and not "who's a bit constipated". U6 might be compared to, say, "He's a diabetic who's a little depressed", where it is the diabetes that constitutes the medically relevant characterization and "who's a little depressed" constitutes some quasi-medical characterization. The last part of U6, while looking like a possible reason for a visit by the patient, glosses over the organizationally relevant aspects of the characterization "routine baby". The characterization "routine baby", then, is one of a class of patients who are healthy but, because they are babies, have grounds for visiting the Clinic.

The reader will notice that until U16, no grounds for a visit by the patient have been proposed. Rather than consider U16 at this time, I would like to make a digression in the analysis of how clinic members
account for a patient's reason for a visit and continue with the examination of this transcript in terms of some of its other organizational features. My reason for doing this are twofold. First, it will allow me to continue to work with the data in a methodological fashion by attending to the order in which it was produced. Second, it allows me to demonstrate how such data can be utilized to uncover organizational features that we may not have been able to formulate, or assume as operable until we pay rigorous attention to their actual, situated production.

A Sociological Treatment of Referring

U6 is inherently connected to the organizational features of the occasion, i.e., the patient was referred to in a medically relevant manner. An examination of Us 7-9 shows an interactional exchange whereby the Nurse offers another characterization of the patient in order to allow Doctor B to identify him. A noticeable feature of this characterization is its "mundaneness". By this I mean that it is not immediately evident that it is or need be related to a conception of medical practice. It is the purpose of this section to discuss how such a characterization can refer to the patient in such a way as to successfully locate that person for Doctor B.

One of the first things to notice with reference to Us 7-9 is that Doctor B's U7, "That's ah the," is not completed by him, and that the nurse's U8, "The commune," follows and completes it. This utterance warrants consideration since it is only by virtue of the fact that it is a completion of U7 that it gets to be seen as a characterization. We are
concerned then with the fit that this utterance has within the ensuing conversation. Let me elaborate this point.

First off, is there a way in which we can account for a completion of U7 such that, if there is to be a completion by another speaker, it will be constrained by certain structural features of the talk that preceded it? Sacks remarks on this phenomenon:

Now in fact, there is perfectly lovely natural data which shows pretty well that persons not only analyze utterances grammatically in the course of those utterances, but, furthermore, they have the results of their analysis, grammatical analysis, available to them while that utterance is yet going on. And can use it furthermore, either on the other's completion or even before its completion. Data consists of this sort of thing...what you get is something like A produces, is engaged in producing a sentence, at some point in it he hesitates, pauses, and B sticks in quote the word he was looking for. Now if we ask how is it that B could do that, then since at that point, leaving aside that B gets the right word, all that would be interesting is that B gets the right class of words. That is to say that he knows that what's being looked for is a noun, perhaps a noun of a certain sort is being looked for so as to be able to stick it in, what he has had to be doing is to see that at that point in an utterance he's analysing only, say, such a noun is eligible while the sentence would yet retain its grammaticality. Now as it happens, if you look at ordinary talk you'll find that it is very frequent that so-far hearers interject completions of the sentences of others in a syntactically coherent way and thereby show among other things that they understand the sentence syntactically that they have been doing their understanding, doing the analysis that allows them to understand while the thing is being produced, so as to have its results available to them while the sentence is yet being produced.

What then, if any, are the structural constraints of U7 that provide for the type of production which occurs in U8?

In rather broad terms, the linguistic structure of U7 consists of a subject and predicate ("That's" is that is) and the definite article.
(Linguists relegate the "ah" in U7 to the arena of linguistic performance and, as such, something which is not available for analysis.) However, we are not doing linguistics here, but interactional analysis and I suggest "ah" in U7 is of importance in describing the interactional structure of the conversation at hand. I will return to a consideration of it in a moment.

Given the linguistic structure of U7, possibilities with reference to its completion (either by the current speaker or by a "so-far hearer") are relatively limited. The subject of the sentence is a demonstrative pronoun and, given that a definite article follows, either an adjective phrase or a noun phrase constitutes possible completions. Thus there are good syntactic reasons for the Nurse in U8 to complete Doctor B's U7 with a noun phrase as she did. However, there are also strong interactional reasons for this.

It has been argued that, first, the use of a patient's name does not always provide enough information to identify the patient. Second, that the use of a medically relevant characterization may also fail to identify the patient for he can be "a routine baby who's a bit constipated" regardless of who he is. Thereby, the identification of the patient is, for Doctor B, still problematic so that U7 may be seen as a search undertaken to extract the baby's identity. The Nurse completes this search by providing a characterization which resolves Doctor B's problem. This is evidenced by U9.

But, how is it that we are able to see U7 as part of a mental "search" and thereby to see U8 as a completion to this search. I suggest that the
answer lies at least in part in the hesitation implied by the use of the aforementioned "ah". A search has the character of a question, and questions are typically followed by answers. Thus the doctor's hesitation can be heard by the Nurse both as grounds and as the proper place or slot for providing a characterization which might solve the problem.

The formulation offered by the Nurse in U8 needs to be elaborated. Clearly, we hear it as a characterization of the patient as something which refers back to "the baby". That U8 is a characterization is self-evident. How it is constructed so that it refers and identifies a particular patient is problematic. What we seem to be dealing with is the type of situation Strawson discusses:

The application of the phrase 'identification of particulars' which I shall first be concerned with is this. Very often, when two people are talking, one of them, the speaker, refers to or mentions some particular or other. Very often, the other, the hearer, knows what, or which, particular the speaker is talking about; but sometimes he does not. I shall express this alternative by saying that the hearer either is, or is not, able to identify the particular referred to by the speaker. Among the kinds of expression which we, as speakers, use to make references to particulars are some of which a standard function is, in the circumstances of their use, to enable a hearer to identify the particular which is being referred to. Expressions of these kind include some proper names, some pronouns, some descriptive phrases beginning with the definite article, and expressions compounded of these. When a speaker uses such an expression to refer to a particular, I shall say that he makes an identifying reference to a particular. It does not follow, of course, from the fact that a speaker on a given occasion, makes an identifying reference to a particular, that his hearer does in fact identify that particular. I may mention someone to you by name, and you may not know who it is. But when a speaker makes an identifying reference to a
particular, and his hearer does, on the strength of it, identify the particular referred to, then I shall say, the speaker not only makes an identifying reference to, but also identifies, that particular. So we have a hearer's sense, and a speaker's sense of 'identify'.

Strawson is clearly into sociological territory when talking about a speaker's versus hearer's sense of "identify", "expressions", "the circumstances of their use", and/or the negotiated character of particulars. The crux of what I take Strawson to be saying is that, when individuals interact with one another, they make references and, whether or not such references succeed in accomplishing an "identification" must be negotiated between speakers and hearers over the course of their exchange. Note that while philosophical literature recognizes that 'referring' and 'identification' are accomplished in the course of various speech situations, it tends to subsume description under some general theory rather than focus upon it as a situationally constructed accomplishment. In contrast to this, our concern is with the latter, specifically, here, with accounting for how U8 identifies the patient in a unique way.

U3 uses a patient's name to identify the patient to the staff. When this fails, a medical characterization is used: U6. When this also fails, another type of characterization is offered. This characterization "The Commune", could have been constructed without any medical expertise. That is to say, it could have been constructed by any competent cultural member. Let me begin to elaborate by attending to the semantic referents of the preceding utterances.

The relationship between names and pronouns is a rather straightforward one that is, a pronoun can stand for a name. Interactionally,
however, "He's" must be recognized as the product of some operation on "John Doe" -- an operation in terms of person and number. Similarly, we may ask the question: What is the referent of "The Commune"? We can assume that "The Commune," "He's a routine baby who's a bit constipated," "John Doe" in U5, "Who?" and "John Doe" in U3 all refer to the patient.

Note that, while dealing with the issue of identifying a particular patient, we have come upon a more generalizable feature of interaction. The world is full of noticeable and identifiable features, and it appears that persons are able to orient to, pick-up-on, and utilize these independent of any professional training or expertise. Identifying a patient via "The Commune" seems to be no different than asking, "Is that the guy who owns the Volvo station wagon?" This routine, common sense way of referring to some person or thing is often sufficient to jog memories, clarify references, and make necessary identifications. Such characterizations seem to be able to do the job that they are intended for. Thus, "The Commune" is not a unique medical characterization, yet it seems sufficient for the purposes at hand. This point will be developed later.

Returning to the data, note that "The Commune" can, in and by itself, be taken as the name of a particular place. Another possibility is that the utterance can be expanded to say something like, "the baby who comes from the commune". It is important that I have retained the definite article for, it is not just a baby who lives in "a" commune but the baby who lives in "the" commune. Thus, I am treating "The Commune" as a place name. Place names can, given certain circumstances, be used to identify phenomena which are not, in the strict sense of the
term, places. Here are a few pertinent examples from Schegloff who tells us that:

...place terms can be used to formulate occupation:

A: You uh wha 'dijuh do, fer a living?
B: Ehmm, I work inna driving school

They can be used to formulate "stage of life":

A: When did this happen?
B: When I was in Junior High School.

They can used to formulate activities:

*A: What's Jim doing?
B: Oh, he's at the ballpark.

*Indicates invented data: here the answer could indicate either work or leisure activities depending on "Jim's" occupation.

Where a place term is used to formulate something other than location, the first question may betnot how that term was selected out of the set of terms that are correct for that place, but rather how a place term came to be used to do a non-place formulation.

If I am right in regarding "The Commune" as a place name, then, following Schegloff's recommendation, I would like to consider how it comes to refer to a particular person. To this end, I will provide the reader with some background information about this patient.

The Clinic provides medical care for a group of families, the Smiths, the Jones, and the Does, who live in a nearby commune. These families constitute "The Commune". It is not merely the fact that they live in a commune that is interesting, but that their living in this way has provided members of the Clinic a suitable term applicable to any member or members of the commune. Of course, this is not to say that an individual member can properly be called by the unit's name. By this I mean
that the name "John Doe" is not synonymous with, i.e., replaceable by "The Commune". What I am saying is that whereas "John Doe" did not adequately identify the patient, the term, "The Commune," was intended to and did locate him. Since membership in the commune was both from the point of view of the nurse (speaker) and Doctor B (hearer) an adequate identifying reference.

References are strongly connected to the shared biographies of speakers and hearers. That is, it is up to the speaker to choose an identifying reference and the nature of that reference is constrained both by the nature of the occasion and by what he can assume that he and his conversational partner(s) know in common about the phenomenon referred to. Sacks elaborates upon this in the following discussion about the construction of a story:

...Now she could easily handle that by having said "Ruth Henderson and I drove down to see Mary Smith who lives in Ventura" but there are some problems about that. If she was going to see somebody that the other person doesn't know, then it could cause a complication to say "We went down to see Mary Smith yesterday" "Oh, who's she?" "She's a friend of mine who lives in Ventura." "Oh, I see." But anybody knows that when somebody uses, instead of the name of the person they went to see, a place, then they're telling you among other things, 'you don't know who I'm talking about', e.g., "I went to a party at La Marian last night" tells you among other things, 'you don't know the person I went to see". Where if you knew who they went to see they would tell you who they went to see. For us, however, the problem is the reverse of that faced by Sacks. Doctor B has not been able to identify the patient either by name, or after obtaining a medical characterization of him. Rather, it was a characterization of the patient's residence that finally enabled an identification.
Us 3, 6 and 8 have been treated as "further information," designed to enable Doctor B to identify the patient. U9, it has been argued, completes this sequence for it is here Doctor B finally manages to identify the patient. U8 leads to an adequate identification of the patient. This is evidenced by U9. In Strawson's terms, U9 is the utterance whereby the speaker "identifies the particular referred to." On the one hand, this seems to satisfy Strawson's model of a speaker identifying a particular, on the other, it provides for the possibility that the entire segment, i.e., Us 3-9, is not an instance of referring but constitutes a case of 'remembering' and 'recognition'. We have been treating the patient's name as something mentioned to a participant who does not know what the particular is that the speaker is talking about. U9 casts doubt on this assumption. How is this so?

The 'Oh yeah' of U9 signifies recognition of successfully attaching a patient to a name. The 'John Doe' is the operable portion of the utterance in that it signifies that the Doctor knew all along who was being talked about. That is, it is not the first time he has heard the name. I suggest that U9 is not an instance of Doctor B identifying 'John Doe' and, hence, not an example of referring. Rather, U9 provides us with rather strong grounds for assuming that Doctor B has known all along who the patient is but had forgotten and has been made to "remember."

The sequence under analysis is thus one of remembering and recognition rather than referring. If I am correct in the analysis of U9, what effect does this have on the previous analysis of Us 3-9?

To begin with, let us consider the bureaucratic situation facing participants to chart rounds. Those personnel who routinely come into contact with patients are engaged in a relationship whereby, because of
the nature of Clinic business, they must or at least should remember who patients are. Because the size of the patient population tying names to patients is an omnipresent problem for the staff. Medical personnel are under a strong obligation to remember who patients are when dealing with them. Staff discussions about patients place a similar obligation on those familiar with a patient to be able to tie patient names to patient problems. Staff are concerned not only that some patient has some medical problem, or that some patient has a problem, but with which patient has which problem. Rounds is not merely an occasion to discuss the medical problems of various patients but an opportunity to discuss and to orient to the problems associated with each particular patient. At the same time, some of the participants are not acquainted with the patient who is being discussed. One of the organizational problems of the Clinic is tying patient names to the problems which constitute the organizational business of the Clinic. Chart rounds is an occasion where such business should be discussed. With these features in mind, let us return to an examination of our data.

U6, while a medically relevant characterization, is also inherently connected to the business at hand. I mean that it is, at least in some ways, adequate for the purposes at hand and will do for any "routine baby who's a bit constipated" regardless of its biography. It is a characterization usable to any personnel in general; one that does not require members know the patient's name in order to find it usable. As such, it is tied to the bureaucratic concerns for getting things done. Following the name references, Us 3 and 5, Doctor A proceeds to state the organizational business that the Clinic has with this particular patient.
Such business need not be concerned with who the patient is, but only with the ways in which he is tied to the organizational concerns of the Clinic. Because Doctor A is the patient's physician, the characterization provided by U6 need not be seen as a construction designed to "help" Doctor B locate the patient. Instead, in a rather strong way, it seems to indicate that the Nurse's formulation, "John Doe" is referentially adequate, that is, a non-problematic matter of routine to the business at hand so that participants can proceed with a review of the chart.

However, this does not undercut the earlier analysis of how the use of a patient's name allows those familiar with him to recognize him and affords those who are not familiar with an opportunity to become familiar with his name and problem. In the first case, the name is used for the benefit of those who know the patient whereas, in the second, its use allows those who are unfamiliar with the name, problem and/or organization concerns thereof, to become familiar with them.

With reference to Strawson's remark, "I may mention someone to you by name, and you may not know who it is," it is now evident that he is neglecting the interactional consequences of such name dropping. One does not mention a name to someone unless they assume that that name will have some meaning for them. People usually make third party references to hearers who they assume will know the mentioned party. In such a case, one is not making an identifying reference to a hearer so that the hearer may, on the strength of it, be able to identify the particular referred to, but rather, in making such a reference, the
speaker assumes that the hearer will remember the person referred to.

Where a third party reference is made and the participants do not know who it is, as is the case in our present analysis, circumstances warrant that the name be connected to some category rather than to a particular patient. That is, it is obvious that the name is the name of a patient and all that now is required of those who are present but unfamiliar with the name is that they associate it with a particular patient. The name is not being used as information to allow anyone to identify someone who they do not know. In opposition to Strawson, it is not the case that when people mention someone to you they expect you to know who that someone is; instead they only require that you know who that someone is in terms of some category or class, e.g., a patient. For all practical purposes, the category of patient serves to identify any particular patient mentioned. For some purposes mentioning someone's name need not require that you know "who" he is, but merely "what" he is in order to identify him for the purposes at hand.

Given our reformulation of the data, the fact that the Nurse repeated the patient's name in U5 can be taken to substantiate our claim that the name is posited as adequate information for the task at hand. It is assumed that Doctor B's "Who?" is a "request for a repeat" rather than for further identifying details. The name alone is adequate for anyone familiar with the patient, and anyone not familiar with the patient knows at least that the name is tied to a patient. So what we have in U3 is the name, in U6 a statement of organizational business
with reference to this patient. U6 treats Us 3 and 5 as providing sufficient information for Doctor B to identify the patient and thereby proceeds to the tasks at hand, namely, a review of the chart, and moreover, the scheduled completion of this session of chart rounds.

Thus, in U9, it is not that Doctor B has identified the patient via the information provided in Us 3, 6 and 8 alone. Rather, he has remembered who the patient is for he knew him all along. The connection is one between a name and a known patient and not a case of some amount of identifying information that would allow someone to establish the identity of the patient. Such information only allows a hearer to remember what he has forgotten, and does not allow him to identify particular individuals in the sense that, say, police descriptions do.

How is it that some items can be seen as "rememberable" such that they can be used as a resource when a third party reference that should be recognized but is now can come to be remembered? In this case, how can Doctor B draw upon his knowledge of the mentioned party, and the relationship which obtains between this party, the speaker and himself in such a way that the mention of "The Commune" facilitates his recognition of what he should have known and, in fact, did know all along.

Our concern, now, will be in explicating how "The Commune" can serve as a rememberable item about this patient.

Earlier, it was stated that a group of patients were known collectively as "The Commune" and that this characterization was also applicable to each member of the commune. It is not only the case that this group of patients are or become known as "The Commune," further it is what they are or become remembered for, i.e., belonging to a
commune. Let us consider the relationship between "being (or becoming) known" and "being (or becoming) remembered for".

Becoming known as a patient can result from one's association with any part of an extensive list of items which concern medical practitioners, one can have a broken leg, be a diabetic, "a routine baby who's a bit constipated," etc. While any of these may be a way of getting known, they may or may not be adequate in terms of their remembrance, e.g., there may be numerable routine babies who are constipated. It is unlikely that the staff will remember an individual patient simply because he suffers from some common ailment since there are numerous patients who have the same complaint and receive the same kind of attention. However, that this patient is a member of "The Commune" is somehow a "rememberable" feature.

A lay notion of how doctors routinely remember their patients might assume that they remember them via their aches and pains. It turns out, however, that they remember patients in the same way that any other member of society might remember a phenomenon, i.e., by associating it with some attribute that is unique to them. Thus, "The Commune" is a rememberable feature, and by linking the patient to it, Doctor B is able to remember what he knew all along.

This section began by treating Us 3-9 as an instance of what Strawson called "referring to particulars." This analysis, however, was re-evaluated and deemed inadequate, for a second examination of the data suggested that it may better constitute an instance of remembering and recognition. What could be more private than an individual's
memory? As it turns out, memory is not the product of individual awareness along, but is shaped via the relations that obtain between the item to be remembered and the ways in which individuals can be helped to remember. The process of remembering is not solely a product of the workings of one mind, but a socially-organized phenomenon.

Organizational Treatment of an Organizational Problem

An encounter between patient and physician is usually arranged through an intermediary such as a secretary. Scheduling accomplishes coincidence between physician and patient. If the patient comes in at time "x", he will see Doctor "y". If circumstances prohibit his seeing Doctor "y", another doctor will see him. Both patients and physicians are aware of this possibility. The fact of scheduling allows physicians to consult the secretary's appointment log prior to chart rounds so as to have some idea of who is scheduled to see whom that day. This does not mean that a physician always does this, however, the possibility always exists. Since our data comes from a session of chart rounds where the charts belong to those patients who are to be seen later on in the day, a discussion of scheduling problems seems to be in order for they constitute an area of concern for the personnel involved in chart rounds.

Before addressing this issue, however, I would like to draw the reader's attention to the manner in which the forthcoming discussion will rely for sense upon those features of clinic organization referred to in the opening section of the dissertation. Specifically, it will require that the reader remember and use certain facts from this section,
e.g., the fact that the clinic operates on an appointment system. At the time that these materials were presented, I stated that I did not know how such details would come to be usable for whatever subsequent analysis was presented. Despite this, I relied and shall rely upon the reader to make use of these materials in order to inform the analysis to be presented. While somewhat of an aside from our present concern, it is interesting how not only does this substantiate our earlier analysis of the standard ethnographic format, but also illustrates some of the ways in which these initial materials come to constitute the background for subsequent materials. Noting how such initial materials are used as a resource for our appreciation of some subsequent analysis reinforces the initial section and contributes as well to the analysis at hand. Let us now return to our transcript and to an analysis of the scheduling problem.

Continuing with our data, Ull constitutes both an organizational problem, i.e., which physician will see the patient given that Doctor A is unable to do so? and a proposed solution to this problem. Our concern is with how Ull handles the problem of who should see the patient. To begin with, let us consider the possible hearings members could engage in.

1. The beginning of a complaint. It is the secretary's job to allocate appropriate time slots for meetings between doctor and patient. The secretary knows which physicians are available at what times and it is her job to schedule appointments accordingly. Thus, the beginning section of Ull, "They've put him a bit early for this afternoon," could
well constitute a complaint, i.e., given Dr. A's schedule, the patient was scheduled for too early a time slot and, further, that "They," the secretary (or the clinic in general) should have known better than to make an appointment for that time. Put crudely, the beginning of Ull provides for the possibility of affixing responsibility or blame to some organizational member who should have scheduled the patient at a time later than that alloted. We can assume that Doctor A had consulted the appointment schedule prior to rounds and is, thereby, in a position to know when the patient is scheduled.

2. A contingency in Doctor A's schedule has developed. Another possible hearing for the beginning part of Ull is that 'this afternoon' is different from any afternoon because something has arisen in Doctor A's schedule. Something which the secretary could not have known about. The 'bit early' seems to suggest this possibility since it undercuts the notion of making a complaint about, e.g., the efficiency of secretarial scheduling. If one is going to make a complaint, it is not that the secretary made the appointment "a bit early", but that she made the appointment "too early". 'Bit early' implies a temporal period short enough in duration not to be problematic with reference to coincidence between physician and patient.

Regardless of which possible hearing is correct (for our business is not one of deciding history), either hearing provides that the beginning of Ull establishes an organizational problem with reference to the speaker. The patient is "a bit early for this afternoon" only with reference to Doctor A's whereabouts. Doctor A is not making some
statement about scheduling procedures in general where e.g., there would not be anyone at the Clinic to see the patient. Given the group character of the clinic, the beginning of Ull establishes the organizational problem of "Who should see the patient?"

The organizational problem provides those other physicians present with the possibility of seeing another and unexpected patient. This, however, increases their patient load. As such, the beginning of Ull, while an organizational problem for the Clinic, allows those candidates for another patient to orient to that fact in terms of either accepting another patient or constructing a possible excuse for not accepting another patient. What constitutes an organizational problem can also be seen by physicians as a personal problem that is, seeing another patient in an already busy work day. What we are dealing with then, is both an organizational concern, i.e., someone has to see the patient, and the personal concerns of Doctor A and the other physicians. Ull provides us with an instance of how the organization of the work day is attended to by the ongoing practices employed by members of the setting.

Given that Ull is preceded by the Nurse's U10, how is it that Doctor B hears himself as the referent to the pronoun 'you' in "I don't know whether he's coming to see me or to see you...." Again we are dealing with possibilities, although in this instance my field notes will serve as a piece of history.

1. **Eye contact.** One possibility is that Doctor A has spoken the utterance while looking at Doctor B providing thereby, in addition to a verbal display, a paralinguistic means for speaker selection.
2. **Medical specialty.** Doctor A is a paediatrician while Doctor B is a family physician. The distinction being that, while a paediatrician specializes in children, a family physician treats the family as a whole. However, either physician could treat the patient and the secretary could have scheduled him to see either of them.

3. **Past medical encounters.** By providing a medical characterization of the patient in Ull, Doctor A gives us warrantable grounds for assuming that he has seen the patient previously. Although we have no data to substantiate this claim, it is equally possible that the patient has been seen previously by Doctor B. Us 7–9 attest to the fact that Doctor B is at least familiar with the patient.

4. **Participants to the occasion.** That the 'you' in Ull refers to Doctor B can be derived from the fact that only Doctor A, Doctor B, the Nurse, and myself were present at this time. Since patients are scheduled to see physicians, the only candidate for the 'you' is Doctor B. Note how participants orient to such contingencies when making or when hearing an utterance. Doctor B is now in a position to see that he will have to deal with the patient. Although the possibility has not yet been explicitly proposed by Doctor A, I suggest that it has been established here.

Note another interesting feature of the segment "I don't know whether he's coming to see me or to see you", i.e., in some strict sense, the patient is not "coming to see anyone" since he is a baby and capable of no such independent act. Rather, he is being brought to the Clinic. However, for the purposes at hand, he is a patient and patients are said to
"come to the doctor" regardless of whatever else they do.

The remainder of Ull reads "...but I shall be sort of squeezed I think to get back in time you deal with it". This is both an explicit formulation of the problem and a possible solution for it, i.e., Doctor B should see the patient should Doctor A be unable to do so. Notice that Doctor A is not refusing to see the patient but establishing good grounds for such a possibility, i.e., he has business elsewhere. The "I think" then sets up this as a possibility rather than a certainty, i.e., the possibility exists that Doctor A will be busy elsewhere and/or unable to make it back in time to see the patient. Although he provides a solution to the problem, namely "you deal with it", this may not be satisfactory to the physician so delegated. Let me elaborate this point.

One way of handling the present situation would be to look at it through some traditional sociological concept such as status. Thereby, Doctor A could be viewed as a senior physician, entitled to specific rights and duties from junior physicians such as Doctor B. When contingencies prohibit Doctor A from seeing a patient, he can, as a matter of course, assign that patient to a less senior physician. However, were the situation reversed, it would be harder for Doctor B to assign a patient to Doctor A. Such a view would be concerned wholly with how various medical personnel relate to each other in terms of power and would shift our attention away from the actual, ongoing sequences of interaction in and through which members produce and sustain for one another their daily affairs. To treat Ull in such a manner would be to
ignore the interactional structure of the occasion. The problem of who should see the patient involves the ways in which the problem is something produced and attended to by participants, and the answers to these concerns are much more complicated than notions such as status and the rest provide for.

A resolution to the problem can be achieved in many different ways -- it can be done tactfully, rudely, gracefully, etc. The interactional structure of the resolution is another interesting feature here. U14 is not merely an instance of a senior physician passing the buck to a junior physician. Rather, it is specifically designed to undercut the notion that one doctor is ordering another to see a patient. Doctor A establishes a reasonable excuse for having to assign the patient to another physician. It is not merely that he does not want to see the patient, but rather that he provides for the possibility that he may not be able to do so and, therefore, warrantable grounds for assigning him to another physician.

That a patient is assigned to another physician does not mean that that physician need accept the patient, nor need it result in a refusal to do so. U14 is an example of how an attempt to re-assign the patient can be forestalled by reformulating the bases of the problem; that is, it is not that there will be no physician available to see the patient at the appointed time, but rather that the patient is not the problem here. The patient is a baby and, whether Doctor A or Doctor B is available or not is not a matter of concern for him. The problem and possible solution lies in the fact that the person bringing the baby to
the doctor is his mother who will have to wait. Doctor B has transformed the "he's" of Ull into the appropriate category of person for whom it would be a problem were Doctor A not available. At the same time, he provides a characterization of the patient's mother that undercuts Doctor A's concern that he may not be able to get back in time by proposing that "Mother's a patient woman". This is not a mere characterization of the patient's mother, but is tied to the reasons why Doctor A might not be able to see the patient on time. Doctor B has undercut these grounds rather than refuse to see the patient. Note that such a refusal could have severe consequences, e.g., an argument might ensue. Thus, for the moment, Doctor A is still responsible for seeing the patient.

What gets treated in the literature as status and role relationships and rights and duties with reference to such relationships, can tell us little about the ways in which members come up with solutions to their everyday problems (problems which I suggest, fall under the heading of "the politics of everyday life"). That people are routinely placed in situations providing them with some degree of satisfaction or dissatisfaction is something one need not be a sociologist to attend to. How such problems come to be negotiated in everyday interaction, is, however, a topic worthy of sociological inquiry and one that will not be answered by providing glosses of the very phenomena investigated. The politics of everyday life is a topic for research investigation and not for theoretical speculation.

In attempting to handle the data as it was produced, I have made
two diversions from the analysis of a patient's reason for a visit.

First, I have discussed Us 3-9 as an instance of memory and recognition. Second, I have attempted to elaborate how an organizational problem is handled by the practices employed by members in handover. At this point, I will return to an analysis of a patient's reason for a visit to the Clinic.

The Reason for a Visit: II

Until U15, no grounds for the patient's reason for coming to the Clinic had been proposed. It should be remembered that U6 only provides us with the parameters that could warrant the patient's visit. In U15, the Nurse begins to formulate the reason. Although, in principle, a patient can see a physician for a wide variety of reasons, members of the Clinic do not engage in guessing games to account for a patient's reason for a visit. It is an inherent feature of medical practice that certain treatments may require repeat visits to a physician. Given this feature, members of the Clinic are able to formulate possible grounds for a present visit on the basis of the patient's past visits to the Clinic. What we are dealing with, then, is that patients can come to have careers as patients. I will address this issue in a moment.

In U15, the Nurse has apparently found some grounds, via recall or by consulting the chart, which allow her to formulate a possible reason for the patient's coming to the Clinic today. Doctor A, in U16, does not simply fail to wait for the Nurse's formulation to be completed, but rather orients to the Nurse's activity of finding a reason for a visit. The "Oh yeah" in U16 does not corroborate the Nurse's finding (as of yet she has not proposed a possible reason for a visit), but
rather by seeing that the Nurse is attempting or about to produce some reason for the visit, Doctor A via his memory of the patient, produces that reason and does so.

The structure of U16 is complex and warrants further comment. Nowhere in the utterance is it explicitly stated that the patient is coming to the Clinic for a shot. Yet, I take it that this is what is being proposed. The patient had a "last shot" and is coming for his "next shot". The elegance here lies not only in remembering that the patient received a shot, but remembering that the patient received a shot as part of a series of shots in a course of treatment, and that the shot received during a prior visit was not the final shot in the series. The use of "last shot" invokes the notion of a "series" of shots in a rather strong way. In addition, I suggest, it serves to reaffirm the earlier characterization of the patient as a "routine baby" where the features of such a characterization provide for such shots. As it turns out in Us 22-25, the shots are a series of immunizations.

It has been demonstrated that members of the Clinic are able to formulate grounds accounting for the patients' forthcoming visit. Empirically, however, the adduced reason for a visit may be proved wrong in the actual encounter between patient and physician. The baby could have become seriously ill and that illness could be the grounds for coming to the Clinic. Nevertheless this possibility never arises in their discussion. The parameters of a visit to the Clinic are, for members of the Clinic, an issue decided and decidable by reference to
their knowledge of the patient and current organizational concerns. Thus what was originally thought to be an organizational problem with reference to doctor-patient scheduling, Us 11-14, is now seen as something which may only require the attention of a nurse, for routine immunizations are given by the Nurse and the patient need not see a doctor in order to receive them.

The above analysis is but one instance of participants to chart rounds formulating or attempting to formulate a patient's reason for coming to the Clinic. This is a regular and recurrent feature of the occasion. I would like to make some general comments concerning the accomplishment of this.

While a patient's reason for a visit is generated from his chart and the accumulated knowledge of the participants, there are various ways in which the patient's relationship to the Clinic is utilized in this accomplishment.

1. **Use of the chart for programmed information.** Consider the following pieces of data:

   ONE

   68. Doc. A Mrs. Jones. I take it she is recently married. **Condyloma podophyllin return in one week.** (Condyloma is a disease term for venereal warts, and podophyllin is the medication used in treatment)

   69. Doc B Ah oh Steve Schwartz had been seeing them applying podophyllin.

   70. Doc. A Um well ah she must be back for more.
TWO

131. Doc A Miss Joan Summer wishes I.U.D. period finished to go take pill and return when she's menstruating. I take it she is coming back now.

132. Doc B ( )

133. Doc A For an I.U.D. insertion

The above constitute instances of how the staff use information about what took place on a patient's last visit to the Clinic in order to account for what will take place today. A written record of what took place previously is placed in the patient's chart and available during chart rounds. The patient's chart may contain information such as "return in one week," "return when she's menstruating," etc. Note that formulating a patient's reason for a visit is an accomplished activity and is not merely discovered by reading the chart. While a chart may contain programmed information, e.g., "return in one week," it must be established why the patient was told to "return in one week". Clearly, the charts are not self-interpreting. Even in the case of such a simple instruction, the staff faces the task of finding some correspondence between the instruction and the ramifications that follow from it.

The drawing up of charts is a member's accomplishment. Those who produced them knew that they may come to be utilized at some later point. Thus, I would like to examine the underlined portions in the above pieces of data as produced to be able to be utilized successfully at a later point. That is, the underlined portions have an order and that is tied to some notions of proper and improper accounts of what transpired between physician and patient. I am treating these instances as routine
accounts of previous contacts which participants to chart rounds use to decide a patient's reason for a visit.

Such accounts, however, can be problematical and commented upon. There are right ways and wrong ways of describing what transpired between patient and physician. Consider the following:

36. Doctor And here's Mrs. Heather Moore who's fifty-one.

37. Medical student Yes I remember her ah Doctor Marsh and I saw her

38. Doctor Is her presenting problem ah um Dr. Marsh hasn't learned the terse brevity of The Clinic yet? (Dr. Marsh is a new physician) sometimes it's a bit (difficult) to know exactly (5.0 sec. pause) pigmented all over including her ah ( ) mucosa with a normal blood pressure

39. Medical student I think her presenting problem was amenorrhoea

215. Doctor Irene Coombs, still on meloral (11.0 sec. pause) Lot of social stuff (7.0 sec. pause) Still on meloral, she's babysitting at home on meloral same dose, more meloral same dose ( ) (present medication) Aside from the fact that she's bored and does some occasional babysitting from time to time I haven't found out why she got on meloral. Do you know Mrs. Irene Coombs, Judy?

216. Judy (the Nurse) She's a schizophrenic

217. Doctor Well now that's the reason there we are...

The above instances illustrate how, describing what took place between physician and patient is tied to certain organizational concerns
of what an adequate description would entail. From these two instances, we may decide that such a description should be brief, describe the patients problem, and account for certain treatment procedures, e.g., the administration of drugs. How then do the underlined portions of our data constitute adequate descriptions usable for deciding a patient's reason for a visit? To get at this, I will present one further illustration. It is an instance where the physician makes a mistake in reading the chart. From this mistake, we may gain insight into the organization of our data.

346. Doctor Mrs. Child, feels much better coughs much better. Pardon me, feels well cough much better. Her pneumonia's cured, treatment nil

347. Doctor B. Mary was seeing her

We may begin by noting that the error made by the Doctor in U346 is an error in sequencing, i.e., the first underlined portion does not provide an adequate account. The "coughs much better" denies the grounds for "feels much better". The doctor recognizes this error in sequencing and provides a correction. He reads it again, this time, he reads correctly what was written in the chart: "feels well cough much better" where "cough much better" provides grounds for "feels well".

The issue of sequencing and record-making has been discussed by Raffel.⁹

The general problem for members is to get some sequence to a set of sentences. To sequence is one aspect of making a record as a factual account of what happened. To sequence in certain ways would be to call into question the factuality of the account...⁹

Examining the sequencing of the underlined portions of our data, what
would be the consequences of reordering, e.g., the underlined portion of U68 to "return in one week condyloma podophyllin". One possibility is that the patient is returning to the Clinic for determining whether or not she has condyloma and, if she does, the treatment is podophyllin. Such an account would still leave in question what in fact transpired between the patient and physician at the time the entry was made into the chart. Why couldn't she be examined and treated then? The "return in one week" provides for the fact that the illness and accompanying treatment account for her reason for a next visit. The patient is made out to be a medical case, requiring visits until the course of treatment is over and she no longer suffers from the illness.

The first part of U131, "wishes I.U.D.", establishes that the patient did not in fact receive an I.U.D. on her last encounter. "Wishes" establishes that the request was not fulfilled on the last encounter and constitutes the grounds for this current encounter. Compare this to, e.g., "wished I.U.D. period finished to go take pill and return when she's menstruating" where 'wished' would then provide the possibility that the patient had since changed her mind. The patient wanted an I.U.D. and still wants one. I.U.D.s are usually inserted when the patient is menstruating. That the patient was instructed to return "when she's menstruating", must be read that she is returning, not because she is menstruating, but for an I.U.D. to be inserted. Consider the underlined portion of U131 were it reorganized in the following way: "period finished to go take pill and return when she's menstruating wishes I.U.D." Such a reordering provides the possibility that she is coming,
not for an I.U.D. insertion, but for an examination to determine if such an insertion is possible.

2. Consulting the organizational policy of the Clinic. The Clinic's policy requires e.g., that a Pap Test (a test for cancer of the cervix) be administered to women who want birth control pills. A patient unable to have the test on a given day can schedule an appointment to return at a later date. When the patient's chart is reviewed, this policy can be utilized in constructing the reason for the visit.

Consider the following:

187. Doc. A Charlen Pinch wants birth control pills, she was in December, she taking anti-anxiety pills when she cannot sleep, one pill every two nights approximately. She must be ah yeah she's a Youth night program

188. Doc. B Umm hum

189. Doc. A Given one month supply of birth control pills and told to return within a month for a Pap so I guess she's in for pills

The patient wants birth control pills and she has to go through a Pap Test in order to get them. The Pap is an organizational policy of the Test in order to get them. The Pap is an organizational policy of the Clinic while her own reason for coming to the Clinic, however, is not for a Pap Test, but for pills.

That staff can decide on a patient's reasons for visiting the Clinic is an essential feature of chart rounds. Chart rounds, I suggest, run contrary to lay conceptions of medical practice. Such a notion might be formulated as, e.g., "the physician should always expect the worst when seeing a patient (i.e., be fully attentive for any possible manifestations of disease.)" Contrary to this lay conception, physicians
find typical courses of action that can account for the patient scheduling a visit to the Clinic. They do not typically view the visit as an open field of contingencies. Rather, once a reason for a visit is found, it is treated organizationally until or unless further notice should prove it wrong as the reason for the patient's visit. The medical history of the patient is held in equilibrium with reference to the organizational concern for the patient's presence at the Clinic.

This suggests then, that physicians emerge from chart rounds with a provisional orientation to the problems of the day's incoming patients. Of course, this is not to say that these provisional orientations are not fully retractable or that, upon encountering patients, physicians do not bring their full medical skills and attention to bear.

Conclusion

This chapter has attempted to present a detailed analysis of the occasion of chart rounds. I have proposed that this occasion is one designed for and accomplished by talk by clinic members. I have discussed some of the activities that are accomplished through talk and have discovered some of the self-organizing features of this occasion via an analysis of transcriptions of actual meetings. I have dealt with the progression problem, the reason for a patient's visit, the adequacy of patient names, and other matters related to the overall organizational structure of conversations. Without claiming that this is all that there is to find or that I have produced an exhaustive analysis of chart rounds, the findings presented in this chapter stand as ethnographic findings, responsive to our concern for maintaining the integrity of the occasion while responding to its self-
organizing features.

Over the course of producing this analysis of chart rounds, it has hopefully been evident that I have in fact followed a "standard ethnographic format" insofar as I have made use of those initial background materials provided in an earlier section of this report. At the time of their construction, however, I did not know that they would come to have this kind of prospective value and it can be assumed the reader did not see the significance of these materials either. This seems to be a general feature of the construction of detailed ethnographic description.

We may also note that the interactional and conversational approach used here has allowed us to illustrate some of the self-organizing features of chart rounds. Such analysis, however, also works the other way round for, by examining some of the data from chart rounds, we have discovered some generalizable features of interaction that operate across scenes and settings. Thus, while engaging in a description of chart rounds, the analysis pertains also to areas beyond the confines of the research setting.
Footnotes: Chapter Three

1. The unpublished work of Harvey Sacks must be mentioned as an outstanding source of intellectual and research stimulation. Since much of his material is only available in a series of unpublished, transcribed lectures given to his classes at the University of California, Irvine, the reader should note that it often has a chatty and casual character rather than that of material submitted for publication. While the style of these lecture materials is casual, the ideas put forth are our main concern. Rather than attempt to paraphrase or condense these references I have chosen to present them as they are within the lectures. This allows the reader to see how conversational materials are utilized in the production of a conversational analysis.


3. Ibid., p. 20.

4. Certain settings and occasions provide for the accomplishment of "core activities" and the talk that occurs within these settings and occasions must be responsive to their "core activities". This is not to say that the talk that occurs must only be about the core activity, but that such talk cannot be absent without some re-evaluation of the character of the occasion. It is in this sense that we can speak of "constraints" on topical talk. For a more detailed discussion see Bruce Katz, Conversational Resources of Two-Person Psychotherapy, unpub. M.A. Thesis, University of British Columbia, Vancouver, 1971.


CHAPTER FOUR

THE RESEARCHER AND THE RESEARCH SETTING

Introduction

Throughout this report we have been concerned with interaction and the procedures one could employ to study interaction. Chapter two was addressed to some of the ways in which a researcher, having gained access to the Clinic, could actually do his analysis. This chapter will be dedicated to an examination of the relationship between the researcher and the research setting. Specifically, it will be concerned with those features which confront the researcher in the actual doing of field work. It will focus upon some of the contingencies that a field researcher must deal with in and over the course of his work. Since much of these materials is related to those issues discussed in chapter two, I would like to go over the major points raised in that chapter.

Chapter two began with a consideration of some of the ways in which a medical sociologist might conduct research if given access to the Clinic. It was suggested that his approach would involve a predecided set of research questions and that the Clinic would serve as the locus from which answers could be obtained. I argued that such an approach would not, indeed, could not, respect the integrity of the research setting. I pointed out that the actual, ongoing behaviour would be seen and used as data supporting or refuting some research hypothesis. This fact alone would preclude any rigorous methodological concern with or attention to the members' point of view and/or with the self-organizing features of the Clinic.
Next, I considered traditional ethnographic description which has, as a fundamental concern, an analysis of how members organize their daily affairs. This looked more promising. However, a closer examination revealed that ethnographies often incorporate what may be called "standard professional concerns" — matters such as kinship organization, religion and magic, and political organization — into their analyses. Such concerns constrict ethnographers in much the same way as a research hypothesis limits our medical sociologist for they tend to channel observations about daily life into preconceived abstractions of singularly professional interest.

This weakness was noticed not only by me but by those anthropologists who promulgated an alternative to standard ways of doing ethnography. This new approach, called cognitive anthropology, has as its focus the production of a description which "understands the organizing principles underlying behaviour." In this respect, it is concerned not only with a setting's organizational features but with maintaining what I have called the integrity of the research setting as well. It is aimed at a cultural description which would allow the reader to behave in ways that would be deemed culturally appropriate by members of the studied culture. It criticized traditional ethnography on the grounds that the questions that it asked and thus the answers that it obtained were not responsive to the organization of the culture under observation. Frake characterized this difference between the two approaches in the following manner:

The only existing field manual for ethnographers, *Notes and Queries on Anthropology* presents a list of queries that an investigator can take to the
field, present to his informants, and thereby produce a set of responses. His ethnographic record, then, is a list of questions and answers. (The tradition in modern anthropology, however, is not to make such a record public but to publish an essay about it.) The image of ethnography we (cognitive anthropologists) have in mind also includes lists of queries and responses, but with this difference: both the queries and their responses are to be discovered in the culture of the people being studied. The problem is not simply to find answers to questions the ethnographer brings into the field, but also to find questions that go with the responses he observes after his arrival.1

While attending to the integrity of settings and occasions this new approach to ethnographic description has also advocated the goal of being able to specify "the grammar of a culture." Rather than predict behaviour, the goal of cognitive anthropology is somewhat like that of the linguist who faces the task of constructing the grammar of a language:

The aims of ethnography, then, differ from those of stimulus-response psychology in at least two respects. First it is not, I think, the ethnographer's task to predict behaviour per se, but rather to state rules of culturally appropriate behaviour. In this respect the ethnographer is again akin to the linguist who does not attempt to predict what people will say but to state rules for constructing utterances which native speakers will judge as grammatically appropriate. The model of an ethnographic statement 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." The second difference is that the ethnographer seeks to discover, not prescribe, the significant stimuli in the subject's world. He attempts to describe each act in terms of the cultural situations which appropriately evoke it and each situation in terms of the acts it appropriately evokes.2

The goal of cognitive anthropology is itself commendable, however, it was pointed out that no description could possibly allow the reader to act in an ongoing culturally appropriate way. The basis for this
criticism derives from the inescapable fact that any description requires first, that the researcher rely upon his own common sense knowledge of social structures when producing a description of a society and, second, that readers use their own common sense to interpret the researcher's description. Thereby, a description that would satisfy the goals of cognitive anthropologists would require the ethnographer to give more detail than would be contained in any setting or domain from which the description is generated. An example may help to clarify this feature of producing descriptions.

The previous analysis of chart rounds is not intended as a description that would enable anyone reading it to act in culturally appropriate ways. Rather, it is a description that is responsive to the self-organizing features of this occasion. To transform the previous analysis of chart rounds into an analysis that satisfies the goal of cognitive anthropology would necessitate that I inform the reader about many other features of the occasion. For example, I would be obliged to tell him that the staff are usually seated during the occasion, that it is permissible to smoke, that it would be inappropriate to "make "faces"; and so on and so forth. It is apparent that a virtually infinite number of items could be added to this or indeed to any description. That is, as Garfinkel has pointed out, the inherent problem for any such description is the problem of the necessary appendage of an et cetera clause. For, as a practical matter of fact or procedure, it is impossible to cite all of those things that any reader would need to know in order to be able to act in a culturally appropriate manner.
It can be assumed that no one treats cognitive anthropology as a discipline which should or even could achieve its expressed goal. I am not advocating a contrary position, but noting that the goal is more arduous and complicated than may first seem to be the case and, finally, that it is pragmatically impossible to achieve such a goal. To require that a description be responsive to "everything one would need to know in order to act in and to be seen as someone who is acting in culturally appropriate ways" is to ask for the impossible for it is impossible to describe that "everything".

This synopsis of chapter two illustrates that cognitive anthropologists have proposed a certain research goal towards which their cultural descriptions are oriented. The social scientist in the field has, similarly, some conception of his relationship to the research setting and the ends towards which his descriptions are directed. Whereas cognitive anthropologists specify the goals or ideals towards which their studies are directed, sociological literature often attempts to give advice and/or detailed instructions about certain ways in which recognizably good social ethnographies are to be accomplished. For instance, there is much literature explicating the roles that the researcher can and should play while studying a community or an organization. In general, such literature attends to the idea that the researcher should make himself minimally disruptive and thereby be maximally productive in terms of the actual collection of data. An entire literature concerns itself with the problems afforded the researcher by his role in the research setting. It could not be reviewed here. However, a considerable part ...
attempts to provide the analyst with instructions on how to act competently while in the research setting.

It is somewhat ironic that, on the one hand, cognitive anthropologists specify a goal which their descriptions should attain to but provide no literature outlining the way or ways in which this goal is to be achieved. While, on the other hand, we have sociological field manuals which attempt to instruct the social researcher in appropriate field methods but espouse no fixed goal which he should attempt to achieve. In sum, while cognitive anthropology has ignored the possibility (or impossibility) of achieving its proposed goal, sociologists have no proposed goal but have made the actual doing of field work into a methodological problem.

The advice offered in sociological field manuals is by and large of a kind that could be readily characterized as "boy scout" advice. The researcher is instructed to be "tactful", "non-disruptive", "adopt a role", and so forth. It is not that such advice cannot be followed but that it does not tell the researcher in the setting how to act in the manner prescribed. Instead, every individual in every particular research setting is required to make practical research decisions for the purposes at hand so as to follow such advice. By this, I mean that what will or will not constitute being "tactful," "non-disruptive" or whatever will have to be decided within an actual interactional context. Field methods texts tend to ignore the interactional context of the researcher and the research setting and to present their readers with a set of vacuous instructions that have to be transformed into practical
accomplishments in the field situation. While the goal of cognitive anthropology is as a practical matter of fact unobtainable, the instructions offered by research manuals are of a problematically empty character. Admittedly, the advice that they offer to field researchers is, in some sense, demonstrably good advice, however, the manner in which that advice is to be implemented by the researcher in the research setting is never itself a topic for examination; hence, the vacuous character of field research manuals.

Neither the proponents of cognitive anthropology nor those concerned with field work methodology have provided us with a thoroughgoing account of how the members of particular research settings manage the properties of a setting (including the presence of the researcher). Nor have they studied the ways in which the researcher manages his field experiences in the setting. In the following pages, it is my intention to examine this relationship between the analyst and the research setting by considering my own research experiences at the Clinic. Instead of treating my presence in the field as an essentially methodological problem, I shall look at some of the ways in which it must be regarded as a necessary part of the description of the Clinic.

The Sociologist as Cultural Stranger

Sociological field work manuals typically attempt to deny the sociologist the benefit of his own cultural expertise. By this, I mean that he is often treated as an "interactional buffoon" who has to be instructed in such things as the proper ways of entering a research setting, identity management, role playing, and role selection. It seems odd that
the adult layman going to a new setting should require a manual of instructions on how to be inoffensive, unobstrusive, and/or non-disruptive. Furthermore, the very act of giving such instructions could be seen as an implication that the author, the reader or both regard the person seeking instruction as socially incompetent. Be that as it may, concerns about methodological proprieties occupy a central focus in the literature of field research. These very methodological issues have seemingly denied the sociologist that which he (in essential contrast to the anthropologist) has in common with the people and society he is investigating, that is, his own cultural competence as a bona-fide member of the society he is studying. Instead, he is cast in the role of a cultural stranger.

There is something intrinsically strange about the assumption that we should instruct a sociologist in proper procedures for the doing of field research. If the sociologist is denied his cultural competence and regarded instead as a stranger to a setting, whatever instructions he may be provided with will not show him to behave in appropriate ways since, paradoxically, this very set of instructions trade upon the assumption that he is a competent cultural member in order to deny that he has this competence. Rather than treat him as a stranger, we should attempt to explicate the relationship obtaining between sociological topics and our resources for the investigation of those topics. Thus, Turner has stated that:

It is increasingly recognized as an issue for sociology that the equipment that enables the ordinary member of the society to make his daily way through
the world is the equipment available for those who
would wish to do a "science" of that world....

Field work methodology treats everyday features of interaction---
entering a new situation, being inoffensive, being tactful, etc., ---
as a suitable topic for investigation by incorporating into its pro-
positions our own common-sense knowledge of how these methodological
problems are resolved on an interactional basis without every making
the researcher's cultural competence itself a topic of sociological
inquiry. Often after tedious discussions of role selection, partici-
pant observation and research strategy the field researcher encounters
what I call the "et cetera clause of field work methodology." An ex-
ample of this et cetera clause is found in Junker's classic on field
work:

It is not possible to specify the combinations and
conditions and thence to write prescriptions for
role choices to match social science problems, if
only because field workers vary so greatly in
respect to identity and self that each must learn
to solve these problems as they crop up. More-
over, the ability to find such solutions and to
reject impossible field work tasks doubtless
develops as each field worker learns more about
self and the repertory of roles possible and
even most congenial for him, given who is is.

After any discussion about the methodological problems which are said to
confront the field worker, his "ultimate strategy" boils down to the in-
escapable fact that he can make use of his everyday common sense about
how to act in or react to any new strange or problematic situation which
he may meet within the course of his work. Therefore, while such
methodological discussions may be interesting, they seem to have little
relative utility for persons doing social ethnographies.
Let us now turn our attention to an examination of some of the everyday contingencies which are part and parcel of any research. Field research is regarded as a practical accomplishment. In order to undertake an adequate investigation of some of its properties, it seems appropriate that we examine actual instances of data from field situations. Many of my experiences at the Clinic have been preserved on audio tapes. It is my intention to examine my tape recordings and field notes in order to explicate some of the contingencies that may confront one in the field. I will consider three specific aspects of my field research at the Clinic: (1) participant or non-participant observation; (2) the manner in which Clinic staff identified me to patients during the course of various medical encounters; and (3) some ethical considerations revolving around the ways in which I was identified to patients.

On Participant-Non-participant Observation

There is a prevailing notion in sociological field work that the researcher has a choice about how he will conduct himself once his presence, as a social researcher, is known to the credentialed members of the setting. He can elect to be a participant observer or a non-participant observer, or various combinations and permutations thereof. While methodologists acknowledge that these two characterizations are ideal types so that it is impossible for the sociologist to be wholly one or the other, sociological literature continues to posit the distinction between the two as an issue of consequence for those engaged in field research. However, the distinction is neither consistently clear nor
free from contradictions. The following field note comes from my research at the Clinic. It is apparent that it can not be easily classified in terms of participant or non-participant observation:

I had just finished recording a medical interview when one of the physicians (Doctor X) asked me if I had ever seen a cyst removed. I replied that I had not. Doctor X then gently pushed me into another examination room and told me that I should "Have a look". I found myself in the presence of Doctor Y, a part-time physician who came to the Clinic every other week. He was preparing the patient for a minor surgical operation.

Doctor Y was familiar to me and with my presence at the Clinic and I assumed that he knew I was a sociological researcher. I was not introduced to the patient. Doctor Y proceeded to remove the cyst. He asked me to hand him certain instruments from a nearby medical stand. I often did this when observing medical examinations since it was anyway of doing something that I thought might be helpful. Doctor Y then showed me the cyst which was located on the patient's stomach. He then made an incision into the cyst and inserted a hemostat (a pair of blunt scissor-like prongs) into the cut in order to spread the patient's skin. Next he asked me to hold the hemostat (which was still inserted in the cyst) while he proceeded to wipe the wound with cotton. He asked me to spread the skin a bit more in order to get all the pus out. I did as requested.

After the procedure was over and the patient had left the examination room I asked Doctor Y if he knew that I was not a medical student. He said that he thought I was a medical student and that he wanted me to have a good look at the procedure. It appeared that the director of the Clinic had not informed Doctor Y about my research project and that he had assumed I was a medical student. Nothing was made out of this incident and, on subsequent occasions, Doctor Y would allow me to observe his medical encounters with patients.

This experience is not merely some "cute example" of one of
those unusual or unexpected contingencies typically encountered and related by those who have done some field work. Rather, it illustrates how field researchers face certain demand characteristics by virtue of the fact that they are accountable parts of an ongoing setting. By demand characteristics, I am referring to what Turner has described as the "situational and contextual features which persons (in this instance field researchers) engaged in everyday routines orient to as governing and organizing their activities." Thus, rather than regard this field note as an indication of an instance when a researcher who was a non-participant observer became a participant observer, I would like to examine some of the exigencies which it so clearly illustrates. These are an inescapable part of any section of daily life, field work of course included and, because this is so, a thorough reconsideration of the utility of the categories of participant and non-participant observer may be in order.

I would like to begin our inquiry by noting that I came to view the operation on the cyst by chance rather than by any purposeful effort on my part. Since I had told the staff of the Clinic that my research was concerned with doctor-patient communication, I assume that they knew that the observation of the removal of a cyst would not facilitate my research. At this point, it is important that the reader understand how my research interests were formulated to and for members of the staff. For this reason I would like to present the reader with the research description that I had constructed for the Clinic. I know that the aforementioned "Doctor X" had read the
following research proposal prior to the incident with the cyst:

Research Proposal

As a consequence of my interest in basic communication processes and because I must satisfy my thesis requirement for the Ph.D. degree in the Department of Anthropology and Sociology, I have decided to focus on certain basic communication processes employed in medical situations. It is hoped that the outcome of the analysis will prove to be of interest to physicians (although this may not be evident at the outset of the research).

Medical practice is not a mechanical operation in which patients enter the doctor's office, are examined and treated, and then depart. Rather, the talk that occurs between a doctor and a patient constitutes an essential component for the effective implementation of medical care. It is this talk, a basic communicative process, which is the proposed area of investigation. The medical consultation exemplifies the situation of parties who have different knowledge and different vocabularies for talking about matters that are of serious import to both of them. How is "adequate" communication achieved under these conditions? More importantly, what constitutes adequate communication between such parties, given that they have different stakes in the process and its outcome?

While many forms of communication are operative in face-to-face interactions (for example, verbal cues, gestures, and facial movements), the focus of my research is on the talk which occurs in interaction. The study takes as its fundamental position that the talk which occurs in various interactive situations (between a doctor and a patient) contributes to the social organization of such situations. Talk is much more than the common-sense notion of exchanging "packets" of information or messages; people, when engaging in talk, also perform activities, and it is these conversational activities performed by both doctor and patient in the course of their interactions, which are my main concern. Thus I am not using a "Telephone Company" notion of communication but rather an interactional one. Doctors and patients interact with one another and such interaction is socially
organized via the talk that occurs between the participants. Hence I am not engaged in evaluating doctor performance. Rather it can safely be assumed that doctors have acquired the competence with which to treat patients and, indeed, have learned to talk long before becoming physicians. However, the talk that does occur between doctor and patient has not been subjected to analysis. It is my intention to do this.

In order to accomplish this task, in October of 1971 I began a study of basic communication processes employed in medical settings. The members of the medical centre generously allowed me to enter their domain and observe the operation of a medical practice. At that time my research strategy provided for a period of time to acquaint myself with a new setting and equally to provide time for the members of the centre to adjust to having a sociologist in their midst. While such familiarity with a setting is an asset, the study of basic communication processes requires a certain degree of precision in data acquisition. In studying talk as a communicative process, it is essential to have a corpus of precise data from which to perform an analysis. When individuals engage in conversation, it is possible to make reference to "what was said in a previous conversation" without being able to produce a verbatim account of the entire interaction. Indeed, to spontaneously produce a literal account of what was said would seem strange. However, while the reliance on one's memory for the reporting of conversational exchanges is adequate for everyday encounters, such a procedure as a methodological technique for studying conversation is hardly appropriate. The study of the communicative processes employed in a complex interaction -- as that between doctor and patient -- requires a detailed record of what was said. For this reason I use tape-recordings as my source of data.¹ The procedure is long and time consuming. The result, however, is a corpus of data that constitutes a written record of a conversational encounter. It is from such records that a study of basic communication processes can proceed.

Since my concern is with the doctor-patient relationship, it would be advantageous to secure a corpus of data consisting of transcripts of recorded doctor-patient interactions. Data would therefore be

¹ Due to the confidential nature of the data required, this proposed research has been cleared and approved by Dr. A. of the Department of X and the Ethics Committee of the Faculty of Medicine.
obtained from those situations involving medical personnel interacting with patients. This would include data obtained from medical consultation, doctor-student interactions, Youth Night Clinic interactions between doctors, students, and patients, as well as doctor-patient interactions of the day-time practice. It is hoped that such a broad focus will also elaborate the mechanisms whereby medical diagnosis is accomplished.

It should be made clear at this time that I am not engaged in an evaluative study of the medical centre. The centre provides a setting from which the data may be obtained. I am extremely grateful and express my thanks to the members of

In order to facilitate the acquisition of data, Dr. Tough asked that I present an inventory of the quantity and type of medical interactions that I wish to examine. The following list will serve as a guideline with reference to the acquisition of data:

1. 30 medical interactions between a medical student and a patient in which a patient's medical history is obtained. These materials would be obtained from the Youth Night Clinic, and will prove useful when I compare them to those interactions in which a doctor elicits a patient's medical history.

2. 25 interviews between doctor and patient. These interviews should be both the patient's and the physician's initial contact with each other. It is in the initial encounter between patient and physician that one may expect to find problems in communication. For example, different languages of language use. Such interviews are of considerable theoretical interest.

3. 40 interviews between doctor and patient obtained from the day-time office practice. These will provide a comparison with those interviews conducted during the Youth Night Clinic.

4. 40 interviews between medical teams interacting with patients. These interviews will be obtained
from the Youth Night Clinic and will provide the comparison set of interviews to those obtained from the day-time practice.

Admittedly my research constitutes a new area of investigation and, from a sociological perspective, a departure from a traditional sociological approach. While the research is exploratory, it is acceptable to my Ph.D. Committee. I am sure that my supervisor, Dr. Roy Turner of the Department of Anthropology and Sociology would be happy to attempt to answer any questions you might have concerning the proposed research.

In presenting this research proposal, I hope to accomplish three things: First, it allows the reader to see what I, as a sociologist in the field situation, told Clinic staff about my research. (This information is seldom offered to the reader of a research report.) Second, it allows the reader to understand what I meant by the "demand characteristics" faced by the field researcher in a field situation. This research proposal was produced at the request of the Clinic's director. Its contents are directed specifically at satisfying his request for a research proposal that would specify both the character of my research and the type and quantity of tape recorded medical interactions that I required. As such it represents a situationally constructed account of my proposed research at the Clinic rather than a formal "academic" research proposal. Thus, for example, the numbers of medical interviews that I requested were not generated out of any "scientific" concerns with having a representative sample of medical interactions, but were generated solely out of my abiding concern with being allowed to remain in the research setting for an extended period of time. It was thought that, by specifying a large number of
medical interactions, it would be possible to obtain sustained access to the Clinic for a longer period than if the quantity of medical interviews was of a lesser number. As stated earlier in this report, I did not have a preconceived research hypothesis about doctor-patient communication, but this absence of a specific research hypothesis and research methodology did not prohibit the construction of a research proposal for the practical purpose of satisfying members of Clinic staff. Third, this proposal should allow the reader to recognize that, when Doctor X ushered me into an already ongoing medical interaction, he was not really assisting me in my research. Not only was there no time in which to set up my tape recorder, but I was also entering an ongoing situation without knowing what had occurred prior to my entrance. In essence, I was being shown a medical procedure rather than being afforded with an opportunity to tape-record a medical interaction.

Objectively, watching the removal of a cyst did not contribute to my understanding of doctor-patient communication. However, I did not indicate that I was not interested in watching the removal of a cyst when Doctor X indicated that he would like me to do so nor did I object when he placed me in an examination room with another physician and a patient. That is, I did not tell Doctor X that I was not particularly interested in watching a cyst removed or that I was specifically interested in tape-recording interactions rather than just observing them.

My reason or reasons for not resisting Doctor X's invitation are not hard to fathom. As a researcher in the field, I felt that I should be polite towards Clinic staff. Regardless of whether or not Doctor X
had a correct understanding of my research project after having read my research proposal, I felt that if he thought I might be interested in seeing a cyst removed or that watching this medical procedure would benefit my research in some way, then it was incumbent upon me to demonstrate some interest in this operation. It would not have been courteous or expedient to have thwarted any possible interest which Doctor X might have in me or in my research by informing him that he did not understand what it was that I was trying to do. Furthermore, to have done this might have had the result that I would not see any future medical examinations. While it is true that I was unable to record this particular encounter, to gain legitimate access to the examination room was, in this period of the research, a step forward for me. Whatever it was that motivated Doctor X to show me the "cyst removal" it was apparent that my theoretical sociological interests were of little or no concern to him.

It is apparent that the subjects in the setting under investigation can have various views about the purposes of the researcher's project. Instead of treating this as a result of some failure or reluctance on the part of the researcher to provide an adequate portrayal of his research interests, it may be better tied to the fact that members of the setting can, and in some sense necessarily, operate with diverse and sometimes discrepant views of "matters of fact" including ideas about what the researcher is doing. Furthermore, any attempt to rectify these discrepant conceptions result in more intrinsic problems than solutions.
Note that neither Doctor X nor Doctor Y felt that my presence in the examination room was something that required further explanation. Further, they saw no necessity that I be introduced to the patient. Furthermore, I saw no need to offer a self-introduction or self-identifier. I did not say, for example, "I am Bruce Katz the sociologist," or "Hi, I am Bruce Katz from U.B.C." I did not engage in a self-introduction for several reasons. However, it is important to note that the staff decided that it was the physician who should properly introduce me to the patient.

At this point, I would like to make clear that I am reporting neither my psychological feelings nor psychological motivations at the time of the operation; rather I am proposing good organizational or structured interactional reasons for not offering any self-introduction. These were my overriding concerns on this and other occasions. To have done a self-introduction on this particular occasion would not only have been inappropriate, it may have proved to be diversely and intricately problematic as well. I suddenly found myself in a situation in which a patient was already on the examination table and about to have an incision made into a cyst on his stomach. I did not know whether the patient would be willing or happy to have a sociologist in the examination room at this time. For me to have created a situation in which there was some possibility of the patient becoming upset seemed to be strikingly inappropriate. Instead, I allowed the setting to furnish an identity for me. That is, I presume that the patient thought that I was a medical student. The grounds that I had
for not engaging in a self-introduction at this time were based on my concerns for (1) the possibility of a problematic situation developing between patient and physician due to the fact that I was a sociologist and not a medical student and, (2) the possibility of jeopardizing my own position as a sociological researcher within the Clinic, and (3) my general concerns for the patient. To have engaged in a self-introduction might have caused interactional problems for this particular situation and lead to further general conclusions about the problems created by the presence of a sociologist. Obviously, these must be avoided.

My recognition of the possible problems and repercussions that could have arisen from a self-introduction did not originate in my having acquired any special sociological competence gleaned from field research manuals. My rationale for acting as I did was based on my own common-sense notions of situational proprieties. That is, I was relying and was able to rely on everyday common-sense notions about "how the social world operates" in order to manage my role in this and other situations.

Although one could ask whether I was a participant or non-participant observer in this particular situation, it was obvious that a "yes" or "no" answer to such a question would tell us little which would be of use to a researcher in a field situation. This incident illustrates the absurdity of the notion that we can easily define either a participant or a non-participant observer. The role of the field researcher, like that of any other members of the world of daily life, is not something
which admits to any easy, fixed definition. The range of interactional circumstances in which he will find himself during the course of his work admits to no hard and fast classification. To categorize the researcher is often to limit the scope and nature of his operations. It is to ignore the rich contextual field of all of our daily affairs. While such exercises may be interesting, they often hide much more than they reveal. How, for instance, could one classify the above interaction between myself and Doctor Y? Am I this at one time and that at another?

Situations must be dealt with on an ad hoc basis in and over the course of their development. They cannot be prepared for, accounted for, or ultimately constructed prior to their actual occurrence. Because this is so, any abstract concern with the role of the field researcher in the research setting is empirically empty. While it is possible to discuss role selection prior to or aside from the actual doing of any such discussion has very little connection to the practical and common-sense solutions that a field researcher must attend to when actually engaging in the daily contingencies of a researcher in a field situation.

Since this is the case, one may start to view the methodological concerns with the proper role selection as pragmatically useless and theoretically empty. Consider the following field note:

Before chart rounds one of the secretaries or one of the nurses usually makes coffee. The coffee machine is one of those industrial coffee makers. After spending some time at the Clinic, I started to make the morning coffee if I arrived a bit before rounds started. One day I just made coffee. No one objected to my doing so nor did they specifically require that I do so. It just happened. In the future, I often made coffee for Clinic staff when I arrived for morning rounds.
Although a simple and mundane feature of my field experience, this piece of data illustrates two specific features, one of these is practical, the other is theoretical. First, the field researcher is not generally incorporated into the daily activities of the society that he is studying. By this I mean that, as a researcher, one does not have a specific task to perform other than that of observing others at their work and forming certain opinions from these observations. However, this in itself can create some very practical problems. For example, as a sort of third wheel at the Clinic, I did not have organizational space which I could call my own. Unlike physicians or nurses, I did not have a desk or an office where I could do any work that I had to do. I purposefully spent a good deal of my time at the Clinic wandering around or sitting either in the lunch room or in an empty examination room. Whenever I was allowed to view a medical examination I tried to be as helpful as possible. For example, I would fill out laboratory information sheets, hand instruments to the physician, take urine specimens to the laboratory and so forth. I attempted to do things that Lofland has described as an "exchange of services":

In terms of "exchange of services" the pure observer role involves a highly imbalanced relation to the participants. They let him watch, but he does nothing for them in return (except in the most long-term and abstract way, in "doing social science"). More immediate reciprocities are necessary. Indeed, in a wide range of emergent circumstances, it will seem highly peculiar if the observer does not volunteer his help...people need rides, loans, messages carried, coffee brought, advice, opinions, defense, illegal goods held, lies in their behalf, and so on, through the entire range of normal friendly relations.
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typical to organized social life. The observer must necessarily engage in such things, unless he is willing to give off the impression of being a very odd, cold withdrawn fellow, "indeed."

Lofland would regard performance of such activities as a feature which is compatible with the pure observer role. He would point out that the researcher must do such things as these to give the impression that he is a right fellow. In contrast to this, my experience at the Clinic taught me that such activities are not so much a component of the observer role as a consequence of the fact that any party involved in an ongoing situation for a long period of time -- and this includes any researcher in a field situation -- has to engage in some activities in order to account for his continued presence in the setting. The performance of activities is not connected solely to the observer role, but constitute an essential part of those demand characteristics which are unavoidable in a field situation.

Secondly, while the performance of these activities may be classed as an exchange of services, their very necessity leads us to question the validity of the theoretical distinction between the participant and non-participant observer. What will be taken as an instance of participation or non-participation? For example, is the fact that I sometimes made the morning coffee going to be indicative of an exchange of services (i.e., non-participation) or is it going to be viewed as a sign that I was, in fact, a participant in the research setting? What theoretical status obtains between my helping during a medical examination as opposed to my making of the morning coffee? When does the researcher engage in an activity that makes him a participant as opposed
to a mere observer? If we cannot specify whether or not some activity is indicative of essential participation, what is the actual status of this distinction? Is it not lacking any worthwhile substance?

Researcher Identification: Accepting Staff Accounts

The previous section alluded to the fact that the social researcher is someone who is not a regular member of the research setting. For example, my presence as a sociologist was not a part of the formal plan or regular workings of the Clinic's affairs. The researcher's presence can create a host of problems for regular members of the research setting and the nature of and the solutions for these problems are not necessarily dependent on any calculable thing that the researcher does. This was illustrated time after time in the manner in which my identity was managed by the staff when they introduced me to patients during a medical examination. Consider the following transcript:

1. Doctor: Do you mind if Mr. Katz sits in with us?
2. Patient: No no by all means
3. Researcher: Great
4. Doctor: ( ) this room right here. Have a seat
5. Patient Tissue (Note: the patient had asked for a tissue)
6. Doctor: Mr. Katz is working with ah the Department of Medicine Faculty of Medicine for the next few months and he's doing ah study of communications and we wondered if it would be all right if he taped what we said to each other it's just for his use
7. Patient: Sure....
It was decided that physicians would introduce me to their patients and seek their consent before allowing me to tape a medical interview. However, an examination of the doctor’s portrayal of my research (utterance 6) shows several notable flaws in the characterization which he offered to the patient. First of all, I was not working with the Faculty of Medicine as is implied here. That is, I was not a certified member of the Faculty of Medicine but was only given a summer student research stipend from the Department of _______ to enable me to do a study of doctor-patient communication. Clinic staff knew that I did not have a medical degree and was not a medical student. They were aware that I was a Ph.D. student in sociology. Given my research proposal, it seems odd that physicians could be misinformed about my identity. Thus, an accurate account of my presence in the examination room might more reasonably be expected to resemble the following:

Mr. Katz is working for the Department of _______ Faculty of Medicine over the summer. He is doing a sociological study of doctor-patient interaction for his Ph.D. dissertation in Sociology at U.B.C. We wondered if it would be all right with you if he taped what we said to each other. It's just for his use.

While such a characterization might indeed be more accurate, it could also delay or be otherwise problematic for the successful accomplishment of the interview. Clinic physicians had to see a large number of patients over the course of any working day. My presence in the examination room was for them an additional feature that they were now required to attend to. They had to manage my presence in the examination room in ways that would still allow them to accomplish their
job in a successful and expedient fashion. One way of coping with my presence would be to offer an account of my identity similar to the above example. However, this might make things more difficult for the staff and for me. To do so allows for the possibility that the patient might question the physician about the presence of a sociologist in the examination room. He might feel easy or uneasy about this. He might ask questions about the purpose of my research, etc. Such questions would contribute nothing to the pragmatic task of treating the medical problem at hand. They certainly would not make the encounter any shorter. Indeed, the patient might balk at the idea or refuse to continue the medical interview. I am proposing, then, that there were good organizational reasons for my having been introduced in the way that I was, and to regard such an introduction as inaccurate or lacking in information is to neglect the fact that my presence in the research setting is, for the Clinic staff, a problem that has to be resolved by them on an ongoing basis. Thereby, while in some technical sense, the account quoted above may be incorrect, it would not be incorrect to say that the physician had, in some way, introduced me to the patient.

Interactionally, the doctor's introduction furnished me with a credentialed reason for being in the examination room. Interactionally, it would not be incorrect for the patient to assume that I was a medical professional, for patients often orient to the presence of medical students in such situations. Furthermore, while it is possible that I could have corrected the physician's portrayal, the consequences of doing this should be fairly obvious. To correct his account would, no doubt, "set the record straight", but it could well result in my being prohibited
from observing or recording any further encounters. My concerns with accuracy would not coincide with his pragmatic concern for seeing a large number of patients with as little wasted time as possible.

Instead of introducing me to a patient, Clinic physicians would often allow the research setting to define my identity for the patient. Since the medical interviews were to be recorded, I would simply position the tape recorder and then inform the physician that I was recording. At this point the physician would call the patient into the examination room. Consider the following piece of data:

1. Patient: (walking towards the examination room)
2. Researcher: Hello
3. Patient: Hi
4. Doctor: Well ah how you doing?
5. Patient: Oh I think I'm just about as good as I'm going to be
6. Doctor: uh good, it's quite a bit better
7. Patient: Yeah
8. Doctor: Oh that's good. All right, now ah you know that we want to cut down the Prenason ah that the um not the Anasec not the Tedral the other one that you started three a day....

This interview continued without my identity or purpose in the examination becoming a topic of conversation. I presumed that the patient identified me as a medical student for the setting provided for this characterization. While it had been stipulated that physicians should introduce me to their patient and ask for the patient's consent
before I would be allowed to do any recording, the procedure was not always adhered to. One way in which Clinic physicians managed my presence was to say nothing at all about my identity and allow the patient to assume that I was a medical student.

In the above situation, for example, the doctor and the patient were acquainted with one another although I had never met the patient. It is customary for a party who knows two other persons who do not know each other to introduce the two people. However, no introduction was forthcoming, nor was the patient informed of the presence of the tape recorder. What was I to do in this situation?

To have introduced myself as a sociologist would have been contrary to the definition which the physician had allowed to develop. This would be problematic in itself but, further, were I to interrupt the physician and introduce myself as a sociologist it would be possible for the patient to conclude that the doctor had purposefully withheld this information from him. Again, although I could have interrupted the physician to remind him of the presence of the tape recorder and/or the requirement that we obtain the patient's consent to have the medical examination recorded, to have done this could have caused some trouble for this particular occasion and have unpleasant ramifications for future encounters as well. I did not think that I should inform the patient about those things that should properly have been conveyed to him by the doctor. Indeed, it seemed more trouble to challenge or disagree with the developing character of the encounter than to allow the physician's unstated definition of the situation to be sustained. The end
result of this encounter was that both myself and the physician
operated as a team in order to maintain a routine in which my identity
was for all intents and purposes that of a medical student. As a
researcher interested in creating as few troubles as possible, I found
it necessary that I offer no self-introduction or mention of the
presence of the tape recorder.

Ethical Considerations

I had to secure approval from the Faculty of Medicine Ethics
Committee before doing my research at the Clinic. Many of the prelimi-
nary obstacles were the result of a concern which the Clinic staff had
about the ethical implications of my research. In retrospect, it seems
paradoxical that many of these same physicians, when confronted by the
situations described in this paper did not inform their patients of my
identity or that they were being recorded. Consider the following two
instances. They are typical of the way many of the physicians handled
my presence during the medical interaction.

I -

1. Doctor: I don't know if I'll even tell
   her that this thing is on

2. Researcher: Umm mm

3. Doctor: I think I'd better not

4. Researcher: Umm

5. Doctor: I don't know how she'll take it
   so let's leave it on

6. Researcher: Um mm

7. Doctor: She saw two students before so
   she's used to students
8. Researcher: Okay, maybe I should just turn it the other way (reference to tape recorder) I don't think it makes any difference leave that like that she's not gonna

9. Doctor: Yeah ah okay (patient entering office) Well Jane how are you. You're looking a lot better than you did the other night

10. Patient:] Yeah....

II -

1. Doctor: Mrs. Jones (pause) Mrs. Jones (the patient is being summoned from the waiting room) Here (doctor is referring to the examination room)

2. Patient: Well well how many doctors have I got (Note: in addition to myself there was a medical student present)

3. Doctor: Do you mind if Dr. Katz

4. Patient: Now I can't hear very good you know

5. Doctor: Do you mind if Dr. Katz and Dr. X (the medical student)

6. Patient: Not at all

7. Doctor: ah stay in here

8. Patient: No I don't mind

9. Doctor: Fine....

Note that these are actual occurrences. They are part of the corpus of transcriptions which grew out of my work at the Clinic. No doubt the reader appreciates the ethical implications contained therein. In the first instance, the fact that I was not introduced or identified to the patient was not simply an oversight, i.e., "a failure to give a proper introduction," rather it must be interpreted as part of a calculated strategy to allow my identity to be furnished for the patient by
the setting. I assume that the patient was supposed to and did in fact assume that I was a medical student. Indeed, my assumption seems to be borne out by the second instance in which the physician specifically introduced me to the patient as "Dr. Katz." Note too that patients were not informed that there was a tape recorder on or that they were taking part in a sociological study. While it seems important that field researchers consider the ethical implications involved in their studies, there are few, if any, descriptions of the ethical problems which field researchers encounter in the course of their work. I suggest that by examining these two instances we can begin to understand that whatever a researcher's own ethical position, there are everyday contingencies inherent in the field situation that prohibit him from exercising his personal inclinations in a unilateral way.

The choices open to the field researcher are organizationally determined by the fact that he is a field researcher in ongoing situations and are not dependent on some "abstract" adherence to a set of ethical principles. The field researcher has gained access to the research setting as a privilege granted to him and does not possess an ongoing right to be in the research setting. Throughout one's research, it is an omnipresent possibility that that access could be terminated. With reference to my own research at the Clinic I had proposed a project that would require that I be present in the research setting for over a year. This proposal however was always subject to termination by Clinic staff if I was, for example, causing trouble. Trouble could constitute anything that staff considered problematic for their everyday occupational
routines. One of the concerns of Clinic staff was with this concept of trouble, specifically the effect my presence might have on patients.

The Clinic was an experimental medical facility and, as such, one of their outstanding concerns was that of acquiring a large patient population. They were always on the alert for anything that might cause that patient population to decline. Needless to say, the presence of a research sociologist was a possible source of trouble, something that could have an adverse effect on the absolute numbers of the patient population. My research into doctor-patient communication, while theoretically interesting, could lead to certain complications since I was not a medical practitioner and/or, in addition to this, I would be using a tape recorder. Given this information, what was I to do? I knew that from a strictly ethical standpoint, the physician should have informed the patient of my true identity, my purposes and the presence of the tape recorder. However, I found myself in an understandable quandary about an appropriate course of action in this situation. Let me elaborate by referring to the two transcripts cited above.

In the first transcript the physician (in utterance 1) infers that the tape recorder should be left on and decides not to inform the patient that the interview is going to be recorded. The physician making these propositions has ostensibly "good medical grounds" for doing so. By this, I mean that the physician has made a decision about the use of the tape recorder in terms of the possible repercussion that it might have for the interview. As he stated in utterance 5: "I don't
know how she'll take it so let's leave it on." I felt it was inappropriate to disagree with the physician. I could have suggested that he tell the patient about the tape recorder. I felt that to do so would have created 1) immediate problems for the situation at hand. It would cause undoubtable embarrassment for the doctor and possible consternation for the patient as well, and 2) a situation in which my stay at the research setting or at least my working relationship with this particular physician would be placed in jeopardy. Obviously, I did not want to do anything that could create a strain in the working relationship that I would have with this and with other physicians. If he felt that it was best to conduct the interview in this manner, I did not feel it was my position to tell him that he was engaging in procedures which might be labelled as unethical. It was he, the doctor who should have final say in matters of the doctor-patient relationship. Here, I was only the researcher.

As a researcher I had to orient to the demand characteristics faced by Clinic staff. Thus, physician's working conditions were part of the constraints bearing upon my actual research procedures. Thereby, while I could have engaged in a discussion with the physician about his decision, to have done so would have interfered with the ongoing concern that the physician should attend to the patient in the least possible amount of time. (Note: this does not mean that the patient should receive inadequate medical care. Rather, it comes from the fact that physicians are busy with a number of patients and do not have time for matters not specifically related to the examination at hand.)
Physicians have a job to do and, from their point of view, they should accomplish it in as short a time as is practically possible. As such, their primary orientation was directed towards the fact that these interviews could be done with or without the tape recorder.

Although it would have been possible for me to have raised these issues at Clinic staff meetings, to point out that some physicians had not introduced me to their patients, informed them that I was a sociological researcher rather than a medical student, and that I wanted to tape-record their interviews would portray certain physicians as unethical and would surely have resulted in the termination of either my research at the Clinic or of the good working relationship which held between myself and the staff. I saw no reason to do this since it was apparent that the staff was happy with things as they were.

In the second instance of data, I was introduced overtly as 'Dr. Katz'. This characterization was both false and deliberately misleading. What was I to do in this situation? To make the necessary correction during the interview (in front of the patient) seemed grossly inappropriate and to correct him after the interview also seemed to be inappropriate. Further, note that the medical student was also introduced as a doctor, although he was just a student. It seems from other experiences at the Clinic that this is the way in which students are typically introduced to patients and the physician assumed that, were he to manage my presence in the room in this way there would be as few problems as possible.

While the two instances quoted above are examples of some of the
ethical problems that a researcher might encounter in the field, they also show how such problems take place within a situational context. To speak of ethical considerations in the abstract alone is to neglect the situational and contextual features of field research. I had done everything possible to assure that my research would be conducted in an ethical manner. Indeed, this was not only my concern alone, it was also of central concern to Clinic staff. It is ironic that a researcher who sought the approval of the Faculty of Medicine Ethics Committee before starting his project should find himself violating certain ethical considerations. However, as I pointed out, this violation was motivated by the inescapable contingencies faced by a researcher confronting an actual ongoing situation rather than by any personal choice alone. It would appear that any discussion on the topic of ethics and field research is indeed empty if it fails to consider the practical contingencies that evolve over the course of any field research. It may well be the case that the researcher's own ethical position will come to be curtailed and transformed by the members of the setting to which he has access.

Conclusion

In the previous chapter we examined some of the self-organizing properties of chart rounds oriented to by participants. We did not address the relationship between collection of data and the researcher's presence in and of the research setting. While this relationship is often regarded as the "other side" of ethnographic description, this chapter has attempted to reconsider this formulation by considering some of the writer's experience during his research at the Community Clinic.
Research manuals often provide the analyst with a set of instructions on how to act during his stay in the research setting. He is supposed to select the role which will most successfully allow him to accomplish his research goals. Ideally, he should try to be non-disruptive, unobtrusive, and so forth. This chapter has attempted to point out the fallacious character of this formulation for the researcher as inevitably in but also becomes a part of the setting and how he is to act is not something which he can control in advance. Rather, whatever the advice he may acquire about how to act in the field, that advice has to be negotiated with the realities of every setting. The essential demand characteristics of the field situation remain unspecified in field research texts.

We focused primarily upon three features of doing field research: participant-non-participant observation, how I worked with patients of the Clinic, and certain ethical considerations arising out of these identifications. While such features are often treated by methodologists as separate topics, I have attempted to demonstrate that each of these can properly be examined in conjunction with actual instances of field data, i.e., that they become substantive topics only when they are responsive to those interactional occasions in which they occurred.
Footnotes: Chapter Four


2. Ibid., p. 124.


5. I recognize that there are materials such as etiquette manuals dealing with appropriate ways of behaving in various settings and occasions and that people do ask "advice" when going to new and strange places. These constitute lay exceptions to this formulation. The point however remains valid for any "instructions" they receive acquire their substance when the person has to attend to the organizational structure of that they refer him to. The type of advice and instructions offered in such manuals is not self-fulfilling, that is, if someone says "you do x when you go there" it still requires that one follow that advice within some organizational context and how that advice should be carried out within such an organizational context.

6. The same point has been made using data from a different setting, namely, grade school testing procedures. It was found that to successfully accomplish a series of tests designed to assess the competence of grade school children, students already need possess the very competence which the testing procedure was designed to assess. See, Robert MacKay, "Conceptions of Children and Models of Socialization" in Hans Peter Dreitzel, Ed., Recent Sociology No. 5: Childhood and Socialization, (New York: Macmillan Company., 1973), pp. 27-44.


9. Author's field note.

10. In the previous chapter reference was made to the "demand characteristics" required of participants to chart rounds. This term is equally applicable to the researcher who must treat his stay within the research setting in a similar way, i.e., as his job. As such, it will have certain features that he must attend to in consequential ways.


12. Technically speaking, medical students are not yet medical physicians. It appears, however, that when referring to these students physicians address them as "doctor".
CHAPTER FIVE

CONCLUSION

Summary and Further Research

In this dissertation I have departed from a standard model of ethnographic analysis in that I have chosen to formulate as a topic matters customarily treated as features of an interpretive schema shared by ethnographers and laymen, and used by them as resources in the construction and interpretation of ethnographic reports. That is, I have focused attention upon some of the methodological and substantive issues which underlie the production of a description.

Chapter one began with a description of the Clinic. I discussed how I had come to select this particular setting and how this selection arose from a fundamental pragmatic need for a setting for my dissertation rather than from any previous experience or special interest in medical sociology. Next, various descriptive characteristics of the Clinic were presented so that the reader might acquire a "sense of the setting." Of particular importance here was the fact that the Clinic operates as a group medical practice so that there is always the possibility that a patient will be seen by a physician other than his own, regular physician; and therefore, in order to facilitate patient care, staff review patients' medical charts in a routine session known as chart rounds. These initial materials also contained an account of the procedures I had to follow in order to gain access to the setting. The reader was informed that, while the Clinic appeared initially to be receptive to sociological research, gaining access proved to involve
certain elaborate procedures. Finally, however, I not only gained access but was allowed to tape-record sessions of chart rounds and other instances of medical interactions. The transcripts made from these recordings would come to constitute the majority of data for this report.

This initial description was intended as a necessary component of an ethnography of the Clinic. By this, I mean that it was expected that I provide the reader with some general information about the problems of research access and the characteristics of the Clinic prior to a more detailed and analytical study of its organization. At this point, however, chapter one took a radical departure. Utilizing a procedure followed throughout most of this study, this initial descriptive material was taken as a topic in its own right. That is, I examined the ways in which it came to be a recognized and recognizable description. My concern was not with those features peculiar to my own description, but was directed towards discovering the generalizable features of any such ethnographic material.

A comparison of my initial description with the opening sections of other ethnographic works suggested that the construction of an accredited ethnography follows a recognizable structure which was characterized as a "standard ethnographic format". That is, an ethnographic report typically presents the reader with a corpus of initial materials as an introduction to the subsequent analytical sections that are to follow. The first part of our task was to specify the relationship which obtained between these sections.

It was found that although these initial materials provide the
reader with a sense of the research setting, the connection between them and the subsequent sections of a report was not one of logical necessity. By this I mean that the reader's understanding of subsequent sections of the ethnography is not dependent on his first having read these initial materials. Such materials seem to occupy the position that they do in order to satisfy a presentational format rather than to satisfy any criterion for the logical ordering of ethnographic materials. Furthermore, it was shown that the construction of these materials is not based on any special scientific competence that the ethnographer may or must possess. Rather, he seems to be able to rely upon his own common sense to provide his readers with a sense of the research setting. These observations pointed to a rather curious fact, i.e., while the main sections of ethnographies are often seen as sections containing issues of theoretical interest, these prefatory sections, although they are standard components of ethnographic reports, are seldom regarded as worthy of examination or analysis.

The next part of chapter one was directed towards an explication of the importance of these initial materials for the construction of an ethnographic description. Traditionally, investigation has been focused upon the substantive content of ethnographic reports rather than on the way in which reports are constructed. In this respect, the chapter focused attention upon a strangely neglected area.

These initial materials were shown to do certain work for the reader with reference to the organization of ethnographic descriptions. First, they underscore the privileged position of the ethnographer
vis-à-vis the reader of the report. By presenting such materials the author establishes himself as someone who possesses expert knowledge about the culture under investigation. Second, while these descriptive materials may constitute a set of resources that the reader may, or should use to inform his reading of later parts of the report, the reader is not provided with a set of instructions on how to utilize these materials when reading subsequent sections. While there is not a logical a priori connection between these initial materials and subsequent section of the report, they seem to occupy the place that they do so that the report as a whole will be seen as a competent piece of ethonographic material.

It was demonstrated that their construction, although consequential for the ethnography, was itself founded in a common-sense set of relevancies. The features that are incorporated into the initial description are selected by the ethnographer after considering such things as the type of audience that the report is intended for, etc.

To say that the ethnographer relies on common-sense procedures in constructing the prefatory sections of his report is to argue that such initial materials are condemned to be common-sense rather than scientific constructions. This, however, is not a criticism for, rather than view it as an inadequacy of ethnographic description, it may be considered simply an interesting, invariant property of an ethnographic report.

Chapter one not only provided an analysis of the initial descriptive materials of an ethnographic report, it also contained an initial
description of the Clinic. This was the opening section of my ethnography of the Clinic. The reader, here, could properly expect a more analytical description of Clinic organization. However, chapter two took as its point of departure a consideration of the relationship between the researcher and the research setting. Thus, it did not provide the expected analytical description of an aspect of Clinic organization, but rather turned to an examination of some of the methodological presuppositions that account for the analysis of chart rounds presented in chapter three.

Chapter two began by considering how a medical sociologist might treat access to the Clinic. It was proposed that such an approach could regard the setting as a mere location from which to gain information for some preconceived hypothesis. This approach was shown to be incapable of describing the organization of the Clinic since it would neither respect the integrity of those aspects of Clinic organization observed nor be responsive to the type of data collected from the daily organizational routines of Clinic life. These points need further clarification.

As a researcher at the Clinic I was in a privileged position to observe and record various aspects of Clinic organization. To simply present the reader with the data which I collected at the Clinic would not constitute an adequate ethnography. What then, should an analysis be responsive to with reference to the data and the social origins of that data? To speak of the integrity of the research setting was to note that data does not exist independent of a social context from which
it is collected. My own data is not just from the Clinic but from a medical interview, from a session of chart rounds, or a lunch conversation, and so forth. To neglect this contextual feature is to disregard a large part of the interactional background expectancies that members (including the sociologist) use and display in their routine interactional encounters. To say that a medical sociology approach would not respect the integrity of the research setting is to say that it treats the stream of Clinic behaviour as something independent of the contextual situations in which it was produced. Such an approach would not be concerned with Clinic behaviour per se but rather, with some preconceived research hypothesis which would allow the researcher to see various behaviours as items that would support or refute his orientation. Such an approach would disregard the context from which the data was collected as itself being a constituent feature of any analysis of that data.

To speak of one's analysis as dependent on the context from which the data comes is to note that it should be responsive to that context. By this I mean that the researcher should be able to handle the details of actual occurrences. He should be able to do this in abstract ways, but, ultimately, his analysis should be responsive to actual social interactions. From the standpoint of an analytical description of Clinic organization, it is clear that an approach, such as that used by the medical sociologist, would contribute little to such a description for it would not be concerned with how the members of the Clinic organize their ongoing social affairs.
An alternative research procedure would be to regard such activities and occasions as phenomena having a structure and self-organizing character which are open to and worthy of empirical investigation. To follow such a research strategy would require one to respect the integrity of the research setting. Traditional ethnography portrays itself as having this concern and for this reason became a topic of inquiry. A consideration of materials from research into the social psychology of experiments showed how traditional ethnographies, while somewhat better than the traditional sociological approach, could not maintain their claim to respect the integrity of other cultural settings. I proposed that such ethnographic reports are often constructed around preconceived topics and theoretical issues which remove them from the matrix of everyday life from which they ultimately originated.

The new ethnography (or, cognitive anthropology) was then considered since it not only made pertinent criticisms of traditional ethnographic description, but also proposed a new research strategy. It tries instead to produce a description that would enable the reader to act in culturally appropriate ways. The direction of the new ethnography seemed to be a recommendation towards the production of a description of some aspect of clinic activity, but its goal seemed to be unattainable. In chapter one it was demonstrated that it is impossible to provide a description that will completely inform a reader since any description relies on a set of unspecifiable features that the reader must necessarily utilize in order to "make-sense-of" what he is reading.

The next task was the selection of a clinic activity from which to
produce a detailed analysis. Chart rounds was selected because it constitutes a recurrent and bounded activity within the Clinic. I then produced a description which I characterized as "an inadequate description" since it did not attend to the self-organizing features of the occasion. Given that chart rounds are specifically designed for talk about patients, I proposed that, by examining transcribed material from tape recorded sessions, it would be possible to discover some of their organizational features. Since chart rounds are a routine interactional and conversational activity, such transcripts should display relevant features of the self-organizing character of the occasion. Thus, instead of attempting to do a description that would enable one to act as a competent member to this occasion, I would attempt to construct an analysis responsive to the self-organizing features of the occasion by examining some properties displayed in the talk of clinic participants.

Chapter four also examined the relationship between the researcher and the research setting, but this time with a different perspective than that of chapter two. It focused on some of the day-to-day decisions that confront a researcher in the field situation and on how he might be expected to conduct himself in and over the course of a variety of situations. It was not the purpose of this chapter to provide a manual or a set of instructions for field researchers. Indeed, it was the hiatus which exists between the goals of ethnographic description and the nature of the instructions offered in field manuals that prompted this examination of my own field experiences at the Clinic. This was directed towards an explication of my own field experiences and, further,
towards an examination of these experiences as indicative of the general problems faced by a researcher in a field situation.

Throughout my research at the Clinic I was engaged in a variety of situations that provoked questions about the validity of many of the taken-for-granted assumptions I had held about the nature of field work. As a student, I acquired an orientation towards the successful accomplishment of field research. By this I mean that I had become aware of the standard topics and problems associated with doing field work: the selection of a research setting, the problem of securing research access, the various social roles one would have to "play", the problem of being a participant or non-participant observer, and so forth. I regarded the doing of field research as something that I had to control and account for. It seemed necessary for me to handle my presence in field situations and it was my responsibility to manage the problems associated with the role of a participant or non-participant observer.

This prior conception of field research was quickly shattered during my research at the Clinic. Such prior concerns concentrated on the social researcher and neglected the fact that his presence is something that the members of the research setting must also manage. In terms of my daily life in the field, it soon became apparent that whatever prior conceptions of field work I had were vacuous and of little pragmatic use when confronting the daily contingencies of a researcher at the Clinic.

Instead of presenting a discussion of role selection or ways of being accepted by Clinic staff, I examined what I referred to as
the demand characteristics of a researcher in a research setting. That is, instead of giving the standard field work topics a privileged position, I placed those topics within the actual interactional context of doing field research. The focus of the chapter was thus directed towards the examination of those situational and contextual features in which I found myself while a researcher at the Clinic.

The reader will remember that my own experience demonstrated that 1) the distinction between participant and non-participant observer lacked substance, 2) the manner in which a researcher's identity is developed may be dependent on the setting's members rather on the researcher, and 3) that this identity selection can lead to certain ethical problems. While it is possible to maintain the distinction between participant or non-participant observation or between ethical and non-ethical research practices, these may have to be discarded if one is to focus on the doing of field work and to investigate the daily demands placed upon the field researcher as they are translated into courses of action.

Chapter four suggests that further research into the daily contingencies of the field researcher and of the actual problems that researchers encounter in their activities could provide the appropriate basis for a discussion of how the research situation can become a source of data for an investigation into the methodology of field work.

In chapter three I presented an interactional analysis of chart rounds. A segment of transcript was examined and, from this apparently uninteresting, mundane material, it was possible to discover the following general features of this occasion: 1) How Clinic staff resolve
the termination of talk about one patient's chart and a progression to the next, thereby accomplishing a solution to what was termed the "progression problem". 2) A patient's name is often not sufficient to referentially identify him to members of the staff, 3) A patient's medical chart constitutes a document-in-use throughout the occasion of chart rounds, and can be consulted to determine his reason for a visit to the Clinic, and 4) Physicians tend to view patients' medical careers as static between visits even though such a view is subject to transformation upon an actual medical encounter with the patient.

That the Clinic staff is able to adduce a patient's reason for a visit during the occasion of chart rounds provides some insight into the workings of organizations. The reader will remember that the Clinic is regarded by staff as a new and innovative type of medical treatment facility. It is staffed by a group of highly-trained medical professionals who are idealistic about the potentialities for community medical care. They regard the Clinic as something distinctly different from other, more conventional medical practices. Yet, despite this progressive ideological perspective, closer examination shows that the Clinic displays some of the standard and invariant features of any organization. Thus, during chart rounds, Clinic staff routinely provide reasons for patient's seeking medical attention.

Chart rounds is not an occasion specifically designed to adduce a patient's actual reason for a visit to the Clinic. However, adducing a patient's reason for a visit is an organizational outcome of chart rounds. This method of determining a patient's reason for a visit
runs contrary to a lay notion of medical practice: "the physician should always expect the worst when seeing a patient," that is, "be fully attentive to any possible manifestation of disease." Contrary to this lay notion, physicians find typical courses of action that can account for the patient scheduling a visit to the Clinic. They do not typically view the visit as something generated by an open field of contingencies. Rather, once a "reason for a visit" is found, it is treated organizationally and "until further notice" as the reason for a visit.

Thus, despite the progressive ideology of the Clinic, the analysis of chart rounds found that the Clinic exhibits standard properties of organizations. The Clinic staff constitutes a group of experts dealing with a clientele. As such, they have standard formats for routinizing contacts with that clientele and ways of dealing with clients that are economic in terms of time and energy, etc. To propose this as a feature of Clinic organization is not to adopt a cynical attitude towards the Clinic or its operation, but rather to demonstrate that, even in a setting which subscribes to an avant-garde concept of medical practice, one will find certain standard and invariant occupational routines and organizational structures.

I have paid relatively little attention to many of the substantive areas of life within the Community Clinic; areas such as doctor-patient interviews, physical examinations, laboratory procedures, medical diagnosis, and so forth. This lack of attention is not stated apologetically. As an ethnographer within the Clinic I had no theoretical framework that
provided me with a "serial" orientation toward ethnographic description. In many ways this proved advantageous. Let me elaborate this point.

Traditional anthropological ethnographers attempt to study some people, group, tribe or society. The ethnographer is regarded as having an expertise with reference to such a group. The product of such ethnographic research is usually a monograph which consists of the component features of the culture. Firth, for example, in his work on the Tikopia has chapters on marriage relationships, family, circles, land tenure, and village life. Either the fact that these components originate from the same setting provide a sense of unity, or else the ethnographer is forced to find theoretical connections between these components which give the ethnographic report a sense of unity. I did not have a research framework that would allow (or force me) to presume that a collection of topics from one substantive area would constitute an adequate ethnography of the Clinic, nor did I have a theoretical framework that would allow me to generate arguments for my ethnography to contain such a serial collection of descriptive topics.

Admittedly, I could have proposed further analyses of other aspects of clinic organization, but for what reason? From material discussed in chapter one, it should be apparent that to adopt such a conception of ethnographic description would be to adopt an unattainable goal since no matter how much one attempted to describe, it is impossible to describe everything. I am not referring to the problem that psychologists might call a stimulus overload, i.e., that there is
so much impinging on the human senses that it is impossible to describe all that our senses react to. Rather, I mean that, as analysts, we have no conception of what would be necessary to describe the "everything" of some cultural setting or occasion. Whatever is discovered to be described is described and whatever set of interests or relevancies the ethnographer has dictates the character of that description. As analysts, we do not have a sense of the description of a setting standing in relationship to that setting in the same way that the items of furniture of a room stand to that room. There is no inventory of the proper items to be described in doing ethnographic description; and it is argued that to adopt a contrary notion is not a viable way of looking at the world as an object of study.

An alternative to this serial strategy might be to examine a single bounded activity, setting, or occasion and focus on it in some detail. This was the procedure followed in our examination of chart rounds. It is apparent that, although we limited our focus of attention to this one activity within the Clinic, there was, nevertheless, an infinite amount of detail to be discovered and attended to. Thus such a procedure should not be presumed to have a minimal amount of ethnographic interest. Because no exhaustive description is possible, it seems a reasonable procedure that we find some describable feature of a setting from which it may be possible to discover features that are generalizable to other settings and occasions.

The materials presented in the analysis of chart rounds are identifiably recurrent particulars for the members of the Clinic. They
are features oriented to by the participants to chart rounds as constituent features of the members' world within the research setting. Where do such findings now lead us? It seems that the particulars discovered via an examination of an occasion with this medical context may be the raw material of socially-organized activities that occur at any time or place. While they are located by the particulars in this medical setting, they seem to be instances of "cultural logics" that materials from other settings and occasions could be organized around; that is, they seem to have the capacity to organize other settings and occasions. Thus, from a detailed examination of one instance of clinic activity, it may be possible to engage in further research into these cultural logics rather than proceeding by examining further materials from other medical settings.

An illustrative example of this latter point is found in Sudnow's analysis of "unit news" in his ethnography *Passing On: The Social Organization of Dying*. He found that once a death occurred within a family, notification of that death to other family members was a socially-organized phenomenon. However, the topic of "unit news" is not restricted to the announcement of a family death nor to hospital settings but becomes a feature of everyday life, available for other ethnographers to investigate vis-à-vis other settings. Similarly, I am suggesting that the analysis of chart rounds has provided us with materials that have the capacity to organize other settings and occasions. I am not proposing that one should compare this analysis of chart rounds with other analyses of similar medical activities, but that further research
should be directed towards how features discovered from an examination of this clinic could be found operative in other settings and occasions.

Concluding Remarks

A dissertation typically ends by presenting the reader with a concluding chapter. The format for such a chapter usually contains not only a summary of the main points of the dissertation, but also an assessment of their significance for the topic(s) under examination. It is not uncommon to end with a discussion of possibilities for further research. The preceding materials of this chapter conform to such expectancies for the ending of a dissertation. I have presented a summary of the highlights and findings of each of the chapters and some suggestions for further research. I feel, however, that this does not constitute an appropriate way in which to end this report. Let me elaborate.

While it is possible to regard the previous section of this chapter as explicating the connections between each of the preceding chapters and as offering proposals for further research, to do so would not only to disregard further possible discoveries about the standard ethnographic format, but also to misrepresent the findings of this report as well. This report has looked back upon certain of its sections as data from which to generate an analysis. I would therefore like to end this report by adopting this procedure and considering the previous sections of this chapter as data from which to derive further features of the standard ethnographic format. This procedure will also allow me to exemplify the differences between this report and a standard ethnographic description.
It appears to be a standard feature of an ethnography that one's report ends with what may be described as a "satisfying" conclusion. By this I mean that the ethnographer does not find that his ethnography has, for example, no termination point, nor is it the case that it leaves the ethnographer without any resolution to his original task or that he finds that there are more problems at the end of his report than when he started his research. I intend no cynicism in referring to this conclusion as satisfying, but merely wish to note that all of the possibilities noted above are perfectly plausible and probable outcomes of an ethnographic report. Yet, the ethnographer is somehow expected to end his work with a tight-knit conclusion that allows the reader to "see" the major findings and highlights of the research. In providing such a conclusion, the reader can view the ethnographic description not only as a description of some cultural setting, but also as a piece of research having some overall theoretical relevance.

When I look back upon the preceding chapters of this report, I find that they do not contain any real termination points nor do they provide any resolutions to major issues. Rather, the chapters represent a series of topics that I have considered as a result of my experience in the research setting. All that these chapters have in common is that they were topics I considered. Thus, the connections claimed in the preceding section of this chapter constitute what I regard as "analysts' connections" rather than any necessary connection. In this respect, my research report is different from a standard ethnography and an examination of this difference reveals 1) what those features of the standard
standard ethnographic format are, and 2) how my report differs from these features.

The standard ethnographic format seems to make two claims that this research report is not making. First, ethnographies either implicitly or explicitly claim that they have covered at least some feature of their setting with a reasonable degree of comprehensiveness and exhaustiveness. That is, the ethnographer has done enough research on some aspect of the setting to be in a position to offer a coherent, clear, and authoritative report of the topic he investigated.

With reference to this first point, my own research at the Clinic does not follow such a format. While my research report contains some items that I have discovered in the research setting, my report does not purport to be an exhaustive description of some feature of the Clinic. Rather, it is a discussion of two or three topics that were touched off by my being in the setting — topics that I felt I was able to write about in an interesting way. These topics are collected in the preceding pages. They do not have the relationship to one another that the standard ethnographic format would seem to require. That is, that chapters are sequential and serial and built upon each other, or that one chapter concerns one aspect of the research setting and another chapter deals with some other aspect such that, when the reader has covered all of the material, he will have a complete description of the setting. I have attempted to do neither. Instead I have talked about a few topics that were made available to me by virtue of having been a researcher at the Clinic. I make no apologies for these topics not having the
relatedness required by the standard ethnographic format.

The second claim of the standard ethnographic format is that it contributes to the cumulative knowledge about some culture or particular feature thereof. That is, an ethnographic report is not viewed as just a description of some culture or features of a culture, but also as an integral part of the knowledge now available on a culture or cultural feature. Hence, other researchers wishing to study that culture should read the ethnographic report since it constitutes a component of the corpus of relevant literature on that which is to be examined. Just as certain materials help the nuclear physicist to keep abreast of recent developments in his field, so the ethnographer's report is seen as contributing to a cumulative body of knowledge on some aspect of a culture's organization and should be read by other ethnographers embarking on research in the same area.

My research, however, does not have this cumulative character and thus departs from the standard ethnographic format. First, the topics discussed are not necessarily setting-related topics. By this I mean that anyone else doing an ethnography of a medical clinic or setting need not read this report since it will not necessarily tell them anything they should know in order to conduct their own research. Secondly, I am proposing that what I have done, albeit an analytical account, is through and through a common-sense one which does not propose to have used a scientific method whereby there is some possibility of "cumulative" findings. Rather, this report consists of a series of analytical accounts of various topics the usefulness of which remains to be
discovered. By this I mean that although they are analytical accounts of various topics, I cannot foretell their utility for subsequent readers; and for me to propose that they will be useful would be to propose an incompleteness that could be remedied were some researcher to spend some time and energy pursuing the topics discussed in this report. I reject this contention and regard the materials of the preceding chapters as inherently incomplete. In this respect I find myself in agreement with Blum and McHugh's conception of unfinished work. They state that:

Another important matter is the status of the phrase "unfinished work." That our papers are not finished does not distinguish them from other pieces of sociology. Rather, the difference is that our incompleteness is grounded whereas theirs is treated as happenstance. Consider, for example, one notion of the future of functional analysis. Committed functionalists will often acknowledge that functionalism has flaws. However, these flaws (a kind of incompleteness) are treated as things to be repaired if only... If only functionalists had the wit, the experience, the foresight, or especially the money. The future for these people becomes the time — sooner or later — when the money will accumulate and temporary failures become permanent successes. Failures are nothing to worry about because, by the nature of the case, they are temporary. A theory has defects only because the theorist happens to be situated in time, space, in an economic structure. This relativization to the conditions of failure is a perfect method for managing in a Goffmanesque sense, but it hardly comes to grips with the trouble. By anticipating termination, they think their work will come to have the final character. They are oriented to the possibility of closure and it is in terms of this that they measure success and failure. We say, on the contrary, that our work, because of the nature of analysis, will always be in need of repair.
Throughout this report I have proposed a critique of the standard procedures used in the construction of ethnographic reports. I have offered some alternatives to utilizing materials from research settings. I have been forced to reconsider many of the issues involved in ethnographic description since, over the course of my ethnography, it was evident that it was not adhering to a standard ethnographic format. Thus I regard this report neither as a "perverse" alternative to the standard ethnographic format, nor as a criticism of ethnography. Rather, I have taken the task of producing an ethnographic description seriously and have produced some reflections about the production of such descriptions that may be worth consideration not only by ethnographers, but by other social scientists as well.

\textit{\textit{Whi\textsubscript{s}um, while it might be possible for the reader to assume that this dissertation has been concerned with improving ethnographic field methods and practices, this is not the case. Nor is this dissertation intended as a contribution to the substantive literature on medical sociology even though it has examined materials from a medical setting. Although it has discussed ethnographic field work in some detail, this dissertation is intended to be a contribution to a small but growing literature in anthropology and sociology which treats the doing of field work as the occasion for reflecting upon theoretical issues. These issues have to do with the warranting of knowledge and the possibilities of transcending everyday experience.}}
Footnotes: Chapter Five


Sacks, Harvey, Unpublished Lectures, The University of California at Irvine.


