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## **On The Linguistic Constitution Of Research Practices**

**A thesis submitted to the University of Stirling in partial fulfilment for the Degree of  
Ph.D. in Sociology and Social Policy**

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## **Abstract**

This thesis explores sociologists' routine research activities, including observation, participant observation, interviewing, and transcription. It suggests that the constitutive activities of sociological research methods – writing field-notes, doing looking and categorising, and the endogenous structure of members' ordinary language transactions are suffused with culturally methodic, i.e. ordinary language activities.

"Membership categories" are the ordinary organising practices of description that society-members – including sociologists – routinely use in assembling sense of settings. This thesis addresses the procedural bases of activities which are constituent features of the research: disguising identities of informants, reviewing literature, writing-up research outcomes, and compiling bibliographies. These activities are themselves loci of practical reasoning. Whilst these activities are assemblages of members' cultural methods, they have not been recognised as "research practices" by methodologically ironic sociology.

The thesis presents a series of studies in Membership Categorisation Analysis. Using both sequential and membership categorisational aspects of Conversation Analysis, as well as textual analysis of published research, this thesis examines how members' cultural practices coincide with research practices. Data are derived from a period of participant observation in an organisation, video-recordings of the organisation's work; and interviews following the 1996 bombing in Manchester.

A major, cumulative theme within this thesis is confidentiality – within an organisation, within a research project and within sociology itself. Features of confidentiality are explored through ethnographic observation, textual analysis and Membership Categorisation Analysis. Membership Categorisation Analysis brings seen-but-unnoticed features of confidentiality into relief.

Central to the thesis are the works of Edward Rose, particularly his ethnographic inquiries of Skid Row, and Harvey Sacks, on the cultural logic shared by society-members. Rose and Sacks explicate the visibility and recognition of members' activities to other members, and research activities as linguistic activities.

## **Declaration**

No portion of the work referred to in this thesis has been submitted in support of an application of another degree or qualification of this or any other University or other institute of learning.

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There are many people to whom I owe a debt of gratitude. Whilst they dismiss their contributions as being less-than trivial, I am grateful to them. I do appreciate that some are too modest to be named, and perhaps some may not wish to be associated with the results of their contributions; I apologise in advance for any embarrassment they may experience.

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## Introduction

### ***Doing Sociological Research***

In 1977, Colin Bell and Howard Newby published their edited collection *Doing Sociological Research*. The stated purpose of this volume was to inform readers that the actuality of sociological research differed from “textbook” accounts of sociological research, if textbooks presented such accounts at all. The editors asked contributors to the volume to write accessible essays that described their conducting research rather than the research itself. In concluding a letter to potential contributors, during the editors’ work of assembling a line-up, Bell and Newby remark

“we hope that all these essays will lead to a wider understanding of research that is already published in most cases and will lead to a more realistic appraisal of *Doing Sociological Work.*”

(Bell and Newby 1977:12)

Such “a wider understanding” includes the input of potential publishers of research, as Bell and Newby observe in their Epilogue (*ibid.* 170-173). Financial constraints are imposed by publishers, which may adversely affect the original plans for the book. Whilst Bell and Newby acknowledge the contingencies arising from the financial aspect of publishing sociological research, they also remark upon the legalities of material. In the epilogue they explain that they did not anticipate how libel laws, and the Official Secrets Act, would impinge upon the books’ contents:

“We apologise to our readers; there were and are even better stories to tell than some of these found in this book. In one case we have lost a complete chapter, and

it is not even clear to us that we can safely discuss it or the reasons for its removal.”

(Bell and Newby 1977:172)

In a sense, the epilogue can be seen as more than just an *apologia*. Through the epilogue the editors address themselves to the stated aims of the book, quoted on the first page above. That is, the epilogue informs readers that the list of contributors, and the contents of their contributions, was not “the whole story” of the edited collection. For reasons beyond the control of the editors themselves, the collection is an adumbrated collection, and contains pieces which “are rather anodyne” (*ibid.*).

Necessarily (as well as etymologically), the epilogue is a *post hoc* reconstruction of constraints upon the editors’ work. If the published accounts of research are *post hoc* reconstructions of research, the contributions in Bell and Newby’s collection are reconstructions also. Whilst these accounts contain details omitted from the published versions, they are reconstructed nevertheless. Garfinkel, Lynch and Livingston (1981:138) formulate such reconstructions as being “independent” of the practices of research. That is, the accounts of research in Bell and Newby’s collection, as well as the published accounts of the research to which these accounts refer, are independent of the practices of research. To use Abraham Kaplan’s (1964:3-11) distinction, the “logic-in-use”—the *doing* of the research—is substituted with the idealised, final written-up version or “reconstructed logic” of the research.

Although the editors acknowledge the *post hoc* character of the book's contents (Bell and Newby 1977:12), this does not overcome its limitations—its failure to address how sociological research is linguistically constituted, i.e. linguistic transactions are incarnate within research practices yet are edited out of the final, published reconstruction of the research.

Whilst *Doing Sociological Research* is a useful complement to textbook presentations of the sociological enterprise, its usefulness is bounded by the *linguistic nature* of research. Research is conducted through the medium of language. Researchers and participants in their research share a language that is known-in-common, a “natural language”. Yet language practices are deleteriously missing from *Doing Sociological Research*: it fails to mention that actual *in situ* (logic-in-use) research practices are suffused with language practices. This thesis attempts to redress the omission of the centrality of language, and argues that “doing sociological research” is, *in toto*, an assemblage of members’ sense-making procedures and language practices.

Bell and Newby’s collection complements introductory textbooks in its reliance on *post hoc* reconstructions of *in situ* practice. In their analyses of sociology textbooks, Lynch and Bogen (1997) refer to this tendency as “asociological”. Whilst Lynch and Bogen examine the treatment of science and scientific knowledge in sociology, their arguments are relevant to Bell and Newby’s collection also. Lynch and Bogen argue that the reiteration of an idealised and conventional conception of scientific method is, following

sociological inquiries *apropos* science and scientific methods, without a sociological basis.

Directions in research have shown that it is through natural language practices that members (researchers and non-researchers), for example, give and respond to compliments; give and receive directions; claim and display understanding; interrogate a murder suspect and be so interrogated; and make racial discriminations. It is through natural language practices that researchers write fieldnotes (Farran 1985), and make sense of fieldnotes (G. Watson 1992). In order to conduct their inquiries, researchers write research proposals and apply for research funds; these activities are natural language, i.e. cultural activities. Representatives of funding bodies read researchers' proposals and applications using the self-same natural language practices that researchers employed to write them.

In this thesis, it is shown that research activities and practices are, through and through, linguistic practices, i.e. that research practices are culturally methodic practices, where "culture" refers to common sense knowledge, and where language and culture are mutually embedded. The following section argues that this culturally-methodic character suggests certain practices—researchers' methods—may be considered as "research methods" in their own right, even though they are not considered to be part of the lexicon of methodology. As such, this thesis provides considerations of activities normally regarded as taken-for-granted features of research.

### ***The Linguistic Constitution of Research Practices***

Lynch and Bogen identify the “standard” conception of research implicated in popular textbooks as follows:

“(1) selecting a research problem; (2) reviewing the literature; (3) formulating a hypothesis; (4) choosing a research method and/or specifying a research topic; (5) collecting the data; (6) analysing the results; (7) reporting findings”

(Lynch and Bogen 1997:488)

This thesis attends to such compartmentalising or idealisation of research practices *via* the explication of research practices as culturally based, linguistic activities.

Regardless of theoretical or methodological options elected, the researcher engages in practical activities which are constitutive features of the research project. Although these activities are not considered as being “research methods” in their own right, they involve the analyst in the same natural language activities—*inference-making, membership categorisation and the documentary method of interpretation*—as those contingent, occasioned accomplishments glossed as “research methods” by sociology textbooks. As ethnomethodological studies of situated activities demonstrate, activities are “procedural” in that they involve work—linguistic work—on behalf of members. That is, sociological studies incorporate a “missing what”, which is explicated by studies in the ethnomethodological programme.

The activities or practices of inquiry are *procedural*. Harold Garfinkel clarifies this term for its conceptual and methodological import: “Procedural means labor” (1996:6).

Garfinkel uses this phrase to underline how the focus of ethnomethodological inquiries is upon practical activities, whereby the use of the word “procedural” intends to make visible how activities are members’ productions. That is, activities are accomplished by members in and through natural language.

The *activities* or *practices* of research denote that research projects are assemblages or series of courses of action. Although this thesis does not attempt to explicate such series *in toto*, they may be summarily enumerated (but not completely listed) as, *inter alia*, reading; writing; looking; talking; listening; writing grant proposals and filling-in grant application forms (as mentioned above); highlighting passages in articles and books; marking Post-it notes with references; locating references in the library; ordering inter-library loans; recognising which pages of a book to photocopy;<sup>1</sup> negotiating access to research sites; “meeting and greeting” informants; explaining the aims of the research project; providing assurances of confidentiality; asking questions; writing fieldnotes; transcribing tapes; identifying thematic continuities; cutting-and-pasting transcribed quotes into the body of the text; assigning pseudonyms; recognising or making determinations of relevance; producing a literature review; compiling a bibliography; producing drafts of the research; writing captions for graphs, illustrations and tables; presenting the research; making corrections suggested by colleagues and refereed journals;<sup>2</sup> etc.<sup>3</sup>

These courses of action are worded, i.e. linguistically organised, linguistically constituted activities. This thesis intends to highlight the centrality of language in research, that the

practices of research are linguistically constituted practices, i.e. are worded entities. The centrality of language to our inquiries is attested to by D.R. Watson:

“Language is important to members and analysts alike simply because in one way or another it is the instrument of all social life and because a vast number of the interchanges that make up social life are *linguistic* interchanges. The world we live in is “language saturated” in all kinds of respects and pervasively involves the mastery of ordinary language; we encounter the world in this way. Thus as analysts of social life we are, whether we to acknowledge it or not, always encountering linguistic transactions in our data gathering.”

(D.R. Watson 1992:2; emphasis supplied)

Research activities are coincident with and in many ways part of everyday activities in that they are all suffused with natural language. However, in attending to the linguistic constitution of activities, i.e. that everyday and research activities involve members' ordinary language practices, this thesis demonstrates how some research activities are culturally-methodic activities. Such activities do not fall under the *aegis* of research *methods*, as commonly conceived.

Interviews, life-histories, participant observation, questionnaires, textual analysis—whilst these glosses, among others, may be collected under the collective gloss “research methods”, this thesis shows that researchers are involved in practical activities or *procedures* which are constitutive of the research. Whilst these activities are not glossed as “research instruments”, these activities are routinised, linguistically organised, common-sense practices. Explication of the routine and methodic character of these activities shows how they constitute, and may be formulated as, “research methods”, or research practices, *per se*.

So following on from D.R. Watson (*ibid.*), this thesis does not confine itself to the research activities of collecting and analysing data as natural language enterprises, but explicates the linguistic nature of research activities themselves. The activities of compiling a bibliography, reviewing literature, writing reports, transcribing recorded materials, disguising identities, advancing and criticising arguments are, *in toto*, linguistically constituted activities. These activities are, in Garfinkel's (*ibid.*) sense, procedural, in that they involve culturally-methodic practical work, where culture refers to the use of common sense or procedural knowledge. As natural language activities, research projects incorporate and rely upon such natural-language phenomena as membership categorisation and *recognition* of membership activities—that is, as a member of a common culture the analyst does not need explanation of natural-language activities.

## **Topicalising Research Practices**

The orientation of the thesis may be glossed as the distinction between “topic” and “resource”. Rather than relying upon people’s natural language practices as a resource, this thesis shall take the “linguistic turn” and consider natural language as a topic for study *per se*, and therefore study it *as language*.

This thesis presents a number of *prima facie* discrete topics of inquiry. However, rather than being disjunctive, the coherence and compatibility of these discrete topics is delivered by the “analytic mentality” and methodological unity with which these topics are attended. It shows that these topics or areas of investigation are not separate entities.

The thesis addresses how *prima facie* research methods are constitutive of people’s common-sense methods. In its approach to analytic methods as assemblages of common-sense methods, by “topicalising” the mundane features of research methodologies, i.e. delivering the procedural knowledge involved in researching social phenomena, this thesis takes what may be called a “methodological turn”.

Although the thesis may appear to be of a broad “scope”, its focus remains narrow. It shows how ordinary methods or linguistic activities are used as analytic methods, and how these ordinary methods are used in research practices outlined in the previous section. In this sense a task the thesis sets itself is one of “textual analysis”, and the delivery of ordinary-logical entailments that are implicated in sociological argumentation.

One of these implications or logical entailments is methodological irony. Methodological ironies occur when the analyst traduces members' orientations, *via* the use and imposition of language games and theoretical devices, which are competitive with members' natural-attitude orientations. As members of the culture, analysts rely and use the self-same linguistic and interpretive practices that "lay" members use—hence Garfinkel's notions of "professional sociologists" and "practical sociologists" are particularly apposite. Recognising the use of membership practices as unexplicated features of sociological enterprises, the analytic task is not to ironicise or traduce members' interpretations but to explicate how interpretations are produced.

This thesis provides readers with examples of methodologically ironic devices in use. The methodologically ironic projects of sociology are contrasted with the non-ironic, i.e. explicative character of some Symbolic Interactionist and ethnomethodological programmes. Methodological irony is avoided through the procedural turns taken by this thesis.

For the purposes of pursuing explicative forms of sociology, I shall discuss the importance of one of the early figures in the reactions against traditional sociology, Edward Rose. At present there is a resurgence of interest in Rose's work and his approach to the investigation of social life. I shall introduce valuable biographic and bibliographic considerations of Rose, where biographical materials are reference points

for bibliographic materials. I shall show how his unique approach to the social world, known as the “Ethno-Inquiries”, is suited to the topics at hand.

I shall also show how ethnomethodological programmes, including the Ethno-Inquiries, provide a focus for the procedural turns taken by the thesis. The gain of this focus is the concerted study of salient features of everyday life as relevant and meaningful to people themselves. For example, the Ethno-Inquiries facilitate the study of members’ categorisation and sequencing activities without privileging one at the expense of the other (D.R. Watson 1997a). The Ethno-Inquiries are concerned with linguistic features of everyday encounters.

The practices of research are amenable to Membership Categorisation Analysis, and the use of natural language practices by sociology incorporates membership categorisations. This is the primordial focus of the thesis, whereby the project looks at how aspects of research procedures are imbued with categorisations and other natural language practices. In this way, the thesis demonstrates cognate concerns *via* discrete chapters on aspects of confidentiality and research methods, which show how a research practice was a culturally methodic activity *via* its use of membership categories. Categorisation activities are, then, axial themes of the thesis.

This thesis includes a discussion of an organisation based upon a period of “participant observation”. The insights provided by this “ethnographic” part of the project drew attention to the use of documents by people in an organisation, and the nature of

confidentiality—how confidentiality was a feature of the organisation, how it was orientated to by members of the organisation, how information e.g. documents came to be defined as “confidential”. (These issues are expanded upon in the following section.)

This thesis also includes discussions of “parenting” sessions inside a young offender institution (YOI). *It is coincidental* that the parenting sessions are organised and conducted by the organisation discussed above. That the corpus of data from the parenting sessions and the ethnographic observations were obtained from the same organisation was purely a matter of happenstance: there are no analytic reasons why this should be so.

Further, the treatment of these materials is different. The ethnographic observations are not submitted as “ethnographic background” to the conversational materials from the parenting sessions (Watson and Sharrock 1991a); nor are these sections designed nor implied to be “mutually elaborative”. The ethnographic observations derive from an entirely separate enterprise within the same organisation. This is not to be taken as a collection of methods for the study of a collection of aspects of the same organisation, in some endeavour to reach an overarching view of that organisation.

### ***Confidentiality: Cumulative Considerations***

Further original features of this thesis are its discussions on confidentiality, which are located throughout the text; e.g. how issues of confidentiality are addressed in sociology, and how issues of confidentiality remain unexplicated features of the sociological

enterprise. So instead of accepting confidentiality as a feature or a resource to be used as part of the inquiries, confidentiality is treated as a topic for inquiry in and of itself. Matters of confidentiality are explored in relation to research practices throughout the thesis, including recording and transcription, observation and interviewing, and reporting. Confidentiality is thereby a leitmotiv within the thesis.

The explication of members' common-sense devices to maintain confidentiality brings confidentiality into the purview of sociology. In so doing it provides a conceptual basis for ethnographic study of confidentiality in organisations.

The thesis presents ethnographic observations made in an environment which was governed by confidentiality, where confidentiality was a "constituent feature" of the settings. It is observable that confidentiality has consequences for research and consequences for participants. The conceptual framework for the study of confidentiality developed therein facilitates the study of the invocation of confidentiality, asymmetries of rights to invoke confidentiality, the production of confidential materials, and the use of confidential materials in research.

The recognition of confidentiality as a product of members' methods enables Chapter Four to make a preliminary investigation of confidential research and its difficulties: the sociologist must keep information confidential, but the research site in turn withholds confidential information from the sociologist.

Chapter Four also examines occasions of information disclosures by members. Organisations are collections of members, so the study of individual member's methods of disclosing and withholding information is necessary for an examination of organisations and confidentiality. Organisations cannot disclose or withhold confidential information; that is a member's accomplishment. Organisational activities are, therefore, members' activities.

This thesis examines the accepted practice in sociology of maintaining confidentiality by providing anonymity to informants; and the consequences for research of such procedures. These consequences include obscuring phenomena for analysis and changing phenomena available for analysis.

Although confidentiality is treated as a topic of inquiry rather than an expected adherence to standard or ethical practice, all identities and identifying details have been changed. The work presented in this thesis adheres to the Statement of Ethical Practice (updated and approved 1996) set out by the British Sociological Association.

This thesis begins by introducing the methodological and conceptual bases which are used in the thesis. These introductory notes are not exhaustive expositions of these research programmes—such enterprises are entire doctoral dissertations in their own right. This initial chapter brings together a number of cognate approaches which share an explicative commitment to members' situated practices, *viz.* Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis, under the heading

“Ethnomethodological Programmes”. Whilst these research programmes are not isomorphic, they are logically compatible with one another. A further cognate approach, Ethno-inquiry, is treated in Chapter Two as part of an intellectual profile of Edward Rose.

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<sup>1</sup> Whilst standing in a queue for the Applied Social Science department’s photocopier, which is situated in 4Y2a—a small, uncomfortably hot, stuffy room at the end of a corridor in the Cottrell Building, University of Stirling, a colleague turned to me and said, “this is all part of the process.” What did she mean: photocopying, or queuing?

<sup>2</sup> D.R. Watson (1997:80) provides an avowedly incomplete list of quotidian textual, linguistically organised materials.

<sup>3</sup> This brief list can be extended indefinitely, and the constituent features of each item within the list can be explicated further (Sacks 1963; Sharrock and Turner 1980).

## **Approaches to the Study of Linguistic Activities: Ethnomethodological Programmes**

In this opening chapter I intend to provide readers with resources with which to approach this thesis on the linguistic constitution of research practices. The considerations contained herein are not exhaustive but introduce the conceptual “underpinnings” of the thesis. The thesis uses conceptual and methodological frameworks derived from Ethnomethodology, Conversation Analysis, Membership Categorisation Analysis, and Ethno-Inquiry. Ethno-Inquiry is treated in Chapter Two, as part of an intellectual profile of the founder of the Ethno-Inquiries, Edward Rose.

This chapter provides, then, some source material on Ethnomethodology, Conversation Analysis, and Membership Categorisation Analysis. Beyond my disavowals of providing comprehensive treatments, it should also be noted at this opening juncture that explications of Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis are continued throughout the thesis. That is, examples and discussions of these methodological approaches are embedded within the text, including illustrative notes.

The title of this chapter, which provides orientation on the conceptual and methodological approaches informing this inquiry, is contestable; perhaps not everyone would regard “Ethnomethodological Programmes” an appropriate title with which to gloss Ethnomethodology, Conversation Analysis and Membership Categorisation

Analysis. However, I do not attempt to consubstantiate or unify Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis in this chapter. Instead, this chapter brings together the related approaches of Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis under the heading “ethnomethodological programmes” because they share common or overlapping concerns. Among these shared concerns are the commitment to worldly inquiry and explication of social order “from within”, i.e. as it is recognisable to members. That is, they are explicative, i.e. methodologically non-ironic approaches. As explicative approaches they work to preserve the “phenomenological intactness” of settings and settings’ phenomena (D.R. Watson 1992:9).

This chapter discusses the conceptual framework of the thesis. This chapter does not compartmentalise approaches, but is concerned with the commonalities and overlaps (rather than the disciplinary boundaries) of Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis.

This form of presentation has important analytic gains: it does not attempt to “bifurcate” Ethnomethodology and Conversation Analysis—contrary to moves made elsewhere, Conversation Analysis in this thesis remains an ethnomethodological programme. (In Chapter Thirteen on the linguistic constitution of bibliographies and literature surveys, it is shown how the ethnomethodological foundations of Conversation Analysis may be downplayed and even edited out.) Furthermore, within this chapter and the thesis *in toto*, “Membership Categorisation Analysis” is not de-

emphasised, nor even a proscribed stream of Conversation Analysis. These arguments are pursued in detail throughout the thesis.

Hence, this chapter should not be taken as the beginning-and-end of conceptual explication. In its provision of background information about the ethnomethodological approach, this chapter recommends materials which inform and orientate readers both to ethnomethodological studies and the forthcoming analyses within the thesis.<sup>1</sup>

### ***Ethnomethodology***

Ethnomethodology developed as a reaction to the stipulative approach to the social world featured in Talcott Parson's normative functionalism. Harold Garfinkel, one of Parsons' graduate students, became increasingly dissatisfied with his supervisor's theoretical framework, which presented members of society as "cultural dopes" (Garfinkel 1967:68), who were governed by societal constraints.

In attempting so account for members' understandings using Parsons' framework, Garfinkel found the Parsonian system occluded members' definitions. According to Garfinkel, there was no room in Parsons' conception of society for members' own understandings of rules, and if sociology *was* to account for members' understandings, it would have to start from different epistemological principles. For Garfinkel, members were not governed by rules, as Parsons' system required them to be, but active users of rules. In a series of studies, Garfinkel shows how—*contra* Parsons—members use rules ongoingly, for "practical purposes", and in occasioned and contingent ways.

Garfinkel's re-conceptualisation of Parsonian sociology is located within his attempt to situate members' use of rules as proper topics of sociological inquiry. The orientation to order was not a search for causal or structural rules which preserved a stable, social order. Instead, Garfinkel's research suggests the study of how members produce social order *in vivo*, through the production of visible and recognisable activities, i.e. such-and-such activities which other members recognise as such-and-such activities. That is, social order is produced as a collaborative achievement by members *via* courses of action. The radical move away from theoretical idealisations of social order, is formulated thus:

“The topic then would consist not in the social order as ordinarily conceived, but rather in the ways in which members assemble particular scenes so as to provide for one another evidences of a social order as-ordinarily-conceived.”

(Zimmerman and Pollner 1971:83)

Garfinkel emphasises that members of a culture (the use of the term “member” will be outlined later in the thesis) are competent users of a language known in common with other members of the culture. This language is referred to as “natural language”, or “ordinary language”. It is through using natural language that members' activities get done, and get done in ways which are recognisable to other natural-language users, i.e. other members. Natural language is not a technical language, e.g. the vocabulary of sociology; yet technical languages are based upon and presuppose competence in the use of natural language.

A feature of natural language is “indexicality”. This refers to the meaning of activities, where the meaning of an activity is contingent upon the context of its occurrence. Members understand actions, such as the raising of an arm, in different ways according to context. For example, an auctioneer in an auction might recognise a raised arm as a bid; a teacher in a classroom might recognise a raised arm as indicating that a pupil knows the answer; a politician at a press conference might recognise a raised arm as a journalist’s claim to ask the next question; etc. A raised arm is, then, indexical (or an “indexical expression”) in that members make sense of the action through reference to the context of its production. In different contexts, i.e. in different settings with different parties to the settings, the hypothetical examples above may recognise a raised arm as a greeting, or part of a mime, or a salute.

In a foundational paper, Zimmerman and Pollner (1971) criticise sociological inquiries which trade upon researchers’ use of natural language, i.e. where natural language is treated as a *resource for inquiry*. Relying upon natural language practices as resources for study incorporates an unanalysed dimension into the research. The result of this incorporation is that the claims of sociological studies are misleading: whilst a researcher may claim that their reports are purportedly “about” a particular substantive topic, the unexplicated use of natural language practices produces studies which are assemblages of members’ common-sense, practical activities.

Rather than taking natural language practices as a resource for study, Zimmerman and Pollner argue that the Ethnomethodological programme takes members’ practices—the

usage of natural language and features of natural language, e.g. indexical expressions—as *topics of inquiry* in their own right.

Ethnomethodology challenges forms of social inquiry which tacitly incorporate members' natural language activities. In an early paper, Garfinkel (1949) encountered the problem of choosing between baseline statistics, *viz.* a discrepancy between the numbers of murders and numbers of defendants. Garfinkel formulated this as “the problem of determining which sentence to use to represent the case” (1949:369). Garfinkel's paper on murder charges reveals the considerable latitude involved in the assembling of data which is “good enough” data for sociological research. Furthermore, it highlights the degree of practical work which is done by members in producing statistics *as* statistics, e.g. indicting a Black for 1° murder following the death of a White; indicting a Black for 2° murder or manslaughter following the death of a Black. That is, sociologists should avoid taking statistics on face value, as the statistics themselves are linguistically constituted: statistics are outcomes of and incorporate members' practical, natural language activities. (This early ethnometodological paper will be returned to in Chapter Four on the linguistic organisation of fieldwork activities.)

Garfinkel identified members' use of a family of natural language practices for assembling the sense of indexical expressions. He referred to this recurrent practice as the “Documentary Method of Interpretation” (Garfinkel 1967). In a recent paper, Garfinkel (1996:18) criticised the use of the Documentary Method of Interpretation,

which he regards as too formal to capture the “haecceities” or uniquely-identifying details of social settings. Garfinkel does not regard the Documentary Method of Interpretation to be adequate for the study of the “just-thisness” of individual, local settings. In this thesis, on the linguistic constitution of research projects, I use the conceptual framework of the Documentary Method of Interpretation in explicating how members, i.e. lay society-members and sociological researchers, assemble sense *via* a historico-prospective orientation to situated events. Garfinkel formulates the Documentary Method of Interpretation as follows:

“The [Documentary Method of Interpretation] consists of treating an actual appearance as ‘the document of’, as ‘pointing to’, as ‘standing on behalf of’ a pre-supposed underlying pattern. Not only is the underlying pattern derived from its individual documentary evidences, but the individual documentary evidences, in their turn are interpreted on the basis of ‘what is known’ about the underlying pattern. Each is used to elaborate the other.”

(Garfinkel 1967:78)

The Documentary Method of Interpretation is a flexible, culturally-shared family of linguistic practices, which provides for the incorporation of further indexical particulars which further elaborate the underlying pattern; or, if necessary, revise the underlying pattern. For example, on first being introduced to a person in a group, that person’s actions may be defined as “rudeness” (an underlying pattern); their actions (indexical particulars) such as avoiding eye-contact, not participating in the group’s conversation, etc., may elaborate the “rudeness” underlying pattern. Such an underlying pattern is subject to revision if one of the group-members reveals that the person is extremely shy.

This new detail, a different underlying pattern, recasts other particulars and existing particulars according to the new underlying pattern.

The Documentary Method of Interpretation is used routinely and ongoingly by members, in making sense of activities and settings. The Documentary Method of Interpretation is used in organisational activities and research activities. I realised my own use of the Documentary Method of Interpretation during a period of participant observation in an organisation, which is reported in Chapter Four. (To repeat, then, explications of notions provided in this chapter are elaborated upon in later chapters of the thesis.)

The explication of natural language practices required methodological principles or study policies which did not distance the research from phenomena of study. A strict research policy to capture the phenomenon of study was formulated by Garfinkel and Sacks:

“Ethnomethodological studies of formal structures are directed to the study of [natural language] phenomena, seeking to describe members’ accounts of formal structures wherever and by whomever they are done, while abstaining from all judgments of their adequacy, value, importance, necessity, practicality, success, or consequentiality. We refer to this procedural policy as “ethnomethodological indifference.”

(Garfinkel and Sacks 1970:345)

“Ethnomethodological indifference” is a methodological procedure involving a phenomenological stance towards the topic of inquiry. Ethnomethodological indifference is an analytic practice for ensuring and maintaining an explicative

orientation to phenomena—as opposed to a “methodologically ironic” position which treats people as “judgmental dopes” (Garfinkel 1967).

Ethnomethodological indifference can be difficult to sustain, and the claims of some ethnomethodological studies have attracted criticism on this issue: Smith (1978) was accused of abandoning ethnomethodological indifference by Cuff (1980); Lynch argues that Goode’s “passionate advocacy” (Lynch 1997:371) of disabled children does not enforce the principle of ethnomethodological indifference, which applies “to the whole of practical sociological reasoning” (Garfinkel and Sacks, in Lynch) equally. Ethnomethodological indifference is not a matter for analytic discretion, for use in some settings or practices but not for others. As Lynch goes on to say,

“Far from being an effort to set up a disinterested social science, the policy assigns epistemic privilege to no single version of social affairs, including sociology’s own professionally authorised versions. The policy of “indifference” should be understood not as a principle that sets up a purified vantage point but as a maxim that encourages a unique way of investigating how social order is constituted.”

(Lynch 1997:371-372)

The attitude towards interactional phenomena taken by ethnomethodology, such as ethnomethodological indifference, works to exclude extraneous or theoretically motivated commitments being ushered into the analysis.

Ethnomethodological indifference, as an analytic practice, is also maintained by practitioners of a cognate discipline, Conversation Analysis. The next section outlines some of the study policies of Conversation Analysis, i.e. its “analytic mentality”, and

the apparatus of early Conversation Analysis which I shall use throughout this thesis on the linguistic constitution of activities.

### ***Conversation Analysis***

The focal aim of this thesis is to elucidate the linguistic nature of research activities. Since research activities involve the inscription as well as iteration of natural language practices, this thesis brings the “apparatus” of conversation-analytic inquiries to conversational and non-conversational settings, e.g. texts. This section of the chapter provides brief explication of Conversation Analysis required for a reading of the thesis. As such, this section is not an exhaustive treatment of Conversation Analysis, but an overview of key methodological options relevant to the project at hand.

Conversation Analysis is a field of inquiry derived from ethnomethodology. Conversation Analysis is a sociological, empirical approach to the study of social order in interactional settings. The orderliness of interaction is a member’s accomplishment, wherein social order is produced by participants themselves. This production of social order within settings is a collaborative production *via* ordinary talk. The sociological nature of Conversation Analysis is appreciable in its examination of utterances in the context of their production, rather than an analysis of the structure of language *per se*.

### **The “Analytic Mentality” of Conversation Analysis**

James Norton Schenkein (1978) outlines the methodological and thematic unity of early conversation-analytic studies under the *aegis* of the term “analytic mentality”. Schenkein uses the term as a “placeholder” for the methodological entailments of

Conversation Analysis. The analytic mentality of Conversation Analysis is a “family” of methodological principles which, through their application, produce analyses that preserve the interaction-as-it-happened and that are warranted by available data. In his edited collection of conversation-analytic studies, Schenkein says

“The materials under study were not elicited, remembered, or invented to provide illustration for some analytic design, to exercise some research apparatus, or to examine some prefigured hypotheses. All the materials [...] are drawn from actual interactions occurring in their natural environments”

(Schenkein 1978:2)

This summarises the conversation-analytic approach outlined in the next section, where I shall enumerate Sacks’ methodological reasons for studying conversation. Schenkein’s placeholder “analytic mentality” gathers together the features of early conversation-analytic studies so that Conversation Analysis, as a methodological programme, has an identifiable and distinctive approach to the study of the social world and social order. This *modus operandi* involves the study of naturally occurring interactional phenomena, rather than research-oriented solicitation of materials for study; the recording of settings for repeatable, real-time analysis; an indifferent attitude towards the phenomena of the setting, rather than making theoretical or moral judgements *apropos* the setting and/or setting-members; the avoidance of *a priori* assumptions about the nature of interaction, i.e. a “nonintuitive” methodology which does not rely on researchers’ remembrances, nor researchers’ speculations, of interactional practices. Hence, an explicative, rigorous and unmotivated analysis of the *in situ* details of practical actions. The explicative or methodologically non-ironic

nature of early conversation-analytic studies ensures that the analyst does not substitute or traduce members' conceptions with competitive definitions.

### Social Order and Social Organisation

Sacks' approach to the problem of order was to examine the achieved orderliness of ordinary, naturally occurring settings. For Sacks, ordinary interactional settings were ideal sites for the analysis of social order or talk-in-interaction because they were i.) natural occurrences rather than laboratory or contrived experiments; ii.) the phenomena of order manifest in interactional settings could be preserved using tape-recordings of conversations; iii.) tape-recordings and subsequent transcriptions could be repeatedly analysed and inspected by others; iv.) the use of tape-recorders did not remove phenomena of order from the context of their production. It is important to note that transcriptions of recordings are not substitutes for the recordings themselves but as-adequate-as-possible textual renderings of the recordings for the practical purposes of observation/analysis, data sharing and publication. (Recording and transcription receive analytic attention in a later chapter.)

Conversation was an expedient site of social order for analysis. This approach was a methodological principle rather than a feature of the analysis of talk. In later chapters, this thesis goes on to discuss a paper by Sacks (1972), which was written before the development of Conversation Analysis as a separate field of sociological investigation. In producing this paper, Sacks used documents, e.g. police training manuals, rather than recordings of conversation. In this paper Sacks remarks that he is "only interested in

the police instantially” (Sacks 1972:282). In a similar vein, in one of his lectures, Sacks outlines how conversation is a locus of inquiry:

“If you’re going to have a science of social life, then, like all other sciences of something or other, it should be able to handle the details of something that actually happens. It should be able to do that in an abstract way, while handling actual details. My research is about conversation only in this incidental way, that conversation is something that we can get the actual happenings of on tape and transcribe them more or less, and therefore that’s something to begin with. If you can’t deal with the actual details of actual events then you can’t have a science of social life.”

(Sacks 1992b:26)

The instantial nature of conversation as a locus for research is evident in research on non-conversational settings. Ryave and Schenkein (1974) use video-recordings of a stretch of pavement, which capture naturally occurring walking activities, rather than recordings of naturally occurring conversational activities. Using “retrievable” data, Ryave and Schenkein look at the production of social order by people walking through a public space. Ryave and Schenkein treat the movement of members through public space as an extraordinary series of activities; in this way they are able to explicate the act of walking as a routinised, coordinated accomplishment of members *in situ*. Ryave and Schenkein observed that members oriented to various procedural rules when walking through public space. For example, members are able to do *produce* “walking-alone” and “walking-together” as “vehicular units”, and members *recognise* these “vehicular units” as persons “walking-alone” or “walking-together” (see below).

The analysis of conversation or talk-in-interaction regards social settings to be ordered and organised by members *ab intra*. Observing the orderliness of settings is not an

analyst's achievement but a conjoint achievement of members themselves, who ongoingly display and monitor the activities of each other within the setting. That is, order is exhibited, maintained and recognised by members; members' recognition of the routine orderliness of social settings is prior to the analyst's study of the setting. In other words, the setting is socially organised before the sociologist arrives at the scene.

The location of social order in conversation-interaction is formulated in an early research report by Sacks, Schegloff and Jefferson (1974). Their "simplest systematics" paper highlights the coordinated, collaborative production of social order *via* a system organised by and oriented to speakers themselves, *viz.* the "turn-taking" system of conversation. The turn-taking system is derived from the "grossly observable facts" of conversation; that is, the characteristic features of turn-taking in conversation which anyone would notice through fine-detailed observation of conversation.

The grossly observable facts of turn-taking and speaker change include the empirically warranted observations that (1) "Speaker change recurs, or, at least, occurs" and (2) "Overwhelmingly, one party talks at a time" (Sacks, Schegloff and Jefferson 1974:700). As I suggested above, these grossly observable facts are produced by members themselves rather than stipulated by analytic *fiat*. The orderliness of conversation is a collaborative accomplishment of the conversationalists themselves; the order that is grossly observable within conversation is a sequential order.

The notion of “turns” in turn-taking is, as I suggested at the beginning of this section on Conversation Analysis, a sociological notion. Rather than analysing the linguistic structure of conversations by breaking conversation down into sentential units, Sacks, Schegloff and Jefferson look at the co-ordination of linguistic activities on an utterance by utterance (or turn by turn) basis; that is, by regarding utterances as the basic units of interaction.

The “simplest systematics” provides a model for the production of social order *via* members’ administration of and orientation to conversational “rules”. These rules are glossed by Sacks, Schegloff and Jefferson as the “turn-constructional component” and the “turn-allocational component”. Turn-constructional components, e.g. utterances and sentences-in-progress, are used by members to project or anticipate the next conversational moment at which they may take a turn at talking. These moments are referred to as “transition-relevance places” (*ibid.* 703). The turn-allocational component is comprised by a “recursive rule-set”; this set of rules is used *in situ* by interlocutors in determining the taking of turns. The allocation of turns is accomplished *via* the selection of the next speaker by the current speaker, or *via* self-selection at the next transition-relevance place. If neither of these options are realised, the current speaker may, or may not, continue to take turns at talking.

Whilst Sacks *et al.* look at the local organisation of turn-taking and speaker change, they argue that the “simplest systematics” model of turn-taking is both “context-free” and “context-sensitive” (*ibid.* 699). These observations are not disjunctive with each

other, however. The “simplest systematics” model of conversation explicates how social order is produced by members within conversation; this is grossly observable despite differences in the numbers of parties to talk, where the conversation takes place, what the parties are talking about, who with (e.g. strangers or intimates), etc. That is, the orientation to and administration of the “simplest systematics” model of conversation is not dependent upon the contextual details of the conversation itself, i.e. it is context-free. The “simplest systematics” model of conversation is also context-sensitive, in that it accommodates changes occasioned by the parties to the setting, e.g. arrivals and departures of persons, changes in topic, or “extraneous” circumstances (such as a knock at the door, or the ringing of a telephone, etc.). Sacks, Schegloff and Jefferson emphasise how their “simplest systematics” model is ongoingly used and oriented to by members, as opposed to a theoretically-imposed organising device, in describing their model as a “local management system” (Sacks, Schegloff and Jefferson 1974:725), i.e. the order and organisation of turns is a collaborative production of the parties to the setting; which is “administered” by the parties to the setting.

Sacks, Schegloff and Jefferson discuss the “particularisation” of talk in terms of “recipient design” (1974:727). By this term they

“refer to a multitude of respects in which the talk by a party in a conversation is constructed or designed in ways which display an orientation and sensitivity to the particular other(s) who are the co-participants.”

(Sacks, Schegloff and Jefferson 1974:727)

Recipient design relates to the context-sensitive character of the “simplest systematics” model. Conversationalists design their talk for their interlocutors by attuning their conversation according to the context of the conversation, and any ordinary identifications, inferences and assumptions they make about their interlocutors. This may be shown in Extract 10 (1) below:<sup>2</sup>

Extract 10 (1)

- |     |     |  |
|-----|-----|--|
| 1   | CR1 | when when you due out                  |
| 2   | CD  | ((laughs))                             |
| 3   |     | I would have got out this month        |
| 4   |     | but now I'm gonna get out in September |
| → 5 | CR1 | did you have an R C                    |
| 6   | CD  | pardon                                 |
| 7   | CR1 | did you have an R C                    |
| 8   | CD  | an R C what's that                     |
| 9   | CR1 | an                                     |
| 10  |     | ((pause))                              |
| 11  |     | you know the review                    |
| 12  | CD  | parole                                 |
| 13  | CR1 | yeh                                    |
| 14  | CD  | last year September last year          |

In this extract, CR1 assumes that CD, as a person incarcerated inside a young offender institution, would recognise the use of the abbreviation “RC” (arrowed utterance, line 5). Her use of this abbreviation occasions a perturbation, whereby CD asks CR1 to repeat the utterance (line 6). CR1 repeats the question, “Did you have an RC?” (line 7), without any expansion or elaboration—CR1 repeats the question assuming that CD possesses knowledge *apropos* the meaning of the abbreviation. In the next turn, CD asks CR1 for clarification of the term (line 8). CR1 begins to reply but hesitates (lines

9-10), and designs the clarification for an interlocutor evidently not aware of the meaning of the term “RC”.

The design of talk for recipients is observable through the reference terms that people select. The selection of reference terms is a feature of the presumption of interlocutors’ relationship to and knowledge of other members. (This feature is illustrated in the later chapter looking at the interactional nature of interviews.) If a speaker presumes that their interlocutor knows a particular person, they select a reference term which, according to this presumption, their interlocutor will recognise. The “preference” operating for the selection of reference terms is that of minimisation: when the current speaker uses “recognitional” reference terms, the preference for minimisation is gradually relaxed, i.e. reference terms are elaborated, until recognition is established. This is shown in the following extract, as the caller (A) uses minimal reference terms according the presumed state of knowledge the answerer (B) possesses. As the reference terms or “try-markers” are displayed as inadequate reference terms to establish recognition—such a display is produced in a next turn (line 4), and the display of recognition is not produced after an elaboration of the subsequent try-marker (line 5)—the caller A concatenates a series of increasingly elaborated reference terms in order to locate recognition:

[Sacks and Schegloff]

1. A: Hello?
2. B: 'Lo,
3. B: Is Shorty there,
4. A: Ooo jest- Who?
5. B: Eddy?

6. B:           Wood <sub>f</sub> ward?  
 7. A:           Oo jesta minnit.  
 8.               (1.5)  
 9. A:           Its fer you dear.  
 (Sacks and Schegloff 1979:20)

The sequential organisation of utterances in conversation is oriented to by interlocutors in an ongoing manner. This is because the production of an utterance constituted an environment or context for a subsequent utterance. This was formalised into the notions of “adjacency pairing” and “conditional relevance”.

Schegloff and Sacks identified a recurrent structure of sequencing in conversation as “utterance pairs” or “adjacency pairs” (1974:238 *et seq.*). They noted that for members of our culture, certain utterances occasion particular contextual environments, e.g. greetings and good-bye’s, and such utterances are regarded as “paired” utterances. That is, they are identifiable as “first pair parts” and “second pair parts”, and that these pair parts are sequentially organised; i.e. they are adjacently positioned. The sequential-organisational structure of adjacency pairs was formulated thus:

“(1) two utterance length, (2) adjacent positioning of component utterances, (3) different speakers producing each utterance.”  
 (Schegloff and Sacks 1974:238)

Recognisable examples of adjacency pairs include greetings—the first pair part is followed by a second pair part, an acknowledgment of the first pair part; questions and answers—answers recognisably follow questions as the next action in a sequence. The recognisability of activities, e.g. questioning and answering, highlights the ongoing

analysis and monitoring of turns by parties to talk. This monitoring is displayed in the next turn or adjacently positioned utterance; if a question is followed in the next turn by an utterance which is not an answer to the question, the producer of the question may infer that their interlocutor was not listening, or is deliberately not answering the question.

The aspect of recognisability is the basis of the notion “conditional relevance”. That is, utterances are recognised to be “tied” together, that a greeting is recognisably followed by a greeting-return; in the above example, a question is followed by an answer. Schegloff provides a working definition of the notion:

“By conditional relevance of one item on another we mean: given the first, the second is expectable; upon its occurrence it can be seen to be a second item to the first; upon its non-occurrence it can be seen to be officially absent – all this provided by the occurrence of the first item.”

(Schegloff 1972a:388-389)

So, from the notion of conditional relevance—where the occurrence of one “type” of utterance occasions another utterance of the same “type”—Schegloff is able to show the sociological import of the turn-taking system. The first utterance determines the position of the relevant utterance in the sequence; that is, it happens in the next turn. For example, when greeting somebody, not only does the greeting oblige and require a return, but relevantly that return should happen next. Conversational actions have, therefore, implications for future conversational actions.

That utterances are tied together in sequence—that the production of a particular utterance requires (or is conditionally relevant upon) the production of another—makes visible the normative order or expectation to speak at a specific point. That conversation has a normative structure is demonstrated by such tied utterances as saying ‘Hello’. Not returning a greeting when a greeting is “supposed” to be returned, *viz.* in next action, opens up to members a range of culturally-based inferential possibilities as to why the greeting has not been returned. This shows how utterances are coordinated, and how the coordination is normative.

If a conversational action is expected to happen by members but does not happen at its relevant position in the sequence, that conversational action is “noticeably” or “officially absent” (Schegloff *ibid.*) A noticeable absence requires explanation, and warrants potentially or justifiably negative inferences to be made. When you say ‘Hello’ or wave to a friend in greeting you may reasonably expect them to say ‘Hello’ or wave back in return. There may be “legitimate” reasons why your friend doesn’t return your greeting, e.g. not seeing you, etc.; and the non-occurrence of a required next action may become a topic of conversation—‘Why didn’t you wave back?’ ‘I didn’t see you,’ etc. Such reasons supplied by your friend may be reasonable reasons if you know that your friend requires spectacles yet habitually chooses not to wear them. However, you may infer that your friend’s failure to acknowledge your greeting was a deliberate action. In so doing your friend’s silence or failure to respond to your greeting becomes a form of insult, indicative that they are “not talking to you”.

If your friend meets your greeting with silence, your friend “owns” that silence. “Owning” silence goes to show the inseparability of membership categories from sequences of talk (and *vice versa*). The “ownership” of silence is generated by the turn-taking system. When a silence follows a speaker’s turn—where it is relevant for the auditor to take the turn—this confers that the silence is properly “owned” by the auditor; that is, silence is “someone’s silence” (Schegloff and Sacks 1974:236).

The notion of “noticeable absence” is not a theoretically driven concept. Members themselves know that certain inferences are available from noticeably absent utterances. Members may use this inferentially-available feature of noticeable absences as a resource in the production of accounts to others.<sup>3</sup>

Theorising (talking *about*) e.g. silence is not a substitute for observing how silence is actually treated by members of a culture in its turn-organised and categorial context. The significance of silence—ownership and the inferential possibilities of ownership—can only be derived from the context of its occurrence. To continue with the example of greeting-return, consider the following transcript:

WOMAN	Hi.
BOY	Hi.
WOMAN	Hi, Annie.
MOTHER	Annie, don’t you hear someone say hello to you?
WOMAN	Oh, that’s okay, she smiled hello.
MOTHER	You know you’re supposed to greet someone, don’t you?
ANNIE	[Hangs head] Hello.
	(Sacks 1974a:227)

In this sequence of talk, Annie fails to say hello at the appropriate, relevant moment; that is, she ‘owns’ the silence. After Annie receives a light rebuke from her mother, who moots a possible reason why Annie had remained silent on two appropriate moments, she still fails to greet the woman. Although the woman excuses her, Annie’s mother upgrades the rebuke (‘You know you’re supposed to ...’) following three failed opportunities to return the woman’s greeting. Admonished, Annie finally says hello.

The sequential ordering of conversation, and in particular the adjacency pairing of utterances, enables the notion of noticeable absences to be used as analytic tool which remains sensitive to members’ definitions of the situation. However, it is also a site of overlap between the sequential order of conversation and the membership categorisational (or categorial) order of conversation. That is, the “attributability” of silence brings not only the turn-taking system of conversation into relief (Sacks and Schegloff 1974:237), but the mutual elaboration of turn-taking or sequencing and activities of membership categorisation.

### ***Membership Categorisation Analysis***

Membership categories are ordinary, “everyday” descriptions and identifications of persons and collections of persons, which are used and applied by members on a commonplace and routine basis, in order to organise the social world in which they live. The use of membership categories is culturally methodic, i.e. membership categories are known and shared within a culture; that is, membership categories are features of the use of natural language. They are constituent features of ordinary language

practices, i.e. culture. Membership Categorisation Analysis (MCA) is an attitude towards and explication of this aspect of people's 'cultural logic'.

In a foundational paper, Sacks (1974a) formulated the bases of Membership Categorisation Analysis. Sacks' aim was to explicate and formalise the practical work of description and recognition:

"What one ought to seek to build is an apparatus which will provide for how it is that any activities, which members do in such a way as to be recognisable as such to members, are done, and done recognisably."

(Sacks 1974a:218)

Sacks used the following child's story as data in his disquisition's on membership categories:

"The baby cried. The mommy picked it up."

(Sacks 1974a:216)

Sacks made a series of observations upon these two sentences, which constituted a child's story. First, that he hears these sentences as describing that the baby was picked up by its own mother, rather than "any" mother. Second, members of a culture hear these sentences in the same way that Sacks himself does, i.e. it is not an analytic re-description of an event but a common sense version. Third, he hears these sentences as a description of activities which follow from each other; i.e. that the baby was picked up after it had started to cry. Fourth, that the baby was picked up because it started to cry, i.e. the reason the mother picked up the baby was because it was crying. Fifth, the

adequacy of this description does not depend on our knowing who the child is referring to in this story, i.e. who the baby is or who the mother is.

Sacks' initial observations on this pair of sentences move towards the basis for explication of Membership Categorisation Analysis; the pair of sentences constitutes a “a possible description” (Sacks 1974a:217) which is recognisable to members and analysts as a plausible description of events. The importance of this point is that sociological description is available to members of the culture without the help of sociologists; description is a lay matter and the production of possible/recognisable sociological descriptions involves lay usage of common sense categories.

In explicating the culturally methodic nature of membership categories, Sacks examines not only membership categories but how membership categories are used and invoked by members. Membership categories are organised descriptions and identifications of individuals, relationships and collectivities people routinely use in making sense of everyday life. Sacks called those collectivities and relationships described and comprised by membership categories (such as ‘family’, ‘stage of life’, ‘football team’) “membership categorisation devices”. Sacks defined membership categorisation devices as

“any collection of membership categories, containing at least a category, which may be applied to some population containing at least a member, so as to provide, by the use of some rules of application, for the pairing of at least a population member and a categorisation device member. A device is then a collection plus rules of application.”

(Sacks 1974a:219)

Sacks described these rules of application the “economy rule” and the “consistency rule”.<sup>4</sup> Members can be seen as incumbents of different categories at the same time. The selection of a particular membership category to produce a description of a person is an occasioned matter, and the category selected may be just one of a number of alternate, relevant and recognisable categories. That is, several culturally available categories may be equally correct descriptors of a person. According to the economy rule, a single category from the list of culturally available categories, which would be equally correct descriptors of a member, is sufficient to correctly describe them. As Sacks said,

“a single category from any membership categorisation device can be referentially adequate.”

(Sacks 1974a:219)

The consistency rule, on the other hand, provides for the selection of subsequent categories to be referentially relevant to the selection of the original category. That is, the selection of a category from a membership categorisation device to describe a person should be coincident with categories used to describe further persons if these further persons can be seen as co-incumbents of the membership categorisation device.

Sacks defines this rule as

“if some population of persons is being categorised, and if a category from some device’s collection has been used to categorise a first member of the population, then that category or other categories of the same collection *may* be used to categorise further members of the population.”

(Sacks 1974a:219; emphasis supplied)

So, for example, if I was to introduce someone as a ‘student from the University of Stirling’, then I would prefer to introduce another person at the same conversation as also being a ‘student from the University of Stirling’, or minimally ‘a student’, if they were also an incumbent of the membership categorisation devices ‘student at Scottish universities’ or ‘student’. The membership categorisation device ‘student’ provides for categories such as undergraduate, postgraduate, mature student, returning student, overseas student, visiting student, etc. Any one of these categories could be selected as categories from the same collection in this hypothetical situation. That is, the membership category would be co-selected from the same membership categorisation device.

Sacks outlined a “corollary” to the consistency rule as the “hearer’s maxim”, the use of which by members provides for the recognisability of description. He defined the hearer’s maxim as

“if two or more categories are used to categorise two or more members of some population, and those categories can be heard as categories from the same collection, then: hear them that way.”

(Sacks 1974a:219-220)

So in the child’s story at the beginning of this section (“The baby cried. The mommy picked it up”), the baby and the mommy are heard by members as being categories of the membership categorisation device ‘family’. Although the category “baby” can be heard to be selected from the aforementioned membership categorisation devices ‘family’ or ‘stage of life’, the occurrence of the second category “mommy” informs the

hearer that the category “baby” was not selected from the membership categorisation device ‘stage of life’ but from the membership categorisation device ‘family’. This leads Sacks to suggest that

“if a hearer has a second category which can be heard as consistent with one locus of a first, then the first is to be heard as *at least* consistent with the second.”

(Sacks 1974a:220)

Sacks (*ibid.*) refers to membership categorisation devices such as ‘family’ as “duplicatively organised” membership categorisation devices.

“When such a device is used on a population, what is done is to take its categories, treat the set of categories as defining a unit, and place members of the population into cases of the unit. [...] A population so treated is partitioned into cases of the unit, cases for which what properly holds is that the various persons partitioned into any case are ‘coincumbents’ of that case.”

(Sacks 1974a:220-221)

So, taking football players as examples, incumbents of the category ‘football player’ may be referenced *via* positions, e.g. forwards, wingers, midfielders, defenders, sweepers or goalkeepers, etc. These categories are treatable as categories comprising a unit, *viz.* ‘football team’. Sacks continues with the example of the baby and the mommy, pointing out that the world treated not in terms of babies and mommies but in terms of families. That is, certain categories are organised such that these categories are recognised to go together and to come from the same membership categorisation device. This property of membership categorisation devices works to show how certain categories are recognised to go together but not all categories are recognised as such. The team sheet on Saturday afternoon includes a goalkeeper, defenders, midfielders and

forwards but does not include an uncle, fathers, and brothers. Similarly, the membership categorisation device ‘family’ is recognised to be constituted by such categories but not those recognised to come from the membership categorisation device ‘football team’, e.g. goalkeeper, substitute goalkeeper.

The foregoing affords Sacks an analytic framework with which to examine features of social life as “category-bound” features. This thesis will expand upon this notion in treating bodies of knowledge, places and possessions as “category-bound” features: knowledge, locations and material objects associated with particular categories or membership categorisation devices, e.g. places recognised by members to be places for pickpockets, or places for beggars, or places for drug-addicts. Locations and objects can mutually inform and elaborate the use of membership categories. A syringe on the ground near to a “shooting gallery” suggests that the syringe was discarded by an intravenous drug-user rather than discarded by a disgruntled hospital worker or a medic; further, that a discarded syringe in or close by a shooting gallery is an “addict’s syringe”, not a “hospital’s syringe”. However, for the purposes of this introduction to Membership Categorisation Analysis, considerations remain tied to Sacks’ (related) notion of “category-bound activities” (Sacks 1974a:221 *et seq.*). The qualifier in brackets has been added to emphasise the relation between the activities of injecting drugs and the categories ‘drug-addict’ and ‘hospital worker’—although not *all* members of both membership categorisation devices inject drugs (e.g. hospital administrators, crack smokers), the activity of injecting drugs is done by some members of both membership categorisation devices.

In Sacks' example of the child's story, it is expectable for and recognisable to members is that crying is an activity that babies do. That crying is a category-bound activity, i.e. crying is an activity done by babies, the baby in the story is heard (*via* the hearer's maxim) to belong to the membership categorisation device "stage of life" rather than "family".

The following extracts are taken from a series of parenthood-counselling sessions that are referred to throughout the thesis. These sequences of talk occur in the final session, on first aid. In demonstrating aspects of emergency first aid on children and babies to the group, the nurse (N) outlines some medical hazards associated with child-rearing. These hazards are presented as "category-bound activities" in the utterances marked with arrows:

#### Extract 69 (6)

- |      |   |  |
|------|---|--|
| 56   | N | we've <u>lifted</u> the chin back  |
| 57   |   | we can also (.) <u>sweep</u> a finger round the mouth just in case there's |
| 58   |   | anything   |
| 59   |   | (0.2)  |
| 60   |   | in there   |
| 61 → |   | I mean children are famous for swa- swallowing bits of lego and            |
| 62   |   | sweets and stuff (.) like that okay (.)                                    |
| 63   |   | but don't push your too far=   |
| 64   |   | =just have a quick sweep round   |

### Extract 79 (6)

1 N th`in`k ab`out k`ids a`gain  
2 → th`e one th`ing th`at=  
3 ↑  
4 ((looks up at ceiling))  
5 → =kids d`o as a pa`stime as well  
6 ↑  
7 ((looks up at ceiling))  
8 ((laugh))  
9 ((turns to camera)) it's great this isn it  
10 CR3 yeah  
11 ((Group laugh))  
12 → the one thing that kids do as a pastime  
13 is get burned don't they`

' = loud hammering

The identification of category-bound activities suggests another maxim, a “viewer’s maxim”. Sacks defines the viewer’s maxim as

**“If a member sees a category-bound activity being done, then, if one can see it being done by a member of a category to which the activity is bound, then: See it that way.”**

(Sacks 1974a:225)

This maxim will be illustrated throughout the thesis in that it bears upon the at-a-glance recognisability of visual settings. For example, a member could be categorised according the activities in which they are engaged at a given moment; this is not a deterministic or unequivocal categorisation, as activities may be associated or bound to more than one category or membership categorisation device, e.g. pilgrims engage in ‘tourist’ activities and *vice versa*. The viewer’s maxim is important for this thesis in that it formulates how inferences are available from visual, at-a-glance appearances.

Localised, self-contained areas of drug-taking and prostitution are salient examples of the categorial order of settings. Indeed, it is the categorial nature of a setting which provides for its “sense of place”. They are also demonstrations of what we can call a “common-sense ecology” of cities: as the following example shows, an habitué or local expert familiar with the common-sense ecology of the place is able to tell a “stranger” or visitor about what happens in certain areas, and, if necessary, where to avoid.

Over the pedestrian bridge, across the river from the residential block, are the post office, a taxi-rank and the museum. To the right is a small car park, which is adjacent to the quay-side, a busy road. On Sundays the car park is the site for the market. An interesting feature of this area is the network of narrow, claustrophobic alleyways, which run between the quay-side and the quiet shopping streets off Church Street near the square.

The nature of the streets is not immediately visually available from the quay-side nor Church Street. It is only by walking adjacent to the alleyways that one notices *les vitrines*, the windows in which prostitutes advertise themselves to clients. (A similar but much more public and visible display of prostitutes’ *vitrines* is seen in Amsterdam.) The other small alleys are loci of drug-taking activities, mostly heroin. (These are dangerous streets. Only the addict, the stranger, or the foolhardy sociologist would walk along them.) At night, the car park is also patrolled by sex-workers.

The recognisability of people's actions, using common-sense membership categorisation activities, is a rich seam of inquiry for future research. As I walked with my informant towards the historic quarter of the city, I was told "Everyone waiting here is waiting for a reason: they are soliciting clients, waiting for a prostitute, or waiting for a drug deal".

Ordinary or natural-language categories are manifest in interview settings, e.g. "*one of the things that perhaps people fail to recognise with policemen is that we are only human beings and we get scared too*";<sup>5</sup> ordinary talk and ordinary texts, such as newspaper headlines, e.g. "GIRL GUIDE AGED 14 RAPED AT HELL'S ANGELS CONVENTION" (Lee 1984). Membership Categorisation Analysis is the study of the use of membership categories and membership categorisation devices, and their application.

### ***Navigation through Public Space: Sequence and Category***

In a previous section, I suggested that the treatment of silences by members, and specifically the notion of "noticeable absences", were indicative of a coincidence between sequence and category. Another site of overlap between the sequential and categorial ordering by members is the use of public space. In the previous section, I mentioned Ryave and Schenkein's study of non-conversational phenomena, *viz.* members' culturally methodic practices in walking through public space. Lee and Watson expand upon this study to look at the reflexive, i.e. mutually elaborative relationship between the sequential organisation and categorial order of public space.

Lee and Watson attend to the ways in which public spaces are socially organised, from within, by members themselves. Applying the analytic framework of Conversation Analysis (e.g. adjacency pair structures, preference, economy and consistency rules, membership categorisation and sequences), Lee and Watson are able to provide formal descriptions of non-conversational phenomena. That is, taking Conversation Analysis forward by identifying interactional “equivalents” (Lee and Watson 1993:65) of conversation-analytic formulations in public space. They show how conversation-analytic demonstrations of “next action” can be applied to sequences of pedestrian flow, e.g. queues. The queue—*prima facie* a topic for “sequential” analysis—is orientated to by persons in terms of categories: next, next in line, head of the queue, end of the queue. The incumbents of these categories in the sequence change ongoingly as the queue progresses, i.e. as the “next” is served “next in line” becomes head of the queue (becomes the “next”); as other people join they become the end of the line.

Lee and Watson advance the foundational work on “the navigation problem” (Ryave and Schenkein *op cit.*) through the use of Membership Categorisation Analysis; hence, walking is not reducible to purely sequential concerns. So Lee and Watson show how public spaces are displays of sequential phenomena (or forms of what Sacks, Schegloff and Jefferson (*op cit.*) call “turn-taking systems”) as well as categorisation phenomena.

Furthermore, they explicate the ways in which turn-taking and categorial organisation inform and elaborate each other using the notion of “category articulation” (Lee and Watson 1993:4). This notion accounts for how people orient to sequences of persons in

terms of categories, and *vice versa*. So for example, a line of people can be seen as a queue, and people within the queue categorised as “head of the queue”, “next in line”, etc.; people can walk through public space and be seen as “being together”; or be seen as a “mother and child”, “passer-by”, a “customer”, or a “beggar”.

Public space is available to members as a visible, recognisable state of affairs. The data upon which their analyses are based manifest the use of certain rules by members in public space. These include the “consistency rule”, where the articulation of categories followed the commonly held assumptions which constitute Membership Categorisation Devices (Sacks 1974a); and “preference rules”, whereby members displayed preferences for certain trajectories or practices rather than others. (These rules are not deterministic; organised practices within flow-files are preferred rather than hard-and-fast options.) These preferences are visible in vehicular formats or “flow files” (Lee and Watson 1993:86 *et seq.*), which orient to maxims e.g. “keep to the left”—even if they are not textually mediated by e.g. a “Keep Left” sign. Flow files tend to adhere to the preference rule “the rule of the road”; as far as possible, members walk *with* the flow rather than against it. When members occasionally go against the flow, such activities are “inferentially rich”.

The rule of the road does not just operate as a preference in terms of trajectories, as in the above example of walking with the flow rather than against it. It also operates *apropos* of the speed of locomotion. The data analysed by Lee and Watson shows how flow files are organised from within by members themselves. A flow of people walking

in the same direction preserves the same serial organisation; this is achieved by persons within the flow file maintaining a consistent speed of walking or “standard pace” (Lee and Watson 1993:89-94). The manifestation of a standard pace does not suggest there exists an overall standard pace for locomotion. Rather, it attends to the endogenous and thoroughly contextual nature of locomotion in public space: the standard pace will differ between flow files. E.g. the standard pace of a queue compared to the standard pace of more mobile formats. Like the preference for trajectories which walk with the flow rather than against it, departing from the standard pace of a flow file can be “inferentially rich”. This demonstrates another intersection of category and sequence in terms of “category articulation”. Members walking at a pace which differs from the “standard” pace can be seen as “rushing” or “dawdling”.

The analysis of the visibility arrangements of public space in interactional terms, *viz.* sequences and categories of members, provides a conceptual framework in which to situate observations on beggars and begging (Carlin 1999; Carlin, Evergeti and Murtagh 1999).

The categorial aspects of public spaces outlined so far in this section have informed an ongoing project into the routine use of galleries and museums. I have observed how walking in public spaces such as art galleries and museums is a normative course of action. Members in gallery spaces recurrently show a dispreference for breaking the line of sight between members and the exhibit of orientation. If members do walk between the exhibit on display and the member attending to the exhibit, they produce an

apology. Producing an apology informs members looking at exhibits that the interruption was a dispreferred option and acknowledges that a violation has occurred, i.e. that it is an accountable matter; at the same time, *qua* commentator machine, the accounting or production of an apology tells the analyst that interrupting the line of sight is a violation of a rule. Apologies can be verbal (“sorry”, “excuse me”) or non-verbal. Non-verbal apologies take the form of an accentuated scurrying between paintings and viewers, and exaggerated dips; in galleries, these non-verbal activities attend to ensuring that the line of sight is not broken, or at least broken only fleetingly. There were exceptions to these apologies. Incumbents of a particular category—young children—did not produce apologies when walking between viewer and exhibit; nor, seemingly, were apologies expected by members viewing exhibits.

These regularised and recurrent patterns of locomotion—a preference for walking behind members and a dispreference for walking between the gallery exhibit and members visually orienting towards it—are observable and demonstrated in the open public spaces. The preference rules apply *mutatis mutandis* in the trajectories of flow files moving past beggars. Members in public space have a dispreference for walking between the beggar and the begging bowl, or the busker and the vessel used to collect coins (e.g. bowl, cup, cap, instrument case, etc.) (Carlin, Evergeti and Murtagh 1999).

This socially organised feature of pedestrian traffic was used as a resource by members begging on a pedestrian bridge. This bridge was regularly patronised by a man playing the harmonica. He had a terracotta bowl in front of him to collect coins. However, he

had positioned the bowl in the middle of the bridge. By standing still in the same place, he remained outside the traffic flow, whilst his bowl was placed within it. In passing by, members manifested a preference not to walk between the man and his bowl. At one end of the bridge, a group of beggars sat outside the flow files, on a step. However, they positioned bowls across the exit of the bridge, adjacent to the access ramp. Even though the bridge was too narrow for members to pass by unproblematically, unless in single file, the group did not interfere with members' trajectories of locomotion. However, the confines of the bridge opening ensured that, at a minimum, members had to step over the bowls: if people chose not to "see" these beggars, they saw the bowls.

The social and visible arrangements of flow files highlighted a difference between stationary begging, whereby members doing begging are seen to be standing outside flow files and "demonstrably 'waiting on the sidelines'" (Lee and Watson 1993:92); and practices of "breaking" or "interrupting" the flow momentarily in order to beg change from passers-by within the flow. This is not a comparison of e.g. silent begging *versus* whispering begging, or stationary begging *versus* ambulatory begging. Rather, this refers to observed instances of begging activities whereby the person doing begging standing outside the traffic flow used the internal order of flow-formats to engage the attention of members within the flow (Carlin, Evergeti and Murtagh, 1999).

The final section of this chapter looks at the relationship between Ethnomethodological programmes and sociology, what Garfinkel (1996) refers to as "Formal Analysis".

### ***Ethnomethodological Programmes and Formal Analysis***

As this thesis shows, members' common-sense practices—how members accomplish activities through natural language *in situ*—are the “proper” topics for investigation. Whilst explicating members' usage of natural language practices, this thesis addresses researchers' reliance on and incorporation of natural language practices as constituent features of the routine practices of sociological projects.

Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis are approaches to social phenomena which differ from Formal Analysis, or conventional sociology. Their subject matter, and their purviews, are fundamentally different from conventional sociology. What conventional sociology uses as its resources for study are topics for Ethnomethodological inquiries, i.e. they constitute a *gestalt*. The programmes of Formal Analysis and ethnomethodology are so fundamentally different that one cannot be judged in terms of the other. With their radically different projects, the claims made by Ethnomethodological programmes are not the same as the claims made by Formal Analysis.

It is a mistake to set up Ethnomethodological approaches in competition with conventional approaches because they discuss different kinds of social or worldly phenomena. (Herewith the (contradictory) claims and characterisations that Ethnomethodological programmes are conservative, apolitical and amoral.) Doing so compromises the enterprises and subscribes to the terms of reference set by conventional sociology, which ethnomethodology originally set out to challenge.

It is a mistake also to attempt the *incorporation* of ethnomethodological programmes into existing frameworks. That is, to adopt an ethnomethodological or conversation-analytic approach to a feature of the problem at hand as defined by Formal Analysis. Researchers should not assume that they can treat part of the project as being treatable with e.g. questionnaires and another part of the project as being treatable with Conversation Analysis. As my forthcoming comments on Bjelic (1994) will show, these are different projects rather than separable parts of the same project.. This is *not* to say that work from ethnomethodological programmes should not be used to inform projects. However, it would be a different order of analysis to examine a problem using ethnomethodological programmes *and* e.g. Feminist Theory. The ethnomethodological and formal-analytic programmes are “incommensurable, asymmetrically alternate technologies of social analysis” (Garfinkel and Wieder 1992 *passim*), whereby incorporation is not just unsuitable but impossible. What formal-analytic programmes use as a resource for study, ethnomethodological programmes take as a topic of study (Zimmerman and Pollner 1971).

Indeed, what may be ‘seen’ from one point of view is *necessarily* obscured from the view of another. As Sharrock and Watson argue,

“The relationship between ethnomethodology’s point of view and that of any other sociology is not like that between two perspectives on the same thing, such that these can be conjoined to produce a more ‘rounded’ picture of the object of attention. The relationship between these points of view is, rather, much more like that between the elements in a *gestalt* switch such that there is—again—a

discontinuity: one can look at things one way or the other, but they are discrete and alternate, not additive, ways of seeing things, such that what is seen from one point of view ‘disappears’ when seen from the other.”

(Sharrock and Watson 1988:60)

Formal Analysis regards certain phenomena to be within its purview, to be suitable topics for investigation. The selection of methods for the inquiry of these phenomena is in turn determined by its purview. This circularity, a hermeneutic system, is a feature of the “methodogenic ontology” (Wieder 1980:76) of Formal Analysis, i.e. its methods of inquiry limit it to the investigation of certain phenomena yet these phenomena are available for analysis only *via* these methods. The “methodogenic ontology” is a feature and an epiphenomenon of the “plenum”, that is, the full assembly of theoretical and conceptual assumptions which constitute an approach. The implication of this is that the unexplicated use of natural language and interpretative practices, tacitly relied upon in the plenum as resources for study, results in a methodogenic ontology which is unable to topicalise the actual production of phenomena. So when Formal Analysis *claims* to study phenomena, we may see that it is actually *doing* otherwise.

The mutual exclusiveness of theoretical and methodological options may be illustrated by physicists’ attempts to explain physical phenomena:

“The general theory of relativity describes the force of gravity and the large-scale structure of the universe, that is, the structure on scales from only a few miles to as large as a million million million million (1 with twenty-four zeros after it) miles, the size of the observable universe. Quantum mechanics, on the other hand, deals with phenomena on extremely small scales, such as a millionth of a millionth of an inch. Unfortunately, however, these two theories are known to be inconsistent with each other—they cannot both be correct.”

(Hawking 1988:1-12)

Witness the “*Lebenswelt* structures” of Galileo’s pendulum, i.e. the unspoken, tacit assumptions that are “constituent features” (we could say “equipmentally incarnate”) of the pendulum. Dušan Bjelic (1994) reproduces a demonstration of Galileo’s pendulum. He found that Galileo did not leave instructions for the replication of his pendulum experiment. As such the reproduction of Galileo’s pendulum is heuristic and its accomplishment is a series of “first time through” (Garfinkel, Lynch and Livingston 1981) activities. Bjelic argues that the achievement of “the praxiological ‘inventory’” (Bjelic 1994:6) for reproducing the pendulum demonstration is therefore imposed upon each subsequent physicist.

We may note that Bjelic’s reproduction of the pendulum demonstration involves general theory, the swinging of weights suspended from a bar. If, however, Bjelic were to have addressed the structure of molecules in the air and in the lead fishing-weight as contact was made, he would have to have used quantum mechanics. (This may be clearer if Newton’s Cradle—the “executive toy”—is considered: the impact of swinging weights against each other on a molecular level.) My point is that Bjelic would have entered into an entirely separate experiment: he could not have attempted to reproduce Galileo’s pendulum *as well as* studying the impact of molecular structures in the same demonstration. The approaches to these physical phenomena are mutually exclusive and exist within a *gestalt* relationship: e.g. the vase/silhouette of profiles, which is “seen” either as a vase, or a profile of two faces, but cannot be seen as both at the same time; or the Toulouse Lautrec sketch of the beautiful young/ugly old women, a figure-

ground effect whereby “One cannot constitute both women at once” (D.R. Watson 1994b:16).

So the physicist may perform an experiment and interpret any “discoveries” using the general theory of relativity, or quantum mechanics, but not both at the same time: the use of the general theory or of quantum mechanics are, perforce, different enterprises requiring of different experiments. Irreconcilable forms of discovery within an experiment are, at least, different experiments.

All forms of sociology are necessarily “partial sociologies.”<sup>6</sup> Ethnomethodological programmes, in common with other forms of sociology, have certain purviews; they have phenomenal parameters. The purview of conversation-analytic research is naturally occurring conversational phenomena. It is the application of this purview, rather than the purview *per se*, which enables studies from ethnomethodological programmes to provide or explicate the workings of interactional processes in different “types” of settings.

We may also notice that ethnomethodological programmes are not addressed towards substantive areas in the way that formal-analytic forms of sociology attempt—Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis are *modi operandi*, or, rather, approaches to the *modi operandi* of people in producing recognisable activities in their daily lives.

A study of murder interrogations (D.R. Watson 1990) may illustrate this. What may, *prima facie*, constitute research in criminology, demonstrates how power is interactionally organised (as opposed to the reification of POWER as a theoretical construct) and maintained within settings; how people produce stories, persuasive accounts and claims to states of knowledge; how people make racial distinctions and discriminations; and distinguish victims and suspects, through ordinary activities. As such, the topics of study are endogenous to the setting and warranted by the setting. It is not concerned with “macro” issues (as traditionally conceived), which are external to the setting, because the structure/agency or micro/macro nexus is *outside of* the ethnomethodological project. Moreover, the dichotomous nature of the structure/agency is not natural nor inherent but

“the product of a set of assumptions about the aims and character of theorising, rather than the necessary pre-condition of sociological inquiry.”

(Sharrock and Watson 1988:57)

The structure/agency dualism is a “derivative” of the tendency to contrast “society-seen-from-within” and “society-seen-from-without”, as a *fait accompli*. Society-seen-from-within is that province of everyday life, the *Lebenswelt*, in which people make decisions according to situations; whereas society-seen-from-without is the organisation and governance of people by institutional structures. The conception of society-seen-from-without is a “*modification*” and “*idealisation*” of society-seen-from-within (Sharrock and Watson 1988:58-59; emphasis supplied). We may also note how this relates to the notion of topic and resource: not only is society-seen-from-within used as a resource for the idealisation of society-seen-from-without, i.e. without society-seen-from-within

there could be no idealisation of society-seen-from-without, the very task of idealisation is a topic for ethnomethodological inquiry. Analysts of society-seen-from-without rely (*must* rely) upon society-seen-from-within in order to produce their analyses. This reliance goes unacknowledged yet is incorporated into the (idealised) account of society-seen-from-without.

“Those working within the tradition of social science theorising can—indeed must—disregard the extent to which their activities are rooted in and organised through an understanding of society-seen-from-within, since these matters are (from the point of view of theorising) entirely irrelevant. However, ethnomethodology cannot so disregard them, for it is concerned with precisely the ways in which these *theoretically* irrelevant are *practically* essential and consequential.”

(Sharrock and Watson 1988:59; emphasis supplied)

We see, then, that the analytic interest is in ordinary talk, and how ordinary talk is the conduit of accomplishing courses of action. It is “talk-in-the-service-of-a-task rather than talk *per se*”<sup>7</sup> which comprises the purview of conversation-analytic study. This elides the implicit tension in analysing talk in institutional occasions and institutional settings: “work talk” *versus* “talk at work”.

The study of conversation addresses the activities which are accomplished through conversation. For example, D.R. Watson (*ibid.*) explices the use of people’s routine procedures of ordinary talk to elicit a confession in an interrogation. The distinction I have attempted to draw may be clarified if we consider “interrogation talk” *versus* “talk in interrogation”. Talk is ordinary talk. Institutional or “work talk” is, *a fortiori*, ordinary talk in a setting as made institutionally relevant by members. The police

officer and suspect do not engage in “interrogation talk” during the interrogation, as though there existed forms of talk *qua* interrogation talk, briefing talk, lobby talk, cafeteria talk, etc.: they continue to engage in ordinary talk in pursuance of their activities.

Authors, *via* their tacit use of membership categorisation devices and “instructions for reading”,<sup>8</sup> e.g. the titles of articles, monographs and edited collections, furnish the reader with an *a priori* scheme of interpretation in terms of social context and membership incumbencies, or “predisposing interpretative saliences” (D.R. Watson 1997a:n.5). As Watson argues, the very work of transcription and transcript conventions provide contextual cues for the reading of a transcript as a transcript of talk of such-and-such a kind; unavoidably so, given the cultural practices<sup>9</sup> of transcribing talk and reading a transcript of talk. In attending to an utterance in a transcript we also attend to the institutional/categorial identities; i.e. as an utterance produced by such-and-such. For example, the speaker in this extract

- 07 D: °wanta open f'r me if y' c'l'd  
 (4.0)  
 08 D: °um hum  
 (Anderson 1989:88)

is categorially identified as a dentist. D.R. Watson goes on to say that, in so doing,

“the transcriber is doing the organisation's, or its incumbents' work: it is the task of co-participants themselves to display categorial incumbency where relevant.”

(D.R. Watson 1997a:52)

For the present discussion, it is important to note

“that a) the methodological implications [of providing contextual cues, e.g. categorial designations, within the work of transcription] for conversation analysis have not been followed through, and b) the claims made in conversation analysis disattend these implications.”

(D.R. Watson 1997a:fn.5; my brackets)

We can see that ethnomethodological programmes are “self-contained”: their analytic coherence is maintained through their independence and through the rejection of distortions from outside its programme, e.g. the structure/agency distinction and accepting “creeping incorporationism” *via* the use of Ethnomethodology or Conversation Analysis as extras or corroboratory schemes in constructivist projects. As D.R. Watson notes,

“any additive exercise would be incoherent, for it weds a set of non-ironic analytic practices to a set that is essentially and necessarily ironic, and that self-avowedly sets up a competitive attitude to members’ own conceptions of society”

(D.R. Watson 1992:17)

It is, then, appropriate to reassess the application of the ethnomethodological programme. In taking an independent stance, as opposed to striking postures, we may show that the very autonomy and internal coherence of the programme affords perspicuous studies of social phenomena. It is the adaptation of ethnomethodological programmes *ab intra*, rather than its adaptation to Formal Analysis as a subjacent “take” towards substantive sociological topics, which maintains its analytic integrity in the attempt to stretch or further the boundaries of inquiry. From this perspective we can appreciate the exemplary moves by Lee and Watson, who are engaged in internally

“respecifying” the conversation-analytic programme and applying it to non-conversational settings, e.g. the categorial and sequential analyses of public spaces and queues (Lee and Watson 1993). These are moves *from within* the conversation-analytic programme, rather than derogation or dilution; they are suggestive of a way forward for future ethnomethodological enterprises.

### **Conclusion**

This thesis continues these moves from within ethnomethodological programmes, in addressing the linguistic constitution of research practices. Treating the activities of research as, *au fond*, linguistic activities, provides analytic purchase on the unproblematised use of methods in research. Further, taking research activities as topics of inquiry in their own right makes available the culturally methodic character of activities hitherto taken for granted by researchers.

This chapter has introduced some basic considerations on Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis for the readers, for the reading of the thesis.

The following chapters provide dedicated considerations of Edward Rose, the founder of Ethno-Inquiry and an influential figure in the formative development of ethnomethodological programmes. Rose’s importance for the study of the linguistic constitution of research practices is his thoroughly language-oriented corpus of inquiries undertaken over five decades.

Rose's works are reference points in this thesis. Accordingly, Chapter Two provides readers with an intellectual biography of Rose, in which I point towards ongoing themes and features within his work, and methodological continuities with the work of Harvey Sacks. Crucially, Rose emphasises the importance of members' understandings, how they are realised *via* their culturally-shared, ordinary language practices, and how they prefigure the competitive or methodologically ironic glosses of members' knowledge in sociologists' reports.

Chapter Three provides a list of Rose's works as an integral feature of language-oriented inquiries. Chapters Two and Three are designed to be read in conjunction with each other; each chapter informs the other. We are then ready, in Chapter Four, to move on to explore a realisation of the linguistic constitution of research practices during a period of fieldwork.

<sup>1</sup> As one author contends, "Although the following does not presume to have the status of a bibliographical survey, some starting-points for further reading can be referred to" (Haegeman 1996:61).

<sup>2</sup> This extract is examined for different interactional features in Carlin (1994:126-127).

<sup>3</sup> E.g. telling a "dirty" joke (Sacks 1978:254), where a noticeable absence is a "puzzle". The solution to the puzzle is the point (or punchline) of the joke.

<sup>4</sup> What Sacks (1974a:219) describes as a "reference satisfactoriness rule" and a "relevance rule" respectively.

<sup>5</sup> This utterance is extracted from an interview; more extensive extracts from this particular interview are reprinted throughout this thesis.

<sup>6</sup> Geraldine Lee-Treweek, personal communication, 23 February, 1998.

<sup>7</sup> Roger Slack, personal communication, 2 March, 1998.

<sup>8</sup> This is John Lee's (1984) notion. I shall return to instructions for reading and titles in Chapters Seven and Thirteen.

<sup>9</sup> That is, reading and writing from left to right.

## The Work of Edward Rose: An Intellectual Profile

This thesis, on the linguistic constitution of research, draws comparisons between inquiries that take members' use of language as a topic *versus* as a resource. At this juncture, following introduction to the ethnomethodological approaches which inform the research, this thesis presents a profile of Edward Rose. As Rose's work is referred to ongoingly as the thesis progresses, a discrete overview of his methodological orientations and intellectual developments is appropriate. Rose's work is important to the considerations of this thesis because Rose affords primacy to the use of language in his inquiries. This profile is accompanied by a bibliography of Rose's works, which is featured in the subsequent chapter.

This chapter also brings the reading and writing of biographies into greater salience. Materials used in the production of a biography are mainly textual/oral-aural, i.e. language based materials. (Examples of such materials include personal letters, published and unpublished writings, speeches, personal documents, archived sources; also reminiscences and conversations.) Biographies are reconstructions of a person's life by a biographer, which are based upon fragments of inscribed and iterated particulars. The biographer necessarily imposes their own *imprimatur* on materials through the practices of transformation which these materials undergo; that is, turning materials into a biography transforms those materials.

The linguistically organised nature of biographical and autobiographical materials is illustrated by Paul J. Baker's (1973) study of an unfinished project on the history of US sociology. Luther L. Bernard conceived a project to assemble the life-histories of US sociologists into a single volume, as a service to the profession. He proceeded by inviting sociologists across the country to write their own life-history. According to Baker, the letter of invitation to contribute to the collection included a format for contributions. This format

"included such items as educational background, occupations before and after entering sociology, various influential teachers in sociology and other fields, other significant intellectual influences, various research projects conducted, and 'any other influences, vocational, professional, individual, general, political, social, economic, etc., which have helped to mould your career as a sociologist'"

(Baker 1973:243)

The important point to note about this is not the required format of the life-history but that the invitation to contribute a life-history to the project was textually mediated, i.e. it was linguistically constituted. Baker reprints the life-histories written by W. I. Thomas and Robert E. Park. Again, it is to be noted that the reconstructed versions of their life-histories are textual artefacts, i.e. linguistic materials.

The materials used in assembling a biography are linguistically organised materials. Furthermore, the outcome of the *transformation* of these materials, i.e. the biography, is itself linguistically organised.

This profile has been written using various sources: the extant work listed in the dedicated bibliography; audio-taped conversations, lectures and seminars; personal communications with Rose, and with his colleagues and students; versions of his *curriculum vitae*; historical materials Rose personally made available to me and at the Edward Rose Collection, the Archives in the Norlin Library at University of Colorado, Boulder.

This chapter intends to provide readers with access to the world, or *werald*, which “is made up of people and their things” (Rose 1992:39); as such it talks about Rose’s projects, and some of the people Rose encountered in his inquiries. It locates his work within the human sciences along a continuum that spans nearly seventy years. It suggests influences and continuities that may be found within Rose’s work, remarks upon his input and integral contributions at the early stages of the development of ethnomethodology, and how his concerns led him to establish a separate and distinct school of thought. Further, it points towards some affinities, contiguities and generic “family resemblances” between Rose’s seminal works and the analytically suggestive programmes of Harvey Sacks.

According to John Heritage, Wittgenstein’s notion of “family resemblances” refers to those properties which

“display overlapping similarities and resemblances rather than universal and finitely specifiable features.”  
(Heritage 1978:90-91)

In this way, it is possible to indicate (though not necessarily enumerate) areas in which the fields initiated by Rose and Sacks intersect, i.e. “points of contact” or commonalities between approaches. Using the metaphor of family resemblances, it is shown that the analytic programmes of Rose and Sacks are cognate, rather than isomorphic or identical. Whilst there are differences between Rose and Sacks, e.g. what constitutes data for study and how data are analysed, there are broad areas of overlapping concern, or family resemblances. These include common concerns with language and the use of language by members, and the disinclination to accept sociological concepts in favour of focalising members’ own orientations towards phenomena. This explicative approach indicates a cognate analytic mentality between the work of Rose and Sacks.

### ***Bibliography qua Biography***

Although I do not claim that this chapter is a biography of Rose, it necessarily contains “biographical” material, in that it refers to his professional activities, research interests and teaching commitments. Whilst the range of materials drawn upon are mentioned above, and Rose himself has given generously of his time, the sources of information available to me would not have produced an adequate biography. Furthermore, the production of a biography is a different project from this intellectual profile. (In any case, Rose has written two volumes of his autobiography so far. However, his autobiographies pertain to the period of his life before entering academe as a teacher and researcher.) Biographical materials that are presented are, primarily, germane to the circumstances of the production of his work. Such biographical materials inform the

bibliography in the following chapter, e.g. by explaining why Rose was involved in Gestalt psychology, with Edward Tolman, which led onto his period of study in Frankfurt with Max Wertheimer. That is, biographical materials provide the sense of bibliographic materials, and *vice versa*. This chapter, and the subsequent chapter containing a bibliography of Rose's work, are mutually informing and mutually elaborating. The analytic bibliography informs the intellectual profile, and the intellectual profile informs the analytic bibliography.

The Belgian anthropologist of communication, Yves Winkin, has been engaged in writing biographical and explicative pieces on Erving Goffman for a number of years. For the current project, Winkin's biographical project on Goffman is notable for his considerations of confidentiality. Confidentiality is a major theme in this thesis. Winkin is concerned with the diffuse breach of confidentiality *re* Goffman's life, since Goffman had been very careful to keep his private life private.

Winkin does not approach (auto)biography<sup>1</sup> uncritically, as being sociology *simpliciter*; Winkin's approach is more sophisticated, looking at how biographies are written and informed sociologically, and how biographies constitute members in different ways. His concern is that the methods of producing a biography, and what a biographer made of materials used in its production were made available to the reader (Winkin 1999:30).

Winkin suggests one possible way of writing an intellectual's biography is to identify dispositions or the "habitus" of the individual. In terms of Goffman's habitus, Winkin

points to an approach which had been culturally-established *via* Chicago-School sociology (1999:34-35). Ultimately, however, Winkin (1999:36) notes that this is an unsatisfactory approach: the notion of habitus does not explain why no other “Goffmans” emerged from the University of Chicago; nor, by such extension, why there was only one Harvey Sacks, who had been one of Goffman’s doctoral students. That is, the notion of habitus is not “uniquely adequate”<sup>2</sup> to the writing of biographies. Earlier in his reflections on writing intellectual biography, Winkin notes how influences are problematic for biographers.

“There is never a way to definitely confirm or disconfirm such ‘influences’. [...] They are discarded because they cannot be confirmed – and then retrieved and replaced because they cannot be refuted.”

(Winkin 1999:31)

The nature and extent of ‘influence’ is corrigible and debatable, but again is only linguistically available; different biographers could provide different emphases of influence. That is, ‘influence’ is “subjectively problematic”. John R. E. Lee coined this term to indicate how alternate definitions of the same phenomenon can be advanced, i.e. matters of interpretation for members. (The notion of subjectively problematic phenomena is returned to *re* bibliographic matters in Chapter Thirteen on “constituting bibliographies”.) Another anthropologist acknowledged that the extent and exactitude of ‘influence’ was a problem for writers themselves, not just their biographers:

“[o]ne only knows, and that incompletely, what the major intellectual influences upon his work have been, but to attach specific names to specific passages is arbitrary or libelous”

(Geertz 1968:v)

Nevertheless, it is possible to intimate a number of writers who, directly or indirectly, affected Rose's approaches to the world at different times. Rose was influenced<sup>3</sup> by his teachers Margaret T. Hodgen; the anthropologists Clyde Kluckhohn, Alfred L. Kroeber, Robert H. Lowie and the ethnologist Paul Radin; the historian Frederick J. Teggart; and Edward Tolman, once President of the American Psychological Association. Other key figures for Rose at various stages of his academic career have included Herbert Blumer, once President of the American Sociological Association; the pragmatist John Dewey;<sup>4</sup> Harold Garfinkel; Edmund Husserl, founder of phenomenology; Harvey Sacks, founder of conversation analysis; William Isaac Thomas, once President of the American Sociological Association; and Florian Znaniecki, founder of the Polish Sociological Institute, the *Polish Sociological Review*, and once President of the Polish Sociological Association as well as the American Sociological Association. These intimations are inferences warranted by (and acknowledged in) Rose's work.

This suggests that another approach to biography in sociology is perhaps possible *via* bibliographic analysis. That is, a more radical form of intellectual biography that makes visible a natural history of changes and continuities of analytic options. In the analytic bibliography in the next chapter, various changes of emphases are apparent: *inter alia*, early laboratory studies of rats and *Gestalt* psychology, large and small-scale studies of "culture", applications of the productions of small "cultures" in laboratories, diachronic and etymological analyses of ordinary and professional sociological language, and ethnographic investigations into diverse settings and professional domains.

Such bibliographic analysis for purposes of intellectual biography is not as episodic as might be intuitively presumed, e.g. taking calendrical points such as publication dates as standing proxy for current research interests. As the bibliography of Rose shows, and as active researchers are aware, different projects are not necessarily compartmentalised, and may have been progressed concurrently. Bibliographic analysis highlights common, ongoing and recurring themes within Rose's work, some of which have antecedents in his previous studies.

Following Winkin, this profile includes the presentation of "biographemes". Biographemes are elementary units of biography, which Winkin describes as "meaningful fragments of a whole" (1999:21). (Winkin's use of the word "biographeme" derives, etymologically, from the work of his tutor, R. L. Birdwhistell, on "kinemes": kinemes are the fundamental units of body motion in interaction; Birdwhistell called the study of body movements in interactional settings "kinesics".) Biographemes are inscriptions of basic and identifiable details of a person's life, which comprise an explanatory or descriptive narrative. As mentioned above, the biography informs the bibliography, and *vice versa*. For instance, the circumstances of works prior to his appointment at and retirement from the University of Colorado at Boulder, Rose's principal institutional affiliation.

***The Oral-Aural/Textual Record of Rose on the Move*****From Colorado to California**

Rose's grandfather had taught chemistry at the University of Michigan. His father secured a position as Head of Metallurgy Department at the Colorado School of Mines, before going into industry as the Superintendent of a zinc mill at Blende, outside Pueblo, Colorado. Rose was born in Pueblo and grew up in Berkeley, when his father's work required him to move to California. So Rose and his family had connections with Colorado, and its university at Boulder. CU Boulder was the Alma Mater of some of his relatives, and where he would spend most of his academic career.

Rose entered the University of California, Berkeley in the fall of 1927. As the credit system required students to declare their intention to major in specific subjects before admission to certain courses, Rose majored in different fields in order to take the courses that he wanted, including anthropology, architecture (which enabled him to take a course in sculpture), art, history and psychology, with Edward Tolman. Whilst he was still a senior at Berkeley Rose conducted some laboratory experiments, which opposed the fashionable Watsonian theories of animal behavior—treating rats and people as machine-like beings. Rose's experiments seemed to support Tolman's anti-mechanistic theories of learning. Rose set up adjacent two-door discrimination boxes, in which rats were confronted with different colored exits to move through the apparatus and reach food. The experiment determined that rats could learn contrary discrimination habits. Rats learned to discriminate between exits as they passed through the boxes, displaying a preference for the white-colored exit in the first discrimination box, then preferring

the black-colored exit in the second box. This research, which was repeated and verified by Tolman himself (Tolman 1934) became Rose's first publication (Rose 1931). In the Spring of 1931, Rose majored from University of California at Berkeley with a baccalaureate degree in Art and Anthropology.

### Frankfurt

Unable to find a job after graduating with an A.B. degree, Rose decided to continue his studies for another year, in Europe. Tolman arranged a one year's visiting research position with Max Wertheimer, the founder of Gestalt psychology, at the Universiteit of Frankfurt am Main, Germany. Prior to the unionisation of the shipyards, Rose got a job as a mess boy for the crew of a ship, sailing the canals between San Francisco and New York. Thus Rose crossed the continent, before sailing across the Atlantic to Hamburg, *en route* to Frankfurt.

On his arrival, Rose conducted further laboratory experiments establishing discrimination habits in rats. Willis D. Ellis helped Rose out as a researcher at Frankfurt. Although Ellis was without laboratory experience, he could afford to buy the necessary rats.<sup>5</sup> Tolman's anti-behaviorist turn, which was taken by Rose using laboratory experiments with rats to elaborate upon learning theories, was new to Frankfurt am Main. During his visit to Franfurt, Rose attended lectures given by Wertheimer and by Karl Mannheim. Before leaving Frankfurt, Rose produced a series of diagrams (Rose 1932) charting the world or environment as it was seen by rats. He noted that the production of a diagrammatic representation of the rat's world, as

“experienced” by the rat, was a different proposition than a similar exercise for human beings (*vide* Item #139).

### Berkeley

After his return to Berkeley, as a Research Assistant from 1932 to 1933, Rose elaborated<sup>6</sup> upon the experiments conducted in Frankfurt. A complex of discrimination boxes allowed rearrangements of the discrimination situations which confronted the rats (Rose 1939). Thus, the rats’ acquired or learned orientation to sequential or temporal decisions, and spatial awareness, could be recognised.

From 1934-35, Rose worked as a Research Assistant in the Department of Social Institutions at Berkeley. Here he met with Robert K. Merton who had come to Berkeley to deliver a lecture. Rose’s work, including courses in economics with Margaret Hodgen and Frederick Teggart, led to his Master of Arts in Social Institutions, which was conferred in the Spring of 1935. The Great Depression ensured that Rose was continually involved in graduate studies until the Spring of 1935. That same year, between his MA and taking up a post as Teaching Fellow in the Department of Economics at Stanford (there being no sociology department at that time), Rose worked as a Personnel Supervisor at the State of California Relief Administration, Oakland, California.

### Stanford

In 1936, whilst teaching at Stanford and having started working towards his Ph.D., Rose did some research on behalf of the Bureau of Rural Research, based at Palo Alto,

California. Robert E. Rapp, the Director of the Bureau of Rural Research, Department of Agriculture, was another Ph.D. candidate and a Teaching Fellow in Economics, who worked in the office next to Rose. Rapp hired Rose to write twelve studies on twelve California counties having unemployment and relief problems, along with a thirteenth general survey of rural relief problems in California, issued by the Works Progress Administration (William R. Creech, personal communication, 5 September 1996).<sup>7</sup> Rose secured the first-person accounts of poverty problems experienced by people—especially those who had migrated from the “dustbowl” areas into California—in these areas, a corpus of material which predates John Steinbeck’s novel *The Grapes of Wrath* (Rose, personal communications: May 15 1996, October 9 1996; D.R. Watson 1997:viii).

The oral examinations for progression onto the doctoral dissertation were conducted in 1937; with several interruptions, Rose then went on to work towards his Ph.D. From 1937-38, he briefly returned to the personnel work at State of California Relief Administration. During the period from late 1939 until the November of 1940, Rose entered the business world to manage The Weed Control Company in Berkeley, California. He then spent a whole year as Personnel Supervisor at the U.S. Government National Youth Administration in Richmond, California. Rose became acquainted with William I. Thomas, who arrived in Berkeley from Harvard when his wife, Dorothy Swaine Thomas, got a job teaching sociology at Berkeley’s School of Agriculture.

Rose (19 October 1994, Item #140) notes how his work was influenced by Thomas' notion the "definition of the situation," the experiential nature of social life as formulated by Znaniecki's (1934:36ff.) "humanistic coefficient" (also taken up by Garfinkel [1996:12-13] in a recent paper) and by Thomas and Znaniecki's (1918-20) pioneering study of individuals and their acculturation.

"Their work, *The Polish Peasant in Europe and America*, was then and is still the most important work written on the modern world as it has been filled with people of our time or just before we came on the scene. It's important, not especially for their fine Methodological Note that introduces the work, but just because it deals with *people* as requiring explanation and understanding"

(personal communication, June 12 1996; emphasis supplied. Cf. D.R. Watson 1997:vi).

The long-standing concern with Znaniecki's notion of the humanistic coefficient, shared by Rose and Garfinkel, suggests their involvement in cognate enterprises and common outlooks. However, whilst Garfinkel developed ethnomethodology as a Schutz-based sociological reaction to the Parsonian notion of social order and social systems, Rose notes (20 October 1994, Item #141) that he "arrived" at his position *via* a different route: from Gestalt psychology, through to the realisation of *gestalts* or forms of worldly activities as produced and arranged by people themselves.

Rose's Ph.D. was interrupted again, this time by the Second World War. With the increased likelihood of being sent to Europe as part of the war efforts, Rose was forced to begin a different Ph.D. dissertation upon his leaving the National Youth Administration in the November of 1941, which was completed in March the following year. (Later in this chapter I suggest that antecedents of some of his later work are

contained in this thesis.) While he waited to be drafted he then worked for another full year with Vlad Ratay, as a statistician with the Bureau of Public Assistance, Social Security Board, at the Regional Office in San Francisco. During this time, Rose documented and assisted those American citizens of Japanese descent who were forced to leave states on the Pacific Coast during the Second World War.<sup>8</sup> Two years later, and under different circumstances, Rose and Ratay (then a representative of the United Nations Relief and Rehabilitation Administration) would meet again in Rome.

#### Mobilisation of Private Rose #39040002

In February, 1943, Rose was drafted as a private in the Infantry of the U.S. Army. Whilst on a tour of duty in Algiers, Rose was given a job at the News Division of the Psychological Warfare Branch, which published a daily newspaper. This paper, *Basic News*, was intended to tell the truth as it was known, on a day to day basis, about the progress of the War. Initially Rose served as the Librarian; however, he became responsible for rewrites, transferring news from the teletype machine into readable formats for publication, and occasionally editing copies. In the October of 1944, Rose got his “old job” back, as Librarian at the News Division in Rome, and was appointed Managing Editor of the Central Desk and of the whole news service in the January of 1945.<sup>9</sup>

Whilst Rose was still in Rome, George Hildebrand, one of Rose’s colleagues at Berkeley and at Stanford, learned of an opening at CU Boulder and put Rose’s name forward.<sup>10</sup> Morris Garnsey offered the post to Rose, who held it until his return in

October, 1945. Rose remained equivocal about an academic career, and the success of his art exhibitions whilst he was soldiering in Europe posed a dilemma for him: should he make his money as an artist in San Francisco, or enter university life seriously?<sup>11</sup> At the end of November, the Monday after his demobilisation, Rose travelled with his wife Evelyn from San Francisco to become an Assistant Professor in Sociology in the Department of Social Science at the University of Colorado at Boulder. Rose was hired to teach Anthropology at Boulder. However, the University was able to hire Omar Stewart, who received his Ph.D. in Anthropology at Berkeley. So Rose taught some courses in Anthropology and most of his courses in Sociology.

### ***Sociology in the Laboratory***

The influence of his teachers Kroeber and Teggart provided Rose with an ongoing curiosity with “culture.” Rose’s interests in culture, language and language use, combined with the influence of Tolman and his laboratory experiences, would find their apogee in his famous demonstrations known as the “Small Languages” productions or “experiments.”<sup>12</sup> Rose involved persons in the laboratory productions of small languages, which highlighted the complexities of natural language use through the treatment of natural language activities as “anthropologically strange” (Garfinkel 1967:9).

The innovative Small Languages project was the logical outcome of a series of earlier projects, in which Rose had engaged in various studies of language and culture in laboratory environments. Although the laboratory was used as an analogy for the

encapsulation of worldly phenomena (e.g. E. C. Hughes 1971a:159), the laboratory *per se* had not been seen as a legitimate research site for sociological investigations. However, during the Nineteen-Fifties and Sixties, Rose attempted doing sociology in the laboratory.

Rose conducted a series of studies, which were foregrounded in his formative laboratory experiences and anthropological interest in cultures, with the intention of producing simple, worldly cultures in laboratory settings. The original and creditable features of Rose's laboratory demonstrations were their unalloyed non-psychologistic characters. For example, the use of Rorschach cards was not inclining towards any forms of personality assessment, but for the elicitation of cultural productions. Unlike those who offered psychological explanations, Rose remained in the domain of the social, in and of the world: phenomena were not located or transfixed "within the skull" but were "out there" in the socially available world. This avoidance of such "mentalism", as discussed in his "Amsterdam lecture",<sup>13</sup> is a constant throughout Rose's work. As such, this is an elective affinity between the work of Rose and Garfinkel, who were both proceeding on an anti-mentalism project.

Within an investigation into "the actualisation of culture out of experimental transactions" (Rose and Felton 1955:383), Rose and Felton "sought to produce culture in the laboratory and to specify some of its experimental operations" (Rose and Felton 1955:383). The small, simple, laboratory-specific cultures made visible "culture histories" (Rose and Felton 1955:386) as observable and reportable matters, i.e. as

*accountable* matters. In so doing, this study worked to bring culture histories into the purview of natural sociological inquiry; that is, to realise cultural productions and culture histories as *inquirable* matters.

Cultural productions were size and time limited:<sup>14</sup> groups or cultures were small (three group-members), therefore *manageable*, and the length of each session of group responses was strictly determined. These sessions constituted “closed” societies (Rose and Felton 1955:384); contrastively, the rotation of group-members, bringing a change in personnel or group-membership for subsequent sessions, constituted “open” societies (Rose and Felton 1955:384). Each group-member responded to images on Rorschach cards, and the responses suggested the inquiry of the cultural processes “invention”, “borrowing”, “habit” and “culbit” (responses that displayed the cultural processes of borrowing and habit) (Rose and Felton 1955:384). That is, of the observed responses within each session, the earliest identification of a feature of a card manifested invention, repetition of their own responses manifested habit, and the repetition of another’s response manifested borrowing. When a group-member joined another group, they would take with them the borrowings, habits and culbits, i.e. the common understandings acquired and produced in the previous group. Tracking the movement of personnel, and the concomitant movement of ideas, provided for the analysis of “cultural histories” (Rose and Felton 1955:386).

The notion of cultural histories appears again in the small languages demonstrations, and which, *exceptis exceptiendis*, displays an “elective affinity” (D.R. Watson 1997:v)

between Rose and Sacks. The affinity is exhibited not in the critical differential status they afforded to mock-ups (Garfinkel and Sacks 1970:363; Sacks 1984:25) but in the particular aspect of succession. For Sacks, that two events occurred consecutively constituted a “series”. Rose stated that if a production

“consists of more than two events, its significance will still derive from involvement in a con-sequential set of happenings and circumstances that form a history”

(Rose 1964:8)

In this pertinent respect, the occurrence of series and histories, a parallel with the analytic programmes of Rose and Sacks may be conjectured.

Rose and Felton's (1955) notions of the circulation of ideas within groups, leading to the cultural dissemination of symbolic/linguistic communicative innovations, is supported by observations on the “closed system” (Sudnow 1967:25) of information regarding treatment and patient care that exists at the county hospital, in which the

“lack of daily contact with the ongoing outside world of medical practice provides for a general technological stagnation”

(Sudnow 1967:25-26)

With the continued absence of any input of new diagnostic and surgical techniques, the medics' level of knowledge remains static. Rather than being resistant to change, opportunities to change or experiment with new treatment methods or alternative practices do not arise. It is from this viewpoint that the laboratory study is shown to be

“an investigation of provocations of cultural *development*” (Rose and Felton 1955:391, emphasis supplied).

Rose recognised that the small cultures and small languages produced during this time were coherent with, and illustrative of, Edward Tylor’s nineteenth-century definition of “Culture and Civilisation”, that is “...capabilities and habits acquired by man as a member of society” (Tylor, quoted by Rose.)<sup>15</sup> Instead of determining what culture is, is not, or might be, according to any *ex situ* analytic or theoretical engagements maintained by commentators, Rose restores the faculty of determinations of culture to its producers. Rose’s doing sociology in the laboratory does not treat culture as *sui generis*; Rose did not take culture for granted, as a given. The *in vivo* analysis of cultural establishments, i.e. the articulation of objects or understandings from other groups, demonstrates that culture was produced by participants themselves, wherein the productions of small languages and small cultures were attuned to and

“explored the dependence of ... particular [cultural] responses on membership in laboratory societies”

(Rose and Felton 1955:384; parentheses added)

Although Rose’s approach was a novel departure, its outcomes were located within members’ practices. Rather than the use of laboratory environments for study,<sup>16</sup> this emphasis on members’ work suggests an affinity with other ethnomethodological programmes.

## Translation

When different languages encounter each other, the collaborative outcomes resulting from the production of successful and unsuccessful worded practices, in terms of their mutual intelligibility, are thrown into relief. In so doing, translation becomes a legitimate topic for sociological inquiry. Rose (1967a) provides a series of encounters, accounts of the interaction between such small languages, i.e. cultures, as produced “in the laboratory”. These encounters between small languages make visible fundamental features of the practical sociological work of members themselves. The communication, i.e. cultural exchange between the small languages, revealed “rudiments of natural sociologies advanced by every person” (Rose 1967a:147). Rose sees such communications between small languages as demonstrations of the development of natural sociology. To put this crudely, as sociology is based upon an extant natural language, the development of a new language might indicate the fundamental categories for cultural exchange, or throw them into sharp relief. For Rose, the categories used by people themselves are prior to the categories used in sociology:

“Nominal and pronominal representations have been put to use so long in this world before professional sociology started its classifications that they may be described as primordial sociological outlooks.”

(Rose 1967b:9)

The tension between preservation and transformation of worded phenomena, a demonstration of Sacksian analysis, was taken up in a later conversation between Garfinkel and Rose. How the common-sense wording of an original is rendered into

the common-sense rewording of a translation, whether the translation is “faithful” or “literal” (the notion of “gist”) (Garfinkel and Rose 1978:2), requires the work of an unexplicated set of members’ accomplishments that are uniquely suited to the here-and-now practicalities of handling materials to hand. A later study on translations,<sup>17</sup> taking as an exemplar the translation of a passage from English to Japanese, followed by its re-translation from Japanese back to the original English, examined and reported upon preservations and transformations exhibited in the translation/re-translation process. The topicalising of translation as a “hidden” topic of investigation evinces translation to be, as Rose states, “a natural way to force you to attend to the details of an original version” (Garfinkel and Rose 1978:11).<sup>18</sup>

### ***Rose and Ethnomethodological Programmes***

Rose had developed the reasoning behind his “English Record” (1960a) and “Uniformities in Culture” (1962) papers during the late Nineteen-Fifties. At a conference in New Mexico,<sup>19</sup> they became sources of agreement between Rose and Harold Garfinkel. It was through their discussions and argumentation *apropos* of these papers “on the use of words as cultural units”<sup>20</sup> that they became friends and intellectual colleagues. They shared a common dissatisfaction with sociologists who assumed the culture, that is, those pursuing current sociological approaches who, tacitly and unrelievedly, relied upon their ordinary, common-sense knowledge as members of the culture yet edited out the very same abilities, achievements, viewpoints and voices of people themselves.

Rose had various levels of input into the emergent field of social investigation that came to be known as Ethnomethodology (Watson and Sharrock 1993). Harold Garfinkel made fulsome tributes to Rose's long-standing contributions at the First International Conference on Understanding Language Use in Everyday Life at the University of Calgary, 1989;<sup>21</sup> and acknowledged his participation in print.<sup>22</sup> Rose was the principal investigator on a number of research applications (see the bibliography in the following chapter for some examples). As the grant-holder, he was able to channel monies into activities, e.g. secretarial and stenographic support as well as research itself, which enabled ethnomethodological inquiries to be conducted and reported.<sup>23</sup> So Rose facilitated the development of ethnomethodology through his distribution of grant monies, allocating funds to his colleagues, so that the emergent approach could be printed and made available. He participated in its original, formative discussions along with Garfinkel, Egon Bittner (who was later given access to research sites with the police<sup>24</sup> by Rose on the Larimer Street or "Skid Row" ethnographies) and Harvey Sacks, and in early debates between Ethnomethodology and "traditional" sociology.<sup>25</sup> Later contributors to their discussions included Emanuel Schegloff and David Sudnow.<sup>26</sup>

Through their pioneering research, taking "respecifications" of sociology as means to explicate people's ordinary, common-sense reasoning procedures, Cicourel,<sup>27</sup> Garfinkel, Rose and Sacks independently developed their own schools of thought and traditions. Rose brought an ethnographic dimension and emphasis to the study of

people's practical, worldly activities. During the Nineteen Seventies, whilst he was teaching at Boulder, Rose's investigations came to be known as "the ethno-inquiries." The cohort of investigators was referred to as the "Colorado School" ethno-inquirers (Kaplan, 1982:2) or, in Jon Driessen's sardonic term, "Mountain Time" sociologists. Although they are different fields, Rose's Ethno-Inquiries share various "family resemblances" with Ethnomethodology and Conversation Analysis (though to a lesser extent with Cicourel's Cognitive Sociology and acquisition of social structures), e.g. their explicative, language-orientated, non-ironic and non-mentalistic approaches towards social investigations.

In 1960 Rose helped to found the Institute of Behavioral Sciences,<sup>28</sup> at the University of Colorado, which originally included Morris Garnsey, Kenneth Hammond and Omar Stewart. Physical space for the new Institute within the University was at a premium. After detailed planning,<sup>29</sup> the Institute was established in a refurbished church and in his grandmother's old house beside the campus. He was the first Director for two years until he secured Ozzie G. Simmons, from Harvard, to take up the post of Director; Rose then became one of the Institute's Project Directors. Rose talked with Willard Waller who taught at summer sessions at Boulder (personal communication, 6 January 1997). Rose knew Talcott Parsons mainly through Ph.D. dissertation committees at Harvard, which Parsons directed at this time (personal communication, 25 March 1997); however, he also attended Parsons' original presentations, when Parsons was moving away from being a purely systems theorist towards advocating evolutionary models of societal change.<sup>30</sup>

During the Nineteen-Fifties, Rose's thinking about the "study of society" had undergone marked changes. He had been asked to write a textbook on sociology. His preparatory notes for this book (Rose 1997), wherein the emphasis is not on the study of society (however that might be conceived by conventional sociology) but on the study of the world as known by people, reveal the "obstinate difficulties" (Rose 1961:4) encountered in the accounting for people's practical, decision-making activities within the determinations of "textbook sociology." The radical turn of Rose's approach from "social" inquiries to "worldly" inquiries can be seen in a successful research proposal he submitted to the United States Air Force Office of Scientific Research (Rose *ibid.*)<sup>31,32</sup> but the eventual book proposal on attending to the commentary machine (Rose 1964), which was supported by Llewellyn Gross, a social theorist of State University Of New York at Buffalo, was not accepted for publication.

The implicativeness of this distinction between social and worldly inquiry is established through diachronic analysis. The word "world" is derived from the features *wer* (man) and *ald* (age): the *werald* refers to the "Age of Mann," or "humanity". So "worldly" inquiries are inquiries which take up the study of people and those things people do or have done to them, throughout the course of history to the present. Herodotus, therefore, can be seen as the first "ethno-inquirer".<sup>33</sup> "Place the emphasis on the world, not on the explorer" (personal communication, 15 May 1996), i.e. to give people's worldly experiences primacy in sociological studies rather than arrogating definitional

privilege as “The Great Sociologist”.<sup>34</sup> The explorer, the historian, and the ethno-inquirer encounter people and their things which are happening or have already happened: Herodotus wrote a series of histories of people in the world that he encountered, whether through stories or observations on places he visited. Similarly Columbus, as Rose notes, did not “discover the world” but told others what the world itself had revealed to him. Hence the Ethno-Inquiries preceded Rose.

### ***Professional Activities***

From 1951-52, and through the summer of 1955, Rose was a Visiting Lecturer in Sociology, University of California at Berkeley; at the time, Herbert Blumer was the Chairman of the Sociology Department. Rose was the Chairman of the Department of Sociology at the University of Colorado at Boulder from 1955-57, and then later from 1960-63. He participated in concurrent conferences sponsored by the Behavioral Sciences Division of the United States Air Force Office of Scientific Research at the University of New Mexico, in the summers of 1957 and 1958.<sup>35</sup>

As an Adjunct Professor at the Albert Schweitzer Institute/Colleges in Switzerland in 1969, Rose chaired the dissertation committee of Clifton M. Wignall. Like Rose, Wignall also studied at Berkeley’s Department of Social Institutions (although Wignall admired Teggart, he arrived at Berkeley too late to meet him), and then later with Sir Edward Evans-Pritchard at Oxford; Rose and Wignall would work together on ethnographic research at the South Catchment, Phoenix, Arizona; and collaborate on a paper derived from Wignall’s studies of mental hospitals. Between 1969-72, and

throughout “University Year for Action”, Rose was an Instructor on VISTA, a Master Teaching Programme, at a Sioux Reservation in Pine Ridge, South Dakota. This activity was followed in 1973 by a Visiting Professorship at Fort Lewis College in Durango, Colorado, a position Rose took up again in 1978.

From 1970-78, Rose was the Editor of the *Sociolinguistics Newsletter* (now *Sociolinguistics*), the journal of the International Sociological Association’s Research Committee on Sociolinguistics, when he stepped down to become Associate Editor until 1985. He was Associate Editor of the *International Journal of the Sociology of Language*, and was the Editor of a special issue on “Sociolinguistics and Ethnography.”

Rose dedicated research time to education, conducting inquiries into classroom activities and educational processes. He chaired the Academic Disciplines Committee of the Study Commission on Undergraduate Education and the Education of Teachers, whose members were Kenneth Boulding, Edwin Fills-the-Pipe, Harold Garfinkel, Robert C. Hanson, David Hawkins, Peter Ossorio, Judith Warren Little and Murray Wax. (personal communication, 1 September 1996).

Among the courses that Rose directed throughout his teaching career at the University of Colorado were “History of Social Thought,” “Persons in Society,” “Ideas in Society” and “Sociolinguistics.” After his retirement from the full-time teaching staff in May 1977, Rose continued to give undergraduate and graduate courses including “Expository Writing in the Social Sciences,” “Six Lectures on the Social Thought of the

Moderns: Descartes, Vico, Condorcet, Stael-Hostein, Saint-Simon and Comte,” “Interdisciplinary Studies: An Undergraduate Seminar on Anthropological and Sociological Theory,” “Interdisciplinary Studies: An Undergraduate Research Practicum in the Ethno-Inquiries,” “Seminar on Social Theory: The Ethno-Inquiries,” and “Seminar on Sociolinguistics.”<sup>36</sup>

Rose spent the Spring/Summer period of 1980 as Visiting Professor, Johann Wolfgang Goethe-Universität, Frankfurt am Main. Throughout the Nineteen-Eighties he travelled in Europe and the United States presenting conference papers, and ethnographic evaluations or consultancies for private companies. For example, he conducted consultancy research for telecommunications companies on the use of general and specialised telephone directories (Rose 1986a; 1986b; 1990). In these “professional ethnographies”, Rose returns to some of the methods used in the “Larimer Street” studies, which were conducted for the Denver Urban Renewal Authority. The area in question was subject to redevelopment in an urban renewal project that would disperse the Skid Row residents around the city. In September 1989, the University of Colorado marked Rose’s career and research activities in a celebration entitled “Six Decades of Research and Study in Sociology, Sociolinguistics and the Ethno-Inquires” at the Norlin Library.

The years 1992 and 1993 saw the publication of two of his most significant works to date, *The Werald* and *The Worulde* respectively. These are companion volumes that form a synthesis of his expository and empirical writings, and in which Rose realises

the datum for worldly inquiry: the worded world. Within these works, Rose reconciles those ways of thinking and ways of explicating “society” which have been predominant at various stages in his life. Included in these *modi operandi* are the diachronic and etymological analysis of the English language, the history and development of words and their meanings. The *Oxford English Dictionary* constitutes a record—the “English record”—of the history and meaning of words. The English record documents the meanings and constructions of a natural sociology.

To Rose, the world is complicated by the theorist who provides an ironic gloss of people’s activities. Until then, people themselves are well aware of what they are doing. This lack of coherence between professional sociology and members’ orientations is why Rose argues, and has consistently argued, that the primary and ultimate recourse for sociological inquiry is the world, where the world is constituted by the words of people themselves.

In Summer 1994, at the University of Colorado, he gave a course “Field Experience in Sociology,” run with Fred Templeton, which students attended concurrently with Lennard Pinto’s course “Social Action Internship.”<sup>37</sup> Later that year, in the Fall, he was Visiting Emeritus Professor of Sociology at the University of Manchester, UK. During this time he gave lectures and seminars, to staff and students of all levels, speaking with people in groups and on an individual basis.<sup>38</sup>

### ***Consistencies and Continuities***

What may seem *prima facie* discrete stages in Rose's work may be more accurately described as a "seamless web". For example, some procedural antecedents of his famous paper "The English record of a natural sociology" (Rose 1960a) may be found in an early paper (Rose 1948) on moments, or cultural "innovations." Furthermore, antecedents of this earlier paper on sites and distributions of cultural traits, and much of his later work, are visible within his doctoral thesis (Rose 1942). His thesis constitutes a radical critique of "comparative" forms of anthropology (and sociology) in demonstrating how comparative studies involve, *au fond*, practices of classification devised and imposed by analysts themselves. For Rose, comparative analysis

"may be regarded as an attempt to establish classes and types by techniques which avoid as far as possible specifications to which little in the concrete world corresponds."

(Rose 1942:9)

Rose criticised comparison of cultures which were based on identifiable cultural traits, e.g. arrowheads and basket-weaving. Particular cultural traits were not necessarily representative of particular cultures, a difficulty exacerbated by the numbers and sizes of cultures purportedly available for comparison with each other. The identification, recognition and selection of cultural traits by members themselves would produce cultural classifications that were more sensitive to the culture than analysts' manipulations. Laboratory studies of culture, such as Rose and Felton (1955), can thus be regarded as attempts by Rose to identify—in an adequate rather than "arbitrary"

(Rose 1942:3) manner—the way in which cultural traits are lost, maintained and transmitted.

The use of the “culture record” to identify the provenience of cultural innovations, i.e. small events which perhaps have consequences for the many, obtains inquiry into temporal and locational “culture centres” (Rose 1948:256-7). Culture centres are identifiable sites or sources from whence innovations derive. The culture record can also provide for the where-and-when aspect of linguistic and cultural derivations and developments, i.e. diachronic analysis. Throughout his career, Rose elaborated upon his inquiries using diachronic analysis. This served invaluable ‘underlabourer’ work, as it provided him with the logical grammar of moving from the substantive ‘what’ to the procedural ‘how’, e.g. a study of “changes of theory” (Rose 1985).

The establishment of innovations (Rose 1948) is expressed visibly throughout various projects over time: histories of cultures (Rose and Felton 1955); contours of cultural activities (Rose and Willoughby 1958); on new descriptions, lay histories of collective representations and cultural values (Rose 1960b, Item #19); through diachronic and etymological considerations of the “staples” of sociology (Rose 1960a, Item #20; and “Changes of theory”, Item #113), which demonstrate how technical definitions are methodologically ironic descriptions of and departures from shared understandings; analyses of shared understandings or “cultural uniformities” (“Uniformities in culture: ideas with histories”, Item #24); the production of new languages (“Small Languages” projects, Items #32-33, #48, #50-51); on connections between radicals and roots

(“Commonplaces in the roots of language”, Items #55-56);<sup>39</sup> and in comparative studies of (ancient and modern) languages (Items #100 and #146, discussed in “The Snooker Room”, Item #142).

Diachronic analysis is secondary to the retrieval of members’ own understandings or “ethno-ontologies” in the Larimer Street studies. D.R. Watson, in his prologue to the reprinted version of *The Unattached Society*, observes that although diachronic analysis is “downplayed” in the Larimer Street studies, it “could still be used to supplement study in some salient respects” (D.R. Watson 1997:viii). A provisional attempt is made to instate diachronic analysis in a paper on begging (Carlin 1999). Diachronic and etymological analysis re-enter Rose’s work on *theoria* (see the bibliography of Rose’s works in the next chapter).

A common theme between the Small Languages and Larimer Street projects is the emphasis on people’s own definitions of situations, and how they so define them. Just as Thomas and Znaniecki (1958a and b) were obliged to devise a novel, i.e. non-statistical approach to social research in order to investigate their problem, Rose too was required to start afresh in order to preserve people’s own judgements and expressions—as equal to and as vital as professional sociological judgements—within sociological study from the outset. In a paper delivered to the American Sociological Association (Item #51), Rose provided a position statement which encapsulated both the reasoning for and the outcomes of his work:

“When the views of persons are taken seriously into account and treated as natural sources of both materials and theory in sociology, then this general proposition becomes paramount: persons know and can tell what they are doing in society and with culture. [...] And it has its ironic contrary upon which so much sociology is based, namely that persons do not know what they are doing, that they are, to use Garfinkel’s phrase, cultural dopes.”

(Rose 1967b:6)

Thus this statement attests to a direct overlap of the Small Languages and the Larimer Street projects: an explicative rather than methodologically ironic stance towards members’ understandings of those societies of which they are members.

The internal coherence and consistency of purpose between his projects demonstrate commitments to “ethno-ontologies,” to the recovery of people’s understandings of words and things:

“the cultural scientist makes deliberate use of [the humanistic] coefficient, of impressions of the outlooks of others, so as to recognise ethno-ontologies and ethno-ontographies.”

(Rose 1962:174)

The thoroughgoing emphasis on “worldly inquiries” and his exhaustively praxiological orientation to how people—including “human scientists”—make sense of their worlds in ordinary as opposed to technical language, constitute expositions of what Rose calls a “natural sociology.”

In his paper “Waiting for the World to Walk By” (1994, Items #138 and #144), Rose illustrates Sacks’ notion of the “viewer’s maxim” (Sacks 1974a:225) with an articulation of membership categories, which are commonly available to people “at a

glance” (Sudnow 1972:259) in the visual ordering of the world. The attention to visually-available categories shares commonalities with other inquirers’ emphasis on the “social organisation of visibility arrangements” (Lee and Watson 1993).<sup>40</sup> Members are assumed to be competent observers of the social world, and Rose’s presentations show how incumbencies are “*categorially visible*” (Lee and Watson 1993:5; emphasis supplied). Again we may recognise the thematic consistency in Rose’s analytical concerns as well as overlaps with Sacks’ work. Rose is emphasising how the attitude of the individual *qua* analyst is inextricable from the attitude of the individual *qua* member. For all individuals, “lay persons” and analysts *similiter*, “a wide range of categories is visually available, or inferentially available from visible indications” (Lee and Watson 1993:81). Rose shows how by simply noticing two people, an adult and a child, as a “noticeable pair” (Rose *ibid.*) as they walk past him, he selects categories from the Membership Categorisation Device “family” (Sacks 1974a:219) in the actual *in situ* description of the visual field. (Rose’s observations and categorisations are returned to in Chapter Ten.)

Rose’s observations on his sitting on a railing outside Uni-Hill Elementary School (situated opposite the University) before giving a class, “waiting for the world to walk by,” are reminiscent of his 1933 experience witnessing Paul Radin “doing ethnography”. Rose watched and listened to Radin talking with the leader of a Pomo tribe as they were seated on a heavy timber fence (Rose, personal communications, 21 November 1996, 28 March 1997). Rose regards this incident as his doing ethnomethodology without realising it:

“Without noticing what he was up to, there Rose started to do what came to be called Ethnomethodology.”

(Rose 1992:21)

Similarly, Graham Watson reported to me (personal communication, 16 January 1996) that he became involved in “doing” ethnomethodology unknowingly, and independently from developments in the United States, through his participant observation in a White school in South Africa (G. Watson 1970). During the course of his researches he encountered the tacit management of racial identity—by children, families, and schools—and the involvement of non-White individuals and families “passing” as members of White communities. G. Watson found that Whites were ongoingly engaged in *ad hoc* decision-making activities, and “pass-Whites” were ongoingly involved in the work of passing.

The implicativeness of these occurrences is to bring our attention to the ordinariness of human activities, the salience of people’s ordinary descriptions of these activities, and the importance of “vantage points and the purchase that vantage points provide ... for knowing humanity” (Rose Item #144 *op. cit.*). That is, observation of people’s ordinary activities and listening to people’s descriptions.

“Observing Radin sitting on a fence with his ‘informant’, Rose realised that when all the technicised idealisations of ‘research method’ were stripped away, fieldwork consisted, in the final analysis, of mundane conversation occasioned in utterly casual and contingent ways.”

(D.R. Watson 1997b:vi-vii)

This is not to say that fieldwork, or ethnography, is reducible to conversation. The point of this is the recognition that ethnographic and fieldwork activities are embedded within ordinary, everyday activities, i.e. they are *linguistic* activities.

As investigators of the world, we must watch and listen to people in the world. This distilling of the approaches to social or worldly inquiry into its fundamentals or “simples,” watching and listening, is a formulation of the doing<sup>41</sup> and saying parts of the “commentary machine” metaphor (Sacks 1963).

Indeed, the generic nature of membership categorisation activities is exhibited in (*pace* Kurt Weill) “street scenes.” Just as persons may move through a “categorial career” (Lee and Watson 1993:83) in a Parisian market, there exist multiple, mutable categories which are displayed in talk on Larimer Street by the men on Skid Row and people who have to do with these men. As such, Rose secures the linguistic constitution of the Skid Row residents and categorial determinations of self.

A policeman on Larimer Street variously categorises a man on the street according to familiarity with the man (knowing his face and name), the man’s criminal (if any) and work histories, place and length of residence on skid row, and activities on the street. As one police officer told Rose, “I know [men] by names and by actions” (Rose 1965d:39). A policeman’s categories for men on skid row include man, new man, younger man, old timer, drunk, nobody, bundle stiff (man who carries a rolled blanket), thief, or working stiff (man who takes jobs). Some categories reflect activities which

are defined as being criminal, e.g. vag (vagrant), in distinguishing residents (“permanent citizen”) and non-residents, e.g. people outside, average citizen, tourist (person visiting Skid Row for entertainment), and the exigencies of the regular dealing with persons who are under the influence of drink, e.g. John Doe.

A habitué of Denver’s Skid Row instructed Rose how the men and police use the same membership categories in their organising the street. A man may be described by other men as, *inter alia*, an alcoholic or ex-alcoholic, or a bum—whose activities categorially distinguish him from a tramp:

“a bum is a person that makes his living by bumming. A tramp is a person that goes from place to place but will work once in a while”  
(Rose 1965c:67)

The problems that these transferable identities have for social scientists in counting the incumbents of stipulative, *ad hoc* categories are not problems for the very people who find themselves the object of social study:

“The men aren’t waiting around for social scientists to tell them who they are and what they are like”  
(Rose 1965a:16-17)

In counting the heads on Larimer Street, the social scientist has to rely upon the categories that are supplied by the men and those who work with them. It is even more complicating for the social scientists’ schedules that these

“categories and identities of social types are forever being formed, confirmed, displayed and then confounded, violated and changed”

(Rose 1965a:17)

A “slippage in categorial identities” (D.R. Watson 1997b:xi) is experienced by the men on the street in the shifting of their assigned identities as they move from mission to flop-house, from bar to bar, and from booth to stool within the same bar. Rose’s attending to categorial fluctuations as a topic of inquiry, and ensuring that these categories are provided by members themselves, elides the tension between analytically static coding structures and the fluidity of members’ actual categorisations, of which the Larimer Street scene is a perspicuous example.

### ***“Larimer Street”: Skid Row Ethnographies***

The “Larimer Street” study is an axial reference in this thesis. This ethnography of a “skid row” district is an early example of linguistic ethnography. Rose devised a methodology which used people’s ordinary language for the purposes of doing ethnography. The language used by those people encountered in the study was part of Rose’s project.

In 1965, Rose directed a team of researchers (including Gary Barnett, Egon Bittner, Jon Driessen, Tony Gorman, Rolf Kjolseth, Frank Leuthold, John Collins O’Leary and I. J. Singer) in an investigation into the Skid Row district of Denver, Colorado. The area in question was subject to redevelopment in an urban renewal project that would disperse the Skid Row residents around the city.

The corpus of inquiries known as the “Larimer Street” studies is exemplary of Rose’s commitment to the privileging of members’ accounts. Rather than crediting social scientists with expert knowledge of social settings, Rose shows that the authorities on the life of the men on the street are the men themselves. One of the key informants for the Larimer Street studies, John O’Leary (“Johnny the Reader”) joined the research team; as a resident of Larimer Street and an expert on the life on Skid Row, O’Leary was able to introduce members of Rose’s research team to other informants, and provide insights on the adequacies, capabilities and mundaneities of the life.

Almost thirty years later, this procedure was recovered by Reyes Ramos, one of Rose’s students in a study of drug abuse. In the collaborative research process with members of a researched community, Ramos was assisted by researchers who were recovering drug addicts. His research assistants had access to settings and informants, were familiar with the “technical,” i.e. locally-known and used, argot of the “tecato” community, and possessed expert knowledge of drug practices (Ramos 1995:5).

The Larimer Street study is a classic example of qualitative applied social research. This intensive linguistic ethnography is capable of providing practical recommendations, which have been made available by the inquiries with such persons involved in the life of Larimer Street, and are locatable in the records of these inquiries.<sup>42</sup> The recommendations have their provenance in these first-person accounts, and attend to the consequences for persons and the city’s ecology *apropos* of any attempted movement of Denver’s Skid Row (Rose 1965a:116-137). Remaining firmly

within the province of members' categories, which set the parameters of the study, ensured that recommendations would be sensible and meaningful for both subjects and users of the report, the residents and city planners.<sup>43</sup> There exists clear delineation between members' *in situ* and investigators' *ex situ* organising practices. The very categories used by the Larimer Street study are those which are employed by Skid Row residents. The use of first-person accounts in social inquiries was not without precedent; nor was an awareness of problems with the transformation of an interviewee's language into an interviewer's language (Shaw 1930:21-22). However, the Larimer Street study was committed to the *in toto* preservation of ordinary persons' avowals or definitions. The terms and categories of use were the prerogatives of members, not analysts.

Not all the appendices from the Larimer Street project were transcripts of talk. Zimmerman and Wieder (1977) refer to a research procedure using a diary as an "annotated chronological record or 'log'" in which

"[i]ndividuals are commissioned by the investigator to maintain such a record over some specified period of time according to a set of instructions"  
(Zimmerman and Wieder 1977:481)

This procedure is exemplified by the research team on Larimer Street, who provided notebooks and instructions to men on the street:

"Each man was allowed to write what he chose, given the one rule that he would try to produce a running account clocked through the day"  
(Rose 1965f:1)

Generally, however, the materials taken from Larimer Street were tape-recordings of talk with its residents. During one month, June 1965, the inquirers on Larimer talked with over a hundred men on the street, recorded and then transcribed the talk. As Rose said about the Larimer Street inquiries,

“We hoped to find the world’s account right there on [Larimer Street] – according to what we had heard and could capture on our taping machines.”  
(Rose 1995:183)<sup>44</sup>

### **Affinities: The “Voiced Revolution”**

Rose’s “Small Languages” studies, “Larimer Street” studies and, *inter alia*, reports on drug scenes (Item #94) and communicative technologies (Items #116-119) are committed to the presentation of “retrievable data” (Mehan 1978:36). Retrievable data refers to audio and video-recordings of actual worldly events, and such transcriptions from recordings that capture the interactional nature of these activities: the use of the tape-recorder secures cultural productions as “matters of record,” and therefore as “objects of inquiry” (Rose 1967b:3). Through this *modus operandi*, revealing the transparency of materials by making public what conventional ethnographies consider to be residua, facilitates and encourages readers’ cross-referencing of worldly materials at hand and research outcomes. This commitment is exemplified by the “supplementary materials” to *The Unattached Society*, and the rigorous cross-references afforded to them (Rose 1965a *passim*.). This “analytic mentality” referred to in the previous chapter, that the adequacy and coherence of outcomes be able to be adjudged by other constituencies, displays a elective affinity between Rose and Sacks.

The “ethnographics” of Rose’s projects, the work of realising the endogenous, linguistically organised, reflexively-tied “meaningful matters” of settings, i.e. matters meaningful and relevant to members from within those settings, makes available ethno-ontologies which are principal topics of the ethno-inquiries.<sup>45</sup> Taken as a whole, the “Larimer Street” ethnography is a summation of his commitments to securing people’s ordinary language practices, the presentation of retrievable data, and giving paramountcy to people’s definitions of situations in determining analytic categories.

The methods of data collection were mediated by the tape-recording of men on the street and people who had dealings with them. Rose regards the application of the tape recorder as a significant departure for ethnography, referring to it as the “voiced revolution”.<sup>46</sup> This ensured that the words of the experts—people themselves—would not be re-written by the “social scientist” who claims to have expertise of what is already a world known-in-common. To invoke the concerns of his early works, we may consider the first use of the tape-recorder in ethnography to be a “cultural innovation” (Rose 1948).

Rose shares with Sacks a thoroughgoing engagement with the world, with members’ common-sense linguistic practices. Rose’s language game is conferred by members, rather than moves which oppugn members’ definitions through the methodologically ironic use of members’ categories *qua* analytic resources.

The status of conventional sociology as a serious and valid study of society is made problematic in its usage of words and numbers. The reliance upon “unquestionably abstract social categories” (Blumer 1956:684), which already carry lay definitions prior to any attempted redefinitions as technical concepts (Rose 1960a:134), renders conventional sociology vulnerable through its failures to explicate its own logical assumptions and consider within its terms of reference what may or may not be legitimate topics for inquiry. In not recognising that

“almost all of the established technical vocabulary of the discipline of sociology has been taken over from the common stock of words in ordinary languages”  
(Rose 1960a:134),

conventional sociology relinquishes its claims to analytic rigour by using common-sense terminology as unexplicated resources for its “professional” studies. By the nature of disciplines such as anthropology and sociology, researchers are ongoingly engaged in

“studying the categories that Members use, to be sure, except [...] they’re not investigating their categories by attempting to find them in the activities in which they’re employed.”  
(Sacks 1989:254)

### ***Conclusion***

This chapter has provided an intellectual profile of Edward Rose, where biographical materials are presented as “coat-hangers” for bibliographic explication. The following chapter consists of an extended bibliography of Rose’s work, much of which, as I said

at the beginning of this chapter, provide the bases for this profile. In considering the nature of bibliographies and biographies, these chapters complement each other.

These extended discussions on Rose are necessary in a thesis on the linguistic constitution of research practices, because he has initiated such a variety of language-based inquiries. Rose's inquiries demonstrate that research is conducted through language, i.e. is linguistically constituted; and that the understandings of language by members are anterior to the appropriations of language by social scientists.

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<sup>1</sup> See the special issue edited by Liz Stanley and David Morgan—"Auto/Biography in Sociology"—*Sociology* 27 (1) (1993).

<sup>2</sup> Although the "unique adequacy requirement" is a notion used with reference to the expert competence of the observer, I am bringing it to the writing of biographies and providing an alternate approach in support of Winkin's considerations.

<sup>3</sup> "Influence" does not imply agreement. As I indicate below, Rose would have disagreed with the one of his teacher's when he wrote "Culture, as used by American anthropologists, is of course a technical term which must not be confused with the more limited concept of ordinary language and of history and literature" (Kluckhohn 1972:42).

<sup>4</sup> Rose encountered Dewey's work through his mother, Winifred Higbee, who had been a student of Dewey. Rose intended to publish her original notes from Dewey's lectures; however, they were presumed lost or stolen. On a visit to the Norlin Library Archives at CU Boulder, May 1998, I found these surviving notes in a protective box. Rose intends to publish these notes in the future: *vide* the bibliography in the next chapter, Item #171.

<sup>5</sup> Personal communication, June 12 1996.

<sup>6</sup> Initially reported at a Meeting of the Western Psychological Association. Although the paper itself is lost, the abstract is published in the WPA Proceedings, *Psychology Bulletin* 1934 (31):601. *Vide* Item #3.

<sup>7</sup> The citations to these Works Progress Administration "rural relief" studies are listed in R. E. Rapp (1937) *Some Aspects of the Rural Relief Problem in California as Revealed in Ten Selected Counties* Unpublished Ph.D. Dissertation, Department of Economics, Stanford University. Copies of the rural relief studies are held at the National Archives and Records Administration, Washington DC 20408.

<sup>8</sup> It is possible that surviving copies of some of these reports are stored on microfilm in archives at The Library of Congress (LoC personal communication, 5 July 1996). Federal Emergency Relief Administration (FERA) records are found in the California State Archives.

<sup>9</sup> Read about Rose's "prosaic adventures" as a soldier, on artwork, and on the exigencies of the production of a daily newspaper, in *The Great War Brought To Algiers By Edward Rose*; and in *The Great War Brought To Rome By Edward Rose* (Items #148 and #149 in the next chapter).

<sup>10</sup> Winkin (1999:31) observes that shortly before this, Dennis Wrong mooted to Erving Goffman that he move to the University of Toronto. Winkin's observation relates to the nature and extent of influence of people on others, *viz.* personal and/or intellectual; and how influence is so categorised by biographers.

<sup>11</sup> Rose was at war when his artwork was first viewed in public. A dedicated exhibition took place in 1942, at the Paul Elder Gallery, San Francisco. More paintings were displayed in group exhibits in 1943, at the California Annual Watercolor Exhibit, San Francisco; and at the 1947 Invitational, Canon City Art Society, Colorado. (Correspondence relating to his artwork and to the exhibitions can be found in *Algiers* and *Rome*, where some of Rose's works are reprinted.) He and his wife, Evelyn, participated in the same artistic community in San Francisco.

From 1981 to 1984, some of his Cibachrome and Mylar prints were exhibited individually in Frankfurt am Main, Kassel, Tuscany, and Colorado; and at a six-person Invitational at The Station Terminal Art Space, Boulder, Colorado. A limited number of grey-scale prints of these works, accompanying the writings by William Blake and Ludwig Wittgenstein, appear in small editions. (1991) "Paintings and Prints." (1991) "Worlds Of Visionary Fancy." (1992) "The Spirits Of Ancient Kings." (1992) "Τηε Σπιριτο Οφ Ανχεντ Κινγσ." (1992) "The Gentle Lamentation Of Thel." (1992) "Philosophical Investigations With Pictures." (Items #121-126 in the dedicated bibliography). The production of these particular prints, whilst Rose was in Frankfurt, is documented in a journal—"The Graefs of EROS" (1981, Item #91). Doing painting and the adequacy of the artist's work in general is discussed by Garfinkel and Rose (1978, Items #72 and #155). Commentaries on the prints are found in exhibition notes (1981) "Sights in the Worlds of William Blake," Item #95, University Programme Council; (1981) "Graefs, A Statement Accompanying Exhibitions"; "Art." Audio-taped lecture given to students at the University of Colorado at Boulder, July 20 1994 (*vide* Item #137). See the bibliography in the following chapter for the itemisation of some of Rose's artwork.

<sup>12</sup> The "Small Languages" project is explicated in detail in Slack (1996). This profile sets out the Small Languages as part of the continuum of his work. From Rose's corpus, however, the thesis outwith the profile concentrates on the "Larimer Street" study and ethnographics as they pertain to the linguistic constitution of research practices.

<sup>13</sup> The full title of this oral presentation is "The Presentation Machine. A Conversation with Harvey Sacks: An Analysis with Changes and Connections." This is a comprehensive statement on the Ethno-inquiries. An extract of this paper appears in *The Herald* (Rose 1992:324:341). A French translation is published in full by Alain Coulon in *Cahiers de Recherche Ethnométhodologique* (Rose 1993). The title of the French translation is incorrect: it mistakenly presents the title as "Conversation avec Harvey Sacks: Analyse avec Modifications et Corrections" rather than "Connections". This is both a mistake and a misnomer: Rose is explicit that he is not "correcting" Sacks.

<sup>14</sup> Limiting the parameters of comparative study in terms of manageability and analytic sufficiency is returned to in a long-term project, the exposition of worldly signs, in different languages; *vide* Item #146.

<sup>15</sup> Rose quoted this clause from Tylor's definition, from his *Primitive Culture*, in several of the "small languages" and "small cultures" reports.

<sup>16</sup> During the "Manchester lectures" of October 1994, Rose spoke of the reaction to his laboratory studies of culture. Garfinkel had been critical of the laboratory demonstrations of culture histories (Rose and Felton *ibid.*), which initially caused "friction" between them and was the beginning of their professional collaborations and personal friendship. Rose and Garfinkel actually met at the 1957 conference, where Garfinkel read his "Uniformities in Culture" paper (Rose 1962), which Rose presented at the Second Conference the following year. Rose argued against Garfinkel that the laboratory studies were not simulations, but experiments designed to uncover properties of natural language and natural language learning (Rose, 19 October 1994, Item #140). Again, this returns Rose to Tolman's interest in learning theories and the development of language.

<sup>17</sup> Some materials are reported in Takahara and Rose (1978); *vide* Item #74.

<sup>18</sup> See the section on stenographic records in the Chapter Seven.

<sup>19</sup> *Vide* the asterisked footnote on page 193 of the (1960) version of "The English Record"; page 131 of the reprint (1962).

<sup>20</sup> This is quoted from the legend on the offprint of "The English Record" and "Uniformities in Culture" (Washburne 1962).

<sup>21</sup> An adumbrated version of this acknowledgment is reprinted in Garfinkel and Wieder (1992:203-204).

<sup>22</sup> Including H. Garfinkel (1963) "A Conception of, and Experiments with, "Trust" as a Condition of Stable, Concerted Actions." O. J. Harvey (Ed.) *Motivation and Social Interaction* 187-238. New York: Ronald Press, p. 187; H. Garfinkel (1967) *Studies in Ethnomethodology*. Englewood Cliffs, NJ: Prentice-Hall (see the Preface and Chapter Two).

<sup>23</sup> Correspondence between Rose and Garfinkel at the CU Archives indicates the extent of this role.

<sup>24</sup> Bittner (1965a).

<sup>25</sup> E.g. Hill and Crittenden (1968).

<sup>26</sup> Sudnow was also a participant in Hill and Crittenden (1968) (the "Purdue Symposium").

<sup>27</sup> Rose and Cicourel first met at sociology meetings in Seattle in 1958.

<sup>28</sup> "Behavioral" was used instead of "Social" because the social, physical and biological sciences fell under the *aegis* of the Arts College. This innovative reclassification of "human" and "natural" sciences later became the Institute of Behavioral Science through a typographical error (*vide* Item #140).

<sup>29</sup> *Vide* Rose and Gruber (1960); Item #21. This technical report sketches proposed layouts for the Institute.

<sup>30</sup> For accounts of one such presentation at the Suicide Prevention Center, Los Angeles, with Harold Garfinkel, Erving Goffman, Talcott Parsons, Edward Rose, Harvey Sacks, with Edwin Shneidman presiding, *vide* Items #120 and #139. (This second recording was referred to as "the quiet room" in a subsequent conversation, which took place in the snooker room adjacent to the Senior Common Room, University of Manchester (Item #142). Hence the labeling of conversations as "The Quiet Room" and "The Snooker Room".)

<sup>31</sup> According to a progress report (Rose 1964, Item #30) on research to the funding agency, regular and occasional participants in events funded by this grant included Egon Bittner (then University of California Medical Center, San Francisco); Lindsey Churchill (then Department of Sociology, UCLA); Marvin Cummins (then Department of Sociology, CU Boulder); Harold Garfinkel (then Department of Sociology, UCLA); Erving Goffman (then Department of Sociology, UC at Berkeley); Evelyn Hooker (then Department of Psychology, UCLA); Henry Lennard (then Bureau of Applied Social Research, Columbia); Craig MacAndrew (then UCLA Medical Center); Saul Mendlovitz (then the Law School, Rutgers University); Martin Orne (then Harvard University Medical Center); Talcott Parsons (then Department of Social Relations, Harvard University); Edward Rose (then Department of Sociology, CU Boulder); Harvey Sacks (then Department of Sociology, UCLA); Emanuel Schegloff (then Department of Sociology, Los Angeles State College); Edwin Shneidman (then Suicide Prevention Center, Los Angeles); Marvin Scott (then Department of Sociology, UC at Berkeley); Charles Slack (then Department of Psychology, Brooklyn College); Anselm Strauss (then University of California Medical Center, San Francisco); David Sudnow (then University of California Medical Center, San Francisco); Laurel Walum (then Department of Sociology, Los Angeles State College); and Carl Wertham (then Department of Sociology, UC at Berkeley).

<sup>32</sup> At the time of its submission (December 19 1961), Sacks was a Research Fellow and Ph.D. candidate at the Center for the Study of Law and Society, UC at Berkeley. The breakdown of research interests, in the present continuous tense, suggests his paper "The Lawyer's Work" was then in progress. This supports the editorial note on Sacks' paper by Emanuel Schegloff (in Sacks 1997:47-49). However, in an introduction to Sacks' *Lectures* (1992a:xv), Schegloff mistakenly attributes Garfinkel as the co-principal investigator on this research proposal: the principal investigators were actually Rose and Ozzie Simmons.

<sup>33</sup> Rose makes the following arguments *re* Herodotus and Columbus in a draft version of *Be Fine* pp.182-183. This draft incorporated both Gorman's interview with Anna B. (Item #157) and observations by Rose and Watson (Item #163). This draft was titled *Be Fine: The Great American Epode of Anna Brown* (Rose 1995).

<sup>34</sup> Rose uses this term in a lecture on Ethnomethodology at the Department of Sociology, University of Manchester (October 20 1994).

<sup>35</sup> Participants with whom Rose was principally involved with at these conferences include Harold Garfinkel, William McCord, Anatol Rapoport, William Scott and Michael Wertheimer. The outcomes of these conferences are published in the Willner and Washburne collections. As mentioned earlier, the 1957 conference marked the first meeting between Rose and Garfinkel following Garfinkel's critique of Rose and Felton (1955).

<sup>36</sup> Recordings of many of these extemporaneous lectures are stored in the Edward Rose Collection at University Libraries, CU Boulder. A draft catalogue of the Archives which have been formally processed so far, under the supervision of Cassandra Volpe, is available. The Edward Rose Collection

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is provisionally listed in Fehr and Stetson's (1990) bibliography of Ethnomethodology and Conversation Analysis.

<sup>37</sup> L. Hughes, personal communication, 30 March 1997.

<sup>38</sup> Rose's first-hand knowledge of W. I. Thomas and his work informed the writing of the late Alexandra Korol. Rose confirmed what some regard as an apocryphal story about Thomas' finding letters in a garbage can in a detailed and lengthy conversation with Alexandra, which—as I remember—began after lunch in the Senior Common Room. For Thomas' recollection of the event, see Baker (1973:250). For statements by Thomas, see Blumer 1939. Also Rose's audiotaped seminar—Item 140, Chapter Three.

<sup>39</sup> This paper is an unfinished review of connections between "radicals," roots through root-senses in Indo-European languages, and connections between radicals through roots. Rose was forced to abandon work on this study because the computer he was using at the time could not cope with such large tables of data. (Rose, personal communication, 3 April 1996.)

<sup>40</sup> The series of studies contained in Lee and Watson (1993) constitute "respecifications" of Ryave and Schenkein's (1974) study on solving the "navigational problem"; the respecifications are in terms of membership categorisation analysis.

<sup>41</sup> Rose (1991) notes that in Sacks' original version of this paper, the commentary machine had a "moving" part, which was written up as the "doing" part for publication in a journal administered by graduate students, and edited by Fred Templeton.

<sup>42</sup> *Vide* the supplementary materials to *The Unattached Society*. Additional material or "supporting documents" were published along with the final report; Rose enjoins readers to consult these documents, arguing that they are "far more intriguing, word for word" than the finished report for the Denver Urban Renewal Authority because the appendices contain "ideas actually expressed by the men of Larimer Street and by persons who know these men best" (Rose 1965a:ii).

<sup>43</sup> A response to the challenges of the Larimer Street study is found in an editorial comment of *The Denver Post* (1965), which concludes that following Rose's study "no one can plead that the facts on which to base wise action are not at hand, in clear and readable form."

<sup>44</sup> This is quoted from the draft version of *Be Fine: The Great American Epoche of Anna Brown*, referred to above in the section on Rose's relation to the developing ethnomethodological inquiries (Rose 1995).

<sup>45</sup> Rose used the term "meaningful matters" to gloss whatever those things may be which members themselves, rather than analysts, realised, recognised, found relevant or important (Item #142). See next.

<sup>46</sup> Again, this term was introduced by Rose in "The Snooker Room" (Item #142). See previous note.

## The Works of Edward Rose: An Analytic Bibliography

This chapter presents an extended bibliography as a companion analytic source to the intellectual profile in the previous chapter. As this is the first comprehensive bibliography of Rose's work, considerations of the criteria, considerations, procedures and relevances involved in the compilation of this bibliography are particularly warranted and requisite. The purposes of these introductory notes are to provide explanations of the format of the bibliography and practices of its compilation, rather than annotations to the bibliography. The modest aims of the bibliography itself are to provide greater access to Rose's *œuvre*, and to disseminate awareness of Rose's work more widely.

Although Rose's work addresses generic issues in naturalistic sociology, as shown in the previous chapter, the lack of widespread knowledge of his work has restricted the interest and awareness of its compass. The bibliography presented in this chapter delineates a field within sociology, which constitutes a corpus of methodologically non-ironic inquiry. This chapter, then, makes a contribution to social-scientific communities by compiling this bibliography and making his work accessible.

This free-standing, analytic bibliography<sup>1</sup> is a unique resource for study for the cohort of scholars familiar with Rose's Ethno-Inquiries approach and related disciplines, including Ethnomethodology; and for those encountering his work for the first time.<sup>2</sup> This is not to suggest that all ethnomethodologists are aware of Rose's work, as the corpus of literature or items from it which he has produced is referred to relatively infrequently.<sup>3</sup> Nor do I

mean to imply that awareness of Rose is limited to ethnomethodologists: authorities from various fields recognise his importance. For example, Donald W. Ball used Rose's work on the diachronic bases of sociology extensively, and regarded his work on "natural sociologies" as "an undeservedly neglected paper" (1970:330).<sup>4</sup> It is thus another case of an oriented-to or, in Sacks' terms, "noticeable absence".

Awareness of Rose may derive from "Rose's Gloss" (Garfinkel and Sacks 1970:366), or his renowned clarifications and explications of Sacks' (1963) "Sociological Description" paper,<sup>5</sup> which were originally presented to a conference on Ethnomethodology and Conversation Analysis.<sup>6</sup> However, it would be a mistake to regard Rose solely on the basis of his "English Record" paper, or in terms of "Rose's Gloss": such synoptic assessments neglect the acuity and perspicuity, diversity, quality, breadth and conceptual depth of Rose's output. This analytic bibliography helps to disseminate Rose's work *as a corpus*, as well as bringing it to a larger audience within academic communities. The extensiveness of the bibliography provides readers with access to programmatic writings on Ethno-Inquiry and exemplary ethnographic studies. It will facilitate the recovery and further study of methods which attend to people's definitions of situations.

### ***Compiling the Bibliography: Notes on the Presentation of Materials***

The procedure adopted in compiling this bibliography has been to present materials in chronological rather than alphabetical order. The model for this form of presentation is Emanuel Schegloff's "provisional" or preliminary bibliography of Harvey Sacks' published writings (in Sacks 1985:22-23). However, this bibliography is distinct from

Schegloff's bibliography of Sacks' work because it includes unpublished papers and reports, presentations, lecture series and recordings.

Undated manuscripts are referenced and located in probable chronologies, rather than any definitive order; these editorial decisions are based on *curriculum vitae* and citations. In this respect, the bibliography presented here can be regarded as constituting a form of “intellectual archaeology” and “intellectual biography”. Consequentially, certain undated manuscripts, e.g. Item #15, have been situated alongside or within various trends manifested by Rose's work, i.e. aligned with work which expresses a “family resemblance”; and with professional activities with which he was involved at different times, e.g. Item #47. To reiterate, such judgments are not incorrigible but indicative and may be subject to debate in future considerations of Rose's analytic work. An editorial policy which emerged from compiling this bibliography has been to identify work within what, in the previous chapter, I referred to as the “natural history” of materials. Work is situated within approximate periods of his biography.<sup>7</sup> The biographical materials contained within the previous chapter are thus reference points for bibliographic materials.

The guiding principle in compiling this bibliography has been to include as many clarified references as are available.<sup>8,9,10</sup> In one sense this elides the dilemma of the inclusion/exclusion of material;<sup>11</sup> however, there are the serious matters of significance and relevance of bibliographic items to consider also. Not to include presented papers, for instance, would expunge a large corpus of critical literature; similarly, not to list

unpublished manuscripts or books brought out by independent, quasi-private (and other) publishers<sup>12</sup> would arguably omit some of Rose's finest work.<sup>13</sup> In treating items in the bibliography as *aequales*—as having equal significance in analytic terms without making judgements about which works should take precedence over others—intends to impress upon readers the importance of, and propinquities between, materials herein.

Whilst bibliographies assembled by Rose and under his direction are included,<sup>14</sup> an editorial decision was made regarding the inclusion of administrative documents: reports from and transcripts of committee meetings have not been included. An exception to this editorial practice is the inclusion of a response to a ruling by an ethics committee, which appertains to Rose's insistence on inquiries through observation of, and talking with, people.<sup>15</sup>

This bibliography does not categorise work into subject areas, epochs or whether published, in a conventional sense, in journals or as book chapters. In this bibliography, I adopt a neutral stance towards major or minor publishers, and private or corporate publishing houses, desktop and electronic publishing—categorisations that animate academics increasingly. In keeping with the methodological nature of this thesis, I am indifferent to such categorisations and distinctions.

Classifying documents into published and unpublished works is a problematic activity. As an illustration of the equivocation between published and unpublished work, the *Bureau of Sociological Research* and *Program on Cognitive Processes* reports were

circulated and reviewed, being well-received in professional journals and elsewhere.<sup>16</sup>

Ray L. Birdwhistell noted that the sessions bringing together the “small languages” of Sez and Pique, *A Looking-Glass Conversation*,

“should be of interest to at least two major groups of anthropologists: those who take componential analysis and formalistic theory seriously and those who do not.”  
(Birdwhistell 1968:830)

Birdwhistell also notes how Rose’s work on “small languages” was of criterial relevance not just to anthropology but to discourse analysis, linguistics, semiotics, and sociology.

As noted elsewhere (Fehr and Stetson 1990:474), attempting topical categorisations of a corpus of literature is problematic also. Any form of categorisation tacitly determines the work itself, and determines “what counts” as the work: therefore, it contains (implicative) silences too. It is not intended to be a *catalogue raisonné*, attempting to define the terms of reference of sociological literature; nor to outline thematic developments or methodological and conceptual concerns. Instead, this bibliography allows readers to adjudge for themselves the relevance and usefulness of works for their own projects.

The phenomenon of the relevance of materials is a very important consideration in compiling analytic bibliographies. The phenomenon of the relevance of materials for different projects was identified in a study of the practices of documentation (Garfinkel and Bittner 1967). In a subsequent chapter, it is noted that clinic folders are produced and maintained by medical personnel for practical, organisational purposes. The intended readership of clinic folders are other medics. The compilation and record-keeping of

clinic folders is adequate for this intended readership, but are inadequate for readers with different “organisationally relevant purposes” (Garfinkel and Bittner 1967:191).

Assembling a bibliography can be regarded as a parallel activity, in that assembling a sociological bibliography prefigures sets of relevances readers bring to the bibliography. For example, and as mentioned in the previous chapter, Rose was faced with a choice between art and academia. Rose has been a prolific painter—known especially in San Francisco and the Bay Area early in his career—producing hundreds of canvasses and prints,<sup>17</sup> which have been exhibited both nationally and internationally. For readers interested in Rose’s artwork, a bibliography which made no mention of art would be incomplete; that is, art would be a “noticeable absence”. Statements *apropos* art and the artist’s work are enumerated in the previous chapter. Some exhibition materials are itemised in an attempt not to delimit the sets of relevances that may potentially brought to this bibliography, e.g. Items #91-93, #95, #101-102, #121-126.

Although readers may argue that some bibliographic items may “properly belong” to separate categories, (e.g. artwork, autobiography, ethnographies, ethno-inquiries, sociolinguistics, presented papers, published papers, unpublished papers, books, conversations and symposia), the overlaps that are contained within items so categorised vindicates the decision to organise the material in chronological form. That is, the overlaps would have occasioned a greater *ad hoc* stipulation of categories by the bibliographer. This chronological format thus reflects an endogenous rather than exogenous approach to the bibliography as an “assembled object”. This approach

addresses Rose's own categories, which as the previous chapter showed were derived from categories used by people in the world.

Treating bibliography as an assembled object, rather than the “mere” listing of publications, demonstrates how this chapter is a form of analytic sociology. Reading and compiling bibliographies are natural language activities. The treatment of compiling bibliographies as a series of natural language (i.e. textual) pursuits, as assembled objects, demonstrates how bibliographies manifest discriminations e.g. “academic work”, “art work”, “autobiography”, etc. This “analytic mentality”, which takes bibliographies as inscriptions of natural language activities, displays a methodological coincidence with the explication of other linguistically organised research practices presented throughout the thesis, e.g. confidentiality, pseudonymisation, observation, transcription, and the elicitation of members’ accounts (or “sociological interviews”). Bibliographies as the practical accomplishment of compilers is treated more extensively in Chapter Thirteen.

### **Rose’s Work**

- (1) (1931) “The Establishment by Rats of Contrary Discrimination Habits.” *University of California Publications in Psychology* 4, 21:335-345. Edited by Warner Brown, Harold E. Jones, and Edward C. Tolman. Berkeley, California: University of California Press.\*
- (2) (1932) “The Act.” Johann Wolfgang Goethe-Universität, Frankfurt am Main. A series of diagrams including “A Behavioristic Description of an Act.” (12pp.)\*
- (3) (1934) “A Temporal Basis for a Rat’s Establishment of Contrary Tactual Discrimination Habits.” Paper presented at the Western Psychological Association, Berkeley, California (June 21-23 1934) and introduced by E. C. Tolman. (Abstracted in R. C. Tryon “Proceedings of the Western Psychological Association.” *Psychology Bulletin* 31:604.)‡
- (4) (1939) “Temporal and Spatial Bases Used by Rats in the Establishment of Contrary Discrimination Habits.” *University of California Publications in Psychology* 6,

13:189-218. Edited by Warner Brown, Edward C. Tolman and Harold E. Jones, Berkeley, California: University of California Press.\*

- (5) (1942) *The Classification of Cultural Distributions* Unpublished Ph.D. Dissertation, Department of Economics, Stanford University. (March.) (167pp.)\*
- (6) (1945) "The Roman Notes." (Summer.) (Reprinted in Edward Rose, *The Great War Brought to Rome* Item #149:384-390.)\*
- (7) (1948) "Innovations in American Culture." *Social Forces* 26:255-272.\*
- (8) (1951) "The Determinism of Language." (June 27–August 24.) (Reprinted in *The Jumbler* Item #151:7-11).\*
- (9) (1953) "The Societies." (May 30.) (Reprinted in *The Jumbler* Item #151:73-93).\*
- (10) (1954) "Bibliography of Animal Sociology Collected by the Members of the Seminar in Social Theory, Spring 1954, under the Direction of Edward L. Rose, University of Colorado.\*‡
- (11) (1955) "The Societies." (April 9.) (Reprinted in *The Jumbler* Item #151:109-117).\*
- (12) (1955) "The Production of Culture in the Laboratory." Paper read at the Annual Meeting of the Society for Applied Anthropology, Indiana University, Bloomington. (May 5.)\*‡
- (13) (1955) Edward Rose and William Felton "Experimental Histories of Culture." *American Sociological Review* 20, 4 (August):383-392.\*
- (14) (1956) "The Societies." (May 21.) (Reprinted in *The Jumbler* Item #151:135-142).\*
- (15) (no date) "Pronoun Societies." Manuscript. (14pp.)\*
- (16) (1958) Edward Rose and Gary Willoughby "Culture Profiles and Emphases." *American Journal of Sociology* 63, 5 (March):476-490.\*
- (17) (1958) Harold Garfinkel and Edward Rose "Making Apparitions." United States Air Force Think Tank, University of New Mexico, Albuquerque, New Mexico.‡
- (18) (1958) "Social Facts." Paper presented to The Philosophy Club, October 22. (16pp.)\*†
- (19) (1960) "The Organisation of Microcultures." In Dorothy Willner (Ed.) *Decisions, Values and Groups Volume 1* 171-175. Oxford, London, New York, Paris: Pergamon Press.\*
- (20) (1960) "The English Record of a Natural Sociology." *American Sociological Review* 25, 2 (April):193-208. Reprinted in Norman F. Washburne (Ed.) (1962) *Decisions*,

*Values and Groups Volume 2* 131-153. Oxford, London, New York, Paris: Pergamon Press.\*

- (21) (1960) Edward Rose and Howard Gruber "Behavioral Science in the Life Sciences Complex." (June 3 1960.) University of Colorado at Boulder. (10pp.)\*
- (22) (1960) "Race as a Sociological Concept." Presented at the American Association for the Advancement of Science meetings in Denver, Colorado. (7pp.)\*
- (23) (1961) "Methods of Validation of Judgements in Decision Making." Research Proposal. Bureau of Sociological Research, Institute of Behavioral Science, University of Colorado, Boulder. (December 19.) (12pp.)\*
- (24) (1962) "Uniformities in Culture: Ideas With Histories." Norman F. Washburne (Ed.) *Decisions, Values and Groups Volume 2* 154-176. Oxford, London, New York, Paris: Pergamon Press.\*
- (25) (1962) "Interpersonal Synonymity." Paper read at an Institute of Behavioral Science Research Seminar. University of Colorado, Boulder. (February.)‡
- (26) (1963) "Small Languages." Paper read at an Institute of Behavioral Science Research Seminar. University of Colorado, Boulder. (March.)\*‡
- (27) (1963) Henry Lennard, George Psathas, Edward Rose. "On Conversations." Transcript. *Bureau of Sociological Research Report Number 8* University of Colorado, Boulder. (July.) Reprinted (1998) Greeley, Colorado: The Waiting Room Press. (40pp.)\*
- (28) (1963) "The Talion Maker." Transcript of an Ethnomethodology Conference, with Egon Bittner, Lindsey Churchill, Harold Garfinkel, Craig MacAndrew, Edward Rose and Harvey Sacks. (October 24-25.) (66pp.)\*
- (29) (1963) "The Talion Maker." Paper read at an Institute of Behavioral Science Research Seminar. University of Colorado, Boulder. (December.) (6pp.)\*
- (30) (1964) "Methods of Validation of Judgements in Decision Making." Report on completed and continuing research, and research procedures. Office of Research Services, University of Colorado, Boulder. (January 28.) (11pp.)\*
- (31) (1964) "On Listening." An article prepared for inclusion in a collection of articles brought together by Llewellyn Gross of the University of New York at Buffalo. (7pp.)\*
- (32) (1964) Edward Rose *et al.* *SMALL LANGUAGES* Volume I. Edward Rose, "Small Languages, an Introductory Statement": 1-16. Edward Rose, Sandi Lauderdale and Frank Leuthold (Eds.) "The Land of Sez": 17-64. Edward Rose, Sandi Lauderdale and Frank Leuthold (Eds.) "The Other World": 65-97. Edward Rose and David Pratto,

"The Scene in the Box": 98-106. *Bureau of Sociological Research Report Number 14* (November.) Institute of Behavioral Science, University of Colorado, Boulder. (106pp.)\*

- (33) (1965) Edward Rose *et al.* *SMALL LANGUAGES* Volume II. "The Making of Pique." Section 1. The Elements. Editors: Edward Rose, Sandi Lauderdale, Frank Leuthold and Helen Wilson. "The Making of Pique." (107pp.) Section 2. The Developments. Editors: Edward Rose, Sandi Lauderdale, Frank Leuthold and Helen Wilson. "The Power and the Story of Pique", Edward Rose, 1-16. "The Elements" and "The Developments", edited by Edward Rose, Sandi Lauderdale, Frank Leuthold and Helen Wilson, 17-226. *Bureau of Sociological Research Report Number 15* (January.) Institute of Behavioral Science, University of Colorado, Boulder. (226pp.)\*
- (34) (1965) "The Jailing of John Ralph Kruthshift: A Moving Tale Told to Edward Rose." Volume I "Separation: Arrest and Trial." Edward Rose, 1-5, "Introduction: A Critical Event." Volume II "Marge: The Move to the County Jail." Volume III "Aggregation: The County Jail." *Bureau of Sociological Research Report Number 20* (July.) Institute of Behavioral Science, University of Colorado, Boulder. (Three volumes reprinted in Rose and Hughes, Item #154)\*
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- (80) (1979) "The Great Book of the World." (August 18) Manuscript. (15pp.)\*
- (81) (1979) "The World's Record, Dirty Book II." (December 20.) (Reprinted 1998. Greeley, Colorado: The Waiting Room Press. (42pp.))
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- (91) (1981) "The Graefs of EROS." (November 6 1980–January 12 1981) Boulder, Colorado: The Waiting Room Press. (32pp.)\*
- (92) (1981) "The Beta Graefs." Four paintings painted October 1980. (February 11 1981))‡
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- (94) (1981) *The Last Connection: A Story About Heroin Told By Ali Baba To Edward Rose* Boulder, Colorado: The Waiting Room Press. (x+93pp.)\*
- (95) (1981) Edward Rose "Graefs, A Statement Accompanying Exhibitions." (4 July.) This Statement goes with exhibitions of the Graefs at Amerika Haus in Frankfurt, August–September 1981; Biblioteca Communale of Cortona, Tuscany, September 1981; Trident Booksellers in Boulder, Colorado, October 1981; Art Institute, Universität Kassel, November 1981.\*
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- (103) (1982) "Art and Society." Two lectures given at Fort Lewis College, Durango, Colorado. (March.)‡
- (104) (1982) "Epistle to Kaplan: Remarks on the Ethno-Inquiries." *Sociolinguistics Newsletter* 13, 1 (Spring/Summer):18-23.\*
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- (114) (1986) "Changes of Theory." Discussion document. Boulder, Colorado. (April 6.)\*
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- (117) (1986) "Responses to Readings of Nine Conversations on Buying and Selling." Aurora, Colorado: US West Direct. (May 10.) (7pp.)\*
- (118) (1986) "Cheyenne Selections." Boulder, Colorado. (November 30.) (153pp.)\*
- (119) (1990) "Six People Using Telephones and Telephone Directories." Boulder, Colorado. (December 28.) (135pp.)\*
- (120) (1991) "The Presentation Machine. A Conversation with Harvey Sacks: An Analysis with Changes and Connections." A paper drawn with changes and connections from a talk given on July 17 1991, at a session, with George Psathas presiding, at The Conference on Current Work in Ethnomethodology and Conversation Analysis, The University of Amsterdam. A shortened version is reprinted as "A Conversation with Harvey Sacks: An Analysis with Changes and Connections" in Item #128, *The Werald* (1992:324-340); translated into French and reprinted in full in Item #130, *Cahiers de Recherche Ethnométhodologique* (1993:25-40.)\*
- (121) (1991) *Paintings and Prints* Boulder, Colorado: The Waiting Room Press. (14pp.)\*
- (122) (1991) *Worlds of Visionary Fancy* Boulder, Colorado: The Waiting Room Press. (13pp.)\*
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- (124) (1992) *Tηε Σπιριτο Oφ Ανχιεντ Κινγσ* Boulder, Colorado: The Waiting Room Press. (12pp.) (See Item #123.)\*
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- (126) (1992) *Philosophical Investigations With Pictures* Boulder, Colorado: The Waiting Room Press. (20pp.)\*
- (127) (1992) "The Art Of The Word." Paper presented at the International Institute for Ethnomethodology and Conversation Analysis, Bentley College, Waltham, Mass. (Summer.)\*‡
- (128) (1992) *The Werald* Boulder, Colorado: The Waiting Room Press. (xxxv+343pp.)\*
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- (130) (1993) "Conversation avec Harvey Sacks: Analyse avec Modifications et Corrections." *Cahiers de Recherche Ethnométhodologique* Number 1 (June):25-40. Directeur Alain Coulon, Association Internationale de Recherche Ethnométhodologique et Laboratoire de Recherche Ethnométhodologique Université de Paris, France. (See Item #120.)\*
- (131) (1993) Rodney Watson and Edward Rose *Audio-tape* Boulder, Colorado. (September 29.)\*‡
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- (133) (1993) "Comments on *The Theori and Engagement.*" Edward Rose talking with Professor Leonard Pinto and three students. *Audio-tape* Boulder, Colorado. (November 15.)\*‡
- (134) (1994) "The Theoric Construction In The Ethno-Inquiries.' Selections From *Theory In the World*, Chapter Nine in *The Worulde.*" *Studies In Symbolic Interaction* 16 37-62.\*
- (135) (1994) "A Little Girl and Harry Stack Sullivan." *Audio-tape* Edward Rose at Leonard Pinto's Class, Boulder, Colorado. (January 24.)\*‡
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- (137) (1994) "Field Methods in Sociology." A series of lectures delivered by Edward Rose and Fred Templeton, University of Colorado, Boulder. (Summer.)\*‡

- (138) (1994) "Waiting for the World to Walk By." Transcript of talk given at the University of Colorado, Boulder, Colorado. (August 8.) (13pp.)\*
- (139) (1994) "The Quiet Room." Edward Rose talking with Andrew Carlin, Nozomi Ikeya, Roger Slack and Rod Watson *Audio-tape* University of Manchester. (October 18.)\*‡
- (140) (1994) Graduate Seminar. Edward Rose talking with Wes Sharrock, Rod Watson and graduate students *Audio-tape* University of Manchester. (October 19.)\*‡
- (141) (1994) Lecture given to students of Ethnomethodology, by Edward Rose. Introduced by Rod Watson *Audio-tape* University of Manchester. (October 20.)\*‡
- (142) (1994) "The Snooker Room." Edward Rose talking with Andrew Carlin, Roger Slack and Rod Watson *Audio-tape* University of Manchester. (October 24.)\*‡
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- (144) (1994) "Waiting for the World to Walk By." Paper given at a meeting of the Manchester Ethnography Group, with Rod Watson presiding. *Audio-tape* Manchester Metropolitan University and University of Manchester. (October 27.)\*‡
- (145) (1995) Robert C. Hanson and Edward Rose "Knowledge in the World: First Results of a Comparative, Historical Investigation." ISCSC 24th Annual Meeting, June 15-18, Wright State University, Dayton, Ohio. (34pp.)\*
- (146) (1996) Robert C. Hanson and Edward Rose "The Great Object of Inquiry." An introductory statement to the paper "States of Affairs in Six Great Regions of the World." (April 21.) (6pp.)\*
- (147) (1996) Robert C. Hanson, Edward Rose and Zeke Little "A Comparative Analysis of Sorts of Things Known in Five Ancient Civilisations." ISCSC 25th Annual Meeting, June 20-23, California State Polytechnic University, Pomona, California. (28pp.)\*
- (148) (1996) *The Great War Brought To Algiers By Edward Rose* Greeley, Colorado: The Waiting Room Press. (xviii+340)\*
- (149) (1997) *The Great War Brought To Rome By Edward Rose* Greeley, Colorado: The Waiting Room Press. (xix +461)\*
- (150) (1997) "The Unattached Society." *Ethnographic Studies* 1 (Spring):xv-43. School of Sociology and Social Policy, University of Wales, Bangor, UK. (See Item #35.)\*
- (151) (1997) *The Jumbler: University Note Book Entries from 1951 to 1957 on Language, Culture and the Societies* Greeley, Colorado: The Waiting Room Press. (x+171pp.)\*

- (152) (1997) *Stanford Notions and Stanford Opinions* [(1935 September) "Stanford Notions." 1. Summaries (1p.), 2. Inferences (29pp.), 3. Opinions (3pp.). (1936 December 28). Greeley, Colorado: The Waiting Room Press. (33pp.)\*
- (153) (1997) *Boulder Notions* Greeley, Colorado: The Waiting Room Press. (49pp.)\*
- (154) (1997) Edward Rose and Lori Hughes *The Rites of Jailing* Edward Rose: "Winding Up in Jail," xxvii-xxxviii. "A Walk on the Shady Side of the Road," 415-419. Greeley, Colorado: The Waiting Room Press. (xl+419pp.) (See Item #34.)\*
- (155) (1997) Harold Garfinkel and Edward Rose *Mystifying Kinds of Achievement* (Conversation July 15, 1973.) Reprinted 1997 with a new introduction by Edward Rose, "Rose's Gloss on Talk about Achievements." Greeley, Colorado: The Waiting Room Press. (xv+89pp.) (See Item #72.)\*
- (156) (1998) "Enterknowledge." Manuscript prepared in 1956. Greeley, Colorado: The Waiting Room Press. (34pp.)\*
- (157) (1998) *Be Fine. A Story Told by Anna Brown to Anthony Gorman* Greeley, Colorado: The Waiting Room Press. (xxv+114pp.)\*
- (158) (1998) Edward Rose and Andrew Carlin *Audio-tape* Archives, Norlin Library, University of Colorado at Boulder. (June 8.)\*‡
- (159) (1998) Edward Rose and Andrew Carlin *Audio-tape* Boulder, Colorado. (June 9.)\*‡
- (160) (1998) Robert C. Hanson, Edward Rose, and Zeke Little "Worded Things Spoken by Literate and Nonliterate Populations." Paper delivered at the ISCSC 27th Annual Meeting, June 11-14, Reitaku University, Kashiwa City, Chiba Prefecture, Japan. (14pp.)\*
- (161) (1998) Edward Rose and Andrew Carlin *Audio-tape* Boulder, Colorado. (June 22.)\*‡
- (162) (1998) Edward Rose and Andrew Carlin *Audio-tape* Boulder, Colorado. (June 25.)\*‡
- (163) (1998) Edward Rose and Rodney Watson *Brownian Attainments* Greeley, Colorado: The Waiting Room Press. (xii+43pp.)\*
- (164) (1999) Robert C. Hanson, Edward Rose, and Zeke Little "Common Understandings of Things in the World: A Semantic Analysis of 20 Languages, Part 1." Paper delivered at the ISCSC 27th Annual Meeting, May 20-23, 1999, St. Louis. (20pp.)\*
- (165) (In preparation) "Data Record for Contrary Discrimination Habits." Greeley, Colorado: The Waiting Room Press. (33pp.)

- (166) (In preparation) “The Paragon Tablet.” A variety of Items, including lists of worded and dated sorts of things and notably a draft for a Natural Sociology (no date) and a summary Chart of Anglo-Saxon Sorts of Things. (1963-64). (28pp.)
- (167) (In preparation) Edward Rose’s *Classroom Notes on Professor Frederick J. Teggart’s Lectures on The Idea of Progress*, delivered at the University of California at Berkeley during the Fall Semester of 1929. Greeley, Colorado: The Waiting Room Press. (91pp.)
- (168) (In preparation) Edward Rose’s *Classroom Notes on Professor Frederick J. Teggart’s Lectures on Uses of the Idea of Progress*, delivered at the University of California at Berkeley during the Spring Semester of 1930. Greeley, Colorado: The Waiting Room Press. (83pp.)
- (169) (In preparation) Edward Rose’s *Classroom Notes on Professor Alfred Kroeber’s Lectures on Old World Cultures, Anthropology 103*, delivered at the University of California at Berkeley during the Fall Semester of 1930. Greeley, Colorado: The Waiting Room Press. (119pp.)
- (170) (In preparation) Edward Rose’s *Classroom Notes on Professor Edward Tolman’s Lectures on Animal Psychology, Psychology 150*, delivered at the University of California at Berkeley during the Fall Semester of 1930. Greeley, Colorado: The Waiting Room Press. (80pp.)
- (171) (In preparation) Winifred Higbee’s *Classroom Notes on Anthropological Ethics by John Dewey* Lectures delivered by John Dewey at the University of Michigan in 1894, edited by Winifred Higbee and Edward Rose. (Originally prepared for publication in 1961 by Edward Rose and Kathleen Zumbro; prepared again by Edward Rose and Lori Hughes, along with introductory remarks by Lori Hughes and with observations on “A Meeting of Minds” by Edward Rose.) Greeley, Colorado: The Waiting Room Press.\*‡
- (172) (In preparation) Charles D. Kaplan and Edward Rose “A Natural Sociology of Camps.”\*‡
- (173) (In preparation) Edward Rose and Rodney Watson *The Higher Learning* Greeley, Colorado: The Waiting Room Press.‡

## **Conclusion**

This chapter is a service to the ethnomethodological and sociological communities. This chapter presents, for the first time, a full bibliography of the work of Edward Rose. This

analytic bibliography complements the intellectual profile provided in Chapter Two; the bibliography and the profile are mutually informing.

This chapter makes available the practices of assembling bibliographies, and provides analytic considerations on the work of the bibliographer and on the use of bibliographies for sociological inquiries. These themes are extended in Chapter Thirteen, on the linguistic constitution of bibliographies and literature surveys.

Rose's work is a central feature of this thesis, which shall be used to elaborate the major underlying themes of the thesis; *inter alia*, the linguistic organisation of the social world, the linguistic constitution of research practices, the primacy of members' accounts rather than the *post hoc* imposition of methodologically ironic glosses, and the natural language foundations of everyday and professional (including sociological) activities. Rose's work also suggests issues *apropos* the work of transcription, the textual presentation of reports (e.g. titles), interviewing, confidentiality and disguising informants.

As I stated in Chapter One, further explications of the conceptual and methodological bases of Ethnomethodological programmes—Ethnomethodology, Conversation Analysis, Membership Categorisation Analysis and Rose's work—are embedded within the thesis. With conceptual and methodological resources in place, this thesis moves towards the exploration of the constitutive activities of research practices.

Rose's work, as provided in this chapter and the preceding chapter, is cognate with the Ethnomethodological programmes introduced in Chapter One. Rose's "linguistic turn"

enables this thesis to look at how taken-for-granted, mundane activities are constitutive of research activities. That is, how research methods, e.g. “participant observation”, “observation” and “interviewing” are glosses for assemblages of culturally available activities, e.g. membership categorisation, inference-making, and recognition.

<sup>1</sup> I wish to thank the following for their assistance in preparing this bibliography: Jon J. Driessen (*University of Missoula, Montana*); Robert C. Hanson (*University of Colorado, Boulder*); Lori E. Hughes (*University of Manchester*); Charles D. Kaplan (*Universiteit Maastricht*); Rolf Kjolseth (*University of Colorado, Boulder*); Edward L. Rose (*University of Colorado, Boulder*); Viktor Sarris (*Johann Wolfgang Goethe-Universität*); Roger S. Slack (*University of Edinburgh*); D. Rodney Watson (*University of Manchester*); Michael Wertheimer (*University of Colorado, Boulder*). I also wish to thank the University of Colorado at Boulder Libraries, Archives; University of California, Berkeley and Stanford University Libraries; and the Department of Psychology at the University of California, Berkeley.

<sup>2</sup> The inaugural issue of the journal *Ethnographic Studies* reprinted one of Rose’s ethnographic reports on the Skid-Row district of Denver: Edward Rose (1997) “The Unattached Society.” *Ethnographic Studies* 1 (Spring):xv–43. This issue includes an analytic preface to Rose’s work: Rod Watson (1997) “Prologue to Edward Rose’s *The Unattached Society*.” *Ethnographic Studies* (Spring) 1:iv–xii. (Vide Item #146.)

<sup>3</sup> This thesis (*passim.*) goes somewhat towards redressing this *lacuna*. That Rose’s work constitutes a “gap in the literature” (for more detailed discussion of this phrase, see Chapter Thirteen—“Constituting Bibliographies) is suggestive of a topic for future inquiry, of an ethnographic kind, into “networking”.

<sup>4</sup> Cf. also Birdwhistell, below. Ball was referring to a paper published in the *American Sociological Review* (Item #20); however, the diachronic analyses of sociological practices is a recurring topic in Rose’s works (e.g. Items #24, #55, #98, #113, #128).

<sup>5</sup> H. Sacks (1963) “Sociological Description.” *Berkeley Journal of Sociology* 8:1-16. For Rose’s disquisitions on the “commentator machine”, *vide* Items #120 (reprinted Items #128, #130) and #144. For an earlier consideration of Sacks’ paper, *vide* Item #31. The French translation is accompanied by a commentary: D. R. Watson and W. W. Sharrock (1993) “Conversation avec Edward Rose à propos de sa conversation avec Harvey Sacks: quelques observations analytiques sur les ethno-recherches.” *Les Cahiers de la Recherche en Ethnométhodologie* 1 (Juin):41–53. The titles and translation of Sacks’ conversation with Sacks are referred to in the previous chapter.

<sup>6</sup> Conference on Current Work in Ethnomethodology and Conversation Analysis, The University of Amsterdam, July 17 1991. *Vide* Item #120.

<sup>7</sup> To take Item #15 as an example of such alternate placement, its concerns with pronouns and pronoun societies are considered also in Item #51. As such, these items could be situated together. Both of these alternate placements seem to be plausible. This bibliography does not stipulate between possible placements.

<sup>8</sup> Following Morton’s extensive bibliography of Arnold J. Toynbee (1980:ix), all materials verified by the compiler have been marked with an asterisk [\*]. Unseen materials, e.g. Item #17, are recorded according to extant documents, including conference proceedings, *curriculum vitae*, and grant applications.

<sup>9</sup> A portion of Rose's materials—"The Edward Rose Collection"—has been processed in provisional form only. Copies of "The Edward Rose Collection", listing documents, folders, reel-to-reel tapes and cassettes of lectures, seminars, discussions, and interviews, can be obtained by contacting University of Colorado at Boulder, University Libraries, Archives, Campus Box 184, Boulder, Colorado 80309-0184.

<sup>10</sup> However, even when items have been clarified, in some cases the reference details remain incomplete. That is, certain details are unavailable from inspection of items, documentary materials (cf. note 8 above) and searches for information. Items marked thus [†] are missing precise date and/or venue details; page numbering is unavailable (or not applicable) for bibliographic referencing for items marked thus [‡].

<sup>11</sup> Fehr, B.J. et al. (1990:473).

<sup>12</sup> The Waiting Room Press, Post Office Box 83, Greeley, Colorado 80632, USA.

<sup>13</sup> Psathas (1994:71).

<sup>14</sup> Items #10 and #49.

<sup>15</sup> Edward Rose (1977) "A Request and a Statement to the Commission on the Protection of Human Subjects in Biomedical and Behavioral Research." University of Colorado, Boulder. (April.) Item #67.

<sup>16</sup> E.g. Ray L. Birdwhistell (1968) Review of E. Rose (1967) "A Looking-Glass Conversation in the Rare Languages of Sez and Pique." *Program on Cognitive Processes Report No 102*, (April) Institute of Behavioral Science, University of Colorado. *American Anthropologist* (1968) 70 (6) 830. *The Denver Post* (October 18 1965) Editorial: "The Challenge of Skid Row." The practices of exegesis may have disadvantaged the reception of Rose's work, as manifested in a Review Symposium: compare Psathas, *op cit.*, and Laurel Richardson (1994) "Glossing *The Werald*." *Studies in Symbolic Interaction* 16:63-69, with the level and depth of critique offered elsewhere, *viz.* Robert C. Hanson (1994) "The Worlds of Edward Rose: His Ethno-Inquiries Approach," paper presented at the ISCSC 23rd Annual Meeting, Dublin July 7-10, 1994.

<sup>17</sup> Rose's house and studio are full of artwork. As far as I know, no written record of his artwork has yet been produced.

## Confidentiality: Fieldwork and Linguistic Activities

This chapter comments upon a series of issues involved in the allocation of befrienders to offenders serving prison sentences by a non-statutory body. The following observations are reported from a ten-month period of participatory fieldwork, during which I conducted administrative duties in a regional office of the organisation. Thus, this chapter constitutes what Richman calls an “ethnographic slice” (1997:21), but where I was an active part of the circumstances I describe. This follows a methodological preference outlined by Albert B. Robillard, who noted that the researcher “must and must be seen as an adequate practitioner of the behaviour he or she is analysing” (1996:29). This methodological preference, the “unique adequacy requirement” of research, is treated more extensively in the next chapter. For now, it is important to note that I was an “active practitioner”, not just a “participant observer”, inducted into the organisation and trained in its administrative procedures.

The chapter works through a series of practical and sociological matters obtained by and during the fieldwork. First, the practical matters of doing research with an organisation and within organised settings that were, *de facto*, confidential. Second, the confidential nature of information to which I became party had ramifications for research practices, e.g. recording and note-taking. Third, by participating in multifarious activities, in different settings (including “The Office”, group-meetings and training new volunteers) and with different members of the organisation, what constituted confidential information was observed not to be an unproblematic principle

but a context-sensitive, contextually-occasioned matter. Fourth, confidential information was encountered in working with textual materials and in conversations.

The complementary roles of researcher and administrator afforded opportunities to witness the use of members' routine practices. Commonalities with other settings are discerned, which provide conceptual purchase on observations. The uses of information— withholding and disclosing, oral and textual—are thoroughly linguistic uses. Research practices and organisational activities are natural language activities.

Whilst this chapter contains preliminary explications of research methods as linguistic activities, the contributory analyses are returned to in later chapters. That is, this chapter establishes some of the core relevances of the thesis. By assembling a set of analytic resources, this chapter prepares the foundations for forthcoming analyses.

### ***'Doing Researching' in Confidential Environments***

“Confidentiality” was a ubiquitous consideration in the organisation and for all those who had contact with it, e.g. offenders, offenders’ families, volunteers or “befrienders” (known within the organisation as “Buddies”), probation officers, and researchers. I was not the first researcher to be granted access to hitherto highly confidential settings. “Confidential” research environments have included abortion clinics, gynaecology clinics and school examination meetings. Garfinkel (1967)<sup>1</sup> studied jurors’ deliberations, how jurors commonsensically worked at doing “being legal” by “bugging” a jury room in Wichita. The study of the jurors’ common-sense methods in

discussing and reaching a verdict was Garfinkel's focus on what makes the group enclosed in the jury room "a jury".

Like researchers from Chicago Law School, I sat, watched and listened to members making ordinary, common-sense decisions within confidential settings, decisions which had consequences for naïve persons: "naïve" because these persons were unaware of the trajectories of deliberations, i.e. decision-making activities that concerned them; e.g. the defendant has no idea what jurors are talking about behind closed doors, and is instead orientated to the final outcome/decision. Similarly, as this chapter will show, offenders who request befrienders or buddies have no knowledge of the processing of their request: the work, i.e. the practical reasoning involved in the negotiation of allocation, of "the big decision", is irrelevant to the offender. Befrienders' or buddies' meetings are constituted by people talking behind closed doors; they are discussions *in camera*. Offenders will not learn of the decision-making activities of buddies; instead they are orientated to the outcome of their request. The ethnographic materials presented in this chapter outline the temporal administrative stages and transformations that each request goes through before the result of a request is finalised.

Joel Richman conducted research over a long period in a gynaecology clinic in the North West of England. Access was provided by an obstetrician over a drink, which was when the original research proposal was formulated. Hence, access to a confidential research site was serendipitous:

“Getting into gynaecology was very easy. There [were no lengthy negotiations] with the hospital’s gatekeepers; no circulation of a research protocol (written to conceal potentially contentious issues) to all relevant parties (today could literally mean everyone!); no merry-go-round of endless meetings to demand further “clarifications”; no tight parameters set to hedge in the operant research, etc....”

(Richman 1994:172)

In this respect, Richman’s research differed from Whyte, who reports<sup>2</sup> the difficulties in finding a knowledgeable and reliable informant, a “way in” to the life of the men in Cornerville. Richman echoes the invaluable association of Whyte’s key informant, “Doc”, by remarking that his own “presence was legitimated by and indivisible from Bill’s [the obstetrician]” (Richman 1994:173). For my own part, participation in and observations of meetings were distracted by the officer who originally introduced me to the group, who had a habit of drawing attention to my rôle as researcher. Whilst her co-operation was invaluable, my analytic preference during observations of meetings was for her absence, as I was concerned that her continued comments *re* my “sitting-in” might have a debilitating effect on my participation.<sup>3</sup>

It should be reiterated here that the period of fieldwork reported below occurred during the research for and writing of a research thesis with the same organisation. That previous work (Carlin 1994), based around the axis of the topic and resource distinction of the place (centrality or omission) of natural language practices in research (Zimmerman and Pollner 1971), was concerned with an innovative and specialist welfare programme delivered by the organisation, *viz.* the provision of parenthood-counselling courses in prisons for new and expectant fathers. The voluntary nature of

the organisation (pseudonymised, for reasons which are explained in the following chapter—in the section “Locations, Organisations, and the Work of Pseudonymisation”—as “Second Chance”) and the nature of the welfare programme are outlined for newcomers to a course in the following extracts (see arrowed utterances):

### Extract 89 (2)

- 1 CR1 and for you guys who haven't heard of Second Chance before  
 have you  
 2 you haven't no  
 3 we're just er er an organisation  
 4 bit like ( ) I s'pose  
 5 erm who are volunteers  
 6 come in and visit blokes  
 7 ((pause))  
 8 erm  
 9 ((shakes head))  
 10 and then go round ( )  
 11 ( )  
 12 erm  
 13 we're a slightly different branch  
 14 erm

### Extract 3 (2)

- 1 CR1 we actually come in and hold a group on family matters parenting  
 call it what you like ( ) and erm  
 3 just hold a group discussion group  
 4 on bringing up children and er personal relationships  
 5 so whilst we're on that can we ask you (Tim)  
 6 have you got any children  
 7 CD2 I've got one  
 8 CR1 got one aha  
 9 CD2 boy  
 10 CR1 boy  
 11 CD2 boy yeh four years old  
 12 CR1 yeh what's his name  
 13 CD2 Rory  
 14 CR1 Rory

The parenthood-counselling courses were designated thus because of the provision of opportunities to talk openly for offenders, which paralleled the parenthood-training element of sessions. During the period of fieldwork activities reported herein, tensions were iterated by members of the organisation, *viz.* on being volunteers from a volunteer organisation talking to prisoners in prison, who felt that prison staff regarded them as “interfering do-gooders”; trainers on the video-recordings of parenthood-counselling sessions occasionally expressed frustration at the “lack of fit” between the ethos of the organisation and the prison regime. E.g. during constant noise from machinery which disrupts and eventually ends the parenting-counselling session:

Extract 53 (5)

- ((over machine noise))
- 1 CR1 so wha what do you think about this  
2 about the fathers and mothers having an argument (over) the child [ when I was
- 3 CD1 a kid  
4 when I was ( ) [
- 5 CR2 ((laugh)) [
- 6 CR1 ( ) [
- 7 ((Group laugh))
- 9 CD1 ( ) [
- 10 ((machine tool noise))
- 11 ((Group laugh))
- 12 CR2 ( ) [
- 13 ((pause))
- 14 CR1 ( ) ((shakes head))
- 15 CR3 tell the camera what's happening
- 16 CR1 its virtually impossible to continue the class  
17 under all this \*dreadful noise that's going on outside  
→ 18 and I feel rather sorry that the officers couldn't inform us

19           that would be going on outside  
 20   CD2   (       )  
       [  
 21   CR1   so we could go somewhere else  
 22   CD   (       )  
       [  
 23   CR2   (       )  
       [  
 → 24   CR1   but it seems not very clever to ask us to go in to a room  
 25           where this work is going on outside  
       \*      machine tool noise stops

E.g. the precedence of prison-initiated activities over attendance at the parenting-counselling sessions:

Extract 58 (5)

10           but what do you think about the course  
 11           have you have you enjoyed it  
 12   CD2   yeh  
 13   CR1   yeh  
 14   CD2   pity I (       )  
 → 15           cos I got called off to the dentist didn't I  
 16   CR1   yeh that's right (       )  
 17   CD2   ((nods head))  
 18   CR1   and did you come on the first week  
 19   CD2   did I come on the first week  
 20   CR1   no I do:n't think you did  
 21   CR2   no  
       [  
 22   CD2   no I was at the dentist  
 23           ((laugh))  
 24   CR1   that's right  
 25   CR2   that's right you were at the dentist yeh  
 26           ((laugh))  
 27   CR1   yeh you were at the dentist yeh

The organisation recognises and acknowledges that it is only allowed into the prison at the permission of prison authorities. Whilst working in the office, during the period of

fieldwork, the regional officer expressed her fears that any disruption by offenders attending one of the organisation's courses could provide prison authorities with an excuse to refuse the organisation access to the prison in question—or worse, prisons in general—to provide further parenthood-training (or parenthood-counselling) courses. During parenthood-training courses, representatives of the organisation reiterated the position *apropos* adhering to prisoner regulations; i.e. parenthood-counselling sessions were not “time-out” from prisoner regulations.

### Extract 63 (1)

1       CR1   erm I I just wanna quickly say about rules and regulations of the  
        group  
 →      2       which we do:n't actually have any  
          3       apart from having to obey the prisoner regulations  
          4       which you have to (       ) to  
          5       erm so if I could just ask you not to smoke during class okay  
          6       but we have ten minute break half way through when you can  
           smoke  
          7       and also erm no severely bad language orright  
          8       ((pause))  
          9       cos we do:n't like it  
 10      ((laughs))  
 11      CR2   (       )  
 12      CR1   and erm anyway its a good idea um  
 13      you're you're in slightly different company to what you're  
        usually in  
 14      so er you have to be aware of what y'know  
 15      ((pause))  
 16      (       )  
 17      d'you d'you agree with that yeh  
 18      CD    (       )  
 19      CD    (       )  
 20      CR1   its not gonna be a problem yeh

A fear expressed outside parenthood-counselling courses was that any incidence of misbehaviour or assault on volunteers themselves would lead to the withdrawal of their access to penal institutions as an accredited organisation.

This chapter of the current thesis, however, refers to the generic work of the organisation—its *raison d'être*. As it progresses, this chapter provides readers with appropriate sources and conceptual frameworks from corpora of literature in sociology; that is, it suggests further reading materials relevant to the topics-at-hand.

In discussing the “natural history” of the project, it may be of more than anecdotal interest that the production of “Allocation lists” (see below) coincided with my writing a section on members’ routine, sense-assembly procedures (Carlin 1994:51-61) and the Documentary Method of Interpretation. *In sum*, the option of ethnomethodologically-informed programmes, as analytic approaches appropriate to the phenomena of inquiry, was reached at this stage.

## **Fieldnotes**

The period of fieldwork reported below began before other methodological options were thoroughly explored, especially the use of recorded, retrievable data. This presents its own problems, e.g. the use and reliance upon fieldnotes as data. Fieldnotes are linguistically constituted textual artefacts of settings. This being the case, the analyst should exercise caution in approaching fieldnotes as data. Furthermore, observational techniques are unable to preserve the particulars of situated activities without the use of audio/video-recordings. Data which are derived from non-retrievable sources may be suggestive, but their status remains “conjectural”.<sup>4</sup> Writing fieldnotes *in situ* distracts the attention of the researcher; instead of observing the interaction the analyst divides their attention between writing fieldnotes and observing the scene.

The opportunities for taking fieldnotes in “my” setting were not as harried as some researchers suggest regarding their settings (e.g. Lee-Treweek 1996).<sup>5</sup> Whilst working at the regional office, I kept a diary in a ring-bound notebook of times/dates, tasks and office duties I was engaged in: this was to serve a) as a record for the claiming of travel and subsistence expenses; and b) as a resource for updating my *curriculum vitae*, e.g. familiarity with particular office software, being a recent graduate also preoccupied with finding employment. The latter purpose for keeping this diary necessitated the taking of detailed notes. “Office duties” and “administration” are glosses which are full of potential activities, and orientating myself to job-interview questions *qua* “So what did that task actually involve?”, I produced detailed explications of my work-site

activities. These explications proved invaluable: for the original purpose of adding depth to sections of my *curriculum vitae* as well as for attending to the ordinary, common sense practices which are relied upon in accomplishing administrative work, and for highlighting the linguistic constitution of administrative and research processes. As I became accustomed to the note-taking habit of leaving out as little as I practically could (given the constraints of time and office duties, and given the *caveats* above that fieldnotes *per se* are not and cannot be complete records of a setting), so the ring-bound notebook filled up with detailed observations associated less with my own work-site practices than with the work-site practices of members available for observation.

Having been introduced to other buddies as a graduate researcher interested in the work of the organisation, it was treated as commonplace that I should make notes during meetings.<sup>6</sup> Each buddy kept notebooks on their laps during meetings, which they referred to in conjunction with the client report form (see Appendix) when reporting on their client (visits, letters, news of events since the previous meeting) and used to note down information from the meeting. The mere presence of my notebook did not incite suspicion, therefore.<sup>7</sup>

The status of fieldnotes in research can be equivocal. Notes taken “in the field” are textual artefacts which have a reflexive relation to the setting which they describe. That is, they are constitutive of, and are constituted by the settings of which they were made. As descriptions can be extended indefinitely (Sharrock and Turner 1980), these notes are “merely” *aides-mémoires* of the setting. The linguistic activity of writing-up

fieldnotes does not produce a seamless structure or chronological sequence, but instead produces reconstructions which have been “recollected by virtue of topical associations between them.” (Farran 1985:6). Garfinkel realises how fieldnotes contain a “missing what” following his study of lectures with David Sudnow (Garfinkel and Sudnow 1972). The notes they made during lecture on chemistry (e.g. “[The lecture hasn’t begun] =”) were collaborative, and the collaborative achievement of ethnographic accounts (e.g. topical associations made in later talk about the fieldnotes) is not retrievable from ethnographic reports. That is, the outcome of the conversations conducted after observation do not feature as being constitutive of the process. Farran (1985:4) also alludes to different ethnographers producing different fieldnotes of the same setting, *qua* Sontag’s (1979:89) notion of “photographic seeing”. That is, it is the photographer who operates the camera, deciding what to photograph and when to depress the shutter. Likewise, each ethnographer in the same setting decides that different things are sufficiently noteworthy.

### ***Organisational Activities: Confidentiality and Knowledge***

Following Bittner (1965b:247), I observed how the “terms and determinations” of confidentiality varied in use. I use Bittner’s phrase to refer to the asymmetric definitions of confidentiality within the organisation, whereby some members of the organisation defined information as being confidential—confidentiality was contextually-occasioned and contextually-invoked. That is, information was confidential according to certain people and upon occasions of its application.

Certain people had greater rights to invoke confidentiality than others. The invocation of codes of confidentiality were occasioned activities, which perplexed members of salaried-staff and volunteers alike. Invoking codes of confidentiality was somewhat analogous to “pulling rank”; however, buddies reported frequent incidences of other members of the organisation pulling rank when they had no observable authority within the organisation to do so. Thus, staff and volunteers inferred the existence of informal networks, *qua* in-groups and out-groups, which were not reflected by job-titles or position within the organisation. Membership of the in-group, which was perceived by members to have access to the highest echelons of the organisation, became a warrant to invoke confidentiality. Confidentiality was an organisationally located phenomenon, which underwent different determinations within the same organisation.

To illustrate the circumvention of organisational hierarchies, some of the upper management staff were recognised by members to be “out of the loop” *de facto*. As perceived by members, it was therefore pointless taking grievances or issues to such management staff because they were not part of the informal network. Similarly, certain buddies were recognised by members to be “in the loop”, who “had the ear of” the directorship.

The term “organisational activities” refers to organisationally-sanctioned practices and informal structures, e.g. gossip, interpersonal networks, locally-managed circumventions of standard practices (“in this office we prefer to do it this way”), etc.—where “standard” practice accords with The Organisations’ Code of Practice—and is

therefore respecifying the traditional formal-informal nexus of organisation research. The organisation's activities are constituted by standard practices, which are facilitated and complemented by informal practices. A bureaucracy or an organisation has goals, which are sought through formal and informal practices and habitudes. However, organisational activities are members' activities (organisations are made up of members, of course) which are not necessarily in furtherance of the organisation's goals.

Organisational activities are consequential both within and without the organisation. Within the organisation, organisational activities are known to certain members of the organisation but not others, i.e. within the organisation "select" members are party to some organisational (members') activities. "Select" works to show how some members are in possession of knowledge which is not available to all members in the organisation, rather than denoting exclusivity or status: some members may be in possession of certain knowledge that would be of interest to other members of the organisation, e.g. a forthcoming personnel directive from the Head of Personnel may be of interest to members of the organisation, but, through their ordinary methods as competent members of society, staff at the Personnel Department work to withhold this information, to keep it from becoming public knowledge within the organisation. Furthermore, not all members of Personnel staff may be aware of an upcoming personnel directive that would affect the working arrangements or conditions of all members of the organisation, including themselves.

The above example does not intend to suggest a “top down” model of the possession of organisational knowledge, whereby (using Civil Service levels for the practical purposes of categorial illustration) Senior Executive Officers are in possession of more knowledge of the organisation’s activities than Higher Executive Officers, who in turn are in possession of knowledge than Executive Officers, etc. Such a model

“suppose[s] that there must be some connection between what members know and what they do.”

(Sharrock 1974:45)<sup>8</sup>

With such a conception of incremental levels of knowledge of organisational activities, one would expect the Chief Executive Officer to be best placed, or at least to be in possession of the greatest amount of knowledge. As Strauss *et al.* (1971) argue *apropos* the invocation and application of rules on hospital wards, the case is otherwise.

Knowledge of organisational activities is neither distributed vertically, i.e. the higher the grade, the more organisational knowledge is possessed; nor horizontally, i.e. incumbents of particular grades, say Administration Officers, possess the same level of knowledge of organisational activity as each other. Instead, organisational knowledge is possessed by departmental cohorts, or team-like units, which are comprised by incumbents of various grades, e.g. one HEO, two EOs, one AO and three AAs. This “unit-specific” nature of organisational practices, i.e. specificities of organisational activities not only between organisations but between departments within the same organisation itself, is analogous to the findings by Garfinkel *et al.* (1981): the work of

scientific inquiry varies not only between disciplines, e.g. physics and chemistry, but between laboratories within the same discipline (“that’s not how we do things here”, etc.).

So the possession of knowledge regarding organisational activities is itself organised:

“Characteristically, the specifics of who, what, when, and where are well guarded team secrets of cliques and cabals...in all bureaucratically organised settings. From the point of view of each occupational team, there are the specifics that facilitate the team’s accomplishment of its occupational daily round which is none of the business of some other occupational team in the [organisation]. This is not news of course, except that any investigator has to confront it as a fact of [...] investigative life when, for example, in order to decide the import of what is in the record, [the investigator] has to consult materials that are not in the record but are nevertheless known and count to someone.”

(Garfinkel and Bittner 1967:195)

Garfinkel and Bittner are referring here to intra-organisational, inter-departmental protocols or organisational activities. These are not competing or conflicting interests: occupational teams or departments are indifferent to the “how” of different occupational teams’ activities, but are instead orientated to the “business at hand” of other departments’ responsibilities and outcomes. Occupational teams have “propositional knowledge”, i.e. knowledge *that*, rather than (what Watson and Weinberg [1982] call) “procedural knowledge”, i.e. knowledge *how*, of other occupational teams: they know what other departments’ responsibilities and outcomes are and should be, rather than how they are achieved.

Garfinkel and Bittner are highlighting also the problematic nature of organisational research, i.e. how amenable organisations are to adequate and comprehensive study. Persons, e.g. researchers from without are unable to arrive at definitive “findings” about “the organisation” because the organisation is an assemblage of discrete units; an organisational study is a study of units and their members, which together make up the organisation, rather than a study of an organisation *per se*.

To further compound the researcher’s difficulties, as Garfinkel and Bittner note, the unit-specific activities of members results in the omission of relevant particulars from “official” or organisationally-ratified sources, e.g. textual documentation. Information contained in official documents may not be the defining or relevant information, relevant information may not be retrievable from official sources, and the relevant information may only be retrievable from e.g. the gossip nexus. A document contains the outcome of decision-making practices, but does not contain the decision-making practices themselves. (The organisation and organisationally relevant features of clinic folders were brought to the study of bibliographies in the bibliography of Edward Rose, Chapter Three.)

The perceived asymmetry between “insiders”, members of the informal network, and its “outsiders”, was pointed to by buddies when they attempted to invoke the organisation’s codes of practice on confidentiality. I was present to observe instances, apart from the examples regularly reported by buddies, when activities of some members were seen as violations of the codes of confidentiality by buddies. Such

violations were sanctioned under the code of practice with expulsion from the organisation, and salaried-staff and buddies voiced expressions including

“They wouldn’t let me do that”

and

“We wouldn’t get away with that”,

where the pronouns “me”, “we” and “they” glossed the perceived existence of different groups within the organisation: “those who can, and those who aren’t allowed”. “We” is therefore not the “organisational ‘we’” but refers to a sub-group within the organisation.

All information within the organisation was confidential, but information which certain people seemed entitled to was withheld from them by certain other people, seemingly on an *ad hoc* basis. Somewhat related notions are to be found in Lee-Treweek (1994). Lee-Treweek shows how the *ad hoc* invocation of Health and Safety regulations by auxiliaries in elder care homes operates as a “gatekeeping” device (Erickson and Schultz 1982). Visitors did not possess rights to amend the rules: Lee-Treweek found that auxiliaries excluded residents and visitors from the kitchen, although they permitted entry to residents if it suited their purposes (e.g. if a resident wished to help with the washing up). In these cases, and the organisation under consideration in this chapter, the rules—where rules are supposedly set out in the Codes of Practice—are invoked, and invoked as occasioned matters by those who possess rights to invoke those rules, i.e. we can observe how goal posts are shifted “in flight”.

Confidential information was withheld and disclosed; it was manifested in talk<sup>9</sup> and in documents.<sup>10</sup> The months working in the office were fascinating and instructive; that I was “trained” to produce the “Allocation List”, or A. List (a list of prisoners who had requested buddies, which was circulated within the befriending groups for buddies to pick who they would prefer to befriend), was of particular salience.

The compiler of the A. List was using confidential documents, *viz.* letters from prisoners in the production of another confidential document, *viz.* the A. List. The compiler had to summarise information contained within a letter of request from a prisoner and transfer it onto computer before printing it out. There are specific details which must be included on an entry to the list: name, Prison ID Number, Earliest Date of Release (E.D.R.), offence, sentence, previous offences. These details, if not included in the offender’s letter, were written on the “Referral Form”, which was countersigned by the offender’s probation officer. (A specimen copy of the Referral Form is reproduced in the Appendix.) As this thesis on the linguistic constitution of research processes shows, *via* various case analyses, a series of *transformations* is effected in ensuring that those meaningful matters for members’ horizons of relevance are *preserved*. Information contained within the befriending request will be preserved in the pro forma formatting of the A. List; whilst the process of preservation necessarily performs transformations upon this self-same information. These complementary processes are exemplified in more detail below.

### ***Administrative Activities: The “Process” of Befriending***

For purposes of clarity, the administrative process of befriending is summarised before it is detailed. Summarising members’ practices hides the interactional and interpretive work involved in their accomplishment. The mechanical breakdown of activities is not intended to be stipulative but is listed for the convenience of the reader in approaching this section, in which greater explication of interpretive, linguistic practices is provided.

A summary of stages might look as follows:

1. Office receives request for befriending, *via a standard request-form and/or letter*
2. Office acknowledges receipt of request for befriending with *standard letter*, sent directly to offender
3. Office transfers details of request to *A. List*
4. *A. List* distributed at next buddies’ meeting
5. Process of negotiation between buddies
6. Buddy agrees to befriend offender
7. Office sends *standard letter* to buddy: notification of offender’s details (name, identification number, full address), with instructions to contact offender in writing immediately
8. Office sends *standard letter* to offender: notification that the offender will receive a letter from “their” buddy shortly
9. Office sends *standard letter* to Probation: notification of outcome of befriending request—that the offender has been befriended, and by whom

Here, the befriending process has been broken down into stages—a “career”—enumerating its constituent administrative tasks.

The sociological notion of a career is found in a recent ethnography of the International Monetary Fund (IMF). In “an ethnographic case study of technology in contemporary organisational life” (Harper 1997:86), Harper looks at the “before and after” relevances of documents produced within the IMF. He introduces the notion of a “documentary career” as a departure from and furtherance of Everett Cherrington Hughes’ concept of career as it originally applied to people in the world (Hughes 1937). A document is subject to scrutiny within stages of its career; however, other activities which impinge upon the document—“distribution, delivery, archiving and much more besides” (Harper 1997:44)—are not concerned by such matters as the adequacy, relevance or utility of the document. What Harper calls “transformations” are the changes in status of the document during its career—before and after being finalised and distributed. Harper outlines the career of a document as follows:

“[a] staff report starts its career when it is first drafted by a mission team. At this point, certain sorts of concerns begin to show themselves. These have to do with how a team gradually works up and refines the contents of the document. At the next stage in its career, the document is reviewed. Here, there is set of different and partly overlapping concerns to those of the mission team in the drafting stage. Once it has been reviewed, it is then delivered to the DPU [Document Preparation Unit]. [...] Once the DPU has dealt with it, it goes on to what is more or less the last career stage ...—its consideration by the Executive Board.”  
(Harper 1997:4)

Harper's detailed ethnography shows that documents—in this case economic documents—do not “magically” appear out of thin air: they are the eventual outcome of concerted effort by people who meet with each other, discuss, i.e. talk about the document, which is subject to drafting, reviewing and redrafting, and these activities are to be talked about. What this means is that the document’s production constitutes a series of interactions between the author(s) and the reviewers. However, the finalised document does not make available that series of interactional processes to its reader.

Although a listing of an “administrative career”, as outlined above, apparently contradicts my assertion that this is not meant to be stipulative, the activities are routinely performed in the sequential order set out above—the production of documents is ordered deliberately, as explained later.

These stages are dependent on an offender requesting a befriender—standard forms are attached to brochures about The Organisation and its work, and the request form is detachable *via* perforations in the paper. Brochures are enclosed in every envelope sent to offenders and probation officers from the Office; the aim of this “blanket distribution” is to provide the opportunity for potential clients to fill in and submit a befriending request, as staff at The Organisation reason that offenders

“just leave these things lying around. Someone’s bound to pick it up. There isn’t anything to do so they’ll read absolutely anything. And when they realise that if they send in the coupon then someone comes to visit them, they get in contact with us.”<sup>11</sup>

Having transferred befriending requests from letters “on spec” onto the A. List, i.e. having read letters from offenders in detail, this practice of blanket distribution succeeded in its aim (regardless of the veracity of the reasoning). Letters which did not accompany request-forms would typically state that the offender had “seen one of The Organisation’s leaflets”.

The numbered stages are, for members, recognisably parts of a routine, with which the administrator at the Office is familiar. The administrative process only begins on the receipt of a request (1). The administrator acknowledges receipt of the request with a standard letter (2), which is stored in the wordprocessor and printed on letter-headed paper. The administrator must double-check the identification number, which is required in all communications with offenders within the prison system. Prisoners are moved between prisons frequently, and these moves are made at short notice (this is known as “ghosting”); although ghosting causes the families of serving offenders anxiety and great inconvenience, the Home Office justifies the practice on grounds of security and prison harmony. Some prisoners are renowned (in The Organisation and the prison system) for changing their names, or only answering to other names, in attempts to cause inconvenience to prison warders. So where a prisoner’s name and address may change during their sentence, their identification number remains constant throughout. Ensuring that the identification number is correct is the only way of ensuring that communiqués reach the intended recipient. Identification numbers are printed by probation offices and The Organisation in bold typeface.

***Administrative Practices: The Ecology of the Office***

Most letters from the Office are sent *via* second-class post, and all are posted at the same time at the post-box across the road: when the Regional Officer vacates the office in late afternoon, in time to catch the last daily collection. Extended blocks of the administrator's day are spent opening letters from prisoners—which are stamped by the institution and have the prisoner's name and identification number written on: before it is opened, the administrator has recognised it to be a letter from an inmate—inputting the new set of details into the prepared template on the wordprocessor for printing as a standard letter on letter-headed paper; folding the paper so that the name, address and identification number are visible through the transparent window; sealing the envelope and placing it on a pile of envelopes-for-posting, on the window-sill.

Within a small box room, the Regional Officer does not have much spare space with which to store documentary and administrative materials. The door to the room does not open to its full extent because the filing cabinet is pressed up against the door-jam. The desk is pressed against the wall, in the corner, and the Regional Officer moves about the office – from desk to computer and back again – by swiveling around on the chair. The only other furnishings in the room are a sofa, which sits below the window, a small white coffee table, and a wastepaper basket. This remained empty during the period of observations: any documents for disposal which contain any identifying details of offenders (and, from the break-down of stages and documents listed above, this refers, *de facto*, to all documents) are shredded. (The shredder is outwith the office itself.)

A large white telephone, incorporating an telephone-answering machine, sits on the desk. It has a long cord, which enables the Regional Officer to hold the handset while standing up to search through the desk drawers, the orange plastic in- and out-boxes on top of the filing cabinet (and the filing cabinet itself), or to turn around to the computer whilst remaining seated on the swivel chair. (The printer sits on the floor below the computer.) The right-hand top drawer of her desk contained the letter-head stationery. Extra reams of this official stationery were in the large bottom drawer, underneath piles of brochures held together with elastic bands: a brochure was enclosed in every envelope sent out to prisoners, probation officers and any recipients other than buddies. The middle drawer contained blank sheets of A4 paper, which were used when letters to prisoners or probation ran over more than a single page, and for letters to buddies.

Every letter printed out in the office is photocopied for filing in the filing cabinet. Letters constitute “client-files”. The letters that constitute client-files are those letters which have been itemised above: the original request for befriending; a photocopy of the acknowledgment of request; photocopy of the letter to the buddy who elects to befriend the particular offender (“client”); photocopy of the letter to the offender informing them who their buddy is, and when they can expect to be contacted; photocopy of the letter to the offender’s probation officer informing them of the progress of the befriending process; photocopies of any other letters sent from the office regarding the offender (to the offender, to the offender’s buddy, to the offender’s probation officer); letters received at the office from the buddy befriending the offender;

letters received at the office from the offender's probation officer; letters from the offender.

As each subsequent letter is received, or sent (after having been photocopied), it is stapled to the front of the previous letter. This system of filing ensures that the last communication is always the one immediately visible in the client-file.

### ***Intersections of Membership Practices and Administrative Practices***

The request-form asks offenders to provide information on hobbies and pastimes, so that The Organisation can allocate them to a “suitable”<sup>12</sup> befriender. The information provided by the offender, along with practical details (name, identification number, current institution, offence, sentence, etc.) are inputted into a file on the wordprocessor (3). Although this is seen as a routine task in the processing of a befriending request, this administrative task involves the administrator in transferring details from textual materials, e.g. on-spec letters which do not conform to the convenient “box structure” of standard request-forms; i.e. the recovery and transforming of information for administrative purposes. This involves the administrator in interpretive work, of varying degrees of complexity, which is explicated further below.

Compiling the A. List is an essential part of the befriending process. I do not use the word “routine” as a downgrading term, but to indicate its recognised ordinariness for members in that its constitutive tasks are linguistic, practical activities.

Before each meeting of buddies, the administrator prints this document (not on letter-headed paper) and addresses it to the chairperson of the area groups. Since each day's mail is posted at the same time, if a request arrives at the office after the list has been sent to the area chairpersons, there is a whole month before the new request can be discussed at the next meeting. This is a source of delay, and The Organisation is anxious to avoid any negative inferences which may be made by the offender concerned as to the reasons for the delay. This document, the Allocation List, is the textual material which buddies have access to in deciding who to befriend. During the group meeting, the A. List will be circulated around the group (4). Buddies will (or will not), in turn, opt to "take" those mentioned on the A. List. The Chairperson (or Officer, if present at the meeting) provides additional information orally, if they have any additional information to add. Additional information may be "background" information on the offender, e.g. if the offender is known personally to the Chairperson/Officer, if the offender is known to have a "reputation" (other buddies present at the meeting may have encountered the offender, either in the prison visiting room when visiting one of their own clients, or recognise the offender's name *via* clients' letters). E.g.,

"Steve's this huge guy with a shaved head. Massive biceps, covered in tattoos all down here. He's got a lovely character. A real gentle giant."

and

"He's had his beard shaved off for Comic Relief. I didn't recognise him. He had a really long beard that'd been growing for years, and he had it shaved off. Raised quite a lot of money from the staff too."

These descriptions<sup>13</sup> during meetings prompted animated discussions among the buddies, i.e. they generated topics of conversation. According to fieldnotes, the topics generated by these descriptions produced extended blocs of talk before discussions returned to the “business at hand”. The topics these instances generated were on tattoos and charity work inside prisons, where buddies produced “second stories” on prior turns at talk.

The Officer (rather than the Chairperson, who is not party to the original request or communiqués with the offender nor probation), if present, may have other additional information, which they did not include as part of the entry on the A. List at the compilation stage; or information received from Probation. The Chairperson (or the Officer, if present) will also inform those present at the meeting when the befriending-request was received and, therefore, how long the offender has been waiting for a befriender. (The longer an offender’s name has remained on the A. List, the more “urgent” allocation to a buddy becomes.)

Enumerating stages of the befriending process, for the convenience of the reader, reifies and “telescopes” stages which may be of a piece. For instance, enumeration presents as discrete stages the election to befriend an offender from the A. List: the group-meeting (4), at which *it is hoped* a buddy will choose to befriend him, overlaps with (5) and (6). Negotiations between buddies (5), and how the practical production of the A. List influences these negotiations are discussed below. These negotiations between buddies are discursive activities. Whilst reaching agreement is discursive, it is formalised for

administrative purposes when agreement is reached (6). The chairperson then enters the buddy's name onto the A. List beside the name and identification number of offenders.

After formally agreeing to befriend an offender, the buddy receives a letter from the administrator (7), who, if the administrator was not present at the meeting, has been informed of the formal agreement by the chairperson. This is a standard letter, on letter-headed paper, which includes the details of the offender that the buddy has agreed to befriend. As mentioned above, The Organisation tries to instill immediate action *as a policy* when contacting offenders. Hence, the letter received by the buddy instructs them to introduce themselves to the offender, in writing, immediately.

A standard letter is written to the offender at the same time, to be posted in the same batch of mail, which informs the offender of the outcome of the process (8). These standard letters to offenders and befrienders (buddies) are produced simultaneously so that the offender's details (name, institution, identification number) are transferred using the cut-and-paste function of the wordprocessor. However, the administrator takes particular care when informing the offender of the buddy's details that the buddy's real name is not included. Hence, the standard letter to the buddy is addressed using their real name whilst the standard letter to the offender uses the buddy's pseudonym.

The administrator sends a standard letter to Probation (9), still using the text placed on the clipboard with the cut-and-paste facility, printed on The Organisation's official stationery. If details of the particular offender's probation officer are to hand, e.g. on the Referral Form that accompanies the request, this letter is addressed and sent directly to that probation officer—informing them of the outcome of the administrative process of befriending. The administrator also contacts the probation officer if details necessary for the befriending process are omitted from the request-form.

The standard letter to the offender is adjusted or “customised” according to contingencies. These contingencies may vary. If the request is received by the Office after the A. List has been posted to the Chairpersons of the area groups, the administrator informs the offender to expect a delay of one month before the process even begins. As mentioned above, this is to assuage the offender who may infer that the Office was neglecting their request. Customising the standard letter also affords the administrator with the opportunity to clarify any misunderstandings that may have arisen on the part of the offender when they made their request. Written requests often specify details of preferred befrienders. Whilst some letters state that the writer would welcome a befriendeer of either sex, others are more unequivocal about their desires. (When such specifications are encountered, they include variations of preferences: e.g. sex—female, age—18-25; sometimes other characteristics, e.g. height, body shape or hair colour are included also.) In customising the standard letter, the administrator addresses this section of the letter directly. The usual response is

“We are NOT a dating agency!”

and then proceeds to outline the Mission Statement and aims of The Organisation. If the letter from the offender contained requests for information or questions, the administrator endeavours to respond to these requests or, if necessary, inform the offender that their query will be answered as soon as possible.

The “customisation” of the standard letters, in response to information contained within letters of request, is an example of what Sacks and his colleagues (Sacks *et al.* 1974:727; Sacks and Schegloff 1979:16 *et seq.*) refer to as “recipient design”.

The standard letter to the buddy who has elected to befriend an offender contains limited information concerning the offender himself. The administrator tells the buddy of the earliest date of release (E.D.R.), which is written on the Referral Form by the offender’s probation officer. The E.D.R. is set according to the “tariff” of particular offences, the ergodic or expectable-predictable sentences carried by specific offences, which may be, *mutatis mutandis*, “variant to the exigencies” (Parsons, quoted by Garfinkel 1967:124) of sentencing practices and directives by the Home Secretary. Prins notes that comparable offences may not attract comparable sentences when such factors as age, mental state, criminal record, attitude, degree of provocation, harm caused and relationship to victim are taken into consideration (Prins 1982:107). Particulars glossed as “extenuating circumstances”, the preferences of the judge imposing the sentence (benign, condign, severe<sup>14</sup>) towards certain crimes and certain

modes of punishment, i.e. custodial or non-custodial sentences, combined with vicissitudes of “ideologies of justice” (Prins 1982:112), work to influence sentences and tariffs thereof.

Sentencing comes towards the tail-end of a temporal sequence of procedural, common-sense exigencies. Apprehension of an offender, for example, may depend upon features such as assessments by law-enforcers—wrong person in the wrong place at the wrong time; negotiation between law-enforcement officers and families, occasioning the redefinition of activities; and definition of offence within a legally defined and common-sense schemata of related offences. The E.D.R. was crucial in allocation, as we shall see below.

The offence, a detail that must be entered on the A. List, is taken from the Referral Form. However, “offence” is a gloss which can be misleading for potential buddies. As Sudnow says, a distinction is to be made between “necessarily-included-lesser-offenses” and “situationally-included-lesser-offenses” (Sudnow 1965:256). This distinction refers to the legally determined “courses of action”, whereby certain actions are implied as having occurred in the course of other actions, although these “lesser” actions might actually not have taken place. Sudnow uses as an example the charge of murder: a person may be convicted of intent to commit murder, or of committing murder, but not both. The committing of murder necessarily implies intending to commit murder, a lesser offense which is not included in the charge. Sudnow also invites his readers to consider the charge of “statutory rape”, which necessarily includes

the charge of “contributing to the delinquency of a minor”, yet does not imply murderous intent (Sudnow 1965:257).<sup>15</sup>

Prior to Sudnow’s study, Garfinkel (1949) had also noted the problematic “face-sheet” status of statistics of crime; specifically, murder. His recognition and identification of the interpretability of murder charges constitutes a critique of inquiries based upon official statistics, i.e. what are statistics *actually of*?

Garfinkel attended a series of criminal trials in North Carolina, in which the defendant had been charged with murder, thereby witnessing the event-production of official statistics. The trials consisted of and accounted for four types of murder charges, *viz.* Black on White, White on White, Black on Black, and White on Black. That is, the murder cases were both inter-racial and intra-racial. The official statistics incorporated, but did not reveal, those linguistically constituted activities by which the courtroom leveled charges, arrived at convictions and imposed sentences on defendants. Although defendants in cases of W-B and B-B killings, for instance, may be indicted on an identical charge of 1° murder, the trajectories of the cases may differ. The differences in trajectories were, according to Garfinkel (1949:378), located in what are “reasonable” indictments and level of guilt, “as the court sees it” (where “the court” is an assemblage of category incumbents, e.g. judge, members of the jury, and attorneys).

Garfinkel’s observations are elaborated *via* Membership Categorisation Analysis. Murders are “duplicatively organised” in that murderers have victims; murderer and

victim constitute a “standardised relational pair”. Elsewhere, this thesis notes the duplicative organisation of categories, where one category commonsensically implicates another category, e.g. girlfriend/boyfriend, husband/wife, parent/child, doctor/patient; or, in the present discussion, Counsel for the Defence/Counsel for the Prosecution. D.R. Watson brings Sacks’ notion of standardised relational pairs to bear upon data derived from murder interrogations. D.R. Watson (1983:35) notes how the racial categories White and Black also constitute relational pairs. Murders may be imputed to be “racially-motivated” if the relational pairs Black/White and murderer/victim are “mapped onto” each other.

The “face-sheet” nature of offences, which is purveyed by formal documentation and administrative practices, is problematic for befrienders also. The offence that is listed on the Referral Form glosses the details of the offence. Although the offence *for which the offender is currently sentenced* is listed, along with such details as “Previous for x”, “Previous for y”, or “Previous for z”, it does not make available salient (where “salience” is determined by members) details of the nature of the offence. So, “Murder” does not differentiate or reveal “Double Murder”<sup>16</sup> or “Double Child Murder”, offences which are of a “different order” to befrienders in their deliberations or decision-making activities, as shown in their *post hoc* client reports to group meetings.<sup>17</sup> (A specimen copy of a client-report form, which buddies submit to the Chair of the meeting and from which buddies deliver oral presentations on their befriending activities, is reprinted in the Appendix.) To befrienders, these glosses<sup>18</sup> are sources of tension which continually remain unresolved: some befrienders wish to have

access to the nature of offences before agreeing to befriend a prisoner, which would influence their decision, regardless of their obligations to remain impartial and non-judgmental.<sup>19</sup> However, this request is met with rebuttals which maintain that such information remains confidential, and will be discussed with the befriendeer by the offender concerned if they so wish.

This equanimity involves a paradox (in its everyday and philosophical senses), in that during training befrienders are told by The Organisation that offenders are accomplished liars: if befrienders take The Organisation at their word, how can they believe what they are told by offenders?<sup>20</sup> Also, the offender need not inform the befriendeer of the nature of the offence at all. The Organisation argues that trust is built up in the established and developing relationship between offender and befriendeer, whereby if information *apropos* of details of the offence is disclosed to the befriendeer, it will be at the discretion of the offender concerned. If the offender wishes to talk to the befriendeer about the offence, they will do so when they feel that they can trust\*<sup>21</sup> the befriendeer.

This argument is not acceptable to some befrienders, however, who note that the offender does not always reveal information that the befriendeer would find useful; nor do they necessarily provide accurate information. According to The Organisation, this ‘knowledge-gap’ is a useful resource for the offender, who can “distort” the actuality of the situation. The word distort appears in diacritic marks because members of The Organisation were sometimes reluctant to say outright that offenders were liars, whose

accounts of themselves or self-disclosures were not to be taken on trust\*. This position, which derives from the “philanthropic” tenets of The Organisation, is irreconcilable with the training novitiate befrienders receive mentioned above, and with the experiences of experienced befrienders.

Before proceeding to discuss knowledge-gaps, disclosures and the development of trust\* between befrienders and offenders in more detail, this chapter will provide materials extrinsic to the befriending process which bear upon these issues; i.e. corpora of literature which this chapter makes relevant here-and-now.<sup>22</sup>

### ***Disclosing Confidential Information***

#### **Disclosure and Secret-Telling**

Blumenstein (1973) considers the nature of disclosing information: not *what* the information is (this is of no concern to us *qua* analysts) but rather “announcing” that a person is in possession of privileged information that may be of interest to another person, and the trajectories which follow such announcements. Blumenstein concerns himself with relationships between intimates, courting couples, where one indicates to another that personal knowledge may be disclosed at some future occasion (e.g. “I don’t want to talk about it just yet”; “Perhaps when we know each other better I’ll tell you”).

By “trajectories” I mean announcements of controlled disclosure suggest or imply that information will be disclosed when “the recipient” is held in a position of “trustworthiness” by “the discloser”: the discloser shows to the recipient that they are

entrusted with information which is not commonly provided to others; and the recipient realises that upon receipt of this personal knowledge that their status will change

“from that of the masses to someone special (someone trusted, and someone who then knows what others do not)”

(Blumenstein 1973:211)

Blumenstein makes a preliminary exploration of the trajectories relationships can take following the announcement of controlled disclosures. Another trajectory, which as a study of “good times” Blumenstein does not offer us, is where the announcer of controlled disclosure does not disclose or “fails to deliver” a previously announced disclosure—perhaps due to the failure of the relationship before the disclosure is made, the failure to establish a mutual trust which facilitates the disclosure, or a change in the nature of the relationship.

We can see a parallel between Blumenstein’s remarks on personal intimacy with organisational affiliation in the disclosure of information: in considering that information is “to be disclosed only to a select few” (Blumenstein 1973:212), there exists a resemblance whereby the recipient of information is adjudged (by the organisation’s *apparatchik* or disclosers or decision-makers) to be worthy of their trust\*. There are unequal rights of access to information between the *apparatchik* and salaried staff, between salaried staff and volunteer members, and between volunteers and clients. Disclosure will be determined by those in possession of these rights.

Blumenstein is echoed by Rodriguez and Ryave (1992) in their study of secret-telling interactions, when they argue

“The act of choosing to tell someone a secret is a risk that implicates the recipient as special and defines the relationship toward an increased level of trust and solidarity”

(Rodriguez and Ryave 1992:301)

Due to their self-explanatory nature, secrets are seldom encountered as serious topics for investigation. Indeed, they note

“because secrecy seeks exclusivity and evades observability, the facets of secrecy that have been least studied are the actual instances of the telling and receiving of secrets in everyday social situations”

(Rodriguez and Ryave 1992:297)

To collect data on this recalcitrant feature of social life, Rodriguez and Ryave devised a method of “reflexive participant observation” (Rodriguez and Ryave 1992:298). Reflexive participant observation is a variant on the diary method: put simply, people take notes on events that they themselves are involved in. ‘Specially trained’ student informants, ‘armed’ with concealed notebooks and pens, documented those occasions when they were taken into another’s confidence. Reports of these occasions or “secret telling interactions” included the location, situation and relationships of parties to the secret telling, along with conversational details. A *modus operandi* for maintaining confidentiality was introduced at this stage of the research:

“For ethical reasons, the students were instructed to use pseudonyms in their reports and to change any biographical or other references that might give away

the identities of relevant parties. The students were also told not to place their names on the reports”

(Rodriguez and Ryave 1992:298)

The data collected exhibited cases of “explicit” (or announced) and “implicit” (or unannounced) secrets. Implicit secrets are particulars that Rodriguez and Ryave’s research assistants reported that the secret teller would not wish to be revealed, though the particular secret nature was not referred to by the teller; these constituted information that was ‘obviously’ not for public knowledge. Explicit secrets, on the other hand, were particulars which the teller notified the recipient of the secret status of information prior to disclosure.

Rodriguez and Ryave noted that the predominant organisation of secret-telling interactions possessed the following structure: 1. secret teller announces that a secret will be told; 2. recipient agrees to keep forthcoming information secret; 3. the telling of the secret; and 4. recipient responds to the secret-telling.

An example provided by Rodriguez and Ryave demonstrates this structure:

Place: My bedroom  
Who: Interaction between myself and my sister  
Situation: Discussing a common friend whose name is Helen

JAN: I have something to tell you, but promise me not to tell anyone about it.  
I’m telling you because I always tell you everything.

ME: What is it? Go ahead and tell me. You know when it comes to secrets I  
keep my mouth shut.

JAN: You know what, Helen got an abortion. She was three months pregnant  
and she decided not to keep it. She had to borrow from Roy because her  
boyfriend didn’t have the money yet. It’s sad because she wanted to

keep the baby, but she didn't want her parents to know about it. She's still young you know.

ME: That is sad! I wonder if she feels guilty about it? Well, I hope she won't get pregnant again until she's ready for it.

JAN: Make sure you don't tell anyone, okay?

ME: Sure.

(Rodriguez and Ryave 1992:300)

Following Terasaki (1976), the sequence of talk presented by Rodriguez and Ryave manifests a “pre-announcement sequence”. In Terasaki’s analysis, pre-announcements inform recipients that the speaker has some news to tell; and, dependent on the recipient’s response to the pre-announcement, e.g. whether the hearer had heard the news already, the speaker withholds or proceeds with the news-telling. In the secret-telling sequence above, the first pair-part of the pre-announcement sequence is not establishing whether the hearer knows the forthcoming secret. Rather, the pre-announcement provides instructions for the subsequent treatment of the forthcoming secret, *viz.* requesting a promise that the secret will be kept. The second pair-part of the pre-announcement sequence provides this assurance, and the secret-teller proceeds.

Within Rodriguez and Ryave’s data, there were some occasions when the secret teller confirmed the secret status of the particulars after the actual secret-telling, e.g.

(Secret Information.....)

CAROLE: No, she doesn't want to face up to it. I've told her what I thought and she ignores me. Whatever you do don't let on to either one of them that I told you any of this.

ME: Don't worry, I won't.

(Rodriguez and Ryave 1992:316)

Typically, however, the organisation of secret-telling interactions was of the structure 1-4 above. Within Rodriguez and Ryave's data, there were no instances in which a recipient refused to keep a forthcoming secret.

However, the *post hoc* methods used in this study are problematic. Although Rodriguez and Ryave maintain a persuasive argument—that by the very fact of their being secret, secrets and secret-tellings are commonly unavailable for study—a field of phenomena is omitted from their research. The secret-telling sequences are necessarily intuitive: rather than *in situ* naturally occurring events, they are *ex situ* accounts. This may be illustrated by the idealised and sanitised reconstructions contained in the sequences above, e.g. “*What is it? Go ahead and tell me. You know when it comes to secrets I keep my mouth shut*”; and “*That is sad! I wonder if she feels guilty about it? Well, I hope she won't get pregnant again until she's ready for it*”. That corpus of ‘seemingly trivial’ conversational phenomena, e.g. pauses, micropauses, hesitations, repetitions and continuers are interactional, i.e. collaborative events which work to accomplish a conversational task successfully: displaying engagement, eliciting engagement, achieving mutual recipiency, etc.

The diary method (Zimmerman and Wieder 1977) cannot retrieve the fine detail of interactional phenomena necessary for adequate description of the negotiation work involved in secret-tellings. The researchers are orientated to the practical purposes of secret-telling: to the episodic occurrence of secret-telling in interactions, i.e. that the researchers decide that they are to become parties to secret, that a secret is about to be

disclosed. This research procedure does not make available for inspection how they (as researchers) decide that they have been party to a secret, and how the announcement, agreement, telling and response are collaborative accomplishments of the participants.

There is another aspect to disclosing information, which is the denial of disclosure. A “get-out clause” is implicative in members’ provision of *caveats* in the secret-telling sequences. So in the event of any subsequent disclosure of the secret, members can deny that they are the source of disclosure, or medium by which the secret became known by others.

As preliminary cases, we can see how future “deniability”, i.e. the retrospective-prospective aspects of secrecy, is inferentially available from Jan’s announcement of forthcoming disclosure (“*I have something to tell you, but promise me not to tell anyone about it*”), and in both Jan’s and Carole’s admonitions (“*Make sure you don’t tell anyone, okay?*” and “*Whatever you do don’t let on to either one of them that I told you any of this*”), which concern the responsibility of being granted access to privileged information, of being told a secret. These admonitions not only require the listener to keep the forthcoming secret (something that the secret teller has observably failed to do), but show how secret-telling has an in-built deniability, *qua* “you didn’t hear this from *me*”: if the identity of the discloser of the secret information is equivocal, the discloser can disclaim or deny responsibility.

It is incumbent upon the secret holder to keep the secret, not to become a secret teller, so that any subsequent disclosure—not to have kept the secret—will reflect badly on them (unless or until the disclosure is found to have come from another source, another secret teller). If Helen realised that her secret had become public knowledge, i.e. become known by people whom she herself had not entrusted with the secret, Jan would fall under suspicion as a source of non-permitted disclosure; and if Helen traced the source of disclosure to Jan's sister, she could infer that it was probably Jan who had originally disclosed the secret. Thus Jan clarifies the responsibilities shared between herself and her sister: "*I'm telling you because I always tell you everything.*" The consequences of being exposed as someone who cannot or does not keep secrets is twofold: first, telling secrets is one way how people become defined as untrustworthy or a "blabbermouth"; second, they no longer get to hear any secrets.

Hence a "paradox" of secrecy, as identified by Beryl Bellman. In his wide-reaching discourses on the nature of secrets and secrecy, Bellman considers various features of secrets and secret-tellings. For example, he submits utterances such as Jan's requests at the beginning and end of the secret-telling sequence above "*not to tell anyone about it*" and "*Make sure you don't tell anyone, okay?*" to Membership Categorisation Analysis. Following Sacks, he observes that "anyone" is a discrete category: *apropos* Helen's secret, this membership category would include specifically Helen's parents at least. A paradox of secrecy is visible from the secret-telling sequence, wherein Jan instructs her sister that she is about to disclose a secret. However, secret-tellings are disclosures in themselves, i.e. the secret-hearer receives instructions not to disclose the secret at the

same time the secret is being disclosed to them by the secret-teller (Bellman 1981:9). Using Membership Categorisation Analysis, Bellman also discusses the routine occurrence of foreshortened, elliptical accounts, which he refers to as “adumbrated message forms” (1981:19). Bellman argues that adumbrated accounts enable secrets to be transmitted between members in possession of particular knowledge. Although secret-tellers produce an adumbrated account, the account is elaborated by the knowledge particular incumbents already know. When the hearer of the adumbrated account puts the adumbrated account together with the knowledge they already have, the secret is realised. Bellman summarises this realisation as to “put two and two together” (*ibid.*).

We can also see how deniability is an organisational activity used by members. Gossip networks can be relied upon as a resource for the deniability of responsibility for disclosure: if the discloser of (or innocent party to) secret information is suspected to be (or accused as being) the source of disclosure, the discloser (or innocent party) can invoke the existence of a gossip network to deny responsibility of disclosure. The existence of a gossip network also works to obscure and protect the source of the disclosure, its *fons et origo*, in that the holder of secret information that becomes public or common knowledge need not have been the discloser, of course; to have “heard it on the grapevine” works to absolve (partially, at least) a member’s responsibility for disclosure. To be informed that “the grapevine” was the route of transmission of secret information is dispiriting for the individual concerned with the protection of a secret:

the grapevine will not only confound the unequivocal identification of their secret's discloser, but the individual will gloomily infer "Now everyone knows".

It is also a commonplace to learn that information has been disclosed "off the record", or, in Parliamentary language, revealed on "lobby terms". "Off-the-record" information glosses information disclosed informally rather than formally, and which possesses a non-attributable status. If the off-the-record information is subsequently repeated, it cannot be attributed to its source (e.g. "this is the case and [name] said so"). If information is attributed, i.e. a member is positively identified as the source, that member can deny being the discloser or source.

The non-attributability of information and the deniability of disclosure of information shows how members are concerned with what Schütz (1973:19 *et seq.*) calls "future projects". Schütz refers to those actions which could feasibly occur, given that such actions had occurred on another occasion—possibly from having witnessed or experienced such discomfiture in the past. Like the revealer of secrets above, the member or organisation (e.g. a journalist or newspaper) is orientated to the consequences of their disclosure of the sources of their information; the a journalist or newspaper refuses to attribute off-the-record information, to the extremes of fighting legal challenges to identify informants. The attribution of remarks or identification of informants compromises tacit responsibilities to maintain confidentiality, whereby they (the member or organisation) are less likely to become party to privileged information in the future. The identification of e.g. police informers or Members of Parliament

willing to speak “off the record” on matters which they may regard as being in the “public interest” by the police or newspapers concerned—in an ethnomethodological sense, providing “rational grounds” for practical activities—discourages off-the-record information being disclosed to them in future.

The disclosers of information are themselves compromised by positive identification as the source of information: the lives of police informers and their families could be put at risk; Members of Parliament face such censures as losing their party whip, demotion or de-selection, or that their disclosure results in damage to their party or one of its prominent figures. In speaking off the record, then, people orientate to the possibility of being identified and the consequences thereof, what the possible future may hold: a lifetime as a back-bencher, vilification (by ministers, colleagues and party loyalists) as the “maverick” who cost the party its chances in the general election. For the informer, the possible future may hold, *in extremis*, beating, knee-capping, or execution.

From this we can see that secrecy is a *sanctionable* matter all along its course, where sanctions differ at different points on that course. This analysis requires, logically, that we see secrecy as a temporally-organised course-of-action, a dimension which is illustrated by the case-study provided in the following section.

Members’ projects take account of the past along with an orientation to forthcoming or possible forthcoming events. The Schützian notion of “future projects” Schütz (*op cit.*) is shown to be used by members in the course of their everyday activities by Agnes, the

intersexed person (Garfinkel 1967). For the purposes of the current discussion, the case study of Agnes is introduced to explicate, *inter alia*, how members are orientated to time (past, present and futures, plural), courses of action, and the concealment, i.e. non-disclosure of information.

### Agnes: Fear of Disclosure

Agnes was continually focused on future courses of action, and her motivated orientation to the future was unrelenting. Agnes sought the familiar, or expected, so that anticipated circumstances held few surprises for her:

“For the many situations where she knew enough, she would have mapped out possible alternative developments beforehand and would have decided the conditions of her choice of one course or another prior to her having to exercise those choices”

(Garfinkel 1967:168)

In the event of any uncertainty or absence of prior knowledge, Agnes would ‘reconnoitre’ or explore a setting:

“She tried to make herself knowledgeable about critical situations before she had to encounter them”

(Garfinkel 1967:168)

If Agnes’ actions are said to have been instrumental, then they were designedly and resolutely directed towards the concealment of particulars suggestive of a male biography and the prevention of disclosure at any cost:

“The effect of such an orientation was to assimilate events both past and prospective to the status of means to ends and lent to the stream of experience an unremitting sense of practical purposiveness”

(Garfinkel 1967:178)

The disclosure of information which contradicted her sex-status as a woman was prevented by the systematic use of member's common-sense methods of interpretation and the knowledge that others relied upon those self-same cultural methods also. Garfinkel called Agnes' common-sense methods for concealing her sex status “passing devices” (Garfinkel 1967:167).

In the work of “passing” (Garfinkel 1967:137) as female, i.e. the common-sense methods used to be interpreted as female by others who, in turn, are using their common-sense methods, Agnes' ongoing, concerted retrospective-prospective orientations to situations enabled her to present biographical particulars as constitutive of an accountably, consistently female biography; and to anticipate potential futures which might compromise, impugn, or even directly challenge her intendedly-female presentation, such that members using the Documentary Method of Interpretation revise the underlying pattern of her not-male status. Garfinkel formulates Agnes' orientation to future projects as

“Her characteristic situation in passing was one in which she had to be prepared to choose, and frequently chose, between securing the feminine identity and accomplishing *ordinary goals*.”

(Garfinkel 1967:139; emphasis supplied)

Agnes tells Garfinkel how she takes lodgings near to any place of employment so that she can walk rather than drive to work: in case of a car accident that rendered her unconscious, and which might necessitate the removal of her underclothing at a hospital, Agnes anticipates a future project that would contradict her chosen sexual presentation. When required to produce a urine sample at a medical examination for a job, Agnes excuses herself as being unable to urinate: when she realised that the toilet was situated in the office itself, rather than a cubicle which could be secured, Agnes orientated to the possibility that “the nurse, because she was entitled to enter the office, would come in while Agnes was manipulating her genitals” (Garfinkel 1967:143), a future which would compromise her successful passing as female. Furthermore, after she had returned home with the specimen bottle, Agnes was orientated to the possible future that urinalysis might identify the urine as male. In anticipating this possible future, Agnes persuaded her female roommate to provide the sample for her instead. That this [asking someone else to provide a urine sample for substitution with one’s own urine] is an accountable matter is shown by Agnes’ explaining to her roommate that she had an infection which, if detected, might jeopardise her chances of getting the job. In any case,

“It was much to be preferred to forego the job than to risk disclosure, with one condition being dependent [...] on the other”  
(Garfinkel 1967:139)

In her doing the work of passing in these and many other passing occasions detailed by Garfinkel, Agnes states

“It is necessary for me to tell little white lies a lot of the time and I think there are those that... those are necessary and they have to be necessary to accomplish results.””

(Garfinkel 1967:169)

### Disclosure: Relationships of Trust\*

Although Blumenstein (1973) was concerned with relationships between intimates, we may profitably adapt his phenomenological considerations on the “announcement of controlled disclosure” to the establishment and development of a trust\* (or trusting) relationship between offenders and befrienders.<sup>23</sup> In the act of announcing controlled disclosure, the holder of the information (in this case the offender) indicates to the potential recipient (the befriendee) of this information that, at a future occasion, withheld information, *viz.* details of the offence, may be disclosed. Upon disclosure, the recipient (befriendee) recognises that they have been entrusted with information that is generally withheld from the many and disclosed to the few, marking a change of status with respect to the discloser (the offender).

We can see, then, that upon the announcement of controlled disclosure one possible trajectory of a befriendee-offender relationship is the development of trust\* leading to disclosure. However, if the disclosure of information is taken as an indicator of a ‘level’ of trust\* within a relationship, then a befriendee may commonsensically surmise that disclosure of information indicates the development of trust\* on the offender’s behalf, while the continued withholding of information indicates that the “required level” of trust\* had not been achieved.<sup>24</sup>

In this way, an offender may promise future delivery of confidential information as a ‘reward’ for services rendered, e.g. contacting people on ‘the outside’ on the offender’s behalf, bringing in stamps, toiletries<sup>25</sup> or cigarettes, etc., without intending to disclose information. Therefore, the ‘knowledge gap’ can be negotiated and used in manipulation of the befriender.

So there exists a difference or element of conflict: the policy of The Organisation is for all information to remain confidential, and those buddies who would prefer more openness within the befriending process before proceeding to meet the offender in prison. This difference between the policy of The Organisation regarding the confidentiality of all details pertaining to the offender prior to befriending, and buddies who express dissatisfaction with the policy, is not to be over-emphasised. Those who expressed dissatisfaction constituted a minority of buddies encountered in the fieldwork settings. However, the disagreement is real, relevant and salient to those people. According to W. I. Thomas’ aphorism, “if men [sic] define situations as real then they are real in their consequences”. So it becomes a “meaningful matter” ‘even though’ it is meaningful to a minority, i.e. it is not held by the majority with whom I talked.

The buddies who spoke to me about this were aware of the potential for conflict, even if they themselves did not wish to alter this feature of the befriending process. In examining *how* people define situations—in this case how an asymmetry of knowledge appertains to the befriending process, which may be consequential to the befriending relationship itself—the ethnomethodological/conversation-analytic “analytic mentality”

provides for explication of commonsense reasoning procedures without resorting to an argument by authority, i.e.  $x$  is so because *Person X* says  $x$  is so. A principled attitude of “indifference” towards phenomena, such as the iteration of dissatisfaction with a policy some regard as the defining feature of The Organisation—that whatever offence an offender may have committed in the past, they deserve a “second chance”—makes members’ definitions available for analysis. The “analytic mentality” of ethnomethodological programmes allows the analyst to approach accounts as “versions” (Cuff 1980; Smith 1978), which may be subject to authorisation, revision and reinterpretation. As mentioned earlier in this chapter, some of the befrienders would dispute “whatever” offence, as the term “whatever” is a potential gloss for offences and previous convictions they wish they had known about before agreeing to befriend particular offenders. This view is incompatible with the aims of The Organisation. The disjunction between these positions may be treated formally using an ethnomethodological study of versions in texts (Smith 1990). This paper includes a comparison of alternative versions of an event (a riot in the Californian city of Berkeley), presented by a university professor and the mayor with each other. This practice is analytically preferable to undercutting a member’s account by the provision of her own “alternative version” of events (Smith 1978), which is a form of methodological irony.

Compiling the A. List *and* attending group meetings afforded an unexpected “vantage point” for research. By this I mean that I was party to information unavailable to buddies; yet I also witnessed the reactions of buddies to information that was available

“in flight”, as they iterated their concerns. In illustration, the following reports upon how buddies adjudged between offenders *via* information provided on administrative reports. For reasons of confidentiality, this is reported only briefly and in abstract terms.

Elsewhere in the request, the offender would provide personal information, biographical details, family details, extent of contact with outside world, hobbies. One offender, in his letter of request, stated that he<sup>26</sup> had a large family, received letters every day, and friends visited him every month. Another offender, however, stated that he “*had lost contact with his family*”, “*doesn’t receive letters or visits*”, and that he “*has a friend but they live too far away so can’t afford to travel for a visit*”. Through their (the buddies’ and compiler’s of the A. List) usage of common-sense reasoning procedures, that “overlapping family of practices” (Baker 1997a:88) including the Documentary Method of Interpretation (Garfinkel 1967), it was the second offender who was allocated a buddy rather than the first. According to the Regional Officer, The Organisation might have even refused the first offender because they had so much contact with “the outside” and volunteers for befriending are scarce; however, whether they befriend or not, it is the preferred option to befriend requests of the second “type” before the first “type”. Allocation is prioritised through the use of the Documentary Method of Interpretation. It is *via* members use of the Documentary Method that people decide between offenders, according to their interpretations of “immediate need”. In the case of the two offenders whose befriending requests arrived at the same time, it was decided that

“this guy is more isolated than that guy, so allocating a befriender to him is more urgent”.

The outcome or decision is unavailable from this fieldnote. The second offender above was allocated to a buddy before the first offender.

The A. List was distributed among the area groups within the administrative “regions”. Any clients requesting a befriender remained on the A. List until they were allocated, i.e. that entry would be “rolled over” to appear in each subsequent A. List until they were allocated. Hence, a further factor in the prioritisation of allocation would be the time an entry spent on the A. List. As the A. List was distributed on a monthly basis, then for an entry to appear on e.g. three lists constitutes a delay of three months in the process of his allocation to a befriender. A “rolled-over” entry would thus appear alongside new requests for the services of befriending. The Chairperson (or the Regional Officer, if present) encouraged buddies to “take” (i.e. befriend) a client if they had been on the list “for some time” without having been allocated. It was argued by the Chairperson and the Regional Officer that the combined time of the practices of allocation, excess waiting for allocation to occur and the bureaucratic procedure of befriending (see below) amount to a perceived delay for offenders who have made a request. (See above for discussions on limiting negative inferences.)

The E.D.R. is a significant detail. If two offenders present the same details (family circumstances, i.e. family visits, letters, etc.), the one who will be allocated first is the

one with the longest left to run on their sentence. An E.D.R. of “two months hence” is “pointless” or “useless”: the process of allocation itself—receiving a befriending request, placing the request on the A. List, finding a befriender, confirming with the befriender that this part of the allocation process should be initiated, notifying the offender of their befriender allocation, asking the offender to send a Visiting Order (V.O.) to the office so to permit a meeting between offender and befriender, waiting to receive the V.O.—takes approximately two months. However, two years or twenty years are entirely different matters. Like Sacks’ observations on the practical use of hypotheticalised data,<sup>27</sup> illustrated by enumerating how many hairs make a man bald (Sacks 1992a:698) and by lawyers questioning how low is a low-flying aircraft<sup>28</sup> (“At 2000 ft? At 1000 ft? At 250 ft? At 5ft?”)—how do buddies decide whether the extent of the sentence still to run, the E.D.R., is “long enough” to proceed, and how long is “long enough” to be lonely?

The significant question is, *how* do people judge “how long is lonely”. It is commonsensically assumed by the organisation’s members that loneliness is attributable to those serving long sentences, *a fortiori* that the longer the sentence, the “lonelier” the offender becomes. This common-sense assumption informs the decision-making activities of allocation.

The ‘hobbies’ section (see Appendix) assisted buddies in choosing who they nominate to befriend. I attended the buddies’ monthly group-meetings where these “doing choosing” activities, decision-making processes were made available. Details in the

hobbies section gave buddies the opportunity to form a judgment of what the offender might be like, possible common interests, potential opening topics for conversation. A philatelist would befriend a philatelist. Someone interested in cricket might befriend someone interested in sport. However, “sport” was a problematic criterion or index of interest: not everyone interested in sport cares for cricket, for instance. Conversely, not everyone interested in cricket is interested in sport. “Sport” was thus revealed to be a gloss for a multiplicity of activities.<sup>29</sup>

A problem, of course, was that the ‘hobbies’ section was not a statement of interests by the offender; it was a summary account of the offender’s interests by the producer of the allocation report who was using the documentary method in their summarising procedures. The hobbies section underwent a process of transformation, as information provided in the befriending request was transferred into the A. List. As the producer of the A. List, attending meetings at which befrienders would debate and negotiate who to choose on the basis of the ‘hobbies’ section that I had produced, I was troubled at how adequate a reflection the allocation details were of the original request. Pivotal decisions rested on *which* details from the original request had been entered (there was a limited amount of space available) and upon the *order* in which these details had been listed.<sup>30</sup> Listing “stamp collecting” ahead of “cricket” on the allocation entry suggests that the offender is more interested in stamp collecting than cricket; the serial ordering might not match the ordering in the original request, however, i.e. placing “cricket” up-front in the original request may appear as the penultimate item in the allocation entry,

and inferences are made accordingly. If two buddies in a meeting wanted to befriend the same offender, the hobbies section was consulted and dissected e.g.

“You can have [befriend] him because you’re more interested in antiques than I am.”

“Well I’ve never been to Cornwall so you’ll have that much more in common.”

Whilst offenders are befriended *in absentia*, hobbies and “other useful information” became “arbiters” in the decision-making process. In this sense, hobbies and “other useful information” became bases for arriving at an “impression” of the offender (Schwartz 1974).

Baker notes that “Unless I am being unusually calculating, I don’t *decide* to befriend someone” (Baker 1996:6; emphasis supplied), a notion that is shown not to be subscribed to by the buddies in their decision-making activities. Or, stated otherwise, buddies (unlike Baker) *are* calculating, and their “calculations” are constituted linguistically, being revealed to this researcher through the observation of group-meetings and discussions. The linguistic constitution of befriending decisions—listed above at (5)—is recognised through the self-reporting character of talk between buddies whilst circulating the A. List among themselves. Whilst buddies’ reasoning for electing to befriend one offender rather than another are utterances contained in fieldnotes (see *caveats* above), they make available the accountability of actions and how accounts are constitutive of actions. From these utterances above we can see that buddies are involved in the search for “talk-about-ables”—candidate topics of conversation with

persons with whom they are as yet unacquainted. The buddies iterate the realisation of “good interactional reasons” for electing to befriend one offender rather than another.

As Watson and Sharrock ask,

“How do people make courses of action appear rational, effective, judiciously chosen, in accord with rules and requirements, etc. to each other?”

(Watson and Sharrock 1991b:5)

In a sense, the buddies’ accounts for befriending one offender rather than another display how the buddies rationalise and routinise their activities *qua* jurors (Garfinkel 1967). That is, they furnish justifications for choices in which a *particular* buddy would be a more suitable befriendeer to a *particular* offender. Moreover, these justifications are *recognisable* justifications, in that they are recognised to be “good reasons” for selection from the A. List by other buddies.

Another confidential document was the “Area Buddies List” (the B. List). This was a print-out of the names, addresses and contact telephone numbers of all the buddies in each region, indicating which group they were in (e.g. Central Manchester, South Manchester, Oldham etc.). Beside each name was the buddy’s pseudonym. Not all buddies opted to use pseudonyms, but a significant number did so. Ratios are not relevant—it was meaningful to those concerned. Sherri Cavan elucidates this attitude towards the phenomenon:

“I am not concerned with how many Hippies panhandle; or whether panhandling in the Haight is done frequently or occasionally. [...] [W]hat makes a

phenomenon important to consider is the existence of an activity in the community on a routine basis.”

(Cavan 1972:25)

### ***Conclusion***

This chapter has reported on a period of participant observation conducted in an organisation. The fieldwork enterprise was instructive in a number of ways, e.g. the relationship between the researcher and “research subjects”, remaining indifferent to phenomena (rather than, say, espousing the organisation’s or organisational sub-groups’ viewpoints). An unexpected outcome of the participant observation is manifest as one of the major themes of this thesis, *viz.* confidentiality.

It was incumbent upon each buddy to learn the pseudonyms of other buddies, because a reference to a buddy’s real name, inside a prison visiting room, could be overheard by the relevant offender or by another offender who reports the real name back to them.

This security measure was to spark an analytic interest in confidentiality, beyond purely professional obligations to maintain confidentiality. In order to obtain funding to pursue academic research, I had to complete sections of the grant application forms—*itself* a natural language activity—on confidentiality (“protecting my subjects”) and providing guarantees, etc.<sup>31</sup> “Signs of release” are linguistically-organised, conducted through natural language by the analyst and members who give or withhold permission for research. Rather than a standard or platitudinous response to this section, I was able to provide a detailed account of the confidential nature of The Organisation itself, which I had already studied (Carlin 1994), and that I was therefore already sensitive to

the protection of identities. I would be assigning pseudonyms to people who already had pseudonyms.

But what if the pseudonym I assigned to an individual coincidentally happened to be that individual's real name?

The point that this raised, indirectly, was the *ad hoc*, "on-the-hoof" procedure of assigning pseudonyms just might not be so unproblematic in this case. *But there are no qualitative research textbooks which tell sociologists how to go about the business of protecting identities—how to "do" pseudonymisation, how to "do" confidentiality.* Confidentiality merits a perfunctory note or paragraph, which tells sociologists that confidentiality must be maintained—no questions asked.

The following chapter does ask questions about confidentiality. It asks how sociologists disguise identities, and demonstrates that confidentiality is an aspect of the research "process". Furthermore, that this aspect of the research process is linguistically organised.

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<sup>1</sup> In association with Saul Mendlovitz, on a jury project organised by Fred Strodtbeck at Chicago Law School. See also Garfinkel's oral statement regarding the jury room research (Hill and Crittenden 1968:5).

<sup>2</sup> In his "Methodological Appendix". See especially "First Efforts." (Whyte 1955:288-298).

<sup>3</sup> For more on "access rituals" see Harper (1998:74-77); the term is Erving Goffman's.

<sup>4</sup> Schegloff (1988:442) refers to his analysis of a transcript as "conjecture" because it concerns a single instance, rather than a corpus of data. Hence there is a difference between Garfinkel and Schegloff

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within the emphasis of conjecture *apropos* the status of data, as it concerns the principles of recording interaction: Schegloff's conjecture is based on recorded data—"at least what was on the tape had happened" (Sacks 1984:26); whereas Garfinkel's use of "conjecture" refers to reconstructions from fieldnotes. Schegloff's special pleading for "single-case analysis" rather than tracking instantiations of a phenomenon across voluminous corpora of data betrays an ignorance of CA practised outwith the US, and harks back to forms of sociology CA sets itself up against. Sacks often used single instances to elucidate the "just this-ness" of interaction; a classic example of Sacks' generic work from a single instance is "The baby cried. The mommy picked it up." This story or pair of utterances was introduced in the section on Conversation Analysis in Chapter One.

<sup>5</sup> On finding "a quiet place" to write notes and feigning trips to the bathroom. In contrast, Emond (personal communication July 7 1999) iterates how writing fieldnotes occasioned perturbations among the residents of a children's home (e.g. "What are you writing?"), interrupted her observations, and inhibited her participation with the group. Physically removing herself from the setting in order to make notes was not an option, as the children *knew* if that was the case. At the suggestion of the group-members themselves, she introduced a Dictaphone, noting down the participants present when the machine was switched on; the recordings, and transcripts thereof, were supplemented by her research diary.

<sup>6</sup> That I should "engag[e] in writing behavior" (Rosenhan 1973:253).

<sup>7</sup> Unlike the "little black book" of the researcher or evaluator, the unknown contents of which are a concern to personnel (Richman 1994:183).

<sup>8</sup> Sharrock's (1974) notion of "owning knowledge" is treated in more detail elsewhere: see Chapter Eight.

<sup>9</sup> When information was withheld it was an accountable matter, i.e. an account was provided for the withholding of information. The accounts formulated information withheld as "confidential".

<sup>10</sup> Elsewhere, documents have been afforded detailed analytic attention—for use by sociologists and the practical uses of "practitioners" (*inter alia*, Hak 1992; Harper 1997). Whilst Hak's analysis draws heavily from Garfinkel and Bittner's (1967) seminal paper, it is more reliant upon background to professional (viz. psychiatric) practices than explicating the use of documents as constituent features of professional (viz. psychiatric) practices. "Background expectancies" allow a level of indeterminacy in Hak's analysis.

<sup>11</sup> Quote from fieldnotes, a *reconstruction* of a remark made by the Regional Officer. (Such reconstructed quotations were often written whilst on the bus into the city centre from the Office.)

<sup>12</sup> This is the word used by The Organisation, which I have deliberately preserved because members of The Organisation find it misleading, misrepresents the purpose of The Organisation and occasions "trouble" (see below).

<sup>13</sup> Reconstructions from fieldnotes. See *caveats* above.

<sup>14</sup> See Sacks (1988/89) on members' measurements. Members adjudge for themselves if sentences are "too lenient" or "too harsh", what is "appropriate" or whether a sentence "befits the crime"? For the realisation of "minimum features" in if/then situations, i.e. *lex talionis*, see Rose (1963).

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<sup>15</sup> Cf. tournament tennis. Eventual tournament positions necessarily imply participation in various stages within the tournament. To be a “semi-finalist” is a necessary condition of being a “finalist”. Hence, to be a “finalist” makes the reporting of being a “semi-finalist” in players’ biographies for spectators unnecessary; to be a “semi-finalist” makes the reporting of being a “quarter-finalist” unnecessary; and so on. Hence, as an example, a player’s tournament history is abbreviated as e.g. “1997 SF”, rather than “1997 SF, QF, 3<sup>rd</sup> round, 2<sup>nd</sup> round, 1<sup>st</sup> round”, etc. This was not always the case, however. The All England Tennis Championships final at Wimbledon used to be played between the previous year’s winner, i.e. the tournament’s champion, and the winner of the previous rounds, from which the previous year’s winner was exempted. That is, the Wimbledon tournaments used to provide a contest between the existing champion and the most meritorious challenger. (Indeed the final was referred to as the “Challenge Round”). So in approaching early Wimbledon finals, it was correctly inferred that one of the contestants had won the championship the previous year.

<sup>16</sup> The charge “Double Murder” makes available not just the inference that two people were murdered, but that two people were murdered *at the same time*, i.e. that the murders have a “bracketed history”. In contrast, the sobriquet “Serial Killer” makes inferable that whilst more than two—a Sacksian “series”—people were murdered, the murders were not committed at the same time, i.e. that each murder has a “discrete history”. Roger Slack (personal communication, 24 July 1999) alerted me to the ways in which these inferences are procedurally available.

<sup>17</sup> A central theme of this thesis is membership categorisation. It is interesting to note that whilst buddies were determined to remain non-judgemental of offenders, they would iterate reservations and revulsion about offences using membership categories, and Membership Categorisation Devices, including “family” and “stage of life”. Fieldnotes taken at the time report such statements as, e.g. “he murdered both his daughters”, “he killed his mother”, “he battered his own baby”. These associations of activities with categories from the overlapping Membership Categorisation Devices suggest that certain activities are “category-prohibited activities”. (For an example of category-prohibited activities from anthropological fieldwork, see the section on “Inference and Preference” in the following chapter.) This moves toward a putative Membership Categorisation Analysis of violent crime.

<sup>18</sup> See the previous note on the intertwining of offences and Membership Categorisation Devices: whilst an offence may be glossed as “ABH” (Actual Bodily Harm), it does not reveal the nature of the offence as “stabbing his own mother with a Stanley knife” (as opposed to “stabbing a mother with a Stanley knife”—cf. “The baby cried...”). Although charges released to the organisation do distinguish between “Robbery” and “Armed Robbery”, “robbery” does not distinguish between “robbing” and (the hypothetical) “robbing his own grandmother”. One may imagine that the nature of offences affect the status of prisoners within prison hierarchies, in that the (hypothetical) offender who robbed his own grandmother might have a different status from an armed robber. Or one may speculate about the hierarchical status of a murderer who was “in” (convicted and currently in prison) for shoplifting...

<sup>19</sup> Such instances were occasional yet occurred frequently enough for them to be raised as “meaningful matters” to the buddies concerned. Some buddies had several clients who, depending on the length of prison sentences, changed not infrequently. However, those buddies who expressed reservations *apropos* of their clients’ offences stressed that these reservations were offered “only in exceptional circumstances”. Buddies at group meetings concurred that the stealing from one’s *own* baby and the murder of *both* daughters were exceptional circumstances. The cases in which buddies expressed such reservations had been reported in local media, whereupon buddies argued that “everybody” knew the details of the cases already.

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<sup>20</sup> In a discussion about his study of “pass-Whites” in South Africa, Graham Watson (1970) points towards the issues of trust\* (“trust\*” is discussed in more detail below), truth, and disclosure to categorially-defined recipients in fieldwork:

“I did not tell anybody in the school or in educational administration that I was interested in pass Whites. They thought that were I to publish something that displeased them then they could bring my career as a sociologist of education to a halt by denying me access to schools. I got round that one by emigrating—not entirely voluntarily. The political events that led to my departure also prompted some informants to tell me things they had hitherto kept from me. That’s when I got my best material.”

(G. Watson, personal communication 30 January 1997)

That is, when G. Watson was soon to be expelled by the South African government, i.e. seen by members to be independent of and non-aligned with governmental agencies, he was categorised as someone with whom hitherto suppressed and undisclosed information could be entrusted.

<sup>21</sup> Following Garfinkel’s later writings, “trust\*” marked with an asterisk refers to the intersubjective achievement of trust as a situated phenomenon of order, where that phenomenon is subject to contingencies. These contingencies are outlined below.

<sup>22</sup> The occasioned nature of relevance is discussed at greater length in Chapter Thirteen.

<sup>23</sup> In her ethnography of a children’s home, Emond (personal communication 7 July 1999) would deliberately leave her notebook on the table. Individuals could read passages about themselves, but not about others; similarly, they could listen to tape-recordings of themselves, but not of others. That the children felt comfortable about picking up her notebook without it being snatched away is an instance of trust\*, and the development of trust\* between the researcher and the researched.

<sup>24</sup> It is interesting to apply Everett Cherrington Hughes’ (1971b) notions of “good people” and “dirty work” here. In training sessions for novitiate befrienders, the trainees are informed that offenders are overlooked by voluntary organisations—prisons (and prisoners) being perceived as “dirty” or unpleasant, and unglamorous sites for charitable activities—that the work of the organisation fills a gap in the voluntary sector, and that befriending is a marginal activity. Befriending is presented as marginal by telling trainees that not everybody wants to get involved with prisoners and that befriending is a difficult job that not everyone can nor would wish to do (“dirty work”). What some buddies consider distasteful, *viz.* the revelation of the aforementioned offences by clients, are reformulated by the organisation as instantiations of the developing trust\* between offender and befriender. According to training delivered to novitiate befrienders, they are “good people” for volunteering to befriend prisoners. The disclosure of (sometimes lurid) details about offences suggests that the prisoners concerned—those who disclose details to befrienders—see the befrienders as “good people” too.

<sup>25</sup> In training sessions for novitiate befrienders, “toiletries” is a euphemism for unspecified materials. “Toiletries” could refer to anything, and novitiate befrienders were taught to abandon the assumption that “toiletries” meant toiletries, or that toiletries would be used as toiletries. That is, if an offender asked a befriender to obtain toiletries from outside the prison, then the word “toiletries”, ergo the request, should not be taken on trust\*.

<sup>26</sup> The masculine pronoun is used because the majority of clients are males. Although female prisoners do request and are allocated befrienders, the receipt of befriending requests from female prisoners at the Office during the fieldwork period was negligible.

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<sup>27</sup> Lecture 1, Fall 1971, in Sacks (1992b:419).

<sup>28</sup> Sacks' example is discussed in more detail by Schegloff, in Sacks (1992a:xiii). An implicativeness of Sacks' observations on the activities of lawyers is in the demonstration that "legal reasoning procedures" involve ordinary membership activities. Indeed, because the establishment of outcomes as achieved by lawyers through the production of their lay practices are not acknowledged to be "routinised" and specialised features of legalistically mediated courses of action, the reification of and uncritical approach to "legal" outcomes fails to account for the thoroughly linguistic and social nature of lawyers' work. For example, "'facts' cannot be taken as a given by the observer; the largely consensual construction of an hypothetical set of facts is an utterly key activity of the lawyers" (Sacks 1997:5). The mundanity of ordinary activities in the production of specialised features of expertise can be seen also in the "lived work" of scientific discovery (Garfinkel *et al.* 1981). Likewise, the buddies are remorselessly engaged in the lived work or achievement of allocation and befriending, which is shown to be constituted by ordinary, commonsensical activities.

<sup>29</sup> Football, tennis, rugby, croquet may be glossed as "ball games"; whilst chess, Mah Jong, Scrabble, Monopoly can be glossed as "board games". Discus, hammer, javelin and shot-put can be glossed as "field events". Hurdling, steeplechase, and the 100, 200, 400, 800, 1500, 3000, 10000 metres events can be glossed as "athletics", or "track events" ("track" in the US) also. Other glosses for collections of sports include "country" (angling, grouse shooting, fox hunting, deer stalking, etc.); "motor" (Formula 1 etc., drag-racing, stock-car racing, rally cross, etc.; "water" (diving, swimming, water polo, etc.); "winter" (bobsleigh, skiing, slalom, etc.); "indoor" and "outdoor" sports.

<sup>30</sup> Cf. indexing procedures: an index is a list of items—indexed items—but an index is not an exhaustive list of "indexable" items. The manual compilation of indexes, using master documents in wordprocessing packages, is saturated with the practical reasoning activities of the index compiler: which terms in the text are marked as terms to be items listed in the subject index, which terms are relevant and which terms are relevant to each other? Name indexes are less problematic, as the package generates an index from marked names in the text—so long as the author manages to mark all names. Automatically compiled indexes are as useful as the information the author inputs into the concordance file necessary for automatic compilation. Indexes are produced to suit particular relevance's and cannot be attuned to all members' particular project-specific relevances, nor unanticipated relevances, without reproducing the text *in toto*. See Chapter Seven for considerations of the incorporation of ordinary competencies in transcribing stretches of talk, and Chapter Thirteen for considerations of relevance.

<sup>31</sup> See Goodwin (1993:209) for a specimen release form, asking permission from members to record activities for research purposes. Release forms are linguistically organised, worded entities, i.e. they are sites (inscriptions) of natural language activities.

## The Visibility and Linguistic Constitution of Confidentiality

A period of “participant observation” in the office of “The Organisation” demonstrates the ongoing use of the “Documentary Method of Interpretation”. My tasks as a “participant” in the setting involved the compiling and following of various textual materials within the organisation. As shown in the previous chapter, the natural language practices used by befrienders (“buddies”) to work with the documents were coterminous with the natural language practices I had used to compile the documents. These practices included sequencing (in terms of “list-construction”),<sup>1</sup> categorisation and interpretations of inference and evidence. The organisation was “saturated” with considerations *apropos* of confidentiality: protecting the confidentiality of clients and, should they wish to do so, the befrienders.

As previously mentioned, I do not claim that the period of participant observation reported in Chapter Four is unique in terms of the site being confidential, however. Any researcher, through the natural course of their research, may encounter confidential information. The researcher is indifferent to the nature or content of the information *per se*; the sociological report is not an exposé, and the sociologist should not strive to “expose”, i.e. reveal confidential information. Rather, as shown in the previous chapter, the researcher is concerned to explicate how the information is defined or glossed as confidential by members, and thus how confidentiality becomes an “organisational resource” (Carlin 1994:38). That is, how confidentiality is actually used, invoked and relied upon by members for the *in situ* accomplishment of

organisational activities; i.e. confidentiality is a general practice, but it is not to be considered as a *generalised* practice. Confidentiality is to be considered in its *in situ* particularities or haecceities.

However, confidentiality in social research is not necessarily a unilateral matter: confidentiality may be a duplex phenomenon. As shown in the previous chapter, there may be asymmetries of determinations of confidentiality. Put simply, the duplex character of confidentiality is its two-sidedness: the sociologist is concerned with protecting worldly identities (personal names, locations, organisations); yet the sociologist may systematically be excluded from the world, i.e. information may be withheld from the researcher, ergo the study, at the research site. This feature of confidentiality, i.e. that confidentiality is an *oriented-to* phenomenon, is acknowledged by researchers:

“I regret that it was decided by the authorities that issues of confidentiality ruled out any further discussion with local staff, from which my research would undoubtedly have gained”  
(Marsden 1973:xiii)

Throughout the previous chapter, considerations of confidentiality were introduced and developed. A focus on confidentiality shall be maintained within the present chapter also. Whereas the previous chapter examined the occasioned nature of confidentiality in an organisation, and my participation in its confidential environs, this chapter moves towards examination of confidentiality in sociology.

My intentions in this and the subsequent chapter are to highlight the following: first, that confidentiality is a phenomenon for analysis, not just a professional code or set of moral guidelines. That is, consideration of confidentiality as a professional resource that permits the analyst to proceed, i.e. a “facilitating proviso”, by which “gatekeepers” to the research site will only grant permission to proceed with the project if reassurances to preserve confidentiality are forthcoming. (See Richman (1994:172) for an enumeration of such “access rituals”, e.g. protocols, meetings and negotiations.) Secondly, due to their linguistically organised and culturally-methodic nature, that confidentiality, and pseudonymisation in particular, can be conceived of as sociological research methods, and methodic outcomes, in their own right.

### ***Confidentiality as a Research Practice***

Confidentiality is presumed by readers of social science reports. It is expected that confidentiality is assured: that names have been changed to protect real people’s identities, and thus assures readers that they are not themselves vicarious witnesses of real people. In a sense, it may be noted that there is, *mutatis mutandis*, a “standing transparency” (Garfinkel, in Garfinkel and Wiley 1980:6) of the confidentiality of sociological reports. Originally talking about sexuality and normal appearances, by “standing transparency” Garfinkel intends to convey the unproblematic visibility of sex status; how a member’s sex status is available at a glance, and how this visual availability of sex status is taken for granted. Garfinkel’s notion of the “standing transparency” of phenomena provides a connection with Sacks’ inquiries on “recognisability”, which are developed further in forthcoming chapters (on observation

and Membership Categorisation Analysis). The notion of recognisability constitutes a critique of cognitive processing models, which require “a pause in proceedings” for members to make decisions and react to their environs.<sup>2</sup> Recognisability is a gloss for members’ categorisation activities “in flight”.

In borrowing Garfinkel’s phrase, to suggest that there is a standing transparency of confidentiality, I am referring to the taken for granted assumption that sociological reports protect the identities of persons included in the study. Readers’ attention is called to the *non-confidential* status of sociological reports in cases of doubt: e.g. the “Springdale” incident (discussed at the end of this chapter); and Snodgrass’ (1973) investigative, invasive study of Edwin Sutherland and Chic Conwell/Broadway Jones. Over a number of years, Snodgrass researched Sutherland’s files for information on Chic Conwell/Broadway Jones. Sutherland’s original notes referred to a “connection” between Chic Conwell/Broadway Jones and Harvard Law School, which Snodgrass wished to pursue: the Harvard connection could explain the style in which *The Professional Thief* was written, and how Sutherland and Chic Conwell/Broadway Jones became involved. According to Snodgrass, “Broadway Jones” was the real name of the man who wrote *The Professional Thief*; Snodgrass asserts that Chic Conwell’s real name, “Jones”, was “discovered by using background data to locate his birth certificate” (1973:2). Snodgrass hoped to use the real name to investigate the circumstances and background of Chic Conwell/Broadway Jones, by gaining access to genealogical and public records: by uncovering family details, Snodgrass intended to call into question a key tenet of *The Professional Thief*, i.e. the high social class of professional criminals.

Snodgrass' thesis could not have been advanced without a period of investigation into the life of Chic Conwell/Broadway Jones.

The practice of anonymising informants is so pervasive that, if it warrants any mention at all, then it makes a brief appearance outside the body of the text. In so doing, confidentiality is treated *en passant*, whereby the assurance of anonymity and the provision of pseudonyms becomes a marginal issue in the text itself. Such assurances and provisos are seen as being reducible to footnote status, or become tucked away inside the preface or acknowledgments sections. As I suggest elsewhere (Carlin 1997:2), an analogy can be made with the "anything to declare" channels at customs points. Once a declaration has been made, however, the researcher makes an assumption that they have passed "through the gate".<sup>3</sup> No further considerations are made *apropos* how the changing of names affects the research, or how pseudonyms are "constituent features" of the research.

This relegation of a salient yet taken-for-granted feature of social life, and social research, effectively incorporates into the research an "unacknowledged" or hidden dimension.

Curiously, the movement towards transparency of methods in sociological research steadfastly ignores this phenomenon. Inextricably bound up with the use of natural language, the assurance of confidentiality and the replacement of actual names with

pseudonyms are, *in toto*, natural language activities. To previous studies, pseudonymisation is *terra incognita*.

The explication of natural language practices topicalises or problematises pseudonymisation, i.e. taking it as a topic in and of itself. Turning pseudonymisation into a problem, taking it as topic of inquiry, shows how confidentiality is done by members and, therefore, how it is done by researchers. Highlighting “the how” of confidentiality, the work involved in accomplishing confidentiality, provides grounded observations on formulating procedures for engaging in adequate research.

By “adequate” I refer to methods of inquiry which are sensitive to the particular *in situ* requirements of the research site. The inquiry should attempt to preserve what Schwartz (1977) calls the “phenomenological intactness” of a social setting; and preserve the setting’s “haecceities” (Garfinkel and Wieder 1992:203, fn.2), a term that Garfinkel uses to indicate the unique features of a setting: the just this here and now-ness, with just these particular persons present, at this particular time, in this particular place, in these particular circumstances, producing these particular features. This is perforce an “asymptotic”<sup>4</sup> requirement of methods, i.e. a requirement that can only approach—and cannot achieve—the condition of unification of analytic method and object of its inquiry, of phenomenon and description of that phenomenon: by definition, a description of a setting can only be a description. Even if a uniquely adequate description of a setting appears somewhat quixotic, the work of “chasing this windmill” benefits the analysis and those members whose *in situ* work produced the setting itself.

Some readers may object to these exordial commitments, arguing that these considerations are irrelevances, they simply do not matter. Researchers have to protect identities, so stop wasting time, energy and grant money and “get on with it”. Important ethical implications aside—the ethics of ironicising members’ methods and providing for unwarrantable characterisations of “our research subjects”—there exist important, inquirable matters regardless of any overriding principle or obligation to conceal the sources of information. Although this and the subsequent chapter make the seemingly tautological observation, that pseudonyms are names, it emphasises this truism for the benefit of practicing fieldworkers. To see how pseudonyms, false names, work we should and must first look at how real names work. This is necessary because if analysts do not recognise how real names work, they perforce can not recognise how false names affect the study.

### ***“Topicalising” Members’ Activities***

Throughout this chapter I intend to topicalise a practice of sociological inquiry that is assumed to be ‘good’ or at least ‘standard’ or accepted practice in dealing with ‘human subjects’. The verbalisation—literally, to turn a noun into a verb—of ‘topic’ intends to convey how taken-for-granted features of sociological study are turned from resources for study into topics of inquiry in their own right. To topicalise—in an ethnomethodological sense, to ‘do topicalising’—means to turn into topic, to explicate how something is actuated and operationalised used by members (i.e. all of us) in their (i.e. our) everyday lives.

'Doing' or 'to do' is used "to underscore a conception of [activity] as a matter of course in [its] production and recognition" (Ryave and Schenkein 1974:265; my brackets), so that the activities of 'sociological research' and 'writing up sociological research' are *in toto* members' accomplishments. "Doing" indicates the fact that it is "worked at", and that the outcome is "worked up" as an accomplishment. Scientific research also, in its demonstrations, shows itself to be the work of members; and its outcomes—its write-ups—are products of the work of members doing writing scientific research (Garfinkel, Lynch and Livingston 1981). In so doing, the "analytic mentality" (Schenkein 1978) that regards each everyday activity as the concerted accomplishment of members topicalises that activity: the activity is turned by a "bracketing off" operation from a tacit resource for study into a phenomenon, a topic of inquiry *per se* (Zimmerman and Pollner 1971). It is to be noted that Schenkein's formulation of an analytic mentality, or *modus operandi*, is applied in this paper to confidentiality and pseudonymisation also. That is, the conceptions of confidentiality and pseudonymisation are topicalised (turned into topics) as members' conceptions and as members' accomplishments: members and analysts (so my comments are inclusive of those who suspend the natural attitude of everyday life) are involved in doing confidentiality (*doing* establishing the confidentiality of information, (cf. Rodriguez and Ryave 1992, in the following chapter), *doing* establishing the confidentiality of settings in order to confine information within the settings, (cf. Carlin 1994:37), *doing* maintaining confidentiality outside of settings in which information is glossed as "confidential", *doing* invoking confidentiality...); and in the practical accomplishment of these activities they may be

required to pseudonymise actual names, i.e. *doing* pseudonymising is a practical activity for the practical purposes of *doing* confidentiality.

This can be explicated further using Rose's distinguishing conceptions of the "business-at-hand" and "something-else-again". The business-at-hand of sociological research is to protect the identities of its informants; the something-else-again is the work of protecting identities, showing how pseudonymisation (the extraction of real names, the selection of substitute names for actual names, the emplacement of the *ersatz* for the *echt*) is methodised by members.

"Members" and "everyday lives" are all-encompassing constituencies: we as sociologists are members and, whatever our epistemic and paradigmatic inclinations, we are not exempt from what Schütz calls the "natural attitude", however much 'time out' from everyday life and activities of "membershipping" (Anderson and Lee 1979:290 *et seq.*) sociologists would like to think their 'professional' or 'vocational' status affords them. Anderson and Lee are unequivocal: what constitutes "data" for the sociologist are suffused with "membershipping" activities, by which term they refer to the ordinary and routine practices of categorisation as manifest in members' accounts:

"Membershipping represents a way by which persons in social settings categorise objects of knowledge, incidents, events, and other members. [...] It represents the results of their sense-assembly methods as they utilise conventional rules and procedures in practical purpose settings, in order to know their way around, and instruct others as to how to see the world correctly. It is the sense in which members of a culture deal with contingent events, and render

them into categories such that ‘this’ may be found to be ‘another case of’, or ‘similar to’, or ‘the same situation’, as ‘that’.”

(Anderson and Lee 1979:290)

Taking participant observation as their exemplar, Anderson and Lee demonstrate that research activities are inextricable from membershipping activities. “Participants” in the research are continually involved in membershipping activities, and the researcher is continually engaged in the self-same membershipping activities—however, membershipping and the occasioned and contingent nature of membershipping activities are not features of accounts from participant-observation inquiries.

A “member” is not a member of a club. The term neither sets up nor implies a binarism between “members” and “non-members” (Garfinkel, in Hill and Crittenden 1968:121). Rather, the term “member” refers to the ability of persons to act and orientate towards worldly phenomena (people and things) as a “competent” member of society: someone who adequately understands surrounding ecological (people and things within the interactional setting as relevant to members) and interactional (communicative) features, and is in turn adequately understood by others. Garfinkel and Sacks (1970:342-345) refer to this competence as the “mastery of natural language”:

“[P]ersons, because of the fact that they are heard to be speaking a natural language, *somehow* are heard to be engaged in the objective production and objective display of commonsense knowledge of everyday activities as observable and reportable phenomena. [...] What is it about natural language that makes these phenomena observable-reportable, that is, *accountable* phenomena? For speakers and auditors the practices of natural language somehow exhibit these phenomena in the particulars of speaking, and *that* these

phenomena are exhibited is thereby itself made exhibitable in further description, remark, questions, and in other ways for the telling”

(Garfinkel and Sacks 1970:342; emphasis supplied)

Language is the vehicle of interaction, i.e. culture; and natural language, i.e. the language (e.g. Arabic, English, Japanese) that is commonly and ordinarily shared and understood by members, or cultural incumbents, is the vehicle for the production of intersubjectivity: mutually observable, mutually recognisable and mutually comprehensible activities. Indeed, it is the “mastery” of this natural language which is central to the notion of membership of a culture:

“Such mastery includes the ability to understand more than is explicitly said within a strip of talk by situating it both within indigenous frameworks of knowledge, and within the practical circumstances and particular activities that parties to the talk are engaged in.”

(Goodwin and Duranti 1992:28)

It is through using (i.e. practising) natural language that we are able to ask a question and recognise a response as an answer to the question; we are able to recognise and display that we recognise questions if asked of us; and respond to questions in a manner that is recognisable and recognised to be an answer to the question. Natural language practices allow us to identify and categorise persons as incumbents of membership categories; and to adduce members’ activities as being compatible or incompatible with categorial membership (what Sacks called “category-bound activities”).

It is through natural language that we “do” confidentiality. As we shall see, it is through natural language that we establish that information is confidential, produce

confidential documents, invoke confidentiality, disclose confidential information, withhold confidential information, protect identities, keep identities confidential, assign pseudonyms.

### ***The Normative Nature of Confidentiality***

A notion that the attribution of pseudonyms—false names—oppugns the veracity of the information that is perceived to be the substantive topic of inquiry, is alluded to in a collection of statements by persons involved with drug use:

“All the interviews, which were tape recorded, are genuine but because of the sensitive nature of the subjects covered all the names have been changed to protect the confidentiality of those who agreed to be interviewed ...”

(MacFarlane *et al.* 1996:v)

Questions of truth are thorny ones, and raise issues which shall remain outside the parameters of this inquiry. In any case, we can and must remain indifferent to the truthful/untruthful status of accounts: accounts constitute versions, and how competing versions of accounts get to become authorised as “the truth” or as “fact” in preference to other versions is to be treated as data for study (Cuff 1980; Smith 1978, 1990).

Nevertheless, it is perhaps legitimate to ask whether the face-value acceptance of changing details is warrantable in such a normatively organised discipline as sociology: which details? to what purpose? and to what extent? are changes, within and between studies, qualitatively consistent?

The changing of those characteristics which the researcher adjudges to be identifying characteristics is not the end of the matter. Whyte's observation, in his methodological appendix to the later editions of his original study, provides uncomfortable reading for those concerned that assurances of the confidentiality of research adds a measure of finality, that it somehow means confidentiality for everyone, and for all time:

"It is certain some of these people [included in the study], and perhaps many of them, will read his research report. If [the researcher] disguises the name of a district as I have done, many outsiders apparently will not discover where the study was actually located. I am still surprised to encounter people who locate Cornerville some hundreds of miles from its actual locale. The people in the district, of course, know it is about them, and even the changed names do not disguise the individuals for them. They remember the researcher and know the people with whom he associated and know enough about the various groups to place the individuals with little chance of error."

(Whyte 1955:342)

Whyte's observation suggests that the assurance of confidentiality is not a cast-iron guarantee: even if people's identities are protected from readers, the participants of the research site are conscious of their own identity and the identities of others. Ruth Emond (personal communication, 16 December 1996) vividly demonstrates this transparency in her study of children in residential care. Emond resisted the analytically unwarranted imposition of pseudonyms, a phenomena treated in more detail below, allowing the local cohort of children who comprised the research site to choose "private names", i.e. names which were agreed to remain known only to herself and the individual. In Goffman's terms, Emond and the cohort of children in her study constituted a series of "teams", "by virtue of the cooperation which they maintain in order to sustain a given definition of the situation" (Goffman 1971:108) and the identity

of each child was a “team secret” shared by Emond and individual children. The commitment to the maintenance of these “team secrets” was one-sided, however, as Emond later learned that the children had shared and compared their private names within a group.

Whilst the researcher may experience some discomfiture, however, any ramifications of identification for participants themselves could be extremely serious. In regards to his (1970) study of the educational system in South Africa, in which some people claimed ownership to a different racial identity by “passing for White”, Graham Watson says

“Colander is a pseudonym. There’s lots of stuff in the book that is intended to mislead bloodhounds. By getting Jack Simons to write the preface I ensured that the book would be banned in South Africa and that some people would be spared potential embarrassment.”

(G. Watson)<sup>5</sup>

This is a crucial case of the consequentiality of confidentiality. Graham Watson’s study was written and published during the apartheid era in South Africa. Any “potential embarrassment” caused by identification might include significant consequences for the way of life of an individual concerned and their family.

The normative nature of confidentiality is also attested to by the reaction following the publication of *Small Town in Mass Society*, and the opprobrium the authors received from their peers. That is, a norm was made visible through its being breached. (The notorious “Springdale” incident is returned to later in this chapter.)

### ***The Visibility of Notices of Confidentiality***

It is to be noted that the confidential status of identifying terms was made explicit in most of the works consulted, but not all. A study in which the notice of confidentiality is “noticeably absent”, is William I. Thomas’ and Florian Znaniecki’s *The Polish Peasant in Europe and America*. This does not mean, however, that Thomas and Znaniecki did *not* disguise the identities of senders and receivers of the letters they document in their study.<sup>6</sup> Rather, it suggests that there may be a “natural history” of confidentiality in sociology. Certainly, by the time of Vivien Palmer’s textbook—*Field Studies in Sociology*—on sociological methods was published the following decade, it was made explicit that sociologists provide assurances of confidentiality (1928:173) and ensure that they store their data confidentially (1928:194).

In a review of extant sociological literature we can see how the notification of confidentiality is concertedly relegated to an extra-analytical level, i.e. notices of confidentiality are incidental particulars, treated as being outside the purview or relevance of the project. Previous studies have also isolated textual materials as data, and suggested they exhibit certain formal properties.

In *The Polish Peasant in Europe and America* mentioned above, Thomas and Znaniecki examine persons’ gradual adaptation to cultures different from their own. A large source of data for this study are letters to and from family members who have moved away from their relatives. They provide a taxonomy of peasant letters which, whilst ostensibly written for different reasons, serve the same purpose. The types of letters

they identify are “ceremonial” letters, in which the absent family member sends a letter which serves as the speech that would have been made if they were present at the occasion; “informing” letters, which updates the absent relative of family news; “sentimental” letters written to remind the absent relative of the loving relations at home; “literary” letters, like ceremonial letters, sent as proxy for the writers themselves for recitation at meetings; and “business” letters, sent if the sender cannot attend a meeting in person (e.g. due to distance). Thomas and Znaniecki suggest that “the peasant letters can be considered as variations of one fundamental type” (1958:303). The fundamental type is the “bowing” letter, the expression of “familial feelings” despite the distance and period of separation. According to Thomas and Znaniecki, the function of bowing letters, and hence the types of letters enumerated above, is “to manifest the persistence of familial solidarity” (*ibid.*).

A later study brought formal properties between suicide notes into focus (Jacobs 1967). Jacobs’ argues that the theoretical thrust of his paper is “trust violation” (1967:64), although the concept is introduced abruptly and he does not intimate how or why it was selected, nor how it relates to his analysis of suicide notes. Jacobs examined 112 suicide notes and suggested, like Thomas and Znaniecki (*ibid.*), that they could be separated into categories, wherein notes displayed the same properties. Briefly, Jacobs assigned notes to such categories as “first form notes”; “illness notes”, which were further categorised between “sorry illness notes” and “not sorry illness notes”; “direct accusations”; “notes of instructions”; and “last will and testaments”.

In this section I do not attempt to categorise notices of confidentiality, as do Jacobs, and Thomas and Znaniecki with suicide notes and peasant letters respectively. Such categorisations are analyst's achievements and stipulations, which thereby potentiate the introduction of a form of methodological irony. Instead, in this section I shall introduce notices of confidentiality and their placement. It is observable that where notices of confidentiality do occur, they are placed outwith the body of the text. That is, confidentiality is treated as an expected feature of reports from investigations, and notices of confidentiality are not required to contribute towards the investigation itself. The examples provided in this section show notices of confidentiality that have been placed in the acknowledgments, as appendices, prefatory remarks, and as footnotes. A commonality between notices of confidentiality in the literature is not the placement, but the relegation.

Elizabeth Bott includes a notice of confidentiality in her preface:

“Because of our promise that they [the families interviewed for the study] should remain anonymous, their identities have been disguised, and all names are fictitious.”

(Bott 1957:xi)

This prefatory nature is exhibited elsewhere by William Foote Whyte:

“Since fictitious names are given to all characters in the book, I cannot acknowledge directly the help of local informants.”

(Whyte 1955:ix)

and by Studs Terkel "With few exceptions, pseudonyms are used throughout" (Terkel 1970:11). Babuscio's prefatory notice of confidentiality contains a redundant polemic:

"Names, places and minor details have been altered in every instance, with the exception of those few accounts which have already been published elsewhere. This determination to protect the anonymity of individuals, I would add, reflects my own wish to respect the privacy of all those concerned."

(Babuscio 1976:xiii)

Notices of confidentiality are also found in acknowledgment sections, exhibiting a *quid pro quo* character; i.e. the observance of conditions agreed with the informants in relation to their cooperation with the research:

"My chief debt, of course, is to the streetcorner men themselves and to their women and children. Unfortunately, pseudonyms had to be used throughout to protect their anonymity but they themselves know who they are and I thank them."

(Liebow 1967:xvi-xvii);

"Literally dozens of private individuals also helped me; many of them did so on condition they would not be named. ... 'Providence' is of course a fictitious name, as are the names of people concerned; but nothing else has been altered."

(Parker 1985:373);

"All the women were assured that first names only would be used and that fictitious names could be substituted if they wished; most adopted for the latter. Names of villages were also changed or omitted at the woman's request"

(Hooks 1991:xi);

"Suffice to say, strictures of confidence prevent any elaborate discussion of method, problems of access, etc. ... [detailed methodological description would] violate promises of anonymity guaranteed to the subjects involved; for similar reasons, no direct quotations will be presented."

(Ball 1970:17)

We can infer from Ball's comment that maintaining confidentiality constrains his analysis and leaves him less room for manœuvre, or at least less room than he would

wish for. Hooks' notice of confidentiality (*ibid.*) has a different status from the other notices presented, in that it gives respondents some control over confidentiality.

The restrictive and constraining aspects of confidentiality are made explicit by Sutherland, who laments that the reproduction of certain particulars within a study is *verboten*, and tacitly so given by the normative character of sociology: court citations perforce identify corporations; the materials that can be presented without compromising the confidentiality of cases are not as convincing as *echt* documentation; and that

“a person can get a vivid realisation that the behaviour of... corporations is criminal behaviour only by reading many reports of decisions against them. Something is lost, in this sense, since many detailed and documented cases could not be presented without revealing the identity of the corporations.”

(Sutherland 1949:xiii-xiv)

Such a “vivid realisation” does not constitute an epiphany, of course: the criminal nature of corporations' activities is not only textual but titular, as the first contact readers have with Sutherland's study is its title, *White Collar Crime*. (The placement of titles receives further attention in Chapter Thirteen on the linguistic constitution of bibliographies and literature reviews.) An “underlying pattern”, then, that the corporations are involved in criminal acts, is set up *ab initio* as an “[instruction] as to how to it should be read” (Lee 1984:69). Lee explicates instructions for reading as the procedural or “membershipping” activities involved in reading e.g. newspapers; how we read and how we orientate to a text *apropos* of its title. Sutherland (*passim*) tells readers unequivocally that the behaviours of the corporations therein are criminal.

Indexical particulars, i.e. further details extraneous to the pattern, and which are available for interpretation using that pattern, that work to confirm, elaborate and support this underlying pattern, are presented so that the corporations are evidently to be seen as engaging in criminal behaviours. That corporations are seen this way is an outcome of the reliance on the common sense ordering of the text and the common sense interpretation of the ordered text; i.e. how Sutherland used membership practices to produce this “preferred reading” (D.R. Watson 1992:15) without appealing to such further documentation, and his reliance on readers using their membership practices to endorse this intended conclusion, and in turn readers’ use of membership practices to “discover *for themselves*” the veracity of Sutherland’s analysis, without recourse to this legally authenticated documentation. So with or without these detailed cases, through their using the Documentary Method of Interpretation (Garfinkel 1967), Sutherland’s readers would see how the behaviours of corporations were adjudged to be criminal by the gradual unfolding or building up of a weight of evidence against them.<sup>7</sup>

Wiseman’s study of Skid Row alcoholics features the notification of confidentiality as an acknowledgement:

“[Names] are [...] research observers that I can publicly acknowledge. Other observers must remain anonymous. [...] Nor can I acknowledge by name the men of Skid Row and the agents of social control who took time to talk about the loop...”

(Wiseman 1970:xx-xxi);

as an appendix:

“In using this key [to identify respondents], it should be kept in mind that all names are pseudonyms.”

(Wiseman 1970:287; emphasis supplied);

and as a condition of research in her preface:

“Pseudonyms are used for the situs city, county, and state, for the institutions there, for all Skid Row alcoholics, and for all agents of social control interviewed or observed. This is in compliance with the National Institute of Mental Health protocol on the use of human subjects in research. If any of the names used bear any similarity to those of real persons or places, this is unintentional.”

(Wiseman 1970:xvi)

Whilst her table of informants (Wiseman 1970:287-290) is not orientated to topicalising pseudonymisation *per se*, Wiseman does use extra-nominal characteristics to form composite identities:

“Ages and past skills or occupations of alcoholics, where available, are also changed slightly or interchanged among respondents to avoid identification”

(Wiseman 1970:287)

However, Wiseman furnishes categorial determinations which are so interpretable through members' ordinary reading procedures (D.R. Watson 1997:52).

Notices of confidentiality may be afforded footnote status:

“All place names and personal names have been altered”

(Gallaher 1964:285:fn.1);

“Hightown is a pseudonym for the town in which the school was situated. Any reader with a little knowledge of the demography or the educational provisions of northern towns will speedily recognise it from the descriptive passages that follow. I feel it is extremely important that in any public discussion its

anonymity should be preserved. All other proper names have been similarly disguised."

(Lacey 1970:xvi, fn.1);

"Names of gangs are omitted and disguises used whenever such procedure seems advisable. Major criminal gangs are designated throughout the study by triple letters."<sup>8</sup>

(Thrasher 1927:8, fn1);

"The identity of all persons referred to in documents in this chapter and throughout the book, has been disguised. Where names occur, they are fictitious."

(Zorbaugh 1929:105, fn.1);

"Names have been changed to preserve anonymity"

(Schegloff 1972a:374)

Schegloff, in providing categorial determinations within the collections of persons who are common-sensically expected or accepted to answer the telephone at the Police Desk, does not make explicit the extent of any alterations within his data: in the telephone conversation extracts with the emergency services, e.g. the ranks of "Sergeant ----" and "Officer Novelada".

This contrasts with D.R. Watson,<sup>9</sup> who is concerned that the *transformation* of transcript data for the practical purposes of maintaining the confidentiality of persons' identities is subject to the *preservation* of other productionally relevant features, e.g. Membership Categorisation Devices. In demonstrating the "*activity of implication*" (D.R. Watson 1997:86; emphasis supplied) of textual material, how a particular forensic report (D.R. Watson 1997:86-87) accomplishes the work of linking a suspect with a homicide and predisposes readers of the report to this interpretation, D.R.

Watson transforms, i.e. pseudonymises the names contained therein; whilst preserving its essential characteristics.

Thus, the (male) “Detectives”, “Alan J. Rimsky” and “Michael D. Holt”, both of the “Evidence Unit”, file a forensic report to the “Commander of the Homicide Bureau”, a “Lieutenant” by the name of “Donald O. Corcoran”. D.R. Watson transforms persons’ names, whilst preserving the ‘forms’ of names (first name, initial, surname) and their sub-sets within the collection “policemen at Factory City Police Department”; the policemen’s location within the Police Department is also retained, i.e. the division between the Evidence Unit and the Homicide Bureau. The diminutable character of the victims’ names, “Herb” and “Hank”, their categorisation as “victims” and more specifically “Homicide” and “Assault” victims, is also preserved.

Anonymisation is visually available from interviews transcripts produced in the Larimer Street studies. The following blocs of talk are reprinted from the introduction sequences in a series of interviews on Larimer Street with members of the small Japanese community:

GORMAN: Well, I'll just start by saying that the date is August the eleventh and that my name is Anthony Gorman. And maybe you'd give me your full name?

- K: My name is Ed K.
  - G: How old are you, Ed?
- (Gorman 1965b:5)

GORMAN: Let's see, what is the date today? August the eleventh?

MRS. M: August the eleventh, yes.

G: My name is Anthony Gorman, and what is your full name?

- **Mrs. M:** My name is Susie M.  
**G:** Susie. Could you tell me, then, about your family, who your children are, and what your husband does for a living?  
(Gorman 1965b:19)

The turns marked with arrows are responses giving their full names, as requested in the prior turns by Anthony Gorman. In the transcriptions of the tapes, however, the full names have been edited, reprinting the forenames only. The identifiers of the speakers, *viz.* K. and Mrs. M., are similarly disguised.

In demonstrating the suffusion of membership categories in conversation-analytic studies of “institutional talk”, D.R. Watson (1997:52) observes that transcripts often provide readers with categorial designations before utterance. Hence, some conversation-analytic transcripts constitute an “instructed reading”. To borrow D.R. Watson’s observations, the pseudonymised status of transcripts from the Larimer Street studies is attested to and confirmed *ab initio*, as the disguised identifier is recognised before the full (disguised) name.<sup>10</sup> The notice of pseudonymisation—“the proper names of persons and places have been changed” (Rose 1965c:3)—is contained within a different report from the sequences extracted above. So in the Larimer Street reports, that identities have been disguised is realised through reading the transcripts. Confidentiality, the anonymisation or disguised reference to persons (i.e. pseudonymisation) on the street, is visible from the transcripts themselves.

The examples from Larimer Street reports above show that anonymisation of persons can be made visible through the work of transcription of naturally occurring talk,

rendering a notice of confidentiality redundant. This is shown by the highlighted omission of names, e.g. "G\_\_\_\_\_," "Missiz R\_\_\_\_\_," and "Mistuh T\_\_\_\_\_." (Pomerantz 1980:193). Pomerantz thus preserves the productional nature of the address terms, i.e. "*Missiz*" as a spoken-phonetic form of "Mrs.", and "*Mistuh*" as a spoken-phonetic form of "Mr.". This form of presentation of transcripts provides the analyst greater flexibility in say, the provision of actual identifying characteristics, e.g. categorial determinations of gender and marital status; however, such treatment of persons' names fails to preserve loci of conversational detail, vital productional particulars of talk. As D.R. Watson argues,

"proper names for persons deserve to be analytically treated not as trivial phenomena, but as being of highly consequential social or interactional significance"

(D.R. Watson 1981:91)

Pomerantz, like all analysts of members' talk, was confronted with the intricacies of proper names: their indexicality and embeddedness within a conversational sequence. She decided to excise the names from the transcript (we do not know whether the first letters are indeed the first letters of the actual names) as a means of protecting identities. In so doing, worldly phenomena—the production of a person's name and the consequences of that production—were lost. Or, at least, their phenomenological intactness was distorted.

The review of extant sociological literature also shows how the notice of confidentiality can be subsumed within the larger text:

“Attention was focused on one station in East Belfast, given the fictitious name of ‘Easton’...”

(Brewer 1994:238)

It is important to note here that a real name is framing a false name: the RUC station at which ethnographic observations were made is pseudonymised as “Easton”, a designation derived from its placement in East Belfast. (Further examples of this pseudonymisation technique are presented in the later section on locations.) Another notice of pseudonymisation within the text is found in an account of a law firm:

“My aim ... to describe some aspect of the actualities and character of a particular firm of ‘radical lawyers’, which for the purposes of this paper I will be calling Gregsons.”

(Travers 1994:249)

As intimated above, it is to this pseudonymisation of locational and organisational research sites that will be considered in a later section.

### ***Names and Pseudonyms in Social Inquiry***

“It is standard policy among researchers to protect the identities of subjects, respondents, informants, by providing pseudonyms for actual person and place names.”

(Jefferson 1974:1)<sup>11</sup>

The following observations do not challenge the expectations of research subjects, that identifying characteristics will be concealed; nor the commitment of researchers to protecting these identities. Rather, the following is intended to challenge the expectations and unexplicated cultural methods of researchers used in this concealment. This research is designed to focus awareness on the procedural knowledge and methods

involved in the compliance with the standard policy of changing subjects' identities. It intends to show that the explication of a researcher's cultural methods can be operationalised for the purposes of choosing a pseudonym: one that does not only suit "the business at hand", the business of doing sociology, writing up reports and changing names in order to adhere to the practice of maintaining confidentiality; but one which is suited to "the something else again," to the talk that constitutes the focus of inquiry (Rose 1994).

In considering pseudonyms in social inquiry, a researcher is, by definition, considering names. D.R. Watson argues that

"the use of proper names in everyday interaction is a deeply pervasive phenomenon in our culture, however taken for granted such uses may be"  
(D.R. Watson 1981:91).

The worldly use of personal names is a feature of the researcher's background understandings, which are brought to the study. People's names, and their pseudonyms, are worded objects; therefore they constitute proper topics for ethno-inquiry.

The topicalisation of researchers' cultural methods involved in maintaining confidentiality shows confidentiality to be a linguistically organised, methodical research practice.

***Inference and Preference: “What’s in a Name?”***

An examination of Irawati Karve’s (1947) brief survey of personal names in India shows how names can be inextricably linked to the situation that a person is born into: when, where, to whom, and under what circumstances.<sup>12</sup>

We see how the choice of a name can be influenced; names may be a reflection of various ‘particulars’. Particulars are details that provide reasons to prefer to choose one name rather than another; and these particulars can be reflected in the name. Lest the following discussion of Karve’s work seems tendentious or “overbuilt” (Sacks 1974a:224), I wish to point out that it is based upon the notions *preference* and *inference*: there exist certain preferences to assign certain names; and inferences can be drawn upon a given name in regards to these preferences. However, I shall show how the “inferentially rich” quality of names can impact upon the assignment of pseudonyms in social research: the assignor (the researcher) can, unwittingly and unwarrantably, assign biographical particulars to the assignees (the pseudonymous informant and any persons who are coincidentally known by that pseudonym).

Some names are indicative of what I shall call locational, sequential, relational, temporal, predicative, and circumstantial particulars. Locational particulars provide geographical and social position, and can be reflected in names:

“each cultural region in India shows its preference for certain personal names and the suffixes which are applied to [personal] names are also different. Within

each cultural region there are again different kinds of names for different castes and sub-regions.”

(Karve 1947:37)<sup>13</sup>

This suggests a problem for the investigator when choosing a pseudonym for an informant: a pseudonym may categorise and locate a person as originating from a particular geographical region and social grouping; names are also given according to deities, whereby in recognising a name one can infer the family deity of individuals. We are beginning to see that names, and therefore pseudonyms, set parameters of location, caste and family deity, thus narrowing the scope for anonymising an individual, whether the individual as informant or an individual who possesses the pseudonym as their actual name.

Sequential particulars denote the serial ordering of siblings, e.g. identifying first-born children. The social researcher should avoid assigning a pseudonym if it conflicts with biographical particulars; e.g. assigning a pseudonym inferring first-born, and then referring to older siblings within biographies; or a pseudonym which infers only child, and then referring to other siblings. The onus is on the social researcher to pseudonymise the name, and change all biographical particulars to be consistent with this pseudonym.

Categorisation devices such as “relational” particulars are indicative of sibling relationships, where a brother (not a son) “receives appellations like dada, tatyā, bapu, anna, babu, etc.” (Karve 1947:39). Reo F. Fortune’s anthropological study of Dobu

Island indicates the complications that arise for the researcher, *via* categories taken from the Membership Categorisation Devices “sex” and “family”. Complications are occasioned by the use of names and naming practices among the Dobu—the taboos on the use of personal names within particular categorial relationships (Fortune 1963:62-68). (The previous chapter referred to such phenomena as “category-prohibited activities”.) The Nobel Prize-winner for Literature, Naguib Mahfouz<sup>14</sup> shows that in Egypt, for example, mothers and fathers, sisters and brothers, cousins, aunts, uncles, nieces and nephews, maternal and paternal grandparents are able to distinguish their relationships to each other using common-sense relational address terms.

Temporal particulars, such as hebdodal and monthly days of birth may also be exhibited in people’s names, e.g. born on a Monday, born on a full moon day.<sup>15</sup>

Predicative particulars refer to the naming of children along the lines of temperament or disposition, e.g. “certain qualities like the happy one, good-natured one, the virtuous” (Karve 1947:39). Circumstantial particulars include pre-, inter- and post-partum events which influence the choice of forename. These names “are all representatives of some trivial or nauseous or disrespectful object and are given to children under special circumstances” (Karve 1947:39), e.g. children who had recovered from serious illness, children born after a series of miscarriages or still-born deliveries can be the recipients of a name which reflects their good fortune to have survived, where its siblings had perished.

So we see from Karve's work that names may be relevant to personal biographies too, and therefore inferences can be made from any assigned pseudonym.

So as a hypotheticalised example, detailing particulars provided by a name, a name could indicate that a child "was born prematurely on a Saturday, in the Uttar Pradesh region to a family of the Brahmin caste, the surviving twin who is thought lucky to be alive, who screamed and cried loudly during its first hours."

We also know that names can be generational, in that they may reflect periodic preferences for children's names (e.g. that corpus of names regarded as "Victorian"); and may be influenced by current events, such as Royal births (e.g. William and Henry, Beatrice and Eugene) and popular figures (e.g. Kylie and Jason). Moreover, this influence is inferentially available:

"to find amongst the English that there is an increase in the popularity of a name that has already been given to a member of the royal family is to warrant the conclusion that people are naming their children after a member of the royal family."

(Sharrock 1974:47)

Names are also suggestive of ethnicity and ancestral ties, such as the patronymic O', which infers Irish descent, and the Gaelic prefixes Mac or Mc, literally "son (of)".<sup>16</sup>

This illustrates that inferences are routinely made on persons' names; similarly, inferences can be made on pseudonyms of these names. The onus is on the researcher not to influence or distort the research, and readers' interpretations of that research, by

the assignation, that is the *attribution to* an informant, those culturally-available predicates which are based upon that name. Pseudonymisation in social research requires more consideration than “Smith” and “Jones”.

### ***Locations, Organisations, and the Work of Pseudonymisation***

This review above shows that researchers endeavour not only to change the name(s) of the person(s) in their studies; locational and organisational identifiers are pseudonymised also.

The quotations from Babuscio, Hooks, and Wiseman (*op cit.*) explicitly confirm that place names have been changed. Brewer, Lacey and Parker, meanwhile, provide specific locational pseudonyms (“*Easton*”, “*Hightown*” and “*Providence*” respectively). Liebow is open that his research was carried out in Washington DC; even “*downtown*” Washington DC prevents unequivocal identification of the actual research site. William Foote Whyte’s

“information is based upon a three-and-a-half-year study of the Italian slum district of ‘Cornerville’ in ‘Eastern City’”  
 (Whyte 1943:xv)

Kurt H. Wolff (1964:233) encloses the northern New Mexico community “*Loma*” in diacritic markings to establish its status as a falsified name. Gallaher (1964) reflects on his follow-up study of the American community “*Plainville*”.<sup>17</sup>

The preservation of gist of original names is manifest in pseudonyms assigned to locations, e.g. “Mid-City”:

“Perhaps it was because the organisation was in fact located in the Midwest, and the city name began with an ‘M’.”  
(Zimmerman)<sup>18</sup>

A published notice to this effect is contained in Mary Morse’s study of “problem” teenagers in different cities within the UK:

“The areas eventually chosen were a northern industrial town, referred to throughout this book as Northtown, a coastal resort in southern England to be referred to as Seagate”  
(Morse 1965:10)<sup>19</sup>

“‘Agnes’ is the pseudonym of a patient” (Heritage 1984:180), an intersexed person, who presented herself to a research team<sup>20</sup> at UCLA.; whilst another patient is identified only by the initials “E.P.” (Garfinkel 1967:149). The professional circumstances of the members of the research team unequivocally identified Los Angeles as the locus of the research with Agnes; it would not have been credible to disguise University of California Los Angeles as, e.g. University of California at Berkeley. That Los Angeles is positively identified warrants the references to other locations in South Western California, e.g. Long Beach, San Fernando Valley, San Francisco, and Santa Monica. Considering the proximity of such locations to Los Angeles it is unremarkable that events in a resident’s biography should occur there; i.e. because Los Angeles is identified, any locations in South Western California thus

become credible locations for events in Agnes' biography. Whether Long Beach or Las Vegas were the actual locations of events does not matter.

Garfinkel does, however, pseudonymise two locations which are crucial to Agnes' whole biography and presentation as a "natural, normal female" (Garfinkel 1967:122). Agnes was born, raised and schooled in her home town, which was pseudonymised as "Northwestern City" (Garfinkel 1967:118-119). Agnes lived in Northwestern City for the first seventeen years of her life, where "she was recognised by everyone to be a boy" (Garfinkel 1967:120). Agnes reports to Garfinkel how difficult her life was for her in Northwestern City, having to live with a male body.

The second significant pseudonymised location in Agnes' life was "Midwest City" (Garfinkel 1967:120). This was the home-town of her grandmother; Agnes visited her grandmother, intending to find a job whilst presenting herself as a woman. Although Agnes did not follow through with her intentions, returning to her mother's house in Northwestern City the very evening she effected the change, Agnes continued to present herself as a woman for another two years and two months before the UCLA team encountered her.

Location and organisation identifiers may be coterminous. D.R. Watson (1997:86) pseudonymises an internal forensic report as "Factory City Police Department", thus providing the organisation and its location in the same pseudonym. The pseudonym "Factory City" also works to preserve the uniqueness of the common-sense, locally-

known identifier of an industrial city in the US which, in the UK, is analogous to the South Yorkshire city of Sheffield and its locally-known identifier as “Steel City”.<sup>21</sup> The locations of felonies are pseudonymised on a forensic report as species of trees, *viz.* “431 Ash St.” and “826 Sycamore Ave” (D.R. Watson *ibid.*).

To disguise the identity of organisations, Sutherland assigns number and letter codes to individual corporations. He justifies this form of concealment by appealing to precedent and tradition<sup>22</sup> and, paradoxically, by the redundant expression of those moral sentiments enunciated by Babuscio (*op cit.*).<sup>23</sup>

Organisations (e.g. schools, hospitals, residential care homes, law firms, police departments, public assistance agencies, crisis intervention centres, charities and carry-out stores) are commonly assigned pseudonyms. The section above on the visibility of notifications cited the eponymous “*Hightown Grammar*”, the pseudonymised grammar school in the pseudonymised northern town “*Hightown*” (Lacey *op cit.*); and “*Gregsons*”, the eponymously pseudonymised law firm founded by the pseudonymised “radical” lawyer “*Jane Gregson*” (Travers *op cit.*). The locus of many activities within the neighbourhood is disguised as “*The New Deal Carry-out*” (Liebow 1967:17 *et passim*). “*Lakeside Office*” is a pseudonym for a public assistance agency (Zimmerman 1974:128). “*Hazelford Lodge*” and “*Bracken Court*” are pseudonyms in an ethnographic comparison of residential care homes in the South West of England (Lee-Treweek 1994). “*Cohen*” and “*County*” are the pseudonymised hospitals of an ethnographic comparison of private and publicly-funded hospitals (Sudnow 1967).

“*Lifeliners*” is the pseudonymised crisis intervention centre, calls to which provided the data for the analysis by D.R. Watson (1981). D.R. Watson’s admonitory notification of the use of an organisational pseudonym to maintain confidentiality is exemplary in the level of consideration of members’ practices, as opposed to *ad hoc*, analytic interference, as he attempts

“to preserve the nonspecific nature of the organisation’s title. One analytic problem [with preserving the nature of a pseudonym] is that it is possible that prospective callers to the center might be able to infer something from the actual title (as, e.g. they could conceivably draw inferences from some title such as “The Befrienders”, “The Companions”). Still, the pseudonymisation of an organisational title might affect or limit th[e] analysis in some respect, although I believe it does not substantially affect the analysis of members’ explicative work. Whatever inferences callers may initially draw still have subsequently to be imparted with a practical and situated specification...”

(D.R. Watson 1986:92, fn.1).

Within a footnote, then, D.R. Watson outlines the ethnomethodological concerns with the work of competent membership and natural language, *viz.* inference and preference, where the researcher should be attuned to the preservation of accountable and inferentially-available phenomena. In his topicalising the practice of pseudonymisation, D.R. Watson examines the implications of pseudonymising actually produced names, and the unavoidable trade-off between ethical implications and distorting phenomena available for analysis:

“the use of a pseudonym inevitably limits the reader’s access to the phenomena itself and therefore limits the analysis. All I can say is that the same “kind” of title has been selected; beyond that I have [...] placed the interests of confidentiality before any analytic consideration.”

(D.R. Watson 1986:98, fn.8)<sup>24</sup>

A charity that promotes the befriending of offenders in prisons was anonymised as “*The Organisation*” (Carlin 1994). In Ryleian terms the capitalised eponym was, *mutatis mutandis*,

“impartially receptive to any [pseudonym] you please. [“The Organisation”] is not a rival to [actual name], it is a hotel for [actual name] or any other [pseudonym].”

(Ryle 1954:84)

The latinate *caveat* above is crucial: because Ryle was discussing algebraic terms (“*x* is not a rival to 7, it is a hotel for 7 or any other number”), the invoking of his work to explicate the work of pseudonymisation must avoid the conflation of different language games, a form of “category mistake” which Ryle (1966) himself cautions against.

The pseudonym “*The Organisation*” is difficult to sustain throughout the transcripts of talk in the parenting-counselling sessions, however. Names are embedded within utterances, which in turn are embedded within sequences of utterances. Names furnished at one point in a sequence may be utilised at a later point in the sequence. The problematic status of names is occasioned by their production, e.g.

#### Extract 103 (5)

- 1      CD    (       )
- 2      CR1    a- a- actually I think one of the funniest things ever was erm
- 3                one guy er
- 4                he was (.) press-ganged
- 5                can't remember who he was now=
- 6                =but he came in and erm
- 7                he came in with this rather sort of vague look on his face

8 he was told to (.) join the group  
9 and he came in and he said erm  
10 I- I- was told i- i- i- it's about bridge building= [ ]  
11 CR2 humm yeah=  
12 CR1 =is it?  
13 CR2 =(laugh))  
14 CR1 ((laugh))  
15 building bridges  
16 ((laugh))  
→ 17 called [XXXXXXXXXXXXXX]  
18 ((laugh))  
19 you know  
20 ((laugh))  
21 CD ( ) [  
22 CD ( ) [  
23 CR1 ((laugh)) [  
24 CR2 ( ) [  
25 CR1 ah that was really good  
26 ((laughter)) [  
27 but we are (.) building bridges (.) aren't we  
28 CR2 yeh  
29 CR1 yep  
30 different bridges (.) but we're building bridges  
31 I hope we've been able to build a bridge (.) with you  
32 and er  
33 you've been able to sort of feel you can talk to us

In this sequence or bloc of talk, CR1 is relating an anecdote to the members of the group (lines 2-13). She recalls how “one guy” (line 3) was under the impression, a mistaken impression, that the course being provided was on construction, rather than parenting techniques.

His mistake turns out to be obvious, given the organisational name (deleted at line 17, arrowed). Although the name is not reproduced in the transcript, that it is obvious is

noted by CR1 in line 19: CR1 and CR2 are laughing and sharing the joke.<sup>25</sup> At line 15, CR1 formulates what is “funny”: “building bridges”. And that it is an easy mistake that anyone could have made is explicitly invoked at line 19, where she says “*you know*”: “*you know*” here works to show that this is an understandable error.

The members of the group realise the gist of the joke (the overlapping, inaudible utterances at lines 21 and 23), and CR1 brings some closure to the story (“*ah that was really good*”—line 25). The mistake and the organisational name are then used as resources in the subsequent sequence of talk. CR1 comments that metaphorical bridges are being made by the voluntary organisation, between the “outside community” and with inmates serving sentences (lines 27-30). (This reflects the organisational aims as I outlined in the previous chapter, when explaining the origins of my interest in confidentiality and pseudonymisation.) CR1 concludes the sequence by invoking the bridge-building metaphor as an upshot of the parenting course with the members of this particular group.

From this bloc of talk we can see that changing a name has consequences, because changing the name changes the nature of the following talk. *Changing the name embedded within a sequence of talk alters the sequence itself.* Any pseudonym for the organisational name occludes the warrantable inference, the easy mistake, that the course was not about parenting techniques but was about building bridges. It therefore takes away the reason for a tell-able, amusing story. The actual name is used as a resource for ensuing talk. A pseudonym for the organisation’s name occludes the joke.

The crucial point about this occlusion of a sequence is that an organisation's name, and any pseudonym, make available organisational relevances and inferences. Pseudonymisation damages the phenomenological intactness of a sequence of talk; it distorts the data and consequently distorts the analysis.

This extract illustrates that assigning pseudonyms has essentially practical consequences. The interaction is contingent upon, is based around the real name. A pseudonym might not fit; it might be a square peg in a round hole. The above extract also shows how transcribers use pseudonyms for organisations as well as people. D.R. Watson (*ibid.*) uses "Lifeliners" as the pseudonym for a crisis intervention centre. He attempts to capture or preserve the function of the centre with the pseudonym as opposed to alternatives such as "The Befrienders" or "The Companions". People could draw inferences from these names. At first glance these might suggest the centre was a dating agency. D.R. Watson (1986:120) provides the following extract which demonstrates that callers mistakenly infer the functions of the organisation in just such a manner (Co. = counsellor, Cl. = caller):

[Watson]

1. Co. hello Lifeliners can I help you?
2. Cl. you couldn't put me in touch with a prostitute could you?
3. Co. a prostitute? I'm sorry I couldn't no ((3 secs.))
4. Cl. do you know where I could
5. Co. er no I'm sorry I don't think I do ((3 secs.))
6. Cl. alright thank you
7. Co. is there can I do anything else for you?
8. Cl. no that's (all) I wanted
9. Co. no
10. Cl. alright thank you
11. Co. alright thank you

“Lifeliners” is the “same kind” of title and keeps the organisation a suicide prevention agency.

The unintended consequences of pseudonymisation, concerning disguising the identity of an organisation, are illustrated by an ethnographic fieldwork project into matters such as leadership, team building and teamwork, at an emergency services centre. The materials derived from this project (Driessen, Gold and Watson 1997a; 1997b) arrive at a crucial juncture in the research process. Specifically, their intersection addresses the point at which actual identifying details are expunged and replaced by anonymising reference terms—proper names, precise geographical location—in order to protect the subjects of research in accordance with accepted social scientific practice. These reports show how the fixing of anonymising terms to a natural-describable setting distorts the actuality-as-experienced of that setting.

In Garfinkel’s terms, the practices of anonymisation and pseudonymisation risk “losing the phenomenon” of inquiry: the anonymised presentation is not a true (to members) representation of the actual setting. As shall be seen in the next chapter, pseudonyms introduce another phenomenon, a new tier of phenomena, which were not constitutive features of the actual setting. For when anonymising details are mapped onto the researched setting, the setting ceases to be the original object of the research. The practices of pseudonymisation remove the pseudonymised version of the setting further

from the actual setting. The anonymised product runs athwart, rather than parallel with, the actual setting.

Driessens *et al.* (1997b) are attuned to the analytic consequences of disguising and protecting the identity of the organisation, whilst at the same time preserving the essential features of the organisation. This section has just observed how, through the use of the pseudonym “Lifeliners” in order to pseudonymise an organisational legend, D.R. Watson (*ibid.*) attempts to capture and reflect as accurately as possible the name of the organisation whose purpose is staffing a telephone help and advice centre for people contemplating suicide.

Driessens *et al.* (1997b) consider the gist-preserving nature of pseudonyms in order to preserve the character and function of the organisation without leading to its possible identification. Selecting a suitable pseudonym which does preserve the gist of the organisation is made problematic by the limited number of organisations involved in the US. Locating the organisations is made difficult by the geography of the Continent; that is, how many areas within the US could feasibly be the site for such organisations.

The practice of pseudonymisation was reflected in the “recipient design” of the two different versions of the report. The consultancy version (1997a), which was presented to “Mahogany Row”, i.e. the higher offices in government ministries (Washington DC) was attended by those who were in possession of the actual identities of the organisations. The use of pseudonyms was not only unnecessary, then; indeed, it would

have been an accountable matter if pseudonyms had been used for these recipients. The subsequent version (1997b), written for wider academic audiences, was pseudonymised.

In transcribing talk-in-interaction, pseudonyms necessarily obscure the embedded and contingent properties of actual names within talk; the concerted imposition of “*The Organisation*” exacerbated the troubles encountered by the transcriber. So, following D.R. Watson (*ibid.*), I shall use a pseudonym for the organisation which, *mutato nomine*, preserves as much as possible the character and declared profile of an organisation whose legend reads “Creating links between the offender and the community”. The pseudonym to be used thereafter was “*Second Chance*”.<sup>26</sup> However, like D.R. Watson’s “Lifeliners” problem (*ibid.*), I could not devise a pseudonym to preserve the sense of the joke in the extract above, which at the same time was transformed sufficiently in order to disguise the identity of the organisation.

Herein lies the tension for researchers for reports of their projects, between the preservation and transformation of features. Researchers have to transform the name to some extent, but preserve those features that are important to the setting, including the inferences that can be drawn from any details. An example where researchers did not transform the name enough is the infamous “Springdale” incident. This was a community study of a small American town called “Springfield”. The pseudonym that Vidich and Bensman (1958) used was “Springdale”: *this name was not transformed enough* and caused outrage among the townspeople. (For instance, in the Fourth of July parade residents wore labels with the fictitious names that had been given to them in the

book.)<sup>27</sup> Individuals were easily identifiable in the village. Among other issues raised by the reaction to this study, it highlights a difference between public knowledge in a village, and the villagers' public knowledge—the same stories—appearing in print.

### ***Conclusion***

The previous chapter, on fieldwork and linguistic activities, looked at confidentiality in a real-world setting. It reports that within the period of participant observation, I realised how ordinary, routine practices were irredeemably involved in the production of confidential, organisational documents. My own *imprimatur* was not a deliberate authorship but a result of my own common-sense practices.

The previous chapter also noted that the organisation had a code of practice, which established that all information is to kept within the organisation. This code stipulated that no information was to be transmitted to persons outwith the organisation, e.g. to journalists. However, as observed during the fieldwork, confidentiality within the organisation itself was an occasioned matter. The routine use of pseudonyms by members of the organisation exacerbated the problematic nature of confidentiality *apropos* the research site. As a result of these observations, I was made sensitive to confidentiality, and its vagaries, and how confidentiality was used as an organisational resource. This sensitisation lead to this investigation presented in this chapter, *re* the nature of confidentiality as practised and reported by analysts.

Furthermore, this chapter has illustrated that attending to the interactional details of settings, whilst attempting to protect the identities of participants, takes work on behalf of the analyst.

Further data from the parenting-counselling sessions will be presented in the next chapter, as the thesis moves from considering confidentiality and pseudonymisation in extant sociological work towards the pseudonymisation of actual, naturally occurring talk. This aim requires some further elucidation of names and their indexical properties. Pseudonymisation is another locus of Membership Categorisation Analysis, which demonstrates how pseudonymisation is accomplished by the analyst's use of culturally methodic, i.e. natural language practices. That is, it shows how pseudonymisation is linguistically constituted.

This areal theme suggests that confidentiality and pseudonymisation are more than "good practice" in research, but that they are research methods in their own right.

<sup>1</sup> This term is Jefferson's (1979), from an analysis of members' production of (three-part) lists in naturally occurring conversation. Her empirically-based notion of "list-construction" is derived from oral rather than textual materials, as it is used here. Cf. Atkinson (1984:57-73).

<sup>2</sup> "Cognitive processing" implies the existence of a form of "fermata" (Baker 1994). The "fermata" is a fold in time which, unbeknownst to others, allowed Baker's protagonist, Arno Strine, to pause real-time in order to satisfy (and satiate) his prurient curiosities.

<sup>3</sup> In another context, *viz.* education-career counselling sessions, Erickson and Schultz (1982) examine the role of "gatekeepers". Their notion of "gate-keeping" derives from the analysis of counselling interactions: they observed that counsellors would advise students to opt for particular courses—options which led onto further course-options, which led to future career-options—along racial lines. The analogies of gates and gate-keeping may usefully apply to these considerations of confidentiality.

<sup>4</sup> In mathematics, an asymptote is an axis or straight line whose perpendicular distance from a curve decreases as its distance from the origin increases. Asymptotes, lines which will never touch even as they advance ever closer towards each other into infinity, provide an adequate illustration of unique adequacy in the ethnomethodological, i.e. sociological enterprise: it is not that uniquely-adequate description is a Sysyphean boulder that will roll down to the bottom of the hill as soon as it is rolled up to the summit; rather, no matter how high the boulder is rolled, or how sensitive the description is to the setting, the summit is always beyond reach. Uniquely adequate description is a condition that cannot be realised.

<sup>5</sup> Graham Watson, personal communication, 28 January 1997.

<sup>6</sup> Preliminary inquiries, oriented not to the actual identities of letter-writers, *à la* Snodgrass (*op. cit.*) but to whether identities had been disguised, is problematic. The manuscripts and notes contained in an archive collection—The Florian Znaniecki Papers and Addenda in the Department of Special Collections at the University of Chicago Library—refer chiefly to Znaniecki's subsequent work. The Department of Special Collections only has a limited number of papers by Thomas; Thomas asked his wife, Dorothy Swaine Thomas, to destroy his papers after his death. I wish to thank Robin A. O'Sullivan and Daniel Meyer at the University of Chicago for their assistance on this matter.

<sup>7</sup> A recent example from outwith the corpus of sociological literature may help to illustrate the Documentary Method of Interpretation. The social security minister and Conservative whip, Andrew Mitchell, was alleged to have acted as a whip on a Commons committee—leaving the committee room for consultations with the whip's office and hurriedly returning to the Committee's proceedings, and in so doing “breaching the confidentiality of the work of the Committee”—to have behaved “inappropriately”. The Conservative MP for Portsmouth North, Peter Griffiths, who was also on the Commons Standards and Privileges Committee with Mitchell, said “I did not think anything extraordinary about it at the time, but in retrospect it [the allegation] has shown a different light” (quoted in *The Guardian*, 20 January 1997). Griffiths exhibits the common-sense use of the Documentary Method to interpret events: the indexical particular, that a Committee member should rush out of the room and back again did not seem “extraordinary”, is re-interpreted upon the introduction of an underlying pattern (the allegation that Mitchell was consulting with Conservative whips outside the committee room) through which Mitchell's to-ing and fro-ing is seen through “a different light”.

<sup>8</sup> E.g. “WWW”, “TTT” (Thrasher 1927:14); “YYY”, “XXX” (1927:19).

<sup>9</sup> D.R. Watson, personal communication, 14 January 1997.

<sup>10</sup> It is to be noted that this is not always the case, however. In the following chapter, the provision of real names will be considered in the section on “The inspectable properties of names”.

<sup>11</sup> Quoted with permission. I wish to thank Gail Jefferson for providing me with copies of her unpublished papers, and her encouragement to treat pseudonyms as serious objects of sociological inquiry (personal communication, 22 April 1997).

<sup>12</sup> Although caste names may point towards criterial personal histories, they do not necessarily encapsulate nor clarify them. Indeed, “[Ghurye] noticed that caste names are very often the names of trades, but that these are not the only names: ethnic or tribal names, names of sects and names indicating still other features are also found.” (Dumont 1972:136). Caste names are considered briefly below.

<sup>13</sup> On this point, other anthropologists support Karve: “whereas professional castes are often designated by the name of their trade, their subcastes usually take the name of the territory or locality.” (Dumont 1972:199). However, this *prima facie* similarity is located within disagreement concerning the nature of the caste system. A caste’s name may be a “*professional label*” (Dumont 1972:327; n.26a), an assertion based upon the work of Blunt, who proposed a theory of caste formation as a process of accretion (collection) rather than scission (division) of people. As such, caste names are ‘umbrella-terms’ for collections of people who perform certain tasks, which are so collectable; rather than divisions in a task-specific taxonomy, whereby discrete labels are applied to performers of discrete tasks. (In a Wittgensteinian sense, there are “family resemblances” between these tasks.) Indeed, Dumont summarises Karve’s argument *apropos* castes and subcastes as rather than subcastes being subdivisions of castes, that castes are formed by the accretion of subcastes. For Dumont, this is an untenable position “for it is tantamount to saying that there is no caste of washerman or dhobis but only washermen of such-and-such a kind (subcaste).” (Dumont 1972:101).

<sup>14</sup> In *The Cairo Trilogy*, London, New York, Sydney, Auckland, Toronto: Doubleday.

<sup>15</sup> Literally, then, temporal particulars furnish resources for the nursery rhyme “Monday’s child is fair of face...” This is the theme of a study on Ashanti personal names (Jahoda 1954). Jahoda set out to investigate a belief among the Ashanti that personalities are determined by the week-day of a person’s birth.

<sup>16</sup> In his discussion of kinship usages among the Crow, Lowie remarks “The tie between a person and his patrilineal kindred appears also in the bestowal of names. An honorific appellation is often derived from a father’s clansmen, and the nickname an individual bears may reflect not his own but a “father’s” or a “paternal aunt’s” predilections” (Lowie 1948:71).

<sup>17</sup> Gallaher (1964:285, fn.2) also notes that the original researcher, James West, had written under a pseudonym (Carl Withers).

<sup>18</sup> Don H. Zimmerman (personal communication, 22 April 1997).

<sup>19</sup> It is noted that, *qua* the notices of confidentiality reproduced above, this notice is contained within the introduction to the study, which was written by Robert Beloe.

<sup>20</sup> Comprising H. Garfinkel, R.J. Stoller (psychiatrist and psychoanalyst), A. Rosen (a psychologist); Agnes’ endocrinological details were studied by A.D. Schwabe, D.H. Solomon, J.P. Burnham and Stoller (Garfinkel 1967:119-121).

<sup>21</sup> D.R. Watson, personal communication, 15 January 1997. Certain conurbations possess locally-known and culturally available “monickers” (cf. Polsky 1969, in the following chapter), e.g. Edinburgh is also known as “Auld Reekie”, because of the smell of hops used in its breweries; Manchester is also known as “The Rainy City”, due to its levels of precipitation; Detroit, Michigan is also known as “Motortown”, due to its traditional car manufacturing industries. Jaipur, India is known as “The Pink City”, whereas Petra, Jordan is known as “The Ruby City”, on account of the reddish stone. (For changes in residents’ nicknames for cities, and see also Strauss 1968a:23).

<sup>22</sup> Viz. "the identity of criminals is frequently concealed in scientific writings about living offenders", (Sutherland 1949:xiii), a rationale which invites clarification on social research and the 'rights of' and 'obligations to' deceased offenders in this particular case, and deceased people in general.

<sup>23</sup> "[T]he objective of [White Collar Crime], which is the theory of criminal behaviour, can be better attained without directing attention in an invidious manner to the behaviour of particular corporations", (Sutherland *ibid.*).

<sup>24</sup> Notice that in this quotation, D.R. Watson also draws our attention to the all-inclusive nature of the ethnomethodological notion of membership, which comprises readers of the research as well as the participants (or 'subjects') of the research and the researcher.

<sup>25</sup> In Chapter Seven, I explain the difficulties involved in transcribing the corpus of tape-recording from which this sequence is extracted. These difficulties were predominantly due to the quality of the recordings themselves, but also the equipment which I was using to transcribe them. The transcript was amended to include slight pauses (e.g. lines 27, 30-31) and latching (lines 10-14) with new technology; phenomena of laughter, as suggested by Jefferson (1979, 1985), are not preserved.

<sup>26</sup> Within this thesis I shall continue to use the capitalised form "*The Organisation*", as an anonymised placeholder for the organisation to which these observations derive; "The Organisation" and "Second Chance" are interchangeable.

<sup>27</sup> This parade was reported in a local newspaper, and cited by William Foote Whyte (1958) in an editorial for *Human Organisation*.

## Confidentiality and the Practical Work of Pseudonymisation: Categories-in-Use

When we are looking at pseudonyms we are perforce looking at names.<sup>1</sup> In the previous chapter, it was suggested that names are chosen *in preference to* other possible names. Karve's study of personal names in India was used to illustrate this. Her study shows how names can be inextricably linked to the situation that a person is born into: a) when, b) where, c) to whom, and d) under what circumstances. In Karve's study we see how the choice of a name can be influenced; names may be a reflection of certain biographical details: details that provide reasons to prefer to choose one name rather than another. Here I shall use the notions *preference* and *inference*: there exist certain preferences to assign certain names; and inferences can be drawn upon these preferences. The "inference-rich" quality of names can affect the assignment of pseudonyms in social research: the researcher can accidentally assign a biography to an informant.

To reiterate concerns from the previous chapter, some names in Karve's study reflect place of birth and social position. This indicates problems: Names, and therefore pseudonyms, set parameters. A pseudonym may categorise and locate a person as originating from a particular geographical region and social grouping. Names can indicate the serial ordering of siblings, e.g. identifying first-born children. The researcher must avoid assigning a pseudonym if it conflicts with biographical particulars; e.g. a pseudonym which infers only child, and then referring to other siblings. There are events or "special circumstances" during pregnancy, labour, and in

the first few hours and days that influence the name; e.g. children who had recovered from serious illness, children born after a series of miscarriages or still-born deliveries can be the recipients of a name which reflects their good fortune to have survived, where its siblings had perished.

So we see from Karve's work that names may be relevant to personal biographies too, and therefore inferences can be made from any assigned pseudonym. The onus is on the social researcher to pseudonymise the name, and change all biographical particulars to be consistent with this pseudonym. This illustrates that inferences are routinely made on persons' names; similarly, inferences can be made on pseudonyms of these names.

Although this chapter makes the seemingly tautological observation that pseudonyms are names, it emphasises this truism for the benefit of practicing fieldworkers. To see how pseudonyms, false names, work we should first look at how real names work. Names and naming practices in cultural settings demonstrates that names are "placeholders": names carry meanings for members. When it is recognised that real names are meaningful, it occurs that false names are also meaningful. That is, the pseudonym that we use to replace the real name may also carry meanings. Certain inferences can be drawn from the name. Similarly, inferences can be drawn from the pseudonym. Researchers want to guard against unwarranted inferences being drawn.

From the insights drawn from the previous chapter, this chapter moves towards the study of how names are occasioned, that is, names within interactional settings. From

empirical observations on the use of names in interaction, this chapter looks at how to replace occasioned names with pseudonyms.

### ***The Accountability of Names in Conversation***

When we attempt to explicate members' practices in fine detail, i.e. the detailing of sequential and categorial determinations made by members at the utterance by utterance level, names—and therefore the assigning of pseudonyms—achieve a hitherto unnoticed salience.

The research practices of recording and transcribing naturally occurring conversations open up a field of interactional phenomena.<sup>2</sup> Garfinkel calls these phenomena "observable-reportable" (Garfinkel, in Hill and Crittenden 1968:9), because they are available for inspection (observable) and thus available for comment (reportable). This provides sense to the Garfinkelian term *accountable*: what is account-able is anything of which an account can be produced. And what is accountable is open to inquiry. That is, they become "inquirable properties".<sup>3</sup>

Such uniquely variant features in conversations as continuers, laughter and silences are open to investigation in their own right; the analyst can pursue their inquiry unhindered by exogenous constraints on the analysis imposed by confidentiality. The naming of persons—referring to names of persons absent or party to the talk—is similarly an ordinary feature of conversations. However, in research projects, the naming of persons is *extra-ordinary* in that, unlike continuers (such as "mm-yeh" or "uh-huh"),<sup>4</sup> names

have to be changed. The common occurrence of names is treated as exceptional, in that actual names must be excised and replaced by surrogates, pseudonyms. At this fine level of detail, however, a problem arises:

“when names occur in talk, they occur as embedded and productionally relevant components of that talk, such that a pseudonym, no matter how artfully selected, can obscure data and potentially exclude from research interesting and consequential domains of phenomena.”

(Jefferson 1974:1)

The exceptional nature of names in sociological reports mentioned above is obtained by dint of accepted research practice; yet this requirement is not paralleled by any attention to how names actually operate, nor the environments in which names are occasioned. Jefferson provides an example of transcribed conversation to illustrate the indexical properties of names and how they are embedded within sequences of talk. This is an extract from a telephone call between Guy and Agnes, who have a daughter named Barbara, on their wedding anniversary:

[Jefferson]

AGNES	Thirty seven years ago <u>tonight</u> we were on our way to <u>Ventura</u> .
GUY	Mm. What do you know about <u>that</u> .
AGNES	Santa <u>Barbara</u> hh huh [huh
GUY	[ <u>Barbara</u> . Uh huh
AGNES	We got a <u>Barbara</u> <u>didn't</u> we
GUY	Mmyuh <sup>5</sup>

In her discussion of this extract, Jefferson notes that “At one level the import of the data can readily be preserved” by the juxtapositioning of the referential particle “Barbara” with “Clara”. However, Jefferson goes on to say that

“at another level, relevant features are obscured by those pseudonyms. For example, there is a phenomenon of generationally characteristic names. “Clara” may be distinctive compared to “Barbara”, which may be a name commonly given children of that generation.”

(Jefferson 1974:2)

It is crucial for the preservation of the sequence that Guy recognises his daughter’s name in Agnes’ utterance. (That Guy does recognise his daughter’s name is shown by his repeat of Agnes’ emphatic “Barbara”.) Of more significance than the generational nature of names in this sequence, perhaps, is that the changing of “Barbara” to “Clara” would make his association between the place name and personal name nonsensical: the association of place is between Ventura, California, and Santa Barbara. A common-sense reading of the transcript suggests that a baby was conceived “thirty seven years ago tonight”, in Ventura; and that this baby was named after the area of its conception, *viz.* Santa Barbara. If the location is changed to Clara, it becomes Santa Clara. Thus, if Guy and Agnes celebrated their wedding night in Cuba, where does this leave “Ventura”?

We can see then, that names are embedded within utterances, which in turn are embedded within sequences of utterances. Names furnished at one point in a sequence may be utilised at a later point in the sequence. Changing a name is consequential, i.e. it has consequences, because changing the name changes the nature of the following talk: changing the name embedded within the sequence alters the sequence itself. So assigning pseudonyms has essentially practical consequences.

Elsewhere, in her analysis of a sequence of talk between two different conversationalists, Mel and Jean, Jefferson states

“that the pseudonyms used in the transcript preserve one feature of the actual names; that “Melvin” is diminutable to “Mel”, and that “Jean”, while diminutable, is not equivalently diminutable, but diminutable to something perhaps more intimate, “Jeanie”. It is interesting to speculate what the interchange might have looked like if, for example, “Jean” had a name like “Janice”, diminutable to “Jan”. At any rate, it appears that Mel’s remedial resources are constrained by the specific features of the name he must work with. “Jeanie” is perhaps too intimate to use to reciprocate “Mel”, and “Jean” does not reveal a possible intended reciprocity.”

(Jefferson 1972:111, n.19)

The perspicacity of Jefferson’s observations sensitises us to the delicate work involved in the attribution of pseudonyms. Among the intricacies of transcribing natural conversation in order to preserve the phenomenological intactness of settings, or at least in approximating to the actualities of the setting without disturbing or corrupting its internal structure, the transcriber’s work includes the realisation of a pseudonym that

possesses characteristics of the actual name; that the name and its pseudonym contain (a) “family resemblance(s)”.

Further, the transcriber’s task is to realise an adequate and sustainable pseudonym, i.e. a pseudonym suited (“adequate”) to the *in vivo* work of the setting whilst minimising the damage to the internal structure of the talk; and one which is (“sustain-able”) able to withstand this lived work over time. In Rose’s terms, the transcriber realises a pseudonym that does not only suit “the business at hand”: the business of doing sociology, writing up reports and changing names in order to adhere to the practice of maintaining confidentiality; but one which is suited to “the something else again,” to the talk that constitutes the focus of inquiry.

This suggests that pseudonyms should be assigned with caution; that not any pseudonym will do. Transcribers may wish to choose a pseudonym which ‘resembles’ the originally produced name in preference to another candidate pseudonym.

Consider how some names share certain “family resemblances” with each other; they are not coterminous, but intuitively share acoustic ‘similarities’: e.g. Elaine, Jane, Shane, and Wayne; Mark and Marek; Andrew, Andrea and Andreas; Susan, Suzanne and Susannah. “Robert” and “Robin” are co-characterisable, share a resemblance, in that they are both diminutable to “Rob”. “Rob” is not gender-specific, as a diminutable form of the male and female equivalents “Robin” and “Robyn,” “Robert” and “Roberta.”

Referring to the transcripts below, the actual name “sounded like” ‘Shane’; the ‘sh’ sound is also heard in ‘Seamus’, the equivalent Gaelic form of ‘James’. Perhaps the transcriber considered the acoustic similarity of e.g. ‘Wayne’ and ‘Jane’: if ‘James’ can possibly be regarded as a masculine equivalent of ‘Jane’, might this have caused the pseudonymisation of ‘Wayne’ to ‘James’? These are quite tenuous linkages in order to arrive at ‘James’ from ‘Wayne’. Instead, the transcriber attempted to preserve the ‘ay’ sound, which was achieved through ‘Shane’ and ‘James’. However, the ‘sh’ sound (as in ‘ship’), fitted into the transcript, seemed to disrupt the sequence more violently than the ‘j’ (as in ‘jam’) sound. Retrievable data allows such consideration and, if necessary, reconsideration of the suitability (adequacy, sustainability) of any pseudonym.

Distinguishing persons can also be accomplished using names. Consider the distinguishing work of ‘James’: two James’ might be referred to and become distinguishable through devices such as prefixes, e.g. ‘Big James’ and ‘Little James’; ‘Leeds James’ and ‘Scottish James’; and suffixes, e.g. ‘James One’ and ‘James Two’. That ‘James’ is diminutable is also used to identify specific persons. It is sufficient to refer to ‘Jim’ and be properly understood to be referring to ‘Jim’ rather than ‘James’; i.e. the diminution of proper names may not be just a feature of economy, familiarity or intimacy, but may distinguish between homonymous parties to and topics of talk. Indeed, as one of the Bethnal Green community studies remarks, the sharing of forenames within the same family across generations is not necessarily a source of

“confusion”, because “relatives could talk about ‘little Rose’ and ‘big Rose’” (Townsend 1963:132).

### “Assumed” Names

Polsky, in his ethnography of pool room hustling, found that members routinely assumed nicknames for the (practical) purposes of doing hustling. Polsky had initially thought that the nickname would allow the hustler some freedom of activity, i.e. to use their actual name in the hustling of a naïve player; (“naïve” in that they would not be familiar with the nickname). Polsky found, however, that the use of nicknames was otherwise:

“Most hustlers never use their real names (except occasionally with outsiders encountered the poolroom), not because these need to be hidden but simply because they prefer to be nicknamed. Like many other argot terms, these nicknames exist because, to quote the words of more than one hustler, ‘they lend a little color to the game.’”

(Polsky 1969:106)

Polsky distinguishes between the terms “monicker” and “alias”: as mentioned above, hustlers do not use their real names with “outsiders”. Polsky argues that when a hustler situationally required to give a name to outsiders, i.e. in situations that ask for a name, the hustler “temporarily adopts yet another name, a true alias.” (Polsky 1969:106, n.51) Thus a monicker or nickname is known by other hustlers, who may not even recognise a hustler’s real name. (This feature of hustling frustrates law enforcement agencies, who may attempt to trace a hustler using their real name.) However, if a name must be provided, then rather than a real name, or the monicker, the hustler uses a false name—an alias.

Polsky provides readers with a list of some nicknames used by poolroom hustlers:

“Brooklyn Jimmy, Cornbread Red, Spanish Eddy, Sleepy, Blueshirt, Glendale Johnny, Fats, Wimpy, Harry the Russian, Snake, Whitey (three hustlers), Connecticut Johnny, Detroit Whitey, Brooklyn Johnny, Jersey Red, Blacky, Fifth Avenue Red, Miami, Shoes, Cicero, Gigolo, Subway, Country, Peter Rabbit, Weenie Beanie, Skinny Eddy, Squirrel, Fast Eddy, Gypsy (two hustlers), Tallahassee, Boston Shorty, Rockaway the Hat, the Knoxville Bear, Derby, Dago Frankie, Lefty (three hustlers), Iron Joe, Bob the Destroyer, Jimmy Sure-Shot, Daddy Warbucks, and Tommy the Sailor.”

(Polsky 1969:106)

This list of hustlers’ nicknames is reprinted in full in order to consider the following questions: first, are these actual nicknames, or Polsky’s own inventions, i.e. pseudonymised nicknames, in order to protect identities, remembering that hustlers are identifiable from their nicknames? The previous chapter noted that Snodgrass (1973) highlighted this feature of identifiability from alias’, monickers and pseudonyms *apropos* the professional thief, Chic Conwell. Second, if these are pseudonymised nicknames, upon what grounds did Polsky choose them?

For example, is “Blueshirt” a pseudonym for “Greenvest”; “Harry the Russian” a pseudonym for “Larry the Prussian” (or “Larry the Lithuanian”);<sup>6</sup> “Whitey (three hustlers)” pseudonyms for “Blackie (two hustlers)”; “Fifth Avenue Red” a pseudonym for “Sixth Avenue Blue”; “Squirrel” a pseudonym for “Bushy”; “Jimmy Sure-Shot” a pseudonym for “Johnny Dead-Eye”;<sup>7</sup> “Daddy Warbucks” a pseudonym for “Brother Greenbacks”; or “Tommy the Sailor” a pseudonym for “Timmy the Soldier”?

It is surely apparent, as a member, to show how these alternative nicknames were arrived at: each nickname possesses features that provide for possible corollaries of the nickname. In comparing the above lists, the nicknames and possible alternatives may have been arrived at through, e.g., acoustic resemblances or features of names (*Harry* and *Larry*, *Jimmy* and *Johnny*, *Tommy* and *Timmy*); inferentially available details (squirrels have bushy tails); synonymous phrases (*Sure-Shot* and *Dead-Eye*,<sup>8</sup> *bucks* and *Greenbacks*); and membership categorisations, including nationality (*Russian* and *Chinaman*); names (*Harry* and *Larry*, *Jimmy* and *Johnny*, *Tommy* and *Timmy*), numbers (*three hustlers* and *two hustlers*, *fifth* and *sixth*); place names (*Fifth Avenue* and *Sixth Avenue*); colours (*blue* and *green*, *white* and *black*, *red* and *blue*); clothing (*shirt* and *vest*); family (*Daddy* and *Brother*); professions, specifically armed services (*Sailor* and *Soldier*).

There are exhibited above collections of categories and other inferentially available particulars, which are commonsensically derived. In Sacksian and Wittgensteinian terms, the inferred pseudonyms are co-characterisable with the nicknames; they are part of the same “family”.

Such an exercise can be performed on each nickname in the list.

The following extract is from a transcribed conversation between Rose and Ali Baba, a man close to the drug scene in Frankfurt.<sup>9</sup> Rather than attributing a pseudonym to his

informant, Rose provides the informant with an opportunity to assume a name of his own choosing, an alias.

1. R: My name is Ed Rose. I'm saying that to the tape and to you.
2. A: Pleased to meet you.
3. R: The question is this: Do you want your name on the tape?
4. A: Not necessarily.
5. R: Why not? It's all your knowledge. You should get credit for it.
6. A: Well, just -- You know, my experience tells me to be careful. It's not that I don't trust you.
7. R: I'm not going to have the tape. In fact if you say something that you don't want --
8. A: Well, just kind of instinct, you know: always take care, even if there's no reason for it.
9. R: There is a certain reason. You should get credit for the knowledge that is yours.
10. A: Well, still I'd rather be careful.
11. R: You want to be anonymous? Fine. Well, give me a name I can call you by.
12. A: Ali Baba.
13. R: I'll call you Al. Okay?
14. A: All right.

(Rose 1981a:1-2)

Here, we see the informant, Ali Baba, giving Rose an alias invented for the practical purposes of this particular conversation. Is "Ali Baba" a monicker that he is known by on the street? Polksy (*op. cit.*) shows that a hustler uses a "true" alias for purposes of securing accomodation, travel arrangements, etc. Monickers are useful when for example the police are searching for them under that name. For Ali Baba, his visible reluctance to furnish his name for the tape (lines 4-13) could point to a distrust not only of the police, authorities and agencies involved in drugs and drug addicts, but also an awareness that disclosing information may discredit him among people on the street. This research is indifferent to Ali Baba's reasoning for being circumspect: that his disclosure of his real name might in some way affect his relationships with people on

the street is speculative and imputes a motivational scheme which might not in fact exist. Whatever his motives, however, (and to repeat, this thesis is indifferent to his motives), Ali Baba is seen to provide an account or gloss (lines 6-11) for the “withholding” of his name *qua* a caller to a Suicide Prevention Centre (D.R. Watson 1986:91); the reluctance to give a name is shown to be an accountable matter, i.e. an explanation or an account is required.

The assumption of a false name by an informant, rather than the *ex post facto* attribution of a pseudonym, may protect the identity in ways unappreciable to the researcher. As a former heroin addict and dealer, Ali Baba acknowledges the sensitivity of his position in the very fact of speaking to a researcher, even one whom he feels confident with (line 6): he has made a more informed choice of pseudonym than the researcher could.

### **"Assigned" Names**

In talking with the police officers on Larimer Street, Rose secured information on the processual nature of arrests on Skid Row.<sup>10</sup> If a policeman encounters a drunken man, the man on the street is not arrested immediately; first of all, he is sent home. If he ignores this order, and is found on the street later during the officer's shift, he is arrested. The officer waits for a police vehicle to transport the arrested man to the city jail.

Whilst waiting for the arrival of the vehicle, the officer completes the “arrest slip”; the arrest slip, (a ‘top copy’ plus three carbon copies), is designed to detail the arrested man’s name, date of birth and address. As Officer Schalbrack told Rose,

“Many times they are not able to give an address and don’t remember their date of birth and the name is indistinct.”

(Rose 1965c:37)

There is another section, “remarks”, for an officer’s miscellaneous comments. Officer Schalbrack told Rose that

“under remarks, I put the man’s description, his actions on the street that led to the charge, the fact that he was incapable of taking care of himself”

(Rose 1965c:37)

At the bottom of the arrest slip the officer notes any wounds or injuries; this certifies that these injuries have not been sustained in transit from the arrest site to the city jail, or in custody.

The arrest process continues upon arrival at the station: the man must sober up sufficiently to provide the required information, without which the man cannot be “booked”. This may be a time-consuming part of the process. In such cases, a name is assigned to the man *ad interim* for the practical purposes of the process, covering the intermediate condition or period of limbo before the process of booking can be completed:

“In case he is unable to give his name, as happens frequently, and he has no identifying papers and we are unfamiliar with his real name, we have to send

him in as a John Doe, which means that the name on the arrest slip is left blank and the description on the rear is as complete as possible to aid the jail people in identifying him. And then when the man comes to enough to give the name, the name is put on the arrest slip and he is then booked and fingerprinted at that time.”

(Rose 1965c:37-38)

So arresting “a man with no name”, or more specifically a man with as yet no clearly identifiable name, is a routine and routinised feature of the process of arresting men on the street. The absence of a particular required by the accomplishment of a booking of a man, his name, is seen as a temporary state of affairs within the *in vivo* work of booking.

John Doe has been accommodated within the process, i.e. provision is made for the non-availability of the name, whereby an interim, locally-known alias is used in tandem with a complete description to distinguish between similarly nameless individuals. Namelessness, the temporary condition of being anonymous, is regarded as a standard condition, not a non-standard, special circumstance. As Rose argues,

“Anonymity is not simply a trick played on the police or on the readers of documents such as this: it is played out in the experiences of these men.”

(Rose 1965e:1)<sup>11</sup>

People’s names, and their pseudonyms, are worded objects; therefore they constitute proper topics for ethno-inquiry.

### ***“Spelling Out Loud”: The Inspectable Properties of Names***

In the previous chapter, in the section on the visibility of confidentiality, introductory sequences from the Larimer Street studies were extracted to show that the disguising of identities was visually available. In interviews for the Larimer Street studies, interviewees were asked to say their names for the tape, and names were sometimes repeated by the interviewer for clarification. As an example, in one of the “Larimer Trilogy” interviews, Rose asks William Shaw “if you would state your name and spell it so that the secretary can get it.” (Rose 1965e:4). Some names were excised, hence not all names were spelled out. This is shown in extracts from interviews with various people associated with Larimer Street, *viz.* pensioners living in one of the boarding houses:

- GORMAN: My name is Anthony Gorman. The date is July 23, 1965. How old are you, Mr. A?  
A: Seventy-three.  
GORMAN: I wonder if you would mind telling me something about where you were born, and so on?  
(Gorman 1965a:3)
- GORMAN: Let's see. What is your full name again, Anna?  
MISS B: Anna B.  
GORMAN: And my name is Anthony Gorman.  
(Gorman 1965a:15)
- GORMAN: Let's see. The date is July twenty-third, 1965. My name is Anthony Gorman and what is your full name?  
MRS. P: Rose Marie P. I go by – sign all my checks and everything Rose M. I'm registered in the precinct that way.  
G: Where were you born, Rose?  
(Gorman 1965a:42)

Names (and their attendant pseudonyms) are highlighted when they are spelled out by parties to talk. This is an “observable-reportable”, i.e. an “accountable” phenomenon, in other reports from the Larimer Street studies by Edward Rose.

Rose went out among the men on Denver’s Skid Row in order to secure their reasons for being on the Street and their reactions to a proposed dispersal of the Skid Row district, or Skyline, by an urban renewal project.<sup>12</sup> Rose conducted extensive interviews too with such people that had critical opinions on Denver’s Skid Row, including William Shaw, Director of Rehabilitation Services at Colorado State Hospital;<sup>13</sup> Harold Schalbrack, Officer of the Denver Police Department;<sup>14</sup> and John O’Leary, a resident of Denver’s Skid Row.<sup>15</sup>

As mentioned in the previous chapter, the notice of confidentiality appears in the Larimer Trilogy, specifically “A Quiet Strip.” In reporting the interview with the police officer, Harold L. Shalbrack, Rose notes that

“the proper names of persons and places have been changed, with the exception of the names of the police who ... should not be anonymous in view of the favorable reports on them that are collected here”.

(Rose 1965c:3)

Did these officers agree with this procedure? Rose informs Schalbrack of his opinion (Rose 1965c:4); but was permission granted?<sup>16</sup> The Larimer Street studies raise complex/interesting issues *re* confidentiality. This is a distraction from the issues, however: membership categories are “duplicatively organised” (Sacks 1974a:220-221),

i.e. they come in pairs. Although the identities of men on the street have been falsified, the duplicative organisation of membership categories, i.e. where there is a police officer, there is a suspect (Sacks 1972a), the unequivocal identification of an incumbent (*viz.* Schalbrick) of the membership category “police officer” makes feasible the deduction of the protected identities. Further, that membership categories are duplicatively organised means that the unequivocal identification of incumbents of a category, in this case “police officer”, is seen as “accrediting” the work of those members, whereas the anonymisation of members of the co-categorised collection, in this case “man on the street”, is seen as “discrediting” its incumbents. Anonymity can be inferred to protect the identities of persons who would otherwise be discredited by positive identification.

We encounter interviewees’ names at the beginning of the interviews:

- ROSE: Would you give us your name first, please, for the tape? And please spell it for the secretary.
  - SCHALBRACK: Alright. My name is Harold L. Shalbrick, S C H A L B R A C K.
- (Rose 1965c:4);

and

- ROSE: Well, first of all, let’s get some of the mechanical information down.  
What’s your name?
  - O’LEARY: John Collins O’Leary.
  - ROSE: How do you spell O’Leary?
  - O’LEARY: O apostrophe capital L E A R Y.
  - ROSE: O.K.
- (Rose 1965d:3)

This phenomenon is visible in video-recorded interactions also. Names are exhibited in the parenting-counselling talk, and are publicly spelled out. In Extract 11(1) below, CD3's forename name is remarkable, i.e. a remark is made upon it, and he is required to spell it out. The spelling of a name, making a name inspectable, highlights its unique particularities, embeddedness and salience, and exacerbates the difficulties facing a transcriber wishing to preserve the phenomenological intactness of the setting.

### Extract 11 (1)

- 1 CR1 right how about you  
2 CD3 I've got a kid  
3 CR2 me name's James Morley  
4 CR2 James another James  
5 what's your surname  
6 CD3 Morley  
7 CR2 sorry you'll have to spell it for me  
8 CD3 M-o-r-l-e-y  
9 CR2 l-e-y  
10 yep( )  
11 CD3 yep( )  
12 ((pause))  
13 CR2 right can you tell us about yourself  
14 CD3 jus( ) one kiddy  
15 e's a boy  
16 one  
17 CR2 he's one  
18 CD3 one last birthday  
19 CR2 yeh  
20 CD3 ( ) avvunt seen 'im for months

Here, CD3 is asked to provide certain information (line 1). That CR1's utterance "*right how about you*" can be seen as asking for information, including his name, is a member's observation based upon how it is interpreted by CD3 himself and treated in

next action. The utterance “*right how about you*” indicates that others within the group have previously been asked for information, and “*right how about you*” seeks the same sort of information from CD3. This observation is confirmed by CR2’s utterance following the provision of his name: “*James another James*” (line 4), which tells us that CD3 is (at least) the second James in the group.

So this sequence is a “*serialisable*” (Sacks 1992a:288) item: CR2, in her asking for this information from at least a first participant, provided for the making of a series of items, whereby having been given a first item,

“making ‘a series’ can then be done by doing as a second, something which is not independently seeable as such a thing, but is so seeable given the first.”  
 (Sacks 1992:288)<sup>17</sup>

CR2’s utterance overlaps CD3’s surname, which she subsequently asks him for (line 5). Following his repeat of his surname (line 6), CR2 asks him to spell it for her (line 7). CR2 publicises her successful completion of the task in hand in her utterances “*l-e-y yep*” (lines 9-10); CD3 confirms her spelling through repetition (line 11). The inspectable nature of the actual name complicated the preservation of the interaction in its finest details. The transcriber retained the “*e-y*” feature of the surname so that the pseudonym, and its itemisation or spelling, kept this characteristic consonance.

In Extract 66(2)<sup>18</sup> below, the transcriber was concerned to preserve the “double-t-ness” of the offender’s name (lines 5-6; 16-20):

Extract 66 (2)

1 CR1 just to introduce you to The Organisation  
 2 I'm Catherine ( )  
 3 CD2 I'm er Tim ( ) miss  
 4 CR1 you're  
 5 CD2 Tim Suttle miss  
 6 CR1 Tim Suttle  
 7 right not miss  
 8 Catherine  
 9 CD2 ( )  
 10 CR1 okay we're all on first name terms here  
 11 CD2 ((laughs))  
 12 CR1 um (hi)  
 13 CD3 ( )  
 14 CR1 Robert  
 15 right erm ( )  
 16 CR2 Tim what was your surname sorry  
 17 CD2 Suttle miss  
 18 CR2 Suttle  
 19 S-u- double-t  
 20 CD2 l-e  
 21 CR yep  
 22 CR2 and are you ( )  
 23 CD2 ( )  
 24 CR2 and Robert what's your surname  
 25 CD3 Spencer  
 26 CR2 Spence  
 27 CD3 Spencer  
 28 S-p-e-n-c-e-r  
 29 CR2 S-p-e-n  
 30 CD3 c-e-r

At line 24, CR2 turns her attention to CD3 in order to secure his name for the register.

Melvin Pollner defines “explicative transactions” as occasions

“in which what does next will be seen as defining the import or significance of what another did before”

(Pollner 1979:228)

The introductory sequence between CR1 and CD2, at lines 1-11, acts as an “explicative transaction” in that it instructs CD3 what is expected of him when CR2 initiates their speech exchange (line 12). By this time, i.e. from lines 1-11, CD3 has already learned the counsellors’ preferred address terms (lines 3-10); that is, they are “*all on first name terms*” (line 10). He is tacitly instructed too that CR2 is taking the register (lines 16-20), and in the accomplishment of the taking of the register he will be required to provide necessary particulars; furthermore, that he is required to do so is not singling him out among the group but is usually required of members of the group. The explicative nature of the exchange (at lines 16-20) demonstrates to CD3

“that a question routinely asked of him would be a request for his name, and that he should be prepared to spell out his name for the benefit of CR2, which he unproblematically accomplishes to correct CR2’s mishearing (lines 24-30).”

(Carlin 1994:77)

Although the common sense interpretative procedures and linguistic practices used by the participants allows them to achieve an orderly, ordinary sequence, the transcriber encounters difficulties with the sensible rendering of the sequence. At line 2, CR1 introduces herself using forename and surname; her surname appears in empty parentheses because—conveniently for the transcriber concerned to protect identities, perhaps—it was inaudible and so unavailable for transcription. CD2’s surname is similarly inaudible at line 3. Nevertheless, the forenames in lines 2 and 3 are pseudonyms: to expropriate the Yiddish term *Tsoris*<sup>19</sup> (meaning a source of underlying, constant trouble, which Garfinkel used to describe indexical expressions and sociologists’ attempts to “remedy” the indexical properties of natural language); and

applying it to the extraction of real names from sequences of talk and replacing them with *simulacra*, pseudonyms will in each and every case constitute that proverbial square peg that can not be forced to fit.

CD2's surname, inaudible at line 3, is produced again at line 5; the observation that his name is "produced again" is a common-sense one, noting that there had been a word in the utterance at line 3, following his forename "*Tim*", yet prior to the appellation or address term "*miss*". This inaudible word is warrantably interpreted to be CD2's surname. That the transcriber was unable to retrieve the surname is featured by CR1 who, at line 4, produces an utterance that requires CD2 to repeat his name for her. This suggests that CR1 had not heard CD2 properly either.

Through her repetition (line 6) she clarifies the name for the transcriber. However, this means that the actual name is repeated in two successive utterances, forcing the transcriber to assign a pseudonym that "resembles" the actual name in its particulars, i.e. in its embedding within a sequence that defines the parameters for the pseudonym.

Following the greeting sequence initiated by CR1 ("*um hi*", line 12), CD3 responds by providing his name (line 13). Although the name is not audibly produced, the transcriber knows that the name produced at line 13 is "Robert" because CR1 repeats the name in the adjacent utterance (line 14).

At line 16, CR2 displays that she had not heard CR2's name adequately, i.e. adequately for the bureaucratic or "mechanical" (Rose 1965d:3, *op cit.*) purpose of register-taking, which is shown to necessitate not just a name but a correctly-spelled name. This is achieved by the vocalisation, letter by letter, of CD2's surname (line 19). CD2 confirms CR2's spelling by uttering the final two letters in his name (line 20); in effect, completing the spelling commonsensically explicates to, and in turn is interpreted by CR2 to confirm that the letters used in her spelling, and their sequential ordering, were correct.

The pseudonyms "Tim" and "Suttle" were chosen to preserve the shortened, diminutable form for the forename; and, as mentioned above, to preserve the "double-t" spelling within the surname. However, the pseudonym "Suttle" has unintended features that were not adducible with the actual name: that CD2 completes the spelling "l-e" might warrantably suggest that CD2's name was sometimes misspelled "e-l". Further, the pseudonymised name "Suttle" is coincidentally homonymous with the word "subtle", providing greater scope for misspelling than the actual name. If these speculative observations appear somewhat wire-drawn, it should be remembered first that these are made upon false names erroneously embedded within actual talk, rather than actual names within actual talk (what may be called the "nominal intactness" of the setting); and second, that these pseudonyms, carefully selected to minimise the violence exacted upon the setting, provide for inferences which are unavailable from the actual names. This feeds back to the previous discussion of Karve (1947): her survey of Hindu names

in India shows what can be inferred from names, whereas the considerations here show what can possibly be inferred from pseudonyms.

The introductory format between CR1 and CD2, and the negotiation work involved in the clarification of CD2's name with CR2, is repeated with CD3. As CD2 before him (line 16), CD3 is asked for his surname (line 24). In this transcription, however, the transcriber was unable to render the intricacies of the interaction whilst attending to the overriding concern for the protection of identities. Various permutations and candidate pseudonyms were inserted. The pseudonym "Spencer" does not provide for a credible mishearing as "Spence", as the actual name did. The original interaction is preserved, whereby CD3 corrects the mishearing (line 27), spells the name letter by letter (line 28), CR2 proceeds to repeat the spelling (line 29), which is confirmed by CD3's completion (line 30). However, pseudonymisation obscures the fine, acoustic resemblance between the originally produced surname (line 25), its audible repetition by CR2 in her accomplishment of registering (line 26) and its correction (line 27).

Through this examination of the inspectable properties of names we are able to see an "elective affinity" between Rose's school of thought, the Ethno-Inquiries, and the ethnomethodologically-derived field Conversation Analysis.<sup>20</sup> These cognate approaches are concerned to furnish readers with the actual materials which were used in their analyses. Although ostensibly Rose and Carlin (1994) are interested in different things—whereas Carlin's transcripts are oriented to the realisation of people's turn-taking procedures in conversation, Rose's transcripts are committed to securing the

mundaneities of everyday scenes as oriented to by people themselves—the extracts of talk from Rose and Carlin constitute “retrievable data” (Mehan 1978), which are elaborated upon in the next chapter. “Retrievable data” are in turn constituted by audio and video tape-recordings, and transcripts thereof. Retrievable data are available for rigorous review, where the recoverable source preserves the talk as produced by members. However, this proviso is not accommodated by

“[He] said: ‘Spell you(r) name’. [I said] ‘N-I-C-K-Y’”  
 (Ely *et al.* 1996:13; brackets supplied)

The selective presentation of this utterance, which has been isolated from its achieved embeddedness in a sequence of utterances, i.e. the “adjacently paired” utterances (Schegloff and Sacks 1973:238), is a product of the *a priori* linguistic coding of speech events. Only after it has been “Plucked from its native ground” (Bittner 1965b:247), in this case its ineluctable context, can this utterance be deployed to support psychologistic determinations of self, as Ely *et al.* (1996) attempt in their ironicised account of adult-child conversations.

There remain further puzzles about the work of transcription and the pseudonymisation of individuals. The inlying item “[He]” is, *prima facie*, an anonymisation of a member, pronominally characterised as male. (The inspectable “‘N-I-C-K-Y’” is not, from the available data, unequivocally categorisable as male or female; this research is indifferent to the precise category from the Membership Categorisation Device “sex”, and is instead concerned with the transformation of the actual name to the gender

neutral diminution.) Furthermore, how did Ely *et al.* achieve the transcription “*you(r)*” and “[*I said*]”? The rendering of members’ talk in this form is necessarily ironic, standing in competition with how members define situations, and how members arrive at categorial determinations of self.

### ***Pseudonyms and Membership Categorisation Activities***

As we have seen, names are embedded within talk. Names are one locus of categorisation work as done by members; in disattending the importance of names (and therefore pseudonyms) to how people define situations, i.e. by seeing names as being unproblematic, analysts leave unexplicated an arena of salient sociological phenomena which analysts trade upon in their assigning of pseudonyms.

This is shown in Whyte’s study of a street corner Italian slum. Whyte’s imposition of recognisably Italian appellations as pseudonyms is notable:

“Besides Mike’s crowd and Nutsy’s boys, there were three other men who went to make up the Nortons as I knew them. Angelo Cucci, Fred Mackey (Macaluso), and Lou Danaro were all closely attached to Doc.”  
 (Whyte 1955:11)

Whyte tacitly chooses pseudonyms which are ‘recognisably Italian’ in character, “*Angelo Cucci, Fred Mackey (Macaluso), and Lou Danaro*”, thereby displaying the common-sense use of membership categorisation. Whyte’s choice of a diminutable and ‘recognisably Italian’ surname—*Macaluso*, diminished to *Mackey*—is exemplary; exemplary, that is, in the common-sense work needed to use a ‘recognisably Italian-sounding’ surname that was diminutable to a ‘non-Italian sounding’ surname. This is

also exhibited in the assigning of a pseudonymised nickname to a characterisably Italian surname: "Joe Marco, known as Joe Dodge, was a good friend of both men." (Whyte 1955:10).

Rather than relying on tacit, cultural knowledge, D.R. Watson explicates his preferences for assigning pseudonyms as products of his own membership practices. Thus, D.R. Watson differs from Whyte through his recognition and deliberate use of Membership Categorisation Devices in the realisation of adequate pseudonyms:

"Where relevant, I have [...] substituted names that are commonsensically conceived as 'characteristic' of a given ethnic or national origin with names perceivedly from a similar origin."

(D.R. Watson 1981:105)

The procedure for name selection is topicalised, as D.R. Watson highlights the problematic nature of extracting an inlying, actual name and replacing it with another. Where a person's origin is categorially available or inferred, i.e. *where relevant*, D.R. Watson capitalises upon his resources as a competent member of society in order to preserve the integrity of members' accomplishments.

Consider Whyte's use of Membership Categorisation Devices (Sacks 1974a), in the following case of the unit category 'family', to protect identities: was Whyte using categorial alternatives or opposites to protect identities? Consider that, for example,

“Alec had gone to school with a younger brother of Joe Dodge, and he first took to hanging on Norton Street in order to be with Joe”  
 (Whyte 1955:10)

might originally have been “Alan had gone to school with an older sister of Jon...”?

This alternative is propounded by the categories set up and used by Whyte: “Alec” is a member of the category ‘male’, shown by Whyte’s pronominal reference “*and he first took to hanging on Norton Street*”. The pseudonymised pseudonym “Alan” is suggested because “Alec” and “Alan”, using the membership categorisation device “sex”, are commonsensically co-selected incumbents of the category ‘male’. Furthermore, as Jefferson (1974) notes, Alec and Alan contain an acoustic resemblance on the first syllable, “Al”; they are also both diminutable to Al.

“Brother” is an incumbent of the categorisation device “family”. “Father”, “mother”, “sister”, “grandparent”, “uncle”, “aunt”, “cousin”, “nephew” and “niece” are categorial alternatives, i.e. incumbents from the same category, “family”, which provide for co-selection as a device to change an identifying detail. “Sister” is both a categorial alternative, i.e. “brother” and “sister” are members of the same membership categorisation device; and a categorial opposite, i.e. an incumbent of the category “female”. That is, the category “female” goes together with the category “male” to form the membership categorisation device “sex” (Sacks 1974a:219); its co-selection for purposes of disguising an identity is based upon its co-incumbency of the membership categorisation device “family” and not from the membership categorisation

device “sex” which, to members, consists of two mutually exclusive categories. This is because the membership categorisation device “sex” is duplicatively organised, *viz.* male and female; and, as Garfinkel’s study of Agnes shows, to be seen as female is perforce to be seen as “not-male”.

The *Gestalt* nature of the categories in the membership categorisation device “sex” was oriented to by Agnes, in Garfinkel’s (1967) study of an intersexed person. If Agnes managed to produce the “standing transparency of her sexuality” (Garfinkel, in Garfinkel and Wiley 1980:6), i.e. presented herself successfully as for all intents and purposes female, by emphasising her “female-ness” and downplaying any “male-ness”, then persons using the documentary method of interpretation would see her as “female”, or at least “not-male”.

In the light of Agnes’ common-sense consideration, that if her sexual status was interpreted to be not-male then she was seen to be its polar opposite, female, we can assume its categorial relevance to the changing of identities. It is possible to feminise a male informant (or masculinise a female informant) by assigning a feminised pseudonym to the male informant (or *vice versa*), thereby trading upon the taken for granted dichotomy between male and female as categorially opposite collections within the MCD “sex”. Marcel Proust used this method to disguise the actual identities of some of his characters: characters were not just composites of real people, but were sometimes actively assigned different genders by the feminisation (or masculinisation) of their names; Albertine was modelled on a male with the name Albert.<sup>21</sup> As an

illustration, if one of our informants was a male, George, we see that “George” is also an acceptable diminution of “Georgina”; i.e. we can feminise “George” so that rather than changing the name, we are changing George’s sexual status; or “Chris” as a diminution of Christopher and Christine, Christina or an alternatively spelled Krystyna.

The quote from Whyte above also determines the origin of Whyte’s pseudonym for the cohort of men in the Italian slum the “Norton gang” or “the Nortons”. “Norton Street” is a location-specific reference, and the “Norton gang” is a gloss for a local cohort of residents within the vicinity of Norton Street, and those who chiefly associate with these residents, to distinguish their membership of a gang known as the “Norton Street gang” apart from any other gangs within Cornerville.

Nutsy was a key member of the Norton gang, and one of Doc’s closest friends. The “-sy” suffix of “Nutsy”, following Jefferson’s (1974) observations on the use of the pseudonym “Jean” and “Jeanie”, can be seen as an intimate, diminutable form of “Nut”. “Nut” is an incumbent member of the family “foodstuffs”; if “Nutsy” is the chosen pseudonym, taken from the same family of terms as the original name, then Nutsy might have been known as “Berry”, or “Fruity”.<sup>22</sup> Although this interpolation of “actual name” from “assigned pseudonym” apparently constitutes a *reductio ad absurdam*, stretching the credulity of the reader using a hypotheticalised example to an extreme, it demonstrates how co-categorisable objects can suggest suitable pseudonyms. It is possible that Whyte saw “Nutsy” as an incumbent of the same family of terms as “Psycho”.

In *Family and Kinship in East London*, we see the following names: Mrs. Gould, Mrs. Merton, the Sykes family, Mrs. Warner. Young and Willmott (1962) disguise identities of families by using the names of famous sociologists as pseudonyms for the people who appear in their study.<sup>23</sup> Once this has been appreciated, these names are seen as names of sociologists. Hence, in terms of the Documentary Method of Interpretation, pseudonyms are treated under the underlying pattern “names of sociologists”.

In considering that Young and Willmott substituted people's real names with the names of sociologists, in order to protect the identities of their informants, could they have used the names of any sociologist for this purpose? What if Young and Willmott had used the following sociologists' names: Mr. Peräkylä; Mr. Sjoberg; Miss Sorokin; the Zhao family; Mrs. Znaniecki? Is there any difference? This thesis suggests that very different ethnic identities are inferentially available from these names. The reader can infer that community-members possess distinct ethnic backgrounds. For the researcher, the unwarranted imputation of ethnic identities is a distortion in the description of the setting. The close-knit community in Bethnal Green, which migrates to a new estate in Essex, is provided with a different categorial profile, a different “ethnic mix”, via the selection of ethnically recognisable names.

So from these observations we can point to an extant method of doing pseudonymisation. The method involves the use of membership categorisation activities. Researchers disguise identities by realising culturally-available categories and then deploying these categories. The common-sense use of membership categories

in pseudonymisation works as follows: i.) the researcher defines a person as a category incumbent, based on gender or ethnicity; ii.) the researcher excises the name; iii.) the researcher then selects a substitute name from the same, identifiable-recognisable membership category. So in this way the pseudonym is of the same “type”.

In the following vignette, Louis Wirth selects ethnically or recognisably-Jewish surnames, i.e. identifiable-recognisable as Jewish in order that readers will interpret the people so named as being ethnically Jewish:

“When they are all set for business, around ten thirty in the morning, Mr. Cohen, who sells pop, says to Mr. Goldberg, who sells roasted chestnuts and flower seeds, “I’ll bet you a dollar it’s going to rain.” Mr. Goldberg says it won’t, and the bet is on. They go to Charlie, and what he says goes. And as the dull morning business goes on, there is a voice yelling every once in a while, ‘Charley, is it going to rain today?’”

(Wirth 1928:236)

The names selected as pseudonyms by Wirth are names which he recognises to be ethnically Jewish names, and which he relies on readers to recognise as ethnically Jewish names also.

Sacks elucidates the selection of ethnically or recognisably-Jewish names as a method of doing anti-Semitism; this is a subtle method for doing anti-Semitism because it can be practised whilst claiming that anti-Semitism is not being done. The case he refers to occurred in the former Soviet Union, in which perpetrators of economic crime were identified as being Jewish—rather than Russian—*via* the publication of a list of names. The publication of names associated ethnically or recognisably-Jewish names with

economic crimes, and so setting up “category-bound activities”, i.e. establishing an association between category and activity, where members who engage in an activity are so categorised. It has already been noted, in Chapter One, that Sacks demonstrated that crying was a category-bound activity (Sacks 1974a); in this case, the activity was not crying but economic crime. By setting up economic crime as a category-bound activity, it could be seen that economic crime “was something that Jews do” (Sacks 1992a:338).<sup>24</sup> So the presentation of economic crime as a category-bound activity, or an underlying pattern, elaborates and is elaborated by perpetrators of economic crime who are recognised to be incumbents of a category, *viz.* Jews, rather than other categories, *viz.* Russians. Hence the selection and publication of names in a newspaper, which “turned out to be seeable as belonging to Jews” (Sacks 1992a:42). Sacks went on to comment that following the publication of names, which were perceived to be owned by members of the ethnic category Jew, the anti-Semitic underlying pattern would be reinforced:

“you could leave the rest to everybody’s routine procedures: ‘See? Jews are economic criminals, as everybody knew.’”

(Sacks 1992a:42)

Sacks notes that selecting names as being representatives of, or “standing for” a category, is not a random exercise. That is,

“while the right names can work to locate all sorts of things ... names are not always so usable.”

(Sacks 1992a:582)

So whilst in the quotation from *The Ghetto* reprinted above, Wirth pseudonymises names, Wirth selects names which are seen as ethnically Jewish names. Further, Sacks' considerations above inform how such pseudonymisation procedures get done. This is not to suggest that practices of pseudonymisation are inherently racist (or, in the particular case above, anti-Semitic). Researchers trade and rely upon readers' routine practices to assemble the sense of pseudonyms in the way the researcher intends.

The commonsensical ascription of pseudonyms using membership categorisation activities is also evident in the Liebow's ethnography of a Black slum neighbourhood in Washington DC. Liebow provides readers with an alphabetical table of pseudonyms (plus the ages and usual occupations of these pseudonymised men in the carry-out neighbourhood) to be encountered within the study:

“Arthur, Boley, Budder, Clarence, Earl, John, Lonny, Preston, Robert, Stanton, Stoopy, Sweets, Tonk, Wee Tom, Wesley, William.”

(Liebow 1967:28)

The most visible individuals in the study are “Tally (Jackson)”, “Sea Cat”, “Richard” and “Leroy (Allen Brown)<sup>25</sup>. Other pseudonymised individuals are categorially identified, e.g. “Bumdoode, the numbers man” (Liebow 1967:32); “Siserene”, Robert’s wife (Liebow 1967:111); and Thelma, “Richard’s twenty-two-year-old half sister (who was visiting from New York)” (Liebow 1967:140).

Sexual partnerships (permanent or otherwise) are categorially inferred and categorially available through Liebow’s tacit use of serial organisation, Membership Categorisation

Devices and the unexplicated acceptance of “category bound activities” (Sacks 1974a).

Following Sacks’ observations on the sequential ordering of a joke’s telling (Sacks 1978), “Bernice” is to be recognised as Stanton’s partner: the pick-up truck that transports men on the street to work is waived on by Tonk,

“who cannot bring himself to take a job away from the corner, because, according to other men, he suspects his wife will be unfaithful given the opportunity. Then there is Stanton, who has not reported to work for days now, not since Bernice disappeared.”

(Liebow 1967:33)

We learn that Tonk’s reason for staying on the street is his relationship with a woman. Liebow’s serial ordering, i.e. Tonk’s reason for absenteeism, as a first item, constitutes a “*serialisable*” (Sacks *op. cit.*), thereby Stanton’s reason for absenteeism becomes the second item in a series of similar reasons, and the membership categories contained within, e.g. wife, Bernice as a recognisably female name, instruct readers that Bernice and Stanton are (or rather, were) a couple.

Seemingly “equivocal” relationships between men and women could be clarified for others, and freed from the suspicions and inferences of sexual activities, through the mutual adoption of “pseudo-kinship structures” (Liebow 1967:166-174); e.g. male and female friends could decide to “go for cousins” or “go for brother/sister”.<sup>26</sup> Whether the exact kinship relationship was known was irrelevant: that a couple had opted to “go for cousins” or “go for brother/sister” was enough to establish to those outside the relationship that the relationship was not of a sexual nature. Sexual activities were, therefore, categorially available in that if persons were known or seen to be kinship

related, i.e. persons who were members of the collection “family”, then the activities within that relationship were inferred not to be of a sexual nature. That “Emma Lou” becomes one of Tally’s sexual partners is categorially available from their moving into the same room (Liebow 1967:192); if a man and a women were not genuinely related, or were not ostensibly related, i.e. not going for cousins or going for brother/sister, the available inference was that they were engaging in sexual activities with each other.

The nature of sexual activities within sexual relationships was normatively sanctioned and category bound, with certain activities being proscribed. “Good” or “real” men did not indulge in specific sexual practices; to engage in or request certain activites was subject to general opprobrium, regarded as justifiable grounds for the termination of a relationship, and may become characterised as a “pervert”. Thus, a man could move from the membership category “good man” to the category “pervert” if his engagement in or request for these activities became public knowledge on the street.

Equivocality in membership categories was also noted by Anderson (1923). He noted that the common-sense use of membership categorisation activities had purchase on the imputation of homosexuality: if a man was seen in the company of a boy, the membership category “homosexual” became inferentially available. Anderson remarks that

“Often boys will refrain from travelling with adults, even well-behaved adults, because they realise that they will be under suspicion.”

(Anderson 1923:145)

Indeed, this may be seen as a case of “guilt by association”. It is not that homosexual activity is presumed if an adult *known* to be homosexual is in the company of a boy, but if *any* man and boy were seen together, homosexual activity could be inferred.

Membership categorisation, and the provision of category-bound activities, are commonsensically relied upon in the aforementioned studies by Hooks, and MacFarlane *et al.* Hooks (1991) presents interviews with Guatemalan women; like Whyte (1955) and Liebow (1967), Hooks (1991 *passim*) assigns pseudonyms of a ‘recognisable’ ethnic, i.e. Spanish-type: *viz.* Margarita, Amparo, Doña Irma, Yvette, Yolanda, Lydia, Maria de los Angeles, Vitalina, María, Victoria, Margarita, Gloria, Margarita, Juana, Elena, Claudia, Ester, Dolores, Carmen, Elizabeth, Clemencia, Catalina, Marta, Irma, Manuela. Note Hooks’ triplication of the pseudonym ‘Margarita’; it is, of course, commonsensically acceptable and expectable that Hooks encountered women of the same name, hence assigned the same pseudonym to several interviewees. The repetition of names, *viz.* the duplication of the name ‘John’, is found in a set of first-person accounts on drugs and drug-use (MacFarlane *et al.* 1991).

In their collection of interviews with people associated with (or disassociated from) drug use, MacFarlane *et al.* manage the linkage between categories and activities routinely associated with those categories: “*Robert was busted for possession of cannabis*”, “*Guy drinks and uses cannabis socially*”, “*Ben regularly takes Ecstasy*”, “*Ian is into the hard stuff*”, “*John will try anything*”, “*Sara is a crack addict*”. These

categories are used as titles for sections, or accounts within the text. This thesis returns to these pseudonymised titles, and their categorial organisation, in the “Making connections” section in the later chapter on bibliographies.

### “Functionalised” Pseudonyms

Another method used by researchers to anonymise and pseudonymise members is to conjoin a name and a function.<sup>27</sup> This idea is similar to Welsh naming conventions.<sup>28</sup> The conjoining of a pseudonym with a function shall be referred to as a “functionalised” pseudonym.

In his autobiography, Jeffrey Bernard anonymises people according to characteristics and occupations: “Coffee Ann”, “Johnny Handbag”, “Sid the Swimmer”. As these functionalised pseudonyms appear in an autobiography, there is a likelihood that some people will know who these persons really were. However it will only be a small circle: using functionalised pseudonyms is limiting recognisability to whom those Goffman called “the wise”. Those who remain oblivious to potential identities are, in Goffman’s terms, “the own” (1968:31-45).

The method for pseudonymisation follows such a procedure as follows: i.) select a function performed by a person; ii.) re-name the person after the function. Louis Wirth employs functionalised pseudonyms, e.g. “Charlie the Policeman” (1928:236). Prime examples of this are the ethnographies of low-life in hotel lobbies, and confidence tricksters, by Robert Prus. Similar to Edwin Sutherland, who co-authored *The*

*Professional Thief* (1937) with a thief, Prus co-authors these ethnographies with a card shark. Prus pseudonymises the card shark by naming him after his occupation. So if you search for these ethnographies (on the shelf, or in the library catalogue) you look for R. C. Prus and C. R. D. Sharper (1977). Throughout this thesis, reference is made to Rose's "Larimer Street" studies. One of the men on the street is sometimes identified as "Johnny the Reader", who reads books in a booth in the Gold Mine Bar (Rose 1965c:1). That is, a reference term derived from naming him after the activity he is frequently found doing.

An interesting case of functionalised pseudonyms is found in a study of scientists and their work. Using bold typeface, Goodwin (1995) emphasises how the pseudonyms reflect different scientific disciplines—"Phyllis", the Physical Geographer; "George", the Geochemist (1995:251)—and different tasks on board the research vessel, e.g. "Warren the Winch Operator" (1995:254).

### **The Pseudonymising 'Machine'**

We are realising that the protection of identities is an accomplishment by members using their common sense interpretive methods. As names are worded entities, so too are pseudonyms; and the attribution of a pseudonym to replace an actual name is a worded accomplishment. The work of pseudonymisation involves the concerted use of membership practices, and is thus constituted by cultural machinery. This is not a mechanistic or analytically-defined term; rather than being of equipmental origin, the "machine" is a metaphor, indicating its tacit and unproblematic use by everyone in the

course of their activities. A cultural machine is a worded machine, i.e. members' natural language-based, natural language-using, linguistically constituted courses of action in making out in the world.

The "machine" is a gloss for the methodised and systematised cultural procedures used unrelievedly by members in the transaction of everyday activities. That assemblage of common sense methods is, then, a member's machine. The machine is the "how" an activity is accomplished by members, i.e. it is a doing machine. The machine is the common-sense understanding of events, i.e. it is an understanding machine. *So the machine is the topic for inquiry.* The machine is the coordinated, concerted, intersubjective work of members doing understanding together.

The cultural machinery refers to that set of membership activities or common-sense methods which are used by persons in their everyday lives. The pseudonymising "machine" is the common-sense cultural machinery used by members in assigning pseudonyms to people in the world. Further, the pseudonymising "machine" provides for the realisation of adequate and sustainable pseudonyms.

### ***Conclusion***

So far, this thesis has presented a cumulative treatment of confidentiality. This thesis has engaged itself with showing how confidentiality "gets done" by people. An earlier chapter on the linguistic nature of fieldwork and fieldwork activities discussed how

confidentiality is practised by people in the world. The subsequent chapter examined how confidentiality is practised by sociologists.

This chapter has shown that an examination of the use of pseudonyms in social inquiry is a study of names in social interaction. People's names are commonplace yet receive little attention in sociology.

This chapter shows how people themselves assume names, e.g. nicknames, stage names, pseudonyms; and are in turn assigned names by others, e.g. nicknames, pseudonyms to protect identities, or "conveniences" until a person's real name is established. The example used from the Larimer Street studies was "John Doe"—the man on Skid Row too inebriated to tell police his name. (John Doe and Jane Doe are also the names assigned to thus-far-unidentified bodies in the mortuary.)

When sociologists get near to their "subject matter", watching and listening to real people, they encounter those occasions where a person's name is spelled aloud. This exacerbates the problematic task of replacing a person's name with a pseudonym.

The considerations thus far have argued that confidentiality is a research method in itself, one that receives little to no attention in methods courses or sociological texts. Further, for all the extensive literature and coverage in undergraduate courses, ethnographic methods remain collections of ill-defined, unexplicated courses of action.

Confidentiality, like other ethnographic research techniques, is a series of rule-of-thumb procedures. These rule-of-thumb procedures are, *au fond*, linguistically organised.

The following chapters bring further considerations of research practices as sites of practical reasoning. The next chapter looks at the linguistic activities of producing and reading transcripts. Chapter Eight brings together issues of cultural practices, including the use of ordinary language and membership categorisation activities. The considerations therein build on the problems of data discussed to that point, and elucidates further problems of data which are elucidated in subsequent chapters, e.g. the linguistic or culturally methodic organisation of interviews, observation and textual presentation.

<sup>1</sup> Etymologically, “pseudonym” comes from the Greek *pseudo* (false) and *onoma* (name).

<sup>2</sup> The research practices of recording and transcribing are afforded detailed consideration in the following chapter.

<sup>3</sup> Dušan Bjelic, personal communication, 21 May 1996.

<sup>4</sup> Schegloff identifies that “[i]t is by way of the status of items such as uh huh and mmhmm as demonstrations of continued, coordinated hearership that we may appreciate the fact that they are among the few items that can be spoken while another is speaking without being heard as ‘an interruption’” (Schegloff 1972a:405). See Jefferson (1993:350-351) for a note on the *in vivo* work (*viz.* as “passive recipiency tokens” and “reaffirmation” tokens), accomplished by such items in conversational interaction.

<sup>5</sup> From Jefferson (1974:1). Reprinted with permission.

<sup>6</sup> “Prussian” is mooted because it ‘rhymes with’ “Russian”, (and the kingdom of Prussia was happenstantially adjacent to Russia); whereas “Lithuanian” is mooted because the Baltic republics Lithuania and Russia were both member states of the Union of Soviet Socialist Republics. However, the alliterative character of “Larry the Lithuanian” may not have been contained in the original monicker. Alliteration also featured in Whyte’s pseudonymisation of the nickname “King of the Kids,” given by

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Doc and Danny to the already-nicknamed Nutsy (Whyte 1955:10). Effectively, then, Whyte pseudonymised a nickname of a nickname.

<sup>7</sup> Or “Dead-Eye Johnny”, produced by the selection of an incumbent of the common-sense family of terms indicating a person’s unerring accuracy in shooting, combined with a diminutable form of a name which preserves the “j” (as in ‘jam’) and the familiar suffix “-y”, whilst juxtaposing the sequential ordering of the monicker’s components.

<sup>8</sup> It is interesting to note how the combination of particular terms as hyphenated items (e.g. “-shot” and “-eye”) are commonsensically understood as members of that family of terms, yet whilst other combinations appear nonsensical. Consider the commonsense meanings of “One-Shot” which, unbeknownst to his children, had once been Atticus Finch’s nickname in Lee (1960:103), “crack-shot”, “sure-shot”, “dead-shot” and “dead-eye”. These hyphenated terms are seen as incumbents of the family of terms that refers to marksmanship or markswomanship, i.e. “Dead-shot” and “dead-eye” are interpretable as being synonymous with each other. However, “one-shot” and “one-eye” are not seen as synonyms, ergo they are not seen as members of the family of terms used to refer to a person’s shooting ability.

<sup>9</sup> R = Rose; A = Ali Baba. The line numbers have been added for the convenience of the reader in referring to the speakers’ turns.

<sup>10</sup> An alternate account of an arrest, that is, from a man having been arrested, told by John Ralph Kruthshift (Rose 1965b).

<sup>11</sup> From the introduction to a series of diary entries written by men on the street.

<sup>12</sup> Reported in Rose 1965(a), *The Unattached Society*.

<sup>13</sup> Reported in Rose 1965b.

<sup>14</sup> Reported in Rose 1965c.

<sup>15</sup> Reported in Rose 1965d.

<sup>16</sup> Rose informs Schalbrick of this procedure in an introductory bloc of talk. Schalbrick’s first utterance is “That sounds quite reasonable.” (Rose 1965c:4).

<sup>17</sup> Lecture 04.a. (Spring 1966).

<sup>18</sup> This extract is analysed for its explicative properties elsewhere (Carlin 1994:74-75).

<sup>19</sup> “That means a kind of an obstinate, unyielding trouble that does not go away if you use the usual remedies. It is not that it is all that devastating; but on the occasion on which you encounter it, you cannot be indifferent. Perhaps you would like to be indifferent, but this is impossible.” Garfinkel, oral statement in Hill and Crittenden (1968:3).

<sup>20</sup> For a strong formulation, viz. “Conversation Analysis Ethnomethodologically-conceived”, *vide* D.R. Watson (1998 *passim*).

<sup>21</sup> For a sample discussion of composite characters, and the “originals” from whom they are derived, see Painter (1989), Chapter 10, “The Guermantes Way” 147-168 (esp. 150 *et seq.*).

<sup>22</sup> Nutsy could well have been derived from a habit of eating pistachios, or hailed from a family of peanut farmers.

<sup>23</sup> I wish to thank James Valentine (Department of Applied Social Science, University of Stirling) for drawing this to my attention.

<sup>24</sup> Cf. “A *Kwaku* naturally WOULD do this kind of thing” (Jahoda 1954:193, emphasis supplied).

<sup>25</sup> Full names were not common knowledge among the men in the carry-out neighbourhood. Even close friends might be ignorant of each others’ full names. Readers learn the full names of Tally and Leroy through their relationships with women. Leroy signs a love letter to “*Charlene*” using his full name (Liebow 1967:154). He signs a subsequent letter to Charlene as “*Mr. Nobody*” (Liebow 1967:155); did Liebow pseudonymise an already anonymised note?

<sup>26</sup> D.R. Watson (1978) explicates members’ categorial practices this entails.

<sup>27</sup> In Goscinny and Uderzo’s series of *Asterix* books, the Gauls in his village had names which somehow reflected their character and/or occupation they did. For example: the blacksmith was called *Automatix*; the bard with an awful voice was called *Cacophonix*; the druid was called *Getafix*; the quarryman was called *Obelix*; a Briton, partial to beer, was called *Dipsomaniax*.

<sup>28</sup> For example, in the cartoon *Ivor the Engine*, the train-driver is called “Jones the Steam”.

## The Linguistic Organisation of Transcripts

The central theme of this thesis is an examination of the natural language practices routinely employed in the routine work of doing sociological research.

This chapter opens up a domain of inquiry, taken for granted by researchers as an integral feature of their research projects, as part of the “textbook method” (Lynch and Bogen 1997:488-489). Researchers return from “the field” to the office with notes from observations and tapes of people’s talk at the research site. According to the step-wise idealisation of research identified by Lynch and Bogen (*ibid.*; see the Introduction to this thesis), textbook method represents this as a “stage” of the research project: transcribing the tapes before the “analysis stage” can begin.

This chapter disputes this view of transcribing. Among the issues this chapter raises are: how the transcript incorporates unexplicated dimensions which are topics of inquiry, e.g. the natural language practices of the transcriber and the reader; transcripts are not inert but reflect the theoretical and methodological options of the researcher; the idealised presentation of projects, as identified and criticised by Lynch and Bogen, that research projects can be compartmentalised into discrete “stages” such as fieldwork, transcribing and analysis; that the production of a transcript, *via* its suffusion with members’ natural language practices and cultural procedures, can be conceived of as a research method in its own right.

Techniques for gathering data for sociological inquiries are linguistic activities, i.e. they are conducted in and through natural language. Previous and subsequent chapters examine the use of fieldnotes and fieldnotes as “data”. It is important to note that the status of the claims made for these textual records, written *in vivo*, is not the same as those made for the “taped record” of actual, worldly events. Whereas fieldnotes are selective and contingent upon the memory and note-taking practices of the observer, the ‘taped record’ allows for further transcription and re-transcription. Recordings, and transcriptions thereof, constitute “retrievable data”, which is defined as

“videotape or film for both data gathering and data display. Events depicted on videotape and film are not equivalent to school events *per se*, but audiovisual materials do preserve events in close to their original form. They thus serve as an external memory that allows researchers to examine interactions extensively and repeatedly, often frame by frame.”

(Mehan 1978:36)

The activities of transcribing conversation, the continuous shuttling of tape-recordings of sequences of talk, and consideration of other researchers’ findings and observations, all sensitise the transcriber to endogenously produced, naturally occurring phenomena within the talk. Before looking at the occurrences preserved on tapes—the “taped record”—there follows brief considerations of recording and recordings.

### ***Practices of Preserving the World: Recording***

It may be axiomatic to note that before researchers can transcribe interaction in social settings, the social settings are to be recorded. Practices of recording are, then, analytically prior to practices of transcription.

There are a number of related issues *apropos* recording interaction; *inter alia*, theoretical and methodological options (why record? what to record? and how much to record?), the selectivity of recording, the quality and technology of recording. These issues are of a methodological and pragmatic nature, which may be highlighted by Wieder's notion of "methodogenic ontology" (1980:76). The methodogenic ontology of ethnomethodological programmes, such as Conversation Analysis, regards the recording of interaction settings as a way of getting closer to the interaction than e.g. intuitively organised anecdotes or recollections of the setting. As Sacks said,

"The tape-recorded materials constituted a 'good enough' record of what happened. Other things, to be sure, happened, but at least what was on the tape *had happened*"

(Sacks 1984:26; my emphasis)

The methodological policy of Conversation Analysis is to preserve the endogenous organisation of the setting. As settings are collaboratively produced by the parties to the setting, conversation analysts attempt to take all participants in a setting into account. This methodological policy has ramifications for the recording of interaction settings, and therefore the analysis. That is, recording is constitutive of the inquiry. As Goodwin observes in his manual for recording,

“the decisions [*re* who to include in the frame and how wide the frame is] you make here are in and of themselves a central part of your analysis. Moreover they will shape and constrain whatever you are able to do with the tape subsequently.”

(Goodwin 1993:193-194; my brackets)

Recording, then, is part of the analysis. It is also a form of observation (transcription as observation is considered in a later section below); and structures the research: “any camera position constitutes a theory about what is relevant within a scene” (Goodwin 1994:607). Goodwin regards recording human interaction “as an iterative, progressive process” (1993:194). By viewing the recordings he is able to return to the research site in order to capture—somehow—the salient details of interaction which were not captured initially, e.g. computer screens. The outcomes of recording activities are pedagogic, in that he learns from recordings made in previous settings and can anticipate the details which may have future salience, e.g. by focusing on computer screens.

Recording as much as possible at the research site eschews any problems of partiality *via* the selectivity of recordings. Recordings are selective if, e.g. the analyst focuses on one categorial incumbent at the expense of another; or one category at the expense of another category, e.g. teachers at the expense of students, or *vice versa*, in classroom settings.

Ansii Peräkylä (1997:206) argues that the recordings must be of a sufficient quality in order to be “fully” transcribed. Goodwin’s guide to recording interaction (*ibid.*)

provides practical advice on equipment and procedures which do produce recordings of a certain quality. Although Peräkylä discusses the quality of recordings, the research on parenthood-counselling sessions (Carlin 1994) occasioned considerations of the quality of recordings that the researcher not involved in the recording of settings is given. Due to the difficulties encountered in transcribing the poor-quality tape-recordings that were shown to me by The Organisation, I included a section on “Obtaining useful visual data” (Carlin 1994:115-120) for any future recording of parenthood-counselling sessions.

I use the phrase “technologies” of transcription to refer to interrelated factors involved in the activities of transcribing: i.) the equipment used to transcribe the talk; and ii.) how talk is represented on the page. I shall deal with the technologies of transcription later in this chapter, in the section on the practices of transcription.

The technology available at my disposal during the research on parenthood-counselling sessions affected the availability of phenomena and the quality of transcriptions. For example, adequate transcriptions of, and frame-grabs from the heart massage sequences (“*one and two and three and four ...*”) were not an option with the top-loading Ferguson VideoStar model I had originally used for transcribing the parenthood-counselling sessions. Differing transcriptions were eventuated from the same recorded sequence from the tape. For example, compare the original transcription, Extract 3(2) with the subsequent one, Extract 93(2):

Extract 3 (2)

1      CR1    we actually come in and hold a group on family matters parenting  
 2      call it what you like (         ) and erm  
 3      just hold a group discussion group  
 4      on bringing up children and er personal relationships  
 5      so whilst we're on that can we ask you (name)  
 6      have you got any children  
 7      CD2    I've got one  
 8      CR1    got one aha  
 9      CD2    boy  
 10     CR1    boy  
 11     CD2    boy yeh four years old  
 12     CR1    yeh what's his name  
 13     CD2    Rhys  
 14     CR1    Rhys

and the re-transcription of this sequence with more suitable technology for transcription:

Extract 93(2)

1      CR1    so whilst we're on that  
 2      can can we ask for yours  
 3      ha- have you got any children  
 4      CD2    I've got one  
 5      CR1    you've got one [a:ha]  
 6      CD2                                  [yeah]  
 7      CR1    ↑boy ↑girl  
 8      CD2    boy  
 9      CR1    boy  
 10     CD2    four years old  
 11     CR1    yeah (         ) what's his name  
 12     CD2    Rory  
 13     CR1    Rory  
 14     (2.0)  
 15     (         )  
 16     .hhh (2.0)  
 17     how long since you've seen him  
 18     CD2    er:::m (2.0) seven months  
 19     CR1    oh that's quite a long [time]=

20 CD2 [yeah]  
 21 CR1 =cuz they they alter at that age quite a bit don't they=  
 22 CD2 [yeah  
 23 CR1 =[ ( ) three months thereabouts (0.1) now he's four  
 24 CD2 starts school (November) [ so ]  
 25 CR1 [ ( )]  
 26 CR2 [ ( )] as well

The re-transcription, using new technology, captures sound stretches, (approximate) timings (lines 14, 16, 18 and 23) and shifts in intonations (line 7).

When I watched the sessions using a more advanced video cassette recorder, I heard discrete conversations within the multi-party conversational sequences or “schisms” that I had been unable to hear previously.<sup>1</sup> For example, Extract 100(2) below had been completely unavailable for transcription with original equipment:

#### Extract 100(2)

1 CR1 and also (in) that protein (1.0) ( ) eggs  
 2 (0.2) erm (0.2) is (0.2) e:rm  
 3 CD °salmonella°  
 4 CR1 yeah  
 5 CD2 (hh) salmonella  
 6 CR1 ?sorry  
 7 CD2 salmonella  
 8 CD [salmonella  
 9 CR2 sa:lmon  
 10 CR1 salmon oh (hh) ( ) that time  
 11 a- actually that is a subject we we'll be wanting to talk about  
 12 ( ) about erm (.) soft cheese  
 13 cos they're now saying  
 14 we shouldn't be having soft cheeses whilst they're pregnant  
 15 CR2 yeah yeah  
 16 CR1 because of um (.) what's it called  
 17 (3.0)

18 CD2 [[( )]  
 19 CR3 [[listeria]  
 20 (1.0)  
 21 CR1 ?sorry  
 22 CR2 oh listeria  
 23 CR1 listeria that's right  
 24 listeria

The transcript above does not preserve the phenomenological intactness of the setting *in situ*, however, because of the schisms in conversations within the parenthood-counselling group. This extract is a sequence of topic-talk, inaudible for the purposes of transcription with original equipment, and a conversation which participants themselves had difficulty hearing over the general noise. This is only reflected in the transcript—the appreciable increases in volume for interlocutors to hear, and visible difficulties in hearing (lines 5-10, 18-20). So Extract 100(2) is a sequence of ongoing talk between a group of participants within the parenthood-counselling session, but not a transcription of all the talk that was taking place at that moment.

Throughout the thesis I refer to another corpus of data, separate from the parenthood-counselling sessions, which raises issues of recording. I sought to emulate Rose's Larimer Street inquiries and "interviewing techniques" during conversations with people about the Manchester bombing. The methods of approaching and accessing respondents differ from Rose (1986 item #118), a study in which he hired a stall within a shopping mall, a "mark" standing outside encouraging people to enter. Rose went to visit people, within their "familiar territory" or on their "home ground": people came in to talk and to make observations on their own use of the telephone directory. The study

of the Manchester bombing resembles Rose's consultancy with people, though is more closely "related" to his urban ethno-inquiry of homeless men, *The Unattached Society* (Rose Items #35, #150). I sought out the experts, i.e. ordinary people, at the research site; and collected stories also from people to whom they referred.

In a pool or series of "open-ended" interviews, people were afforded the opportunity to talk about the city centre bombing, and were invited to talk about any other matters if they so wished.<sup>2</sup> Discussions were often time-limited, i.e. determined by the exigencies of informants' routines. Such factors which impinged upon the discussions included, for example, the arrival of customers, closing-time, the next appointment and, in one case, a call to attend an emergency 999 call.

The work-site pressures or constraints encountered during the tape-recorded conversations precluded adherence to all the pragmatic and procedural practices, which shall be referred to here as *Rose's rules for talking with people*:

1. Worry about the *place* where you talk: don't bother to record noises: get people *away from their* places.
2. Worry about the *machine*. Make sure *it* is working in an optimum place.
3. *Never* tape secretly. Show the machine to your *friend* (or whoever you are interviewing). Tell him you will erase whatever he wants. Tell him he can have the tape (though of course try to keep it).
4. *Never* tape or even listen to secrets. If the story gets out, you will be blamed. Tell your friend *never* to tell you any secrets.
5. Let your friend decide what he wants to say. Say only: "Tell me about that." Don't worry about *yourself*.
6. Don't talk beyond telling *the tape* who you are, when and where, and get your friend to give *any* name.
7. Stop everything when there is noise.

8. *Wait for your friend to talk.*  
(Rose, emphasis supplied)<sup>3</sup>

After the interview had concluded, the tape or a copy of the tape was offered to informants (in some cases the offer of a duplicate recording was accepted and fulfilled). The taped record shows that workplace noises do appear, it also shows that I *do* talk; the *interview-as-interaction* is visible in extracts from these conversations. (Extracts from some of these conversations appear within this thesis; see Chapters Eight and Nine.)

The nature of the project and its discussions with “local experts” who talked of their involvement and engagements on the day of the bomb, what they were doing and how they were affected—necessarily required visiting people at their workplace. Therefore, some of the tape-recordings also contain extraneous noise. However, during the project I did take seriously *Rose’s rules of secrets in ethnography*:

“You know the first rule of ethnography: never listen to a secret. The second rule: never promise to keep a secret. The third rule: never believe that secrets are kept—especially in the medical profession and in social services and other official bodies: professionals always gossip about their clients. Fourth rule: don’t be taken in by any claim to confidentiality. Of course on your own and on your honor do keep secrets.”<sup>4</sup>

Rose’s inquiries seek to preserve people’s understandings of the social world in which they live, and to make their understandings available. The methodologically non-ironic nature of the “Larimer Street” (and his subsequent) studies involved the research team not in taking notes during conversations with people on the street—hence transforming

people's own understandings into analyst's commentaries on these understandings—but in preserving these understandings as people themselves displayed and formulated them: "Tony had a tape recorder running while Anna talked" (Rose and Watson 1998:3). Tape-recording produces an archive of people's understandings or definitions, which can be referred to as the "taped record".

### ***The "Taped" Record***

The 'taped record' comprises the worldly phenomena that have been preserved in audio or video forms; the taped record, i.e. the "preservable-reportable" document of phenomena, is the analytic prior of the transcribed record:

"For a journal article, the rich record of complicated vocal and visual events moving through time provided by a videotape must be transformed into something that can silently inhabit the printed page."

(Goodwin 1994:607)

The 'taped record' is approachable like the 'fossil record', where the development of life-forms is traced, as preserved in fossilised forms in rock, and this development is approached using the resources of both specialised (*viz.* geological and paleontological) and ordinary corpora of knowledge and procedures. A found fossil may contribute to a greater understanding of the development of life-forms, recognisable as supportive evidence (an "indexical particular") of existing or pre-existing hypotheses (an "underlying pattern") of the development of this life-form. However, this same fossil may be interpreted by one or more members of the paleontology community as a basis for challenging an existing hypothesis, that is the underlying pattern requires revision, or reinterpretation, suggesting another pattern or thesis of the development of this life-

form. The academic community will debate and dispute the importance of the find. “Debating” and “disputing”, whether transacted through face to face interaction, published (paper or electronic) journals or electronic media, are worded, i.e. linguistic activities: they are accomplished through language. The point being that in recognising that scientific work—the work of scientific discovery, e.g. Garfinkel, Lynch and Livingston (1981) and scientific argumentation—are social, linguistic enterprises. The interpretation of a fossilised form and its place within the fossil record is more than a technically-mediated interpretation but an outcome, or rather *outcomes*, of socially (linguistically) organised, ordinary membership activities.

The ‘taped record’ is approachable like the “English record” (Rose 1960). The English record is the history and development of the English language, as preserved in the *Oxford English Dictionary*, which documents the roots and usages of words. The *Oxford English Dictionary* itemises and defines words, presenting their earliest known use and if possible, tracing the origins of words. It provides the changes in the use of words and if words have fallen out of use. The *Oxford English Dictionary*, the most definitive English record that is available to us, was itself compiled by members using their interpretative procedures, and is based upon the language activities of countless individuals before them. Something said only once, or an instance that is recorded only once, a *hapax legomenon*, becomes in the act of saying part of the language, part of the record. (For example, “hugger-mugger” was used once, by William Shakespeare.) Rose demonstrates how words act as “cultural units”—they are units of meaning.

Words have meanings—meanings with histories, which can be transformed and, occasionally, lost from the record.

“Words are intended to convey meaning, and, for any group of peers, the presumption is always held that some understandings are shared or can be shared through words”

(Rose 1962:159-160)

Ordinary meanings (as opposed to meanings of words appropriated and redefined by sociologists for their own special purposes—see Rose 1960) are uniform features of a culture. Shared understandings are the results of “people putting culture to use” (Rose 1962:159).

Rose consulted the English record and found that many key or “technical” concepts used by sociology were originally “lay” or common-sense words which had been appropriated and redefined by sociologists for their own purposes. In many cases, the appropriating and arrogating purposes of the sociological enterprise set up meanings that were “competitive” with ordinary meanings (Carlin 1994; Watson and Sharrock 1993).

What I have illustrated in this section is that different forms of worldly records have different possibilities for inquirers: fieldnotes necessarily preclude the option to be reanalysed *in their own terms* because of the reflexive or mutually elaborating relationship between fieldnotes and the social setting they describe. The unique, once-only nature of each social encounter requires methods which capture their fleeting

quality. This is not possible with fieldnotes, which are necessarily selective and intuitive.

There is, therefore, a “missing what” (Garfinkel) which is incorporated in fieldnotes, and is incorporated into analyses based on fieldnote data. The “missing what-ness” of fieldnotes is the occasioned use of members’ natural language practices.

Having presented *caveats* on the “interpretation” of fieldnotes above (and throughout the thesis), the reader is aware that “findings” based on fieldnotes are of a different status from recorded, “retrievable” data. From this position, having considered the status of fieldnotes in worldly inquiry, this chapter moves towards considerations of transcripts and transcribing as linguistically organised, linguistically constituted activities. The transcribing of talk, and the reading of transcripts of talk, involve members ongoing use of methods of sense-assembly. Separating transcribing and reading into discrete sections overemphasises and reifies a distinction between them. Issues of transcription are made available through considerations of how members *read* transcripts; these issues inform considerations of how members produce transcripts.

### ***Reading a Transcript***

In a classic study using newspaper articles to examine how membership categorisation activities provide coherence between texts and the titles, Lee indicates that reading is a cultural, i.e. linguistic activity. That is, a temporally organised, linguistic *course of action*:

"I do not read a newspaper by working my way mechanically from the top left-hand side of page one until I reach the bottom right-hand side of the back page. Instead I am guided by the headlines which indicate to me the items which I might find interesting."

(Lee 1984:69)

Members' ordinary, cultural methods are involved when reading transcripts as well as newspapers. Reading a transcript of a talk between Howard Schwartz and Harold Garfinkel suggests some issues of transcription. The transcript can be approached *qua* a "breach experiment" (Garfinkel 1967); however, the breach experiments were contrived situations rather than real-world, natural occurrences, as Garfinkel's study of Agnes had been. The material is a double-spaced typescript, made from a tape-recording on a manual typewriter. How does the transcript get *seen* as a transcript or as a stenographic "record"?

The transcript has to be read in order that the transcription notation can be discerned; the transcription practices are made available through the reader's use of the Documentary Method of Interpretation. That is, the reader "finds" the sense of transcription practices through increasing familiarity: the reader begins to interpret transcript details—"indexical particulars"—using an underlying pattern. This underlying pattern informs future occurrences of such details, and retrospectively provides interpretive sense to previous occurrences within the transcript.<sup>5</sup> In journal articles, transcript conventions are often placed in appendices, i.e. after the transcript, so that the transcript is read before the reader reaches the "how-to-read-the-transcript"

conventions. (This is not a rule, however; e.g. Goodwin (1981) and Schenkein (1978) present transcript conventions before the chapters within their books.)

In the Schwartz and Garfinkel transcript under discussion, the transcriber seems to have elected to represent pauses in speech production with ellipses ( ... ). Not all elliptical occurrences, using the prospective-retrospective interpretation of this underlying pattern, conform to this pattern unproblematically. This is because the ellipses are not standardised.

For example,

H: The thing I was going to say was, minimally, whenever the thing seems to be going on,  
it seems to be linear, at least in the sense that....  
G: You're talking there about...  
H: About typification.  
(Schwartz and Garfinkel 1968:6-7)

If ellipses represent silences, do the number of points indicate a length of silence, where more points indicate longer silences? Similarly, in the example below, is the pause between “not here” longer than the pause between “a you”?

I'm not..here's a...you make ...  
(Schwartz and Garfinkel 1968:8)

At the opening of the transcript, the speakers are not identified. Identifiers only begin to appear at the left-hand margin on the fifth page. However, it is not easy to work back through the transcript from the first identifier in order to decide who said what in the

previous four pages of talk. A speech exchange is preserved—in that utterances are seen to be responses to prior utterances—but the method of transcription does not preserve the “system” of speech exchange, i.e. when speaker change occurs; or more simply, who says what to whom.

Before presenting considerations of the transcript of the Schwartz and Garfinkel discussion, I shall reprint the opening lines—as they appear on the transcript—which provide a basis for some of my observations. It begins as follows:

Let me show you about correctness.

All Right.

You've got a model. Now, consider Warren's (Tenhouten) case of modelling an airplane.

Now, it's got these wings and its got ..... Now, in terms of a correct model, and in terms ... ... ..

Whose case is this?

Warren once wrote a paper.

Oh.

Stuff on model theory and axioms and things like that.

Oh, I see. And he gave as an example ... ... ..

This is one of the ... Well, you know .... I'm going to go beyond his treatment ...

OK.

(Schwartz and Garfinkel 1968:1)

A problem for the reader is the indeterminacy of turn-taking as characterised in this transcript. Using the Documentary Method of Interpretation, readers notice that different speakers take turns at talk, and the changes of speakers are apparently represented on the page using hard returns on the typewriter. However, an indexical particular does not fit conveniently with this underlying pattern. Given that readers are instructed at the very top of the manuscript that there are two speakers,<sup>6</sup> an issue of transcription is presented by the sequence

You've got a model. Now, consider Warren's (Tenhouten) case of modelling an airplane.

Now, it's got these wings and its got . Now, in terms of a correct model, and in terms ... ....

Whose case is this?

The second utterance in the sequence seems tied to the first utterance, in that it provides a description ("it's got these wings and its got ") of the model plane mentioned in the first utterance. However, according to the format of the transcript, which is visually available at-a-glance, the line spacing between the first and second utterances indicates a change of speaker. If this is the case, then given that there are only two persons the third utterance "Whose case is this?" is produced by the speaker of "consider Warren's case of modelling an airplane", which (intuitively at least) seems unlikely. Seeing that the third utterance asks information provided in the first utterance, the reader surmises that the first two utterances are made by the same speaker.

In the aforementioned study of reading newspapers, Lee observes that he

"take[s] it without even considering the matter that there will be some relationship between the headings and the articles to follow."

(Lee 1984:69)

This observation is coherent with the reading of transcripts, as shown by D.R. Watson (1997a). His analysis of the asymmetries of emphasis between the sequential and categorial aspects of conversation shows how readers of transcripts are presented with categorial incumbencies prior to the utterances so designated by these incumbencies. D.R. Watson does not intend to give primacy to Membership Categorisation Analysis at the expense of the sequential analysis of turns, or to see them as alternates, but to highlight the tacit incorporation of membership categories by analysts themselves. A locus of this incorporation is the text itself:

"Article titles, contextualising comments introducing particular transcribed instances and the overall textual environs of the instance are all, frequently, category-rich, saturated by categorial identifications."

(D.R. Watson 1997a:52)

D.R. Watson, therefore, is looking at how the transcript is presented by analysts and how the transcript is attended to by readers. D.R. Watson's study coheres with Lee's (*ibid.*) analysis in that it examines the sense-assembly or culturally-methodic activities which members use in reading. The cultural practices of reading (left to right) "predispose" readers to approach utterances as spoken by incumbents of membership categories the analyst provides.

In the transcript of talk between Schwartz and Garfinkel, the identifiers for the speakers are not consistent, in that Harold Garfinkel is designated from his surname (G) and Howard Schwartz is designated *via* his forename (H). This is derivable through the Documentary Method of Interpretation: the first designation encountered by readers is "H", which could either be referring to "Harold" or "Howard". It is the second designation, G, that tells readers that the prior bloc of talk was "Howard's", not "Harold's". The typed designations are replaced by handwritten ones, from page eleven onwards. When were these handwritten identifiers inserted, and by whom? How did they decide which speaker was which from the transcript, or did they listen to the original tape-recording in order to annotate the transcript *via* the recognition of speakers' voices?<sup>7</sup>

The "look again" character of the transcript, provoked by the inconsistency in line spacing speakers' turns and speaker designations, encourages the reader to approach the remainder rest of this transcript with caution: the reader has become aware that the transcription itself does not render features of the conversation between Schwartz and Garfinkel in a satisfactory manner.<sup>8</sup>

Names may be misspelled, e.g. "Mormon" for Moerman; enclosing items in brackets, e.g. "Warren (Tenhouten)" which occurs in the extract above, is perhaps an unequivocal identification of Warren as "Warren ten houten" by the stenographer/transcriber. According to Sacks,

"in the case, say, of our doing transcription of tape-recorded conversation, or transcriptions of these lectures, then the one predictable thing that will be wrongly transcribed are the uses of names. Names are the prototypical sorts of things for which you may know 'it's an X', but which X it is, you can't be assured via what you heard."

(Sacks 1992a:724)

In the Schwartz and Garfinkel transcript, the transcriber marks uncertainty by inserting spaces within the transcript. Spaces within speakers' utterances are inferable as inaudible words. Do wider spaces indicate a longer stretch of talk which was unavailable for transcription?

Some of these spaces in the typescript have been filled in by hand. Were these annotations made by the transcriber? Were these annotations made with access to, or whilst listening to the original tape-recording?

Some of the transcribed words have been crossed out, and overwritten by hand. For example,

*for      that      type      of  
property*

a number, like, say its evenness, and ~~that's the class~~ ————— when you take each

(Schwartz and Garfinkel 1968:16)

What status do these "corrections" have? Were these corrections made with access to, or whilst listening to the original tape-recording?

Corrections are necessary and commonplace in transcripts, as Rose remarks:

"we have to fix up transcriptions of tapes when a secretary hasn't followed an argument and loses what was in an original discussion. There is a transcription of a tape that you and I made that provides nothing recognisable of what you intended to say about what I intended."

(Garfinkel and Rose 1978:7)

To Garfinkel, the reader is then involved in "reading typist's errors" (Garfinkel and Rose 1978:7). According to Garfinkel,

"Somehow or other the reading that you're doing itself provides a device with which to find fault. You find [the transcriber]; you find [the transcriber] in the properties of [the transcriber's] way of working; you find what [the transcriber] heard on the tape; you find the way in which—you find the phonetic version [the transcriber] gives you as a slip-shod way of *making do with the transcript*."

(Garfinkel and Rose 1978:8; my emphasis)

Rose provides, as an example, how a typist had queried the word *epoché*, spelling it e-p-o-k-a-y in the typescript. (The typist's spelling, which is formulated by the typist—"I've never heard this before"—is contingent on how it is pronounced on the tape.) Rose reports this to Garfinkel, saying that he told the typist "That's fine. I know what you mean." (Rose, in Garfinkel and Rose *ibid.*)

Thus, transcripts are made and read by members. The practical work of reading, however, demonstrates the robustness of the transcript. By "robustness" I am not only referring to the adequacy of the transcription system, nor to the analysis of transcripts. Rather, I am attending to reading as a flexible and routine or commonplace activity.

The Schwartz and Garfinkel transcript raises issues of transcription, *viz.* doing transcribing and reading transcripts. As mentioned above, that identifiers only appear somewhat “belatedly” in the transcript forces the reader to work back through the transcript in order to decide who said what, who took which turn. This reveals a course of reading necessary for assembling sense of the transcript; that is, working back through the text is something readers have to do. So reading is not only temporally organised but is oriented to by members as a retrospective-prospective activity.

Further issues of transcription are discerned in considering the methodological or “elective affinities” between the work of Rose and Sacks. Such affinities and continuities are made visible *via* consideration of their respective purposes of transcription and its analytic prior, recording.

### ***Recording and Transcription – Methodological Affinities***

As mentioned earlier, at the close of the profile of Edward Rose, there is a methodological affinity between the work of Rose and Sacks. This elective affinity begins to be manifest in the use of tape-recorders. However, the identification of this affinity should be qualified: whilst both Rose and Sacks employed tape-recorders, they did so for different analytic purposes.

Rose’s use of the tape-recorder was intended to “make the world available”, i.e. a linguistically constituted, worded world. The “capturing” of members’ formulations of scenes eventuates an opportunity for the investigator to *preserve* the world as it was

worded by members themselves. Sacks' inquiries also mobilised tape-recording technology to preserve members' formulations. However, from the section on Conversation Analysis in the chapter on Ethnomethodological Programmes, there is a shift of emphasis between Rose and Sacks. As mentioned previously, Sacks intended that tape-recorded data substitute (and supersede) intuitive or invented stretches of talk as used by linguisticians. Sacks regarded the tape-recordings as researchable sites for the location of social order. That is, Sacks brought to ethnomethodology's programme an inquirable record of order, as collaboratively and endogenously produced, through conversation by members in settings.

So Rose was interested in people's words and meanings in a more substantive manner than Sacks. If Sacks dealt with meaning it was on a more procedural basis, i.e. how meanings were produced and recognised by interlocutors. A distinction between their use of tape-recorders may be glossed as Rose being concerned with *what* people said, whereas Sacks' concern was with *how* it was said. Rose himself observed this difference between the "how" and the "that" of social inquiries (Rose 1992:23-24).

Rose, following his interest in Znaniecki, regards the recording and transcription of members' talk as an important move in making available members' own understandings, free from the distortions imposed by analysts. Rose's use of transcripts is attended to by Charles Kaplan. According to Kaplan, Rose devised various "rules" for the transcription of talk which were "a methodological application of the humanistic

coefficient" (Kaplan 1989:358). Kaplan reproduces Rose's rules for transcription as follows:

1. Let each phrase start on a separate line
  2. Let that line be rarely longer than this:  
(About ten centimeters)
  3. If the phrase indeed is longer continue the phrase on the following line *indented* five spaces thus:  
(5 spaces)
  4. Indent parenthetical remarks ten spaces whether they come before thus:  
(10 spaces)  
or after a phrase, thus:  
(10 spaces)
- (Rose, in Kaplan 1989:361; emphasis supplied)

Through these rules, Rose makes explicit the practices of transcription he used in the transformation of talk-on-tape to paper. These practices attended to the preservation of the *that* of talk.

Rose's rules of transcription also serve to explicate his *re*-transcription of talk as "poetry". Jon J. Driessen reports an incident (during, or shortly after the Larimer Street project) in which Rose read out to him the words of one of their informants as poetry (Driessen, in Gorman and Rose 1976:6). The talk is elicited, however, and the elicitation is edited out of the account. The opening words are as follows:

Today's  
my dead sister's birthday.

I was born and raised in  
Illinois.  
Ina, Illinois.  
I, N, A.

The town's named after my sister.  
It's a small town.

I had a sister and a brother.  
My sister passed away.

(Gorman and Rose 1976:6—reprinted 1998:1)<sup>9</sup>

These opening verses had originally appeared as one of the supplementary materials to the Larimer Street study:

GORMAN: Today is July 22, 1965.

MISS B: Yeah. My dead sister's birthday.

GORMAN: Your sister's birthday. All right. Now Anna, I did have a few questions I would like to ask you. Maybe you could give me a little idea of where you were born and raised, just a few statements like that.

MISS B: Yes. Illinois. Ina, Illinois. I-N-A. The town's named after my sister. It's a small town.

GORMAN: A small town. Yes. And how large a family did you have?

MISS B: I had a sister and a brother.

GORMAN: I see.

MISS B: My sister passed away.

(Gorman 1965a:15)

So the presentation of talk-as-poetry is not the talk that was originally produced: the practices of elicitation have been omitted.

The practices of elicitation were not always omitted from Rose's transcripts. The transcript of Rose's conversation with the heroin dealer Ali Baba—subject of the rules of transcription quoted by Kaplan (*ibid.*)—preserves the integrity of the talk *as a conversation* (Rose 1981a). The reprinting of other Larimer Street documents also maintains the turn-taking format: whilst the volumes of “The Jailing of John Ralph Kruthshift” were re-typed, one volume of the “Larimer Vignettes” was reproduced

using a scanner. A series of “consultancy reports” on the use of telephones and telephone directories preserves what people actually said—both Rose and the telephone users he spoke with (Rose 1986a, 1986b, 1990).

In considering Rose’s rules for transcription, it is unfortunate that not all the talk-as-poetry is presented in its original form. In the example cited above, from “The Poems of Anna Brown”, the talk-as-poetry can be viewed alongside the original transcript of Anthony Gorman’s interview. This demonstrates the “collapsing” of talk. Rose is aware of the deliberate omission:

“If you’re more interested in Anna’s story than in the way it was brought about, then read [in *Be Fine*] the whole account written down without showing how the story was told and heard.”

(Rose 1998:iv)<sup>10</sup>

However, in the case of other transcripts such as “Leroy II” (Rose 1981b), the reader is presented with blocs of verse without the practices of elicitation, i.e. the words of the informant only, with no means of establishing whether the transcript preserves the *that*.<sup>11</sup> The presentation of talk-as-poetry is problematic, then: with rules of transcription which are intended to preserve members’ understandings, does the transcription of that talk in the form of poetry remain sensitive to members’ understandings? If members do not conceive of themselves as “talking poetry”, then is presenting talk as poetry a form of “methodological irony”?

The majority of Rose's transcripts do, however, remain faithful to the production of talk by the informant *and* the interviewer. These transcripts are intended to display the words and understandings of people themselves. Such transcripts are derived from open-ended discussions with people, by Rose and members of his research teams.

Sacks, on the other hand, did not use transcripts of interviews for analytic purposes. Whilst Sacks' lectures were transcribed for distribution to those interested in the field of the analysis of conversation (Richman 1994:176; Sacks 1992b:336-338), the transcriptions he used as data were not derived from interviews, as Rose worked. Rather, Sacks used transcripts and recordings of naturally occurring interactional settings, e.g. from therapy sessions and telephone calls.

### ***Producing a Transcript***

One locus of the procedural nature of Sacks' interest is evident in his considerations of productional particulars, i.e. how members say what they say: "we have simply tried to get as much of the actual sound as possible into our transcripts" (Sacks, Schegloff and Jefferson 1974:734). As a transcription policy, this contrasts with transcribing talk as it is intuitively understood. For example, by noting that members do not always say "did you", but may produce a particle such as "*dju*" (Sacks 1992a:724). Sacks (1992b:306) also observes that words have "alternate pronunciations", e.g. "because" alternately pronounced as "*cuz*" and "*becuz*", where *cuz* and *becuz* are also alternates to each other. Such alternates are not caricatures, to be read as "funnypaper-English" (Sacks, Schegloff and Jefferson 1974:734).<sup>12</sup> These alternates constitute productional or

“pronunciation” particulars (Jefferson 1983:3), which are located and locatable within sequences of actually occurring talk.

Jefferson notes that productional or pronunciation particulars are “indices” of category membership yet category membership “does not automatically bring with it ‘textbook’ pronunciation” (1983:3-4); pronunciation particulars operate as “dialect signatures” (1983:6) in transcriptions, from which readers of transcripts may infer category-membership.

The retrievability of data enabled Jefferson to investigate the practices of transcription used in transcripts prepared for a workshop on lingua franca conversation. The locus of her concerns *re* the practices of transcription was the recurrent rendering of the word “of”, used by a German speaker, as “off”. Returning to the original tape-recordings from which these conversations had been transcribed, Jefferson found that this recurrent pronunciation had been incorrectly transcribed. The transcription “off” was not the outcome of “instance-by-instance transcribing” (1996:162) but a stereotyped form of pronunciation attributed to the speaker by the original transcriber. That is, this was an example of “transcriptional stereotyping”. The implications of these mis-transcriptions was to distort the natural course of interaction as it had happened, i.e. it did not preserve the phenomenological intactness of the setting. As Jefferson notes, the original transcription was “altogether misleading in its depiction of salient features of [a speaker’s] productions” (Jefferson 1996:168).

This concern with productional particulars impacts upon the practices of transcription. In an oral presentation of the internal organisation of murder interrogations, D.R. Watson (1979:8) provided actual sequences of talk transcribed from police recordings, which included the utterance “whose body was found (dismembered) in the tub at that address”.

[Watson]

1. P: All right, we're investigating the death at two-o-one Patterson
2. S: Two-ten
3. P: Two ten Patterson, whose body was found (dismembered)
4. in the tub at that address. Would you tell me in your own
5. words what you know of this homicide.
6. S: Do you want me to start from the beginning?
7. P: Yes would you please.<sup>13</sup>

According to the use of transcription conventions, an element of doubt *apropos* the hearing of a word is conveyed by enclosing what, to the transcriber, is the most-likely-hearing of the word in brackets (line 3). In the published account of murder interrogations, however, this utterance had been re-transcribed as “whoose body waz foun::d: (1.0) dissem:bered: (.2) in the: tub at: that address” (1990:292). (This transcript is reprinted in full in the Chapter Nine.) The equivocal hearing of “dismembered” problematised the word: repeated hearings, facilitated by retrievable data, enabled D.R. Watson to render the officer's pronunciation of “dismembered” as “dissem:bered:”.

Whilst it remains the case that “dismembered” was, intuitively, the correct word, it is important to note that this is not warranted by salient details of the setting that D.R. Watson alone is party to. Ethnographic details which are extraneous to the tape-

recordings are resisted in Conversation Analysis; that “dismembered”, pronounced “dissem:bered:”, was the term in question is provided for by the interlocutors themselves, not the analyst. As shown in Chapter Nine, the police officer formulates that “we’re: investigating the death (.) of a gir::l” (D.R. Watson *ibid.*). The local detail provided by interlocutors on the tape enabled the transcriber of the earlier version to transcribe the officer’s “utterance as she or he *understood* it rather than how it was *produced.*” (Zimmerman 1988:414; emphasis supplied). So the transcription of the pronounced detail “dissem:bered:” as “dismembered” was a sound-preserving error, i.e. a near version of the “correct” (or actual) pronunciation.

The development in technology, from audio to video-recording, makes available another dimension, *viz.* the visual order, for analysis. Researchers in Conversation Analysis do not make *a priori* judgements regarding the importance of phenomena; as Sacks (*op. cit.*) suggests, activities retrievable from the tape are naturally occurring activities which actually occurred, and are therefore available for analysis. Peräkylä clarifies this attitude towards transcription as

“at the time of transcribing, the researcher cannot know which of the details will turn out to be important for the analysis.”  
(Peräkylä 1997:207)

Consideration of what and how much to transcribe from recordings makes available to inspection the research parameters involved in the practices of recording and transcription. Transcription as a method is employed as a device for presenting, in a

textual format, the interactional features of a social setting as adequately as possible, in order to afford descriptive and analytic precision.

Extract 97(2) (below) was transcribed with the new technology discussed above, and uses the same transcript conventions shown in Extract 100(2) (above). It preserves the turn-by-turn nature of the conversation, as well as other linguistic features, e.g. turn overlaps, continuations of turns, micro-pauses, rising and falling intonations, and variations in volume. It is to be noted, however, that in contrast with Extract 100(2), Extract 97(2) identifies the co-ordination of verbal and non-verbal communication through the ongoing production of turns at talk. In order to render members' non-verbal activities in textual form, the transcript, and the transcription system used to transcribe this sequence, are more complex. Outline arrows ( $\uparrow$  and  $\downarrow$ ) indicate the onset of non-verbal activities. Vernacular descriptions of these non-verbal activities are italicised and enclosed in double-round brackets.

Extract 97(2)

- 1 CR1 a:::m of course the other thing which they do  
2 in a::m [(.) pregnancy now quite early]=  
3 CD2 [°( )°]  
4 CR1 =now is is scans  
5 has anybody seen [those]  
6 CD4 [wha- ]=  
7 =↑ul↓trasound  
8 CR1 yeah  
9 CD ( [ ] )  
10 CR1 [yeah  
11 CD1 yeah I got pictures of mine  
12 CR1 yeah got pictures (.) yeah  
13 CR1 huh I mean that's quite interesting

- 14 CD1 ( )  
 15 CD4 naw:::  
 16 CD5 if you look at it carefully  
*((CD5 arcs a curve with r.h. palm outstretched))*  
     ↓  
 17         you can [see the backbone (1.0)  
 18 CD1           [it looks quite funny=  
*((with l.h. CD5 indicates position of head in relation to backbone from gesture produced at line 17))*  
     ↓  
 19 CD5 [[and the head  
*((CD5 crouches in chair raising knees to chest and hands to shoulders, palms down))*  
     ((CD1 traces cylindrical shape downwards slowly with r.h.))  
     ↓           ↓  
 20 CD1 =[[you can see the back bone and at the end=           ↑  
     ((CD1 returns r.h. up to original position))  
     ((CD5's crouch ends))  
     ↓  
 21 CD1 =it goes (0.2) twisted round           ↑  
     ((CD1 rotates cylindrical shape with r.h. four times))  
     ((CD1's final rotation))  
     ↓  
 22 CR2 turn it upside down yeah  
 23 CR1 yeah  
 24 (2.0)  
 25 yes it's quite fascinating isn't it  
 26 to see it you know (2.0)  
*((CR1 brings l.h. level with her face, extended fingers pointing towards CD1, moves l.h. forward approximately 4"))*  
     ↓  
 27 erm (1.0) a-ctually on the screen           ↑  
*((CR1 retracts l.h. to its original position))*

The practical difficulties and time-consuming effects of transcribing people's physical orientations should not be under-emphasised. I speak from first-hand experience—

*experto crede*—when I suggest that a decision to include gestural features of naturally occurring interaction introduces an extra layer of conceptual and technical obstacles.

Within this thesis “the body” is not conceptualised as a separate, substantive field of research. The body is not “separated out” or privileged by bringing existing analytic commitments to the data. The video-recordings did capture and preserve gestures and body movements, and spatial arrangements, as produced by people themselves. As Sacks argued, this requirement—that an activity actually and demonstrably occurred, is available for “repeatable” analysis—provides the “warrant” for inclusion of an activity as a phenomenon for empirical investigation. For this reason, and for this reason alone, the body may be seen as a feature in the analysis.

However, it is the approach to the body as a topic which differs. Rather than theorising about the place of the body in the social world, and applying a purportedly bespoke set of concepts to a social setting, ethnomethodological programmes would examine how people’s bodies are linguistically constituted by people themselves. E.g. the categorial organisation of pregnancies (prepartum and postpartum states), categorial orientations to growth and physical development (*in utero* and post-delivery). (In the previous sentence I have already introduced four cultural categorisations used in talking about parenting.) This exemplifies the *gestalt* switch between methodologically ironic and methodologically non-ironic forms of sociology: definitional privilege is not afforded to external preferences but to people’s *in situ* accounts and membership activities, *vis-à-vis* the body.

### ***Practices of Transcription***

Patricia Haegeman's (1996:87-110) chapter on transcription is relevant to this thesis on the linguistic constitution of research practices, and particularly apposite to its advocacy of methodological transparency. In opening her considerations of transcription, Haegeman argues

“that work based on transcripts will hardly ever include explicit descriptions and even less justification of the transcription system employed.”

(Haegeman 1996:87)

The work of transcription is made available in retrospective accounts. Goodwin's transcripts are organised where

“the division of talk into lines within a transcript should make visible to the reader how the speaker organised his or her talk into relevant units. I have tried to do that [...], breaking lines at intonational units and indenting the continuation of units too long to fit within the page margins.”

(Goodwin 1994:607)

For the purposes of capturing pronunciational particulars, Jefferson distinguishes correctly-spelled words within her transcripts. For the purposes of analysis, the identification of correct spellings indicates that

“the word was produced in standard form, this was not a transcriptional lapse. Coming across a correctly spelled word with a dot under it, I don't have to wonder if this ‘of’ is the result of a hearing or a lapse of attention.”

(Jefferson 1996:162)

Goodwin provides transcribed instantiations of the organisation of talk and the organisation of gaze in interaction:

JANE: It- *It was (----- + --) so: : (0.3)* °incredible.

[

MEG: I (love) it. = I do.

SPEAKER:

Brian you're gonna ha v- You kids'll *have to go down closer*

[

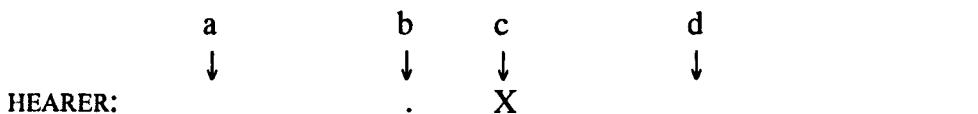
HEARER:

. X

(Goodwin 1981:vii-vii)

Each typographical mark is purposive, and designates a particular feature of interaction.

For example, in the illustration below, the line (d) indicates the direction of an interlocutor's gaze towards another interlocutor; that an interlocutor's gaze is not towards another interlocutor is indicated by the absence of a line (a); dots indicate the movement towards mutual engagement (b); and X marks the onset of mutual engagement (c):



(Goodwin *ibid.*)

Christian Heath's transcribed representations are augmented with illustrations by Katherine Nicholls, derived from video frames; which, as derivations of video representations of the setting, distance readers further from the phenomena. However, Heath argues that the decision to use drawings "rather than the actual photographs" was

motivated by “sensitive ethical considerations” (Heath 1986:xiv). Hence, extra-analytic contingencies have been brought to bear upon the presentation.

A cumulative theme within this thesis is confidentiality. From Heath’s use of an artist’s drawings of video-frames, this thesis recognises that the textual representation of interaction is, then, another locus for considerations of confidentiality. Heath’s study shows how confidentiality is built into visual-pictorial representations.

Prior to the presentation of conversational data, Haegeman (*op. cit.*) outlines the reasoning of transcript notation used throughout transcripts of work-related telephone calls. In an original form of presentation, Haegeman conducts a literature survey of transcription orthographies, the use of transcript symbols in extant work. For each case, she provides rationalisations for notational preferences: e.g. the use of ampersand (&), angle brackets (< >), latching (=), equivocation and editorial comments (including the use of the octothorpe or hatchmark, #), enclosing various non-lexical phenomena and transcribers’ comments in round brackets (( )), marking the beginning ( [ ) and end ( ] ) of overlaps with square brackets, sound stretches, laughter, etc.

Within this thesis, several different forms of presenting talk are apparent. These are:

1. “rectilinear” blocs of talk from interviews; these transcripts attend to some of the productional features of collaborative talk that constitutes “an interview”, e.g. turn-

taking, increases and decreases in the volume of speech-production of particular speakers, pauses

2. transcripts from the parenthood-counselling sessions which attend to the aspects of turn-taking produced by participants within the sessions
3. detailed transcripts from the parenthood-counselling sessions, transcribed using superior technology which became available (mentioned above)

Transcription systems themselves are related to developments in technology and available equipment. Originally, conversations were transcribed using typewriters; now talk is usually transcribed using word processors, and sometimes HTML (Hyper Text Markup Language). The symbols available on typewriters are not always available on word processors (Have 1991:3-4), requiring adaptations to the early transcript conventions—the transcript conventions in Jefferson (1984) shows how some symbols have been amended by hand—and the introduction of newer symbols reflects the availability of increasingly sophisticated equipment. As more and more social phenomena become visible and available for transcription, the symbolic apparatus becomes increasingly complex. So transcript notation is developed ongoingly, due to the identification of phenomena and the textual rendering of phenomena:

“The florin sign [f] is, *for the time being*, used to indicate a certain quality of voice which conveys ‘suppressed laughter’. *I have not yet settled on a symbol for this phenomenon.*”

(Jefferson 1984:216; my emphases)

Despite the range of Haegeman's (*op. cit.*) review, and *post hoc* reconstructions of transcription options, she fails to mention the *raison d'être* of transcription, *viz.* to make available the locally ordered, social-interactional nature of conversational settings. Whilst Haegeman asserts her study is aligned more closely to Linguistics than Sociology (Haegeman 1996:99), she neglects the social dimension of Conversation Analysis. For Haegeman, Conversation Analysis is a "borderline field" which straddles Linguistics and Sociology, a placement that might course discomfiture among some conversation analysts, and risks the creation of category mistakes.

Don H. Zimmerman outlines various principles of transcription for Conversation Analysis. One of his principles suggests refers to transcription *as observation* (Zimmerman 1988:413 *et seq.*; emphasis added). Indeed, for Zimmerman,

"Transcription is observation: the noticing and recording of events of talk that might otherwise elude analytic attention."  
(Zimmerman 1988:413)

By this, Zimmerman intends to clarify how transcriptions make conversational/interactional phenomena observable by making phenomena available for inspection. In effect, the transcription of talk makes the interactional features of talk observable-reportable, i.e. accountable; this enables the analyst to provide explications of the structures of talk endogenous to the conversation as oriented to by interlocutors.

Both Haegeman and Zimmerman cite Gail Jefferson's transcriptions of laughter as examples of conversation-analytic observations, and agree upon the profundity of her explications of hitherto unnoticed conversational phenomena. The intricate and sophisticated works of Gail Jefferson stand as exemplars of the use of retrievable data in the highlighting and explication of social (conversational) phenomena. Her seminal work provides for broader "horizons of visibility" (Carlin 1994:206) within conversation-analytic studies, by making phenomena of unnoticed salience available for rigorous investigation.

Zimmerman (1988:414) notes that rigorous transcription is "an archival task". The use of retrievable data provides for the explication of previously unnoticed phenomena; furthermore, retrievable data affords Conversation Analysis with opportunities to revisit, re-transcribe and re-analyse recordings from the conversation-analytic "archive". Treating transcription as a form of observation, as Zimmerman (*ibid.*) suggests, is complemented by the "archival" nature of retrievable data, in that the noticing and analysis of an occurrence or phenomenon in conversational interaction throws the phenomenon into relief, it alerts or "sensitises" us to the recurrence of the phenomenon within that and other interactions.

For example, Jefferson (1981) describes how, upon discovery of two [instances] of what she characterises as a "post-response-initiation response solicitation" (Jefferson 1981:11) in a corpus of data of German conversation, she began a search through five thousand pages of American conversational transcripts (a procedure only made possible

using retrievable data). If we regard this procedure as being “grounded” then it is grounded in members’ common-sense practices.

“Th[e] confrontation [with the original instances of a solicitation of response after a response is in progress] and the search it entailed, generated an inquiry into a rather more general phenomenon; i.e., the occurrence of post-response pursuit of response”

(Jefferson 1981:14)

A subsequent study (Jefferson 1989a) was realised *via* a similar trajectory. By examining transcripts of conversation for a forthcoming workshop, Jefferson noticed that particular silences occurred, and occurred recurrently in interactions. These were not “noticeable absences”, in Sacks’ terms, in which the production of a second pair-part recognisably did not follow the production of a first pair-part. Rather, these silences were of a “‘standard’ length, *viz.* approximately one second. With her recognition of this approximate standard length of “intra-utterance” silences, Jefferson moved from a “more or less unmotivated scanning” (1989a:166) towards taking intra-utterance silence as a phenomenon in its own right. In another study, on “pronunciation particulars” (*op. cit.*), she notes that

“once we begin to find possible phenomena in pronunciation details which are not in the first place obviously relevant; i.e. details which are not in the first place required to make sense of an utterance [...] or to at least partially account for a range of repairs and understanding-checks [...], then we are either committed to transcribing all the talk in its pronunciation particulars, or to accepting the obliteration of a potentially fruitful data base.”

(Jefferson 1983:12)

To notice these phenomena (*inter alia*, “post-response pursuit of response”, intra-utterance silence, pronunciational variations between speakers and by the same speaker), to initiate a search to find further instances of these phenomena, and to achieve the move from the highly particular to the more general, were only realised through the detailed analysis of fine-grained, retrievable data sources.

Previously, in a study of conversation openings originally published in *American Anthropologist*, Emanuel Schegloff had examined a corpus of five-hundred telephone calls. He arrived at an empirical warrant or “distribution rule” to account for the sequential structures of telephone openings; this distribution rule held that “*the answerer speaks first.*” (Schegloff 1972a:376; emphasis supplied). Cases of overlapping talk—where the caller and answerer talk simultaneously—were noted to be “resolved” by members themselves in preference for the answerer of the call to talk first (1972a:381).

However, there was a single discrepant or “deviant case”, which was not accounted for by the distribution rule. Schegloff remarks that “[n]ot number of occurrences, but common subsumption under a more general formulation is what matters.” (Schegloff 1972a:382). Having identified this conversation opening which departed from the distribution rule, Schegloff noted the options available to him. The first option would be to

“focus exclusively on this case and seek to develop an analysis particular to it that would account for its deviant sequencing. This would constitute an ad hoc

attempt to save the distribution rule, using a technique commonly used in sociology—deviant case analysis.”

(Schegloff 1972a:382)

The second and alternate option, which Schegloff takes, was to re-analyse the entire corpus of telephone-call openings. This re-analysis, according to Schegloff, did not negate the distribution rule but showed that it was “best understood as a derivative of more general rules” (*ibid.*).

The cases of Jefferson and Schegloff quoted above are illustrations of the use of retrievable data, *viz.* transcribed and tape-recorded materials, in the analysis and re-analysis of corpora of data with a particular “analytic mentality”, a commitment to and method of making available the structures of conversational organisation. *Via* the use of retrievable data, previously-analysed corpora of data are available for re-analysis, and previously-transcribed corpora are available for re-transcription. Schegloff’s “Sequencing in Conversational Openings” paper, in particular, highlights the explicative attitude towards conversation (or “talk-in-interaction”) which avoids the introduction of methodological irony by treating data which do not fit into the analyst’s model as atypical or “deviant” cases. So Schegloff’s paper is a perspicuous demonstration of how conversation-analytic models empirically suit the data, rather than attempting to suit data to the models.

Schegloff does not make available how the practices of transcription afford the analyst with the opportunity to rehearse analyses of data corpora. The cumulative and ongoing

activities of rendering talk into text generate analyses *per se*. This is not to suggest that the analysis of a transcript of talk is reducible to the practices of transcribing: Schegloff (*ibid.*) demonstrates how analyses are subject to revision. The point is rather that an incipient or provisional analysis is being assembled through the practices of transcription.

The practices of transcription, *inter alia*, watching/listening to the tape, the repeated viewing/listening of sequences, the transformation of talk into text, the fine-grained and repeated comparisons of transcript with the original video/audio data to ensure coherence between the talk and its textual representation, etc., are all linguistically constituted, linguistically organised, *observational* (and auditory) activities. Transcripts are performe worded entities, which are “worked up” over time.

The ongoing linguistic, observational practices involved in transcribing talk engage the analyst in making provisional formulations about the structures and organisation of the transcribed interaction. That is, the analyst does not arrive at a transcript of talk “cold”. (Additionally, of course, the analyst approaches the transcript as a member; the analyst brings procedural-cultural knowledge—their mastery of natural language—to the transcript.) Transcription, especially using the orthographies of Conversation Analysis, is a time-consuming, labour-intensive activity: the analyst has “lived with” the data over an appreciable period of time before the commencement of the “official analysis stage”, mentioned at the beginning of this chapter. This amounts to an episodic conception of research practices, which does not make allowances for the development of a

provisional analysis which is occasioned by the practices of transcription enumerated above (repeated viewings, comparisons to assess coherence between tapes and transcripts thereof). As Psathas and Anderson observe, transcription “becomes an integral part of the analytic process” (1990:77).

### **Conclusion**

The upshot of these considerations of transcription is that the very practices of transcription generate analysis. The work of transcription is both observation (bringing interactional phenomena into relief in order to produce an analysis on the setting which accounts for its interactional products) and analysis (the ongoing work of transcribing affords insights towards a candidate analysis of phenomena). To reiterate, such analysis “in flight” is not a “final” analysis; but then, as both Jefferson (1981 *op. cit.*) and Schegloff (*ibid.*) show, even the “final” analysis is not final, it is subject to change. The analysis does not start when the transcriber is satisfied that the transcription is a faithful representation of the interaction; the analysis is continually in progress *via* the practices of transcription.

This treatment of transcription is the recognition that transcripts are textual artefacts: the transcripts themselves are, *in toto*, linguistically organised, and that the work of transcription and the work of reading transcripts are a series of linguistically constituted, natural language activities. The approach to transcripts is a cultural, i.e. linguistic approach: analysts bring their own cultural knowledge as society-members to their reading of the transcript. So the linguistic resources analysts use constitute their

viewing of the recording, the work of transcribing, the transcript and their reading of the transcript. As D.R. Watson remarks,

“the data actually comprises our commonsense reading *and* the transcript; it is the transcript-as-read.”

(D.R. Watson 1995a:309; emphasis supplied)

Social interactions are productions of the parties to the interaction, the people present in a setting. The settings are produced through the medium of language, or talk. In transferring the talk from the setting onto the page (cf. Goodwin 1994 *op. cit.*), it has to be recorded (the details of the interaction can not be analysed if the researcher relies on memory to reproduce the interaction), and the recording has to be transcribed.

The theoretical and empirical decisions made prior to the work of transcribing affects the outcome of the transcription. The manner in which the researcher transcribes the talk will define the limits of the study. This is shown within this chapter, and throughout the thesis, *via* the alternate forms of transcripts produced using different technologies and for different practical purposes. Although I attempt to preserve the productional nature of talk in interviews *via* the transcription of turns taken by interviewee and interviewer, pauses and emphases, the transcripts of interviews (the “rectilinear” blocs of talk) and parenthood-counselling sessions exhibit different analytic relevances. Furthermore, the use of superior technology enabled the transcription of phenomena which were inaudible, i.e. unavailable for transcription, using the equipment for the original transcription enterprise. The corpus of the original transcripts appears in Carlin (1994:221-277).

The next chapter is also concerned with data, and the linguistic constitution of data. It pursues the ongoing theme within the thesis of membership, membership categories and Membership Categorisation Analysis. Membership Categorisation Analysis explicates how ethnographic data is problematic *via* the incorporation of culturally methodic, culturally shared, members' practices.

<sup>1</sup> The term "schism" is used in the "simplest systematics" paper (Sacks, Schegloff and Jefferson 1974:713-714) when considering the study of conversations in groups of members, in which separate conversations may be opened and closed during the course of the larger gathering.

<sup>2</sup> An exception was a peripatetic conversation or "guided tour" around the research site and the changes which the explosion had brought about. Like the residents in Emond's study of a residential home (personal communication), my "tour guide" had control of the tape-recorder, recording only what he wanted to record.

<sup>3</sup> Rose, personal communication 21 July 1997. The corpus of tape-recorded interviews shows that secrets were avoided.

<sup>4</sup> Rose: personal communication to Charles Kaplan, 23 February 1988; quoted with permission.

<sup>5</sup> The indexicality of words and retrospective-prospective nature of reading are exemplified by the argot "Nadsat", in Anthony Burgess' *A Clockwork Orange*. The achievement of "discovering the sense" the argot used by Little Alex, the novel's "humble narrator", differs between the book and the film, however. The narration, *qua* commentary machine, coincides with the visual image; a visual image which is based on Stanley Kubrick's interpretation of the meaning of Nadsat terms.

A further example becomes relevant here, from an incident in which the sense of words was assembled without the use of a visual image. When the Prince single "Sign 'o' the Times" was first broadcast on UK television, by Channel 4's *The Tube*, it was accompanied by an animated video, which had been specially made for the occasion by the programme itself. The producers of this video had only the song from which to make the video. As Prince sang the opening line, "*In France a skinny man died of a big disease with a little name*", the word "AIDS" appeared on the video. As such, the video provided the sense of "big disease". With the release of the video made by Prince, the "big disease" was associated with crack cocaine instead. The meaning of the reference to a "big disease" was thus altered retrospectively. As Prince was the writer of the song, his "crack" version was the "authorised" version.

<sup>6</sup> Lee's (1984:69) notion of "instructions for reading" is introduced in the section "The Visibility of Notices of Confidentiality" in Chapter Five. The header of the transcript under current consideration reads "Conference between Howard Schwartz and Harold Garfunkel (*sic*) 6-19-68".

<sup>7</sup> Cf. Schegloff's (1979) paper on identification and recognition in telephone conversations, wherein sequences of talk from telephone openings show that members recognise callers—or come to the

---

realisation that the caller is known to them. In Schegloff's paper, recognition is a sequential outcome. If the corrections on the Schwartz and Garfinkel transcript were made by the transcriber, it is to be supposed that, having transcribed the recording "first time round", they were able to recognise and distinguish between the voices on subsequent listenings.

<sup>8</sup> "Satisfactory" is an adequate term here, if readers remain unsure as to who said what and to whom.

<sup>9</sup> Citing both the original transcription of the interview and the recent reprinting by The Waiting Room Press is problematic; the transcript and reprint are not isomorphic. The headers which appeared within the transcript are omitted from the reprint, replaced with numerals to divide sections. These headers are discussed in Chapter Thirteen.

<sup>10</sup> From Rose's publication notes, in the posthumous reprinting of Gorman's interview with Anna Brown.

<sup>11</sup> Rose's conversation with Leroy was facilitated by Donald Hagler Cole, MD., a psychiatrist in Pueblo, Colorado. I wish to acknowledge Dr. Cole for permission to cite this conversation (personal communication, 18 December 1996).

<sup>12</sup> Jefferson (1996:160) would later refer to her transcription practices as "‘comic book’ orthography", which she uses "to capture pronunciational details with a conventional alphabet". Cf. Jefferson (1983:3).

<sup>13</sup> Reprinted with permission.

## **Membership Categorisation Analysis: Ownership, Possession and Recognising “Normal Appearances”**

This chapter builds upon the explications of membership categorisation activities developed so far in the thesis. This current chapter highlights some routine, constitutive activities that lay-members use, and sociologists as members incorporate as unexplicated resources into their research.

The cumulative considerations of this chapter encourage researchers to “look again” at the data they use and interrogate in their projects. This chapter identifies a series of culturally methodic practices which, when treated as topics of inquiry in their own right, undermine manipulations and refinements researchers employ in order to treat data as being data of a substantive nature.

As I shall set out in the final chapter (on socio-bibliographic studies), the researcher should seek out or be receptive to insights provided by antecedent work. Before making the analytic move from explicating the organisation of talk (sequential and categorial aspects of conversational interaction in interviews) to broadening the discussion of a particular topic (*viz.* security sweeps for suspicious devices), which recurred in interviews, this chapter outlines members’ (*viz.* interviewers’ and interviewees’) culturally methodic practices which are incorporated into the “products” of interviews.

### ***Ownership and Possession: “Hotrodders” and Studies in Membership Categorisation Analysis***

Sacks’ discussions of “hotrodding” provide a procedural basis for taking talk about the bomb as a topic of inquiry and moving beyond the bomb as a topic of talk.<sup>1</sup> This was a characteristic feature of Sacks’ work, whereby he used “therapy talk” to discuss more generic matters. “Therapy talk” is a gloss for talk which is produced in what members define as an orientation to and design for a therapeutic environment: whilst waiting for participants to assemble, whilst the therapist is out of the room, during the therapy session *per se*, etc., rather than a reification of talk-in-a-therapeutic-environment as therapy talk. The specific topic-talk of interviews reprinted in this thesis concerns security sweeps, where people are doing looking for a “suspicious” device, and from looking for a “suspicious” device this thesis moves onto talking about normal appearances more widely conceived.

Sacks’ “hotrodders” studies manifest the coincidence of categorial and sequential organisation of talk; that is, Sacks accounts for both membership categorisation activities and the turn-taking ‘apparatus’—often one in terms of the other—oriented to by the participants to the setting. These analyses are based on transcripts from a therapy session involving youths fascinated by automobiles. Conein (1997) signals how Sacks, through the linkage of knowledge to procedures of membership categorisation—ergo, studies of knowledge-in-use—was a sociologist of knowledge *par excellence*.

That is, Sacks' work is not reducible to the analysis of conversation, where conversation is equated with sequences of talk (D.R. Watson 1999:1). Lee and Watson (1993 *op. cit.*) regard Sacks' Conversation Analysis as providing sociology with a generic methodological approach for naturalistic inquiries.<sup>2</sup> D.R. Watson demonstrates how the application of conversation-analytic technology can be expedited in various settings, e.g. public space, as I explicated in the “navigation” section of Chapter One. Although Sacks studies the interaction within the therapy sessions, he is also able to provide detailed explications *apropos* of how, for example, a particular automobile is an assemblage of parts, and how that assemblage of parts is recognised as a hotrod by those who claim authority to be able to recognise a hotrod. This may be a complicated formulation, but then Sacks' explications are highly complex, as indeed are the activities of practical reasoning he explicates.

Hotrodding, as conceived by members, is a category-bound activity. To anticipate the bibliographic concerns of the final chapter again, it is relevant at-this-precise-point to consider the tacit restrictions on age-membership—from sixteen to twenty-five—of the “beautiful people”, a surfing élite on a strip of Windansea Beach at La Jolla, California (Wolfe 1987).<sup>3</sup> The age-limits of membership in the Pump House Gang were strictly enforced through linguistic activities. The hotrodders too are teenagers. However, Sacks observes (though not *qua* Aristotelian logic, e.g. ‘all hotrodders are teenagers but not all teenagers are hotrodders’) that the hotrodders are teenagers who have a particular orientation to cars, driving, and to other road users, which are distinguishable from “ordinary teenagers” orientations. For Sacks, to ask why hotrodders mobilise their

particular orientations to cars requires consideration of the categories of teenager and hotrodder, and the differences between them. Sacks argues these categories are

“fundamentally different types of categories; the “hotrodder” is, in a very nontrivial sense, quite a revolutionary kind of category”  
(Sacks 1979:8)

For Sacks, this “revolution” is found within the use and ownership of categories. He attaches an importance to the difference between the use of a category by members and nonmembers of that category. This difference in usage indicates

“that there are certain categories which are owned by a group other than those to whom they apply.”  
(Sacks 1979:9)

Sacks outlined this in an extensive manner, using the automobile discussion as starting-and finishing-points for his remarks.<sup>4</sup> Sacks coined the terms “possessables” and “possessitives” to formulate how members themselves recognise, claim and confer ownership; another relevance of these coinages is to elide the analytic problems of the term “possession” (Sacks 1992a:383). Material objects are “possessables” in that they can be owned. Sacks uses a subtle distinction to contrast possessables with “possessitives”:

“[W]e can distinguish between things that can be said to be ‘found’—and such things are ‘possessables’—and things which, having found them, you find them as things somebody has lost, i.e. where what you see when you find it that somebody lost it.”

(Sacks 1992a:384)

Sacks begins by explicating the use of the possessive pronoun “my”. Members claim membership *via* the use of possessive pronouns with Membership Categorisation Devices; Sacks cites as examples “my family” and “my country” (Sacks 1992a:383). Correlatively, members claim ownership *via* the use of possessive pronouns (“my”) with Membership Categorisation Devices, relational pairs and Standard Relational Pairs. Members recognise “affiliations” of pairs, e.g. “my wife”, “my ex-husband” and “my lawyer”, through a maxim such that where affiliations are “duplicatively organised”,<sup>5</sup> the member claiming ownership is an incumbent of one of the “pair-parts” is entitled to claim ownership (Sacks 1992a:383). Possessive pronouns also serve to locate possession as well as affiliation. However, it is not just through the use of a possessive pronoun that ownership is recognisable; that is, the possessive pronoun alone does not presuppose ownership. Affiliation and membership are claimed *via* the tying together of possessive pronouns and categories, which are recognised as belonging to particular membership categorisation devices. Material objects are recognised as being “owned” also, e.g. “women’s magazines”, “men’s magazines”, “adult magazines”, a “baby’s pacifier”.

Whether the material object referred to is categorially related, as in the aforementioned examples, material objects may “confer” possession, i.e. a possessive; Sacks illustrates this with leaving objects “on” or “at” seats or tables, in order to claim possession of seating space. Sacks then wonders if particular objects (rather than other objects) can be used in this way: if a purse is left on a table, does this suggest that the purse has been

positioned on the table to claim ownership of seating space, or has it been left behind when the owner vacated the table? This is, of course, a matter for members themselves.

The variety of material recognised as being “owned” by somebody or some “collectivity” (see below) in sociological reports includes places, e.g. which doorsteps belong to which groups of peddlers (Cavan 1972:195); or which areas belong to pickpockets. Activities of pickpockets, then, are similarly amenable to Membership Categorisation Analysis (see Chapter Ten). Sharrock’s (1974) paper on ownership can be brought to the study of observations on pickpockets, such as Maurer’s observations that the pickpocket

“has a certain proprietorship in his district or street; there is even a sense of pride about it. He may tell you frankly that he *owns* it.”

(Maurer 1964:173; emphasis supplied)

The selection of possessive pronouns is a feature of “recipient design”, whereby possessive pronouns may be used as substitutes for each other but may also be inappropriately selected, depending on the persons being addressed. As an example, Sacks cites the claiming of ownership of a car according to recipiency. Whilst it may be interactionally appropriate to say “my car” among a certain group of people, the car does not necessarily belong to me; it may belong to my mother instead. If my mother were among the people being addressed, I would not be entitled to refer to the car as “my car” (Sacks 1992a:610).

Possessives are then, according to Sacks, “somebody’s”. Furthermore, possessives are recognised by members to be “somebody’s”. That possessives are recognised to be owned, to be “somebody’s”, is a feature of culture, which

“does not merely fill brains in roughly the same way, it fills them so that they are alike in fine detail.”

(Sacks 1992a:245)

This is shown by a boy who, at seven years old, has been socialised into a culture whereby he is able to recognise and assert that something has been deliberately discarded; that is, that an object (*viz.* a crate) does belong to somebody, but that somebody has since abandoned it (Sacks 1992a:386-387).

Possessives are used as a resource by film-makers, whereby possessives are used, to adapt Schegloff’s (1972b) phrase, to formulate place. This moves on from possessives as “somebody’s” to possessives as “somewhere’s”, i.e. objects as standing proxy for somewhere, or *documenting place*. Film-makers can use a shot of e.g. the Eiffel Tower, the Golden Gate Bridge or St. Mark’s Square to indicate the next scene is to be recognised as taking place in Paris, or San Francisco, or Venice; even if the scene was not actually filmed in Paris, or San Francisco, or Venice. These famous landmarks, among others, are seen as being “owned” by specific cities and as documenting locations. Film-makers rely on viewers’ using culturally methodic practices to recognise the purported scene of the action as the location identified by the landmark, which “everybody knows”.

Before the deregulation of public bus companies in the UK, fleets of buses used standard liveries. Hence, if a bus appeared on film or television, the place was identifiable or “glance-determinable” (Sacks 1992b:539) *via* the recognition of its livery. So, if the viewer did not recognise the setting on screen from the environs, then from the presence of a red (or purple, or orange) bus the viewer recognises the setting as London (or Edinburgh, or Manchester). Although Hackney carriages or “black cabs” may be associated with London, the shot of a black cab on film is not as unequivocal at documenting place as a red bus, as various cities in the UK use black cabs. In the US, liveries are category-bound; e.g. to the membership categorisation device ‘stage of life’ (the yellow school buses), and to the membership categorisation device ‘postal organisation’ (the brown square lorries of the United Parcel Service). The trolley-bus amidst traffic identifies a scene in the US as more specifically San Francisco. A shot of a gondola identifies a setting as Venice.

Sacks’ notion of possessables and possessitives can also be applied to members’ knowledge, or corpora of knowledge. Using the analogy of “ownership”, Sharrock (1974) notes how knowledge relates to categories and Membership Categorisation Devices *qua* activities. That is, knowledge may be seen (by members) as possessitive. This chapter will describe how certain bodies of knowledge are seen (by members) to be owned by certain categories and categorisation devices, *inter alia*, mothers and parents, fans of blues music, drug-users, hotrodders. Thus, as well as material objects and category-bound activities, Sharrock notes how corpora of knowledge are recognised

as and are recognisably category-bound to particular membership categories or “collectivities”.

Giving the name of a collectivity to a corpus of knowledge, e.g. “Azande witchcraft”, works not only to describe the corpus of knowledge, but to indicate a relationship between the corpus of knowledge and the collectivity. That is, that knowledge is a possessive, whereby a corpus of knowledge is “somebody’s” knowledge. Sharrock suggests this relationship can be formulated in terms of “ownership”. Referring to a form of medicine as “Baka medicine” does not delimit the collectivity who know or use Baka medicine; the ownership metaphor serves to identify the corpus of knowledge (medicine) with a collectivity (the Baka). Those members who use Baka medicine, if not recognised as Baka themselves, are defined by the Baka (and perhaps by others) as “borrowing” or incorporating Baka medicine into their own medical practices. That is, members are defined by members, and knowledge is attributed to collectivities by members. Here it is important to note that the ethnomethodological programmes regard the definitional work is accomplished by members, not analysts. Alzamora exemplifies this attitude towards activities in a recent study of classroom interaction:

“it is not up to the analyst to stipulate if someone understood or not. Nor is it the researcher’s job to pronounce independently upon whether a given candidate understanding or its confirmation/disconfirmation are ‘correct’ or not: the researcher should not be taking sides or acting as arbiter.

“It is the interlocutor’s job to arrive at such a decision for the practical purposes of continuing the conversation. The research analyst’s job is to look at *how* the interlocutor arrives at the decision of understanding or not understanding and consequently what the structural organisation of the interaction is.”

(Alzamora 1999:77; emphasis supplied)

Similarly, the embourgeoisement thesis—that with increasing wealth and living standards the working class “took on” characteristics of the middle classes in terms of voting behaviour and aspirations—can be conceptualised using Sharrock’s metaphor of ownership. According to the embourgeoisement thesis, the working class people were not just becoming more affluent, their attitudes were changing also. The attitudes were no longer “working class attitudes” but seen as more “middle class attitudes”. The embourgeoisement thesis held that taking on the attitudes and aspirations, which were traditionally “owned” by middle-class people, indicated they were becoming middle class. The ownership metaphor clarifies the introduction of a form of methodological irony, whereby sociology can characterise members by subverting members’ understandings (the notion of “methodological irony” is returned to in Chapter Twelve). Rather than a change in society, workers were suffering from “false consciousness”. Whatever workers might think, they are borrowing attitudes that belong to a different collectivity.

Using the ownership metaphor, Hatch and Watson (1974:176) also attest to Sacks’ identification of methodological irony in comparative social research, wherein knowledge and categories owned by one culture are compared—unfavourably—with the knowledge and categories owned by the analyst’s (Western) culture.

Hatch and Watson (1974) borrow Sharrock’s metaphor of ownership to formalise the ways in which music is characterised by “competent hearers” (Hatch and Watson

1974:162). Competent hearers, in displaying their membership, attribute or characterise music as belonging to or being owned by periods in time;<sup>6</sup> in Sacks' terms above, forms of music can be seen geographically as possessives also—the “Seattle Scene”, the “Mersey Beat”, “Madchester”, etc.

“Members have methods for demonstrably contrasting “Black country blues” as opposed to “Black rural folk songs”, although the difference may be by no means “obvious” to a non-member of the blues community. In fact, non-members often confuse Black, rural, “blues” with rural “folk songs”. This potential confusion, among many others, is used by those in the blues community as an “indicator” of competent membership.”

(Hatch and Watson 1974:163)

Ownership confers obligations too. Members claiming incumbency of the blues community must possess and display the esoteric knowledge which properly “belongs to” or is “owned by” those members or competent hearers who comprise the blues community.

Previous work (Carlin 1994) presents extracts of talk from workshops, glossed as “parenting-counselling sessions”, held in a young offender institution by the volunteer organisation Second Chance. To repeat, the talk is not reified as “parenthood-counselling talk” but talk occasioned within parenthood-counselling sessions. The volunteers who ran the session were three women: two counsellors (denoted as CR1 and CR2); and another counsellor who operated the video-camera but did not participate in the sessions (CR3). In the extract reprinted below, the parenthood-counsellors iterate their “qualifications” for participating in the sessions. Their participation as counsellors in parenting-counselling sessions is ratified *via* their

categorial incumbency, *viz.* mothers. The provision of biographical particulars *apropos* of their family experience (Carlin 1994:187-189) demonstrates to the young offenders attending the sessions that the volunteers own that requisite corpus of knowledge gleaned from their own motherhood:

Extract 90 (1)

1       CR2   erm  
 2               so I'll tell you a little bit about myself  
 3       CD   ((cough))  
 4       CR2   my name's Liz  
 5       ((pause))  
 6               erm  
 7               I'm married  
 8               I got two sons  
 9               one's twenty  
 10          the other one's sixteen just left school  
 :  
 :  
 11          erm  
 12          I'm a great believer in family life  
 13          I think that it's a lot of hard work  
 14          and energy and a a big commitment  
 15          but you get so much back from it  
 16          sss  
 17          you get so much satisfaction  
 18       ((pause))  
 19          a er sense of achievement  
 20          and a sense of security  
 21          knowing you've built that family  
 22          (      )  
 23          and it's very  
 24          it helps you through life  
 :  
 :  
 25       CR1   (      )  
 26          like to tell us about her family experiences  
 27          ((laughs))  
 28       ((pause))  
 29       CR3   I can't film myself can I  
 30       CR1   ((laughs))

31 CR3 er I've got a girl of ten  
 32 a very naughty girl of ten  
 33 very naughty girl of ten but she's lovely  
 :  
 :  
 34 I don't know what to say really  
 35 I love I love family life  
 36 I love being a mother  
 37 I don't want any more children  
 38 one's enough  
 39 one is enough for me to cope with  
 40 erm  
 41 but it but it's great  
 42 it it's it's it's the best really  
 43 ((pause)) even having a naughty girl is wonderful  
 44 ((Group laughs))  
 45 CR1 I've got two naughty girls  
 46 ((Group laughs))

The mothering experience is elaborated *via* expressions of time (lines 9-10, 31-33), which suggest that mothering experiences have been ongoing; and number (lines 8, 46). Implicitly, then, the counsellors have been in possession of knowledge *re* motherhood for years, and repeated with other children. Extract 90 (above) occurred at the beginning of the course, in the first session. In the final session, the organisation arranged for a guest speaker—a nurse (N)—to talk to the young offenders about childhood accidents and first aid. The following extract of talk records how the nurse iterated her own experiential qualifications to discuss first aid:

#### Extract 81 (6)

1 N so that's about the most common things that you'll find that kids  
 2 ((pause)) could do to themselves  
 3 or accidents that could happen  
 4 ( ) oh but if you've got any questions to ask me then do

- 6 before I worked in the red cross I was in the ambulance (service)  
→ 7 and before that I was a medic in ( )  
→ 8 so I've quite a good range of experience of different erm  
→ 9 incidents and treatments and things  
10 so if you've got any questions then y'know  
11 do ask me  
12 ((pause))  
13 ((laughs))  
14 CR2 ( )  
15 ( )  
16 N get the brains working  
17 I'll give you a minute  
18 CR3 if you haven't got questions do you want a five minute fag break  
19 ((pause))  
20 CR2 ( )  
21 CR3 [ because we're going to finish at about half past about half  
past three today okay  
22 N okay

At the arrowed utterances above, the nurse N relates her experiences as a medic in different services. This claim to expertise is seen as the possession of a corpus of specialised knowledge by virtue of her training and professional experience. The expression of ownership of knowledge is similar to the owning of knowledge *via* motherhood, reprinted above.

Having made these observations on members' common-sense orientations to category-related phenomena, including the overlapping features of possession and ownership *re* objects, places and persons, the next section looks at the notions of ownership and possession and their relation to the implementation of research projects.

### ***Ownership: A Problem for Ethnography***

This section elaborates upon Sacks' discussions of "hotrodders". The topicalisation of membership categorisation activities and category-related bodies of knowledge brings unexplicated, though culturally available-culturally methodic practices into relief.

That categorial incumbents are required to display knowledge which they are presupposed to own by virtue of their categorial incumbency presents an ethnographic problem, which is locatable in the linguistic constitution of qualitative methodology, e.g. (covert) participant observation. As Kenneth Stoddart says,

"Research conducted via 'passing' requires that the researcher be seen by members as already possessing what it is he is there to determine, i.e. what every competent members knows. [...] The mere asking of questions became a somewhat 'risky' matter: to ask after the sense of something could be seen by a 'natural' member of the heroin-using community as evidence of my naïveté. I could not have known at the beginning of my research what would have transpired to have been evidence of my exclusion from the community and thus as grounds for some members to *doubt* the bona fide character of my presence in the scene of their activities. [...] I *commonsensically* saw that any evidence of naïveté could invalidate my appearance as a legitimate participant and thus truncate the research at a point not of my choosing."

(Stoddart 1974:174-175; emphasis supplied)

Stoddart's observations complement William Foote Whyte's admonition on participant observation techniques, that

"one has to learn when to question and when not to question as well as what questions to ask."

(Whyte 1955:303)

However, the thoroughly linguistic nature of sociological research—here, covert and non-covert participation—is more fundamental than knowing when to ask questions; the mastery of a natural language enables members to recognise an utterance as a question as opposed to e.g. an answer. During his researches in Cornerville, Whyte roomed with the Martini's, a family with whom he began to learn to speak Italian. By recognising the extent of his knowledge of another language, Whyte states that

“[w]hen I was careful to keep my remarks within the limits of my vocabulary, I could sometimes pass as an immigrant from the village of Viareggio in the province of Tuscany.”

(Whyte 1955:296)

However, a problem for Stoddart was knowing what remarks were within the limits of his vocabulary; or rather, which terms carried particular meanings-in-use for the heroin users in his study. The awkwardness of this situation became apparent for Stoddart after he had “violated” or breached the usage of argot an term, without knowing beforehand that the term carried meaning within the drug community:

“many of the things that heroin users did and said appeared to me as things that might be done or said by ‘anyone’, i.e. *independent* of locations *vis-à-vis* the heroin-using community. [...] I heard and responded [to the word ‘pinched’] not as an element of the heroin users’ argot but as a usage familiar to me by virtue of my participation in another domain, i.e. ‘conventional society.’”

(Stoddart 1974:175)

That is, whilst Whyte was hearing and speaking a language with which he was unfamiliar, *viz.* Italian, Stoddart encountered perturbations using a natural language he regarded as unproblematic.

Stoddart's paper also supports Becker and Geer's (1957) scrutiny of field methods. They argue that certain research methods can only make a partial understanding of a culture available to the analyst. Becker and Geer contend that *caveats* apply to the sociological interview, wherein oral responses to the analyst should not be received or accepted as being unproblematic. The meanings that interviewees attach to words may not be coincident with the analyst's interpretation:

“the cultural esoterica of a group may hide behind ordinary language used in special ways.”

(Becker and Geer 1957:29)

The locus of Stoddart's considerations is the word “pinched”. During ethnographic encounters with a group of heroin users in a large urban area, Stoddart came into contact with a variety of argot terms. *Via* the analytic practice of ethnomethodological indifference, his chapter is not a study of argot nor of a subcultural group, but is concerned with how argot terms are recognised *as* argot terms associated with a particular subcultural group through their use. Stoddart's observations complement the aforementioned chapter by Sharrock (*op. cit.*) in the same edited collection, *vis-à-vis* the “ownership” of knowledge, and the knowledge that members are assumed to possess by virtue of their categorial incumbency.

Stoddart realised the term “pinched” as a Shibboleth, whereby the incorrect usage of the term within the drug-taking community was an accountable matter; not knowing the meaning of the term (or password) “pinched” marked its user as an outsider. The correct usage of argot terms, *qua* litmus test, is indicative of group-membership: “One

thing about passwords of course is that they are passwords for some group” (Sacks 1992b:116).<sup>7</sup> Stoddart approaches his study of drug-users using the procedural policy of ethnomethodological indifference. As such, he does not afford privileged attention to argot but to how an occurrent term is recognised as an argot term. As Stoddart puts it,

“the essay is *not* ‘about’ argot but the procedures involved in the everyday business of hearing and understanding talk—whether ‘opaque’ or ‘transparent.’”  
 (Stoddart 1974:173; emphasis supplied)

Stoddart’s account of conversational encounters can be illuminated by the notion of “explicative transactions”, i.e. examining how an action is treated.<sup>8</sup> Initially, Stoddart does not take the use of the word “pinched” as problematic. However, a series of explicative transactions, available in fieldnotes, retrospectively informs his responses to and use of the word “pinched”. In Stoddart’s treating “pinched” as a synonym for “arrested”—

“I asked Hughie what he’d been arrested for”  
 (Stoddart 1974:175)

—his interlocutor’s answer suggested that the word “pinched” was more specific:

“‘For junk. What did ya think?’”  
 (Stoddart *ibid.*)

The explicative property of this exchange derives from the correction, through which Stoddart learns that his response was inappropriate for its recipient: “pinched” is not being used synonymously with “arrested”.<sup>9</sup> “Pinched” is almost synonymous with the

phrase “being busted”, however, which is defined as “being arrested for the possession of drugs” (Cavan 1972:152)—whereas “pinched” is specifically referring to heroin.<sup>10</sup>

An instance of Stoddart’s use of the term “pinched” is also reprinted:

“During the course of a conversation Hughie asked me if I’d been to the Family Café recently. I replied that I had and he asked ‘Anybody been pinched?’ Earlier in the week I’d heard that Lynn, a lesbian friend of our mutual acquaintance, had been arrested for vagrancy. I thus replied: ‘I heard Lynn was.’ Hughie expressed some surprise and said: ‘Jeez, I didn’t even know she was using.’ I told him what she’d been arrested for and he said ‘Oh, I meant for junk.’”

(Stoddart 1974:178)

By passing as a *bona fide* member of the heroin-using community, Stoddart is assumed to own a certain corpus of knowledge by other heroin-users; this corpus of knowledge includes the community-specific meaning of the argot term “pinched”. Thus, when Stoddart inadvertently topicalised the word “pinched”, by asking for further clarifications, his requests are treated as sarcasm rather than as indexical particulars that challenge an underlying pattern *apropos* his competent membership.

Stoddart’s paper contributes to concerns of categories and ownership by detailing how incumbents of a category are recognised to be in possession of a certain corpus of knowledge, and how categorial incumbents are obliged to display the competent use of category-bound knowledge. This returns the reasoning or explication of categorisation to Sacks’ assertion that “hotrodder” is a revolutionary category. As mentioned at the beginning of this chapter, Sacks argued that the category hotrodder is revolutionary because categorial incumbents own and determinate its application. How categories are used is indicative of category-membership, whereby

“if [such a category] is used, [it] would be recognised to be used as a nonmember.”

(Sacks 1979:9)

Sacks cites as an example the Biblical reference “Abraham the Hebrew”, where the use of this category at that time suggests to Biblical scholars that this particular passage in Genesis was not written by a Jew (Sacks 1979:8). Similarly, referring to an acquaintance as “[a] half-caste acquaintance” (Mead 1977:30) suggests that the writer is not a co-incumbent of the category as used. Again, during her first period of fieldwork in Samoa, Margaret Mead discussed lines of succession in unproblematic terms, which revealed her relation to the categories she employed.<sup>11</sup> The articulation of categories is thus seen to formulate the position of the referrer *apropos* the articulated category. Such phenomena differ in their particulars from insiders’ terms for outsiders, found in argots or informal words, e.g. the Pump House Gang referring to old people as “black panthers” (Wolfe 1987). Another example of this phenomenon is found in Francophone cultures, which refer to people on the British mainland as “Anglo-Saxons”. This presents the inhabitants of mainland Britain as a single, unified culture, rendering invisible the ethnic identities of those who would *not* consider themselves to be Anglo-Saxon. The pronunciation of categories may also indicate members’ relationship *apropos* of categories: those unfamiliar with the location of an aluminium works at Distington, near Workington, would pronounce the silent *t*; not only is the *t* silent, the *s* is emphasised, i.e. “*Dizz-ing-ton*”. The radio disc jockey Steve Wright, after a traffic report, apologised on air for mistakenly giving Brighouse, West Yorkshire, a silent *g*. Similar local pronunciations, which act as “passwords” for categorising

insiders and outsiders, are occasioned by Sowerby Bridge, West Yorkshire—"correctly" pronounced to rhyme with *sew-erby*, rather than *sow*; and Towcester, North Hampshire, "properly" rhymed with *toaster*.

The "revolution" is apparent when category-members themselves, rather than nonmembers, determine who is to be recognised or seen as a member of that category. This is not a process of initiation but rather an assessment of a candidate member's credentials or *bona fides* by category incumbents.

The pivotal aspects of the hotrodder category, according to Sacks, are the identifying characteristics of hotrodders. We can see here how Sacks' analysis complements Garfinkel's notions of quiddities.<sup>12</sup> The categorial work done, that is, iterated by category incumbents, reveals the quiddities or identifying characteristics of the category: the categorial features that define a hotrodder according to other hotrodders, i.e. incumbents of that category; and how category incumbents define and recognise other hotrodders as hotrodders rather than categorise them as other teenagers.

The aim of hotrodders is not just to build and drive fast cars, but to build and drive the fastest car. Driving fast is not the necessary or only criterion for being a hotrodder, however; being a hotrodder is implicated with being recognised as a hotrodder by other hotrodders. This is partly why Sacks refers to "hotrodder" as a "revolutionary category", i.e. unavailable to adult intervention. For Sacks, the hotrodder category is

revolutionary not just because it is unavailable to adult intervention but, unlike the category “teenager”, it is incompatible with adult intervention.

Sacks’ discussion of hotrodder as a revolutionary category explicates the “viewers’ maxim” (Sacks 1974a) discussed in Chapter One. The viewers’ maxim holds that if a member can be seen as an incumbent of a membership category, then see them that way. In his discursus on hotrodders, Sacks states that

“If somebody’s driving a car and that somebody could be seen as a teenager, then they’re seen as a teenager.”

(Sacks 1979:11)

Adults were teenagers themselves once (“adult” and “teenager” are categories from the membership categorisation device “stage of life”). The category “teenager” is available to adults, e.g. category-bound activities (“that’s the sort of thing that teenagers do”, etc.), and the category “teenager” is visible for everyone to see. The category of “hotrodder” is special in that recognising a teenager does not necessarily enable the adult (or teenage) observer to recognise a hotrodder. In that being a member of the category hotrodder is dependent on the recognition of membership by other incumbents, intervention by adults is impossible because it “sets up an administration of the phenomenon which is not theirs.” (Sacks 1979:12). Hence, a production model of a car may be a car for an adult; however, for a hotrodder, a production model car is a source of auto parts:

“A new car is not something that is to be driven as it happens to be purchased, but that one now goes to work on.”

(Sacks 1992a:603)

The hotrod is in the process of development through the assemblage of a body from an *X*, an engine from a *Y*, combined with a rear end from a *Z*.

As competent hearers of the blues community engage in categorising and naming styles of blues (Hatch and Watson *op cit.*), so incumbents of the revolutionary category “hotrodders” categorise, describe and name cars. Cars have alternate yet not necessarily contradictory descriptions. The selection of descriptor by a speaker makes available to hearers the relation the speaker has to a corpus of knowledge *apropos* cars; e.g. describing a car as a “Ford” *versus* a “Thunderbird”. Indeed, the naming of cars and auto-parts by constituent members of the category hotrodders is “elaborate” (Sacks 1992b:117). Cars and auto-parts are given names by hotrodders, which are not the standard names of production models.

The naming of cars is not only a device for defining categorial incumbents:

“It’s not just that we’re giving it a name so as to avoid calling it by the name that everybody else is calling it by, but that what anybody else would call it wouldn’t be correctly naming it.”

(Sacks 1992b:118)

That is, in that hotrods are “conglomerates of various brands’ products” (Sacks 1992b:118), cars and auto-parts have names which are ratified by hotrodders themselves. By “ratified names” I refer to names for vehicles and auto-parts, which are

acceptable names of vehicles and parts for hotrodders. Hotrodders, in claiming to be incumbents of the category, are obliged to display the corpus of knowledge that hotrodders are presumed to own. These claims are open to evaluation by incumbents of the category. For example, describing an engine as a motor is seen as a correctable:

- KEN: Hey – Hey check this: I went down to uh H-Hollywood All--- down to the Holla- Ho:lywood, (1.5) uh Automotives?
- AL: Mm hm?
- KEN: And they told me how I could stick a th-uh:: Thunderbird motor? (1.0) – in my Jeep? And I bought a fifty-five // Thunderbird motor.
- ROGER: Not ‘motor,’ ‘engine.’ You speak of electric ‘motor’ and a gasoline ‘engine.’
- KEN: Okay
- (AL): ( )-
- AL: Yer no/ /t
- KEN: ‘Engine.’ // Okay–
- AL: ‘Internal combus-tion.’
- KEN: Alright, so // lookit
- ( ): mhhh
- KEN: I moved this thing in the Jeep, yesterday, an’ it took me all day to do it.
- ROGER: Why the // hell you gonna put a Ford in a Jeep.
- KEN: (An’ nen I)
- KEN: This thing is the hottest thing in town!
- ROGER: Fords aren’t hot,
- KEN: That Ford- In comparison to that old four cylinder I had in there it’s hot,
- (Sacks 1992b:114)

This corpus of knowledge includes the ratified names of assemblages of parts, the esoteric names of individual parts, how models of the same brand compare with each other and how they compare with models from other brands.<sup>13</sup>

For adults (or more correctly, for non-hotrodders), installing a new engine into a car is a large undertaking; it is an accountable matter as to why a new engine is being fitted rather than buying a new car instead. As mentioned above, hotrodders aim to build the fastest car they can (“the hottest thing in town”). So for hotrodders, it “goes without saying” that replacing the engine is intended to make the car go faster, i.e. that engines are replaced with more powerful engines. For hotrodders, replacing the engine is not an accountable matter in the same way as it is for non-members of the category “hotrodders”. To Sacks, Roger’s request for an account for Ken’s actions, “Why the hell you gonna put a Ford in a Jeep?” “locat[es] the world in which the talk is going on” (Sacks 1992b:116). If a hotrodder had been asked *why* he was replacing the engine, the question would have challenged the questioner’s claim to membership of the category, or, as Sacks is quoted above, the questioner “would be recognised [...] as a nonmember.”

Not all youths driving fast cars are hotrodders. Their status as a hotrodder is contestable, as conversations within the therapy sessions show. Within the therapy sessions, Sacks shows how certain youths claim to be hotrodders, while others (hotrodders) claim the authority to adjudge these claims. A claim may be made for a certain auto to be a hotrod, which may or may not be accepted. Similarly, a claim may be made to be a hotrodder, but to be in possession of a vehicle which is acknowledged to be a hotrod by incumbents of that category does not necessarily guarantee that the claimant will be accepted as a hotrodder.

This section has looked at members' usage of ordinary language activities as reported in published accounts, and how categorial features such as ownership and possession of knowledge and activities are routinely available in everyday life. For example, the routine use of the viewer's maxim to recognise a person as an incumbent of a particular category.

### ***From "Hotrodders" to "Normal Appearances"***

This section continues with the established themes of visibility and recognisability. It uses the framework of Membership Categorisation Analysis outlined in this chapter to show how the category-related features of persons, places and objects are practical matters for members in their routine, professional activities. This section anticipates future concerns within the thesis *apropos* the linguistic constitution of research practices, by introducing a corpus of data derived from "interview environments". Thereby, this section ushers in issues arising from the bombing of Manchester.

Sacks frequently referred to the therapy sessions in his work, using the data to explicate a large array of social phenomena which he elaborated upon during "oral disquisitions" (Coulter 1995:327), including, *inter alia*, considerations of topic, stories and story-telling, jokes, membership categorisation, identification, inference, introducing new categories (or "character appears on cue"), measurement systems, logic, methodology, conversational activities (e.g. greetings, introductions, invitations).<sup>14</sup> In an editorial note, Jefferson observes that

“[o]ne way that Sacks worked with a phenomenon was to present a rather elaborate consideration of it at one point and thereafter track it across the various materials he examined”

(Jefferson, in Sacks 1988/89:45)

This particular *modus operandi* has implications for this thesis: members (users of natural language, i.e. people as well as the analyst; e.g. interviewees as well as the interviewer) themselves suggest and identify topics for further analysis, and organise that analysis. Such topics as identified by members receive attention and consideration *via* a wide variety of settings and circumstances. For example, in looking for a bomb people are involved in an “incongruity procedure”.

Searching for a bomb is a tremendously complicated and highly pressurised activity, and my considerations on the work of looking for a bomb should not be taken as a trivialising set of observations. I do not wish to imply, for instance, that looking for a bomb is a simple activity, which “anybody” could do; nor that looking for a bomb solely requires the use of one’s common-sense knowledge. Looking for a bomb is a demanding operation *in extremis*, and the demands are exacerbated by intuitively apparent factors such as time: find and defuse the bomb before it explodes, injuring or killing people within the vicinity. However, looking for a bomb relies upon members’ “competence at observing scenes” (Sacks 1992b:235). A security sweep for explosive devices in settings such as public libraries or railway stations requires competent members—members familiar with the “normal appearances” of the setting, to remain vigilant *apropos* the normal appearances of the setting

“so as to say that the scene was as it usually is, or that it wasn’t what it usually is but some variant of what it usually is.”

(Sacks 1992b:235)

Looking for a bomb is an activity which is related in its constituent features to other activities, and my considerations of the work of looking for a bomb point out analogous situations where the recognition of a person or thing as a particular “kind” of person or thing is a consequential phenomenon. In this way, some formal properties of activities are presented in a sociological manner.

“Looking” is a generic, cultural activity. Whilst a janitor looks at an assemblage of objects and “recognises trash” (Schwartz and Garfinkel 1968:31), so too does a seven-year-old boy mentioned above (Sacks 1992a:386). The expertise of police officers in looking and recognising is developed out of ordinary competencies. Sacks notes that this is both “irksome” and constraining for police officers, for officers have to be prepared to articulate the reasoning behind any arousal of their suspicions, which in any case are subject to a criterion of not being unreasonable to lay persons:

“[The] warrant in particular cases is that the inference made is one which ordinary persons would make.”

(Sacks 1972a:288)

As the police rely upon commonsense practices and incongruities, so they rely upon the intervention and vigilance of lay persons. This is observable in public places, where members of the public are asked to report any suspicious objects or packages. In places such as railway station foyers and airports, members (referred to as “passengers”) are

requested to report suspicious packages and not to leave baggage unattended (see Mona's comments below). Physical objects and places undergo transformations according to the "finite provinces of meaning" that are put to use. Places and objects can be reconstituted according to different language games:

"For the police, objects and places having routine uses are conceived in terms of favorite misuses. Garbage cans are places in which dead babies are thrown, schoolyards are places where molesters hang out, stores are places where shoplifters go, etc."

(Sacks 1972a:292)

In the Summer of 1994, Fred Templeton and Edward Rose gave a course—"Field Experience in Sociology"—at the University of Colorado, Boulder, through the Department of Sociology. In one lecture, dated June 15<sup>th</sup> 1994, Templeton alludes to this section of Sacks' paper:

"In the city, there are a limited number of places that I can go without becoming conspicuous [...] Police look to people who are out of place. [...] You see a man around my age hanging out around playgrounds, people raise their eyes: 'What in the world is he doing—"loitering", the word becomes—about a playground?' You're supposed to have a *reason* for being places, except a relatively limited number of public places where you can be *without* a reason."

(Templeton, in Templeton and Rose 1994)

These notions of "becoming conspicuous" and being "out of place" are reiterated by Phillip Bourgois:

"From the perspective of the police, I am an obnoxious provocation violating New York City's unwritten apartheid laws: the only reason for a 'white boy' to be in the inner city – especially after dark – is to buy drugs."

(Bourgois 1998a:38-39)

Following both Templeton's (*op. cit.*) and Bourgois' (*ibid.*), there must be a reason to be some place at some time, and these reasons may be—for organisational (*viz.* law enforcement) inferences—nefarious.

During a series of interviews after the Manchester City-Centre bombing, members told me of security sweeps and suspicious appearances. The extracts which are reprinted below may seem longer than necessary. However, the use of extensive extracts avoids somewhat the problems of selectivity, whereby analysts may choose to reprint expeditious or “unrepresentative” quotes at the expense of others. Extensive extracts do not rely upon decontextualisation from a bloc of talk. Furthermore, the interactional nature of the interview is preserved (see the following chapter).

As mentioned elsewhere (Carlin 1998), the topics of study were suggested and organised by members rather than the analyst. “Ordinary” people, the participants in this study, are “articulate observers of the local scene.” (Gold 1974:ix). Their articulate observations are made in ordinary language and captured on a tape-recorder. Indeed,

“[this researcher] did in fact interview some very impressive natural social analysts, learned much from them, and relied heavily on their analyses of the [effects that the bomb] was making in their own lives and in the lives of others in the area.”

(Gold 1974:ix-x; brackets added)

Further than this, in re-creating Rose's "Larimer Street" ethnographies, I intended that the natural social analysts *organised* the project through the use of their own natural, social categories.

The raising of this topic was unsolicited, and prompted inquiries into security sweeps in subsequent interviews. Extensive extracts also show the adventitious nature of the generation of topic, by revealing the topics to have been raised by members rather than purposefully elicited from them by the analyst.

Initially, consider the following:

1. AC: Hmm. I was wondering if I could ask what's it like living and (.) and or working in the city centre now?
2. MONA: Well I only work in the city (.) I don't actually live in Manchester.
3. AC: Okay.
4. MONA: I live in I commute from Pudsey West Yorkshire every day. As for (.) for working (.) this end of town I wasn't actually working the day that it happened
5. AC: Okay.
6. MONA: I heard about it on the news and everything. This end of town when I got in on the Monday, I didn't know what to expect, and it was relatively, relatively untouched. Erm, the cordons went off there were various cordons up and eventually there was just the main one round the immediate sort of epicentre of the blast if you like round by the Arndale. And I know for a while (.) people were just going into, gawping and gazing at (.) all that had gone on
7. AC: Hm.
8. MONA: and you'd sort of say "I'm just going out to such-and-such a shop" and then you'd remember it wasn't there anymore! ((laughs)) There was that

kind of thing. And there was a feeling of (.) I suppose you'd call it "wartime spirit" ((laughs)) although not having lived through a war I wouldn't know what that was like but sort of a camaraderie. Everybody was determined to get on with (.) with everything. People who had actually been working all had tales to tell of what they were doing when it happened. But that lasted for a couple of weeks or so. And certainly working here in the library we became very much aware of making sure some people didn't leave bags and things around, which in a building like this where you come to study, people do!

9. AC: Hmm.

10. MONA: So security was heightened a great deal, there was a lot more awareness. I'm not saying we were complacent in the first place but

11. AC: Hmm.

12. MONA: you know, we were chasing up people, and if bags were left they were being carted away

13. AC: Hmm.

14. MONA: this kind of thing. Erm, but after about a month or so, it was just "aw we get on with it". And even now (.) erm, it's surprising how much of it has sort of, I don't know (.) is normal, you know.

In this extract, then, Mona herself raises the locally-specific issue of security in a public library. The security issue involved is not textually mediated, *viz.* the adhesive stickers in carrels and benches warning of thieves "working in this area". This existing network of notices intends to alert library-users to the possibility of theft, to be responsible for keeping their belongings safe. That is, the security of each individual's own belongings. A new security regimen, where "there was a lot more awareness", impresses upon library-users and library-staff alike that unattended bags within the library impact upon every individual's security:

"And certainly working here in the library we became very much aware of making sure some people didn't leave bags and things around, which in a building like this where you come to study, people do!"

Mona's remark (§8) is illustrative of Sacks' notions of normal appearances and the distinction between possessables and possessives. Unattended bags are familiar features in the library, which do not merit attention. Unattended bags are seen as bags left by a library-user who has left the desk or carrel temporarily, e.g. to browse among the shelves, visit the bathroom, or purchase a cup of tea in the basement café, and will return to collect the bag shortly. Sacks saw this as a "generative property" of possessives (Sacks 1992a:385), in that unattended bags could be seen not just as "somebody's bag" but as documenting a carrel or desk as "somebody's place". From an unattended bag in the library, it becomes inferentially available that "somebody's sitting there". Under the new level of security, however, unattended bags were regarded not as possessives but as potential threats to security, e.g. explosive devices. This new underlying pattern—that unattended bags were not necessarily unattended bags but seen as, potentially, deliberately planted explosives—eventuated in the removal of unattended bags, as iterated in §12:

you know, we were chasing up people, and if bags were left they were being carted away

In a subsequent conversation, another discussant told me about occasions upon which they, similarly, were engaged in attending to the "normal appearances" of a setting. (However, this next setting was not a reference library.) Again, an extended extract is reproduced: as mentioned above, this procedure allows readers to adjudge whether

"relevant" passages are only relevant through having been decontextualised, i.e. whether the provision of "sense" or interpretation is an analyst's achievement. Also as mentioned above, readers may adjudge whether, or indeed to what extent, the quoted remarks have been elicited.

1. AC: Can I just ask then has the place changed for you?
2. FRANK: Changed? Oh yeah it's changed. It's er, it's er I mean for a start er immediate change if you like is not being able to get your sandwiches like you used to do. Cos right across the road at erm (.) on the Exchange building
3. AC: Right.
4. FRANK: There was a little place called Bacon and Eggs. And I think all of our train crew and that we all got our sandwiches from there every morning like (.) breakfast and all and a bacon sandwich was from the Bacon and Eggs but of course it's gone, blew up, gone
5. AC: Hmm.
6. FRANK: Severely damaged. There's Wally's that used to be there that we used to have erm the engraving done for the first aid, er first aid trophies and things that we won competitions in. That's gone. Erm, I don't know (.) I think as many people are using the station as did before
7. AC: Ah.
8. FRANK: er, I don't think there's any change to that. But the surrounding landscape's changed a little bit
9. AC: Ah.
10. FRANK: Basically because of the amount of work that's taking place. I mean, what'll it be like when it's finished, I don't know, but at the end of (.) there's as many people using the place but it it's changed slightly, it there's there's there is a change, a definite change. shops that we went to are no longer there. When you're walking besides the Cathedral and that now there's work being done on the Cathedral there's work being done on a, on the buildings that are that are directly opposite, you've got walk you've got walkways, you're walking round scaffolding, and there's a there's a constant sound of construction taking place off the Arndale day and night

11. AC: Hmm.
12. FRANK: as a as they as work to to rectify the damage and that
13. AC: Hmm.
14. FRANK: So yeah, there's a there's a definite change, there's a definite (.) change to the layout of the station as well. I mean (.) you know it's just (.) but then again that was changing anyway, you know what I mean it was like the the city any city has a normal progression of change anyway new shops
15. AC: Hmm.
16. FRANK: old shops getting knocked down, new buildings going up. I don't know, I don't I don't know that it's (.) the biggest thing of course is the amount of work being taken on the Arndale, and of course it did affect er, er, we had train crew that sign on at Victoria that have to walk to Manchester Piccadilly and that and at Oxford Road, and we have agreed walking times to these places, so we had to re agree the walking times to those places again because initially you couldn't get round the city. Where whereas you could they could walk out walk straight down Corporation Street
17. AC: Hmm.
18. FRANK: er, do a hang a left by the Town Hall and go down the Oxford
19. AC: Hmm.
20. FRANK: the Oxford Road they couldn't
21. AC: Hmm.
22. FRANK: because initially that was all shut off so men they had to go come out of the station, do a right, go down onto Deansgate and then walk the length of Deansgate.
23. AC: Right.
24. FRANK: And then up and into you know what I mean it it's
25. AC: It's a long way.
26. FRANK: It's a long way. Erm, or they had to walk up, turn, they could go up turn go up follow the tramlines up over over erm (.) to Lew the Lewis's

building (.) and then and go down that way, towards Oxford Road. So it was a the the walking time was increased. Erm, Piccadilly the walking time wasn't really (.) er, vastly increased, and it was it was initially because of the, the cordoning of the city because it was about (.) I think that all told it was about six days before they actually it had started to open the city up

27. AC: Right.

28. FRANK: to be able to go across. I mean they were (.) thinking back they were gathering evidence of the I mean if that that van went everywhere. I mean there were bits of it on the roof here and all sorts. Er, so they they they kept it cordoned til they got all the bits of it like.

29. AC: Mm.

30. FRANK: Erm. So it was it was fairly involved you know, it was, it changed things in a lot of ways, I mean, course the (.) even when you restore the walkways and that people have got used to walking the other ways ((laugh)) so they won't go the other way anyway ((laugh)) you know what I mean

31. AC: Yeah, hmm.

32. FRANK: ((laugh)) it's a bit it's a bit like creatures of habit with it, you know, keep walking like ants down there, go left go right. And then you change it. And then when you put it back again they say sod that we're going that way now you know

33. AC: ((laugh))

34. FRANK: It's it's (.) yeah, so there are changes. There are, there are changes that have taken place that you haven't really realised they've changed, but they have, they've been definite changes to the way we go about our working lives. And of course it meant changes within the station because our security awareness went up. Er, right up until the cease fire of the IRA recently

35. AC: Hmm.

36. FRANK: er, whenever there was er er the security level went up, as advised by the [ ], we started doing er (.) hourly checks of the station. (.) We already check the station anyway but

37. AC: Hmm.

38. FRANK: we went to hourly checks whereby we actually detach staff a member of staff to actually walk the station. And it's the car park, and round the building, round, round and round the building kind of thing just to look and see if there's anything suspicious. Packages (.) er, we have curtailed the parking of cars on the front, station front. All kinds of things. You know, it it the these are changes that we never did before.

39. AC: Hmm.

40. FRANK: I mean, some of the initial changes with the (.) er the troubles with the IRA of course was the loss of er lost property offices in pl in major railway stations. Erm, (.) the lockers went because it's their favourite place to stick a bomb you know what I mean

41. AC: Hmm.

42. FRANK: I mean you didn't all you'd need was a quid or something just stick in the thing, open it up shove it in (.) erm, but right to the point now we, we can have a security alert and the first you know about it is when you see all the BT police coming round to sweep the station.

43. AC: °Hmm°.

44. FRANK: And then you'll be instructed then that there's that that staff-wise you've got to do your own sweeps, every hour twenty four hours of the day and night

45. AC: Mm.

46. FRANK: And we've done about four of them, since the bomb

47. AC: Right.

48. FRANK: We've actually been involved (.) when I say four (.) talking maybe, first one I think lasted about five weeks (.) where we actually combed the station day and night. We had members of staff going (.) just walking round

49. AC: °Hmm°.

50. FRANK: To all intents and purposes it was just walking round but they were actually walking round to see that there was nothing suspicious, and that no, er, bags or whatever had been left lying about

51. AC: °Hmm°.

52. FRANK: But I've been in, a couple of times there's been a few false alarms

53. AC: °Hmm°.

54. FRANK: not where we've sent for the bomb squad but where we've (.) taken  
that taken the necessary action to sort it

55. AC: °Hmm°.

56. FRANK: and then discovered it was nothing

57. AC: °Hmm°.

58. FRANK: and then went back. (.) erm (.) the last one we did lasted about  
should have lasted a week went on for three. And then we had the ceasefire  
and we ceased to do it, we're not (.) we still check the station, don't don't  
get me wrong, it's erm, the [ ] on duty still has his daily he he he  
has a a check of the station, usually three times on his shift. He'll he'll check  
out the station anyway as a matter of course

59. AC: Right.

60. FRANK: Er, we had er, all kinds of directives from the thing about advising  
the staff, that's train crew, drivers and everybody basically to be more alert  
to watch what's going on round about them. Er, those are still in force.  
There was there was a there was although I would be willing to say that  
people have since the ceasefire have eased off in that

61. AC: °Right°.

62. FRANK: Erm, (.) yeah there, so there was a, there was quite a lot of changes

63. AC: Hmm.

64. FRANK: to the, to the way we did things

65. AC: Right.

66. FRANK: apart from physical changes to the nature of the surroundings round  
about. Oh yeah. (.) I would definitely say there was.

To exemplify the pursuance of this topic of inquiry, which was suggested by the informants above, the following extract preserves and displays the direct asking of looking for a bomb.

1. AC: That's great! Now I presume you've had experiences of bomb alerts before.
2. PO: Erm, all too often, erm. Generally speaking they are (.) hoaxes, erm but of course we have got a system of recognised codewords (.) something which has developed over a period of time and um if we do receive a message which contains a recognised codeword then obviously we have to take it very seriously and erm, react quickly
3. AC: Mm.
4. PO: and erm so yeah there's there's hundreds and thousands of hoax calls but every now and then you get a coded warning and then
5. AC: Mm.
6. PO: you know you've got a problem
7. AC: Mm. Thankfully it's not always it's a small proportion compared
8. PO: Very small, very small.
9. AC: Mm. And if you get a coded bomb warning how do you go out and find a bomb?
10. PO: Well generally you will have (.) the message will give you a certain amount of information, um, which may not be very accurate (.) erm it may just say erm that there is a particular type of vehicle (.) within an area not necessarily a specific area. Um, you've then got to send officers out as quickly as possible to try and identify where that vehicle is or or erm you know it may just be a erm a threat that there's a device in a building. (.) And erm ((cough)) of course if it's if it's a coded warning then if it's a building you can be evacuating
11. AC: °Mm.°

12. PO: But um one of the biggest problems that we certainly had with the bombing last year (.) um and it's a social issue, really, and that's a matter of respect for authority. Um, I I've I've used the expression before (.) many of my officers

13. AC: Aha.

14. PO: used their full repertoire of social skills to try and get people to go because people don't believe anymore. They don't respect. So um, people were being asked to move, they were being told to move, they were being shouted at, and some of them were kicked down the road (.) because they wouldn't leave (.) until there was a very loud bang and then they left.

15. AC: Mm.

16. PO: But of course um it meant that some people could have been much further away um (.) but weren't and of course one of the big hazards with an explosion of that size is the amount of flying glass. And you know once you get that sort of glass flying then you get a lot of injuries.

17. AC: And you don't know how big the bomb's going to be until

18. PO: You don't! You've no idea. On this particular occasion ((cough)) the fact that is was in a lorry (.) gives you a pretty good indication that it's something special, that it's big and in fact the damage area in the city centre was over a mile across. Um, obviously toward the edges it was only a small amount of damage erm but it was a significant (.) erm distance and of course it certainly taught us a few lessons about um when you when you are trying to secure a scene like that, to put a cordon round it and to get people out, erm we used to work with certain rules of how many yards we should be away at er in the future ((telephone rings)) with things like that we shall just erm get people as far away as possible. ((answers phone))

19. ((PAUSE IN RECORDING))

20. AC: ° Didn't want to record any details of that. °

21. PO: ° It's all right. °

That is, PO's talk on this topic was elicited "more directly" than Mona's and Frank's talk on this topic. I deliberately use the qualification *more directly* because all three sequences of talk reprinted above are extracted from interview situations; that is, *they*

*are all elicited accounts.* Furthermore, they are all *collaborative* accounts, in that their talk was ratified and encouraged by the production of utterances by the interviewer.

However, in response to the questions “I was wondering if I could ask what’s it like living and or working in the city centre now?” and “Can I just ask then has the place changed for you?” both Mona (and myself, *via* my production of cooperative, collaborative utterances) (§8-12) and Frank (and myself, *via* my production of cooperative, collaborative utterances) (§34 *et seq.*), proceed to describe security sweeps and security alerts as part of their answers.

In contrast, I asked the Police Chief the question “And if you get a coded bomb warning how do you go out and find a bomb?” (§9) directly, as a topic for investigation that had been suggested to me by Mona and Frank. Adapting William Foote Whyte, this was a question which I would not have had the sense to ask.

### **Conclusion**

A crucial feature *re* the recognition of “normal appearances” is ownership. That is, does a person’s presence in a certain setting, at a certain time, appear incongruous with the normal appearances of that setting? And does a particular object belong in a certain setting? Members familiar with specific settings, e.g. a railway platform, recognise objects that are out of place on the platform, or do not belong on the platform. That is, members familiar with a railway platform can recognise and distinguish between possessables—objects that belong to persons—and objects that do not belong on the

platform—suspicious objects. Similarly, members familiar with libraries can recognise possessables, e.g. persons' bags and pencil-cases, as belonging to particular library-users: what does and does not belong in the library, whether a bag or pile of books is claiming ownership of a desk or carrel, whether such possessables have been innocently left on a desk whilst the owner of such possessables as a bag or a pencil-case is browsing among the library shelves, or consulting the library catalogue, etc.

Such category-related features as the categorial ownership and possession of bodies of knowledge, the categorial ownership of objects, present a “layer” of practical reasoning to the researcher. In approaching their data for research projects, the researcher who treats data as unproblematic sources of detail on substantive topics falls foul of the topic and resource distinction outlined earlier in the thesis. That is, data such as “interview data” incorporate the culturally shared, linguistic practices of lay-members and sociologists *apropos* membership categories and category-related phenomena.

The researcher could, alternatively, elect a methodological approach which accounted for members' sense-making practices. In recognising the *elicitation* of accounts by the interviewer, and the *conjoint production* of accounts by interviewer and interviewee, we can see that the interview is a research “instrument” based upon members' linguistic activities. The elicitation of accounts, and the collaborative nature of accounts produced in interview settings, shall be treated more extensively in the following chapter. The next chapter shows how interviews are linguistically constituted, socially organised, conjoint activities.

<sup>1</sup> I shall be using, particularly, Lecture 15.2 (Spring) 1967—“Ultra-rich topics” (Sacks 1992a:601-604)—as a precedent or offering legitimisation for moving from the talk about looking for a device, to a discussion about normal appearances.

<sup>2</sup> In a previous chapter (Chapter Six, note 20), I highlight the significance of this *caveat*.

<sup>3</sup> A passage from one of Wolfe’s essays, a pile-up in a motorcycle race in Gardena, California, is extracted by D.R. Watson (1973:5). Anticipating the bibliographic and literature-surveying considerations of Chapter Thirteen, how is “The Pump House Gang” afforded relevance? The circumspection of the fatality involved in the crash is apposite to D.R. Watson’s own observations on the phenomena of public announcement, which he witnessed at Brands Hatch motor racing circuit. D.R. Watson (1996) subsequently produced an extensive reworking of this early paper.

<sup>4</sup> The locus of his considerations is the utterance “In that Bonneville of mine”. This is found in the Group Therapy Session transcript, which appears in variant forms—including a previously untranscribed stretch of talk before the arrival of a new participant in the therapy group, and with different pseudonyms (Sacks 1992a:136-143; 270-280). In an editorial note, Jefferson explains when the new transcription—with the reattribution of pseudonyms—was produced (Sacks 1992a:270).

<sup>5</sup> Sacks uses this term in another context, using a different corpus of data (“The baby cried. The mommy picked it up”) (Sacks 1974a:220), to formalise how certain categories are not treated in isolation but are recognised to “go together with” another category, e.g. husband/wife, ex-husband/ex-wife, lawyer/client, doctor/patient, etc. In this section we are looking at how members recognise ownership through the use of these culturally methodic practices.

<sup>6</sup> Competent hearers iterate “series of measures” (Sacks 1992b:235), e.g. “earlier” and “later” (Hatch and Watson 1974:172) in categorising music. Such “measurement systems” (Sacks 1988/89) are linguistically constituted activities.

<sup>7</sup> Elsewhere (Carlin 1999) I refer to Stoddart’s Wittgenstienian concerns *apropos* “argot-terms-in-use”, when discussing how ethnographers conceive of subcultural groups.

<sup>8</sup> In another setting *apropos* drug-using, Bourgois provides an illustration of Pollner’s notion of “explicative transactions”: “If Doc is to maintain his credibility as an effective shooting gallery manager, he needs to set confrontational precedents in full view of his paying customers and make public spectacles out of any would-be hustlers.” (Bourgois 1998a:54)

<sup>9</sup> Nor with cognate legal procedures, e.g. receiving a parking ticket (Richman 1983:67).

<sup>10</sup> The hippies of Haight in Cavan’s study articulated a different attitude towards arrest for the possession of drugs than the heroin-users of Stoddart’s study: whilst details of “a pinch” cause irritation to an experienced heroin-user (Stoddart 1974:177), details of “a bust” are appreciated among Haight’s Hippies (Cavan 1972:153, 159).

<sup>11</sup> E.g. “... Tata, his eldest son, who was illegitimate ...” (Mead 1963:49).

<sup>12</sup> Here this thesis presents readers with a difference in emphasis in Sacks’ work, involving two styles of analysis or “analytic alternatives”. I have already mentioned how Sacks used “therapy talk” as an

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occasion to talk about generic procedures, rather than to establish the identifying characteristics of talk as therapy talk. These two styles of analysis may be referred to as formal and quiddity-based analyses.

<sup>13</sup> The hotrodders in the therapy session refer to a particular Ford as a "Thunderbird", Thunderbirds being the most powerful model of Ford to a hotrodder. Referring to a Thunderbird as a Ford, however, is a "transformation", i.e. which denigrates it (Sacks 1992b:117).

<sup>14</sup> A comprehensive listing of topics considered by Sacks, based on copies of his mimeographed lectures, appears in Coulter (1976:508-509). These lectures were eventually collected and published by Basil Blackwell (Sacks 1992a and b). Coulter has commented upon the trivialising and delimiting of Sacks' *oeuvre* whereby "'lectures' seems somehow too commonplace a term with which to characterise them" (Coulter 1995:327); and, in a posting to a public electronic discussion group, commenting "that entitling Sacks' lectures in this way [*Lectures on Conversation*] may have contributed to diverting social scientists' attention from the many other treasures therein to be discovered—treasures of a mostly methodological and logical variety" («ETHNO hotline», 6.10.98). Similarly, prior to Coulter's comments, D.R. Watson (1994:185) observed that "Indeed, the title *Lectures on Conversation* might be taken as an inadequate representation of the range of Sacks's considerations and of their implicativeness for the discipline of sociology."

## The Collaborative and Linguistic Nature of Interviews

This chapter builds on the earlier considerations of transcription and confidentiality, to look at another sociological research method, specifically the use of interviews. In line with the central themes of the thesis, considerations of method are made in terms of membership categorisation and methodological irony. The *gestalt* switch involved in the study of formal structures in the interviews, *how* people talk about the bomb (in this particular vehicle, the interview), elides the pitfalls inherent in attempts to uncover or conceive of an interview's "products" as "bona fide 'data'" (Hester and Francis 1994:676):

"Particular interviews are not representatives, they are just local, here and now, occurrences."

(Hester and Francis 1994:679)

In this chapter I shall comment on some under-reported features of interviews as "conduits" for social inquiries, which the topicalising of formal properties of talk makes available for study. These features may be enumerated as the use of membership categories (both by the interviewee and interviewer), how the categorisation of the interviewer by the interviewee may affect the interview itself, interviews and the reporting of interviews (or their "data", their purported products) as methodologically ironic processes, the interview as a speech-exchange system, recipiency and acknowledgments of recipiency (e.g. "acknowledgment tokens"), stories in conversation and the features of stories in interview settings. For example, throughout this thesis I have reprinted extracts from transcriptional renderings of talk in interviews,

which are “ostensibly about” the city-centre bombing of Manchester in 1996. As this chapter shows, the linguistic constitution of research practices, including sociological interviews, the designation of these interviews as interviews about the Manchester bombing is problematic.

### ***Interviews: The Use of Categories and Referents***

That interviews somehow constitute “conduits” (as mentioned above) is a problematic approach to research practices, one which fails to account for the thoroughly linguistic nature of social life, *including* sociological research practices. Sociological interviews are encounters in which the interviewer attempts to elicit information from an interviewee, but it should not be forgotten that the elicitation, provision and receipt of information within interview settings are natural language activities.

“To be sure, we must abandon the entire notion of the interview as a conduit to a separately-conceived object, as a ‘window on the world’, however clear or opaque.”

(D.R. Watson 1998:6)

This is at the heart of sociological inquiries, and ushers in the distinction between topic and resource. When we look at an “interview datum”, is this “datum” actually conceived as being “of” or “about” something; is it a datum which can be used in the service of a substantive topic, i.e. used as a resource, or is it instead a datum in and of itself, i.e. to be approached and used as a topic? As D.R. Watson goes on to say,

“Instead, then, of treating the interview as a more or less corrigible channel to, or perspective on [topics] conceived as independent, free-standing objects, I propose to treat them as actively constituted by and through the interview and as

inseparable from the interactional vehicles through which the interview is conducted."

(D.R. Watson 1998:6-7)

So D.R. Watson (*ibid.*) suggests that sociologists should not see the products or "textual outcomes" of interviews as being data "about" something. A useful approach to interviews is instead

"to treat the interview data as displays of membership categorisation work by interviewees as well as interviewer."

(Baker 1997b:137)

The "rectilinear" presentation of transcripts, mentioned in the previous chapter on transcription, preserves the interactional, collaborative features of the "interviews", and makes available the use of membership categories by myself, as "interviewer", and interviewees.

I could suggest, but cannot stipulate, that interviewees are oriented to responding in their professional capacities, e.g. fire chief, police chief, rail chief, etc.; they display their alignments to the event. That is, an interviewee responding *qua* "how any member of their category would respond". So, for example, does (and if at all, to what extent) the Police Chief talk about the bomb "on behalf of" other police chiefs;<sup>1</sup> had I spoken to another police chief, would I have had the same (or similar) responses? A case of this is exhibited in the following extract:<sup>2</sup>

PC: erm it was low flying and of course (.) that the use of megaphones on the street erm (.) and the other methods it's a question of clearing people away "get them" as far away as possible (.) and erm I think with, with these

type of events with any (.) major disaster (.) erm I think any police officer will tell you this (.) it doesn't matter how good your plans are, you will have an initial phase of chaos (.) where everybody is trying to come to terms with what's happened (.) erm and deal with it erm. One of the important (.) things within that sort of chaos phase (.) is if you like the (.) the confidence and training of police officers, who will not they won't wait to be told what to do

The Police Chief's utterance "I think any police officer will tell you this" is not to be taken as representative of similar *types* of utterance, i.e. this is not being mooted as a theme for analysis; it is an illustrative case of a difficulty we face as analysts in approaching interview "data" *qua* interviews, and the category-membership of interviewer and interviewee. In this utterance the police chief makes an explicit (yet downgraded) claim to inform the interviewer as an incumbent of a particular category: if the interviewee was another member of that category the interviewer would receive the same information. Furthermore, not just another police chief but "any" police officer would display such alignment, would respond in this manner: that is, this information would be supplied by a member of the police regardless of their rank.

The Police Chief does, then, appeal to professionalism in the sense that "any police officer will tell you", or would be able to tell the same thing. In the following extract, the police chief explains that the police officer is a witness to sights that lay persons may not encounter, and how the stories of these sights are part of a "police culture".

1. PC: Mind if I smoke?
2. AC: Please go ahead. ((cigarette lighter flipping)) This is a guess, and then you'll have to, have to correct me, I guess it would have been the central topic of conversation here for

3. PC: A long time, a long time. Erm (.) we we see so many things you know, policing is a unique job, um, no day is the same, um, you see all sorts of strange and wonderful things, um, which are sort of part of the police mythology. The, the anecdotes people can tell about the human race are quite startling. Um (.) I can remember going into an alcoholic's house, some years ago, and this alcoholic drank (.) cans of Carling (.) um Special Brew, and every time she drunk one she threw the empty can on the floor. And when she finally died, the whole ground floor of that house was eighteen inches deep in, with empty cans.
4. AC: °Good grief.°
5. PC: Startling! But it's human!
6. AC: Hmm.
7. PC: Erm, just this last weekend we had a man attempt suicide. Um, now a popular method, in a car putting a hose in and dying from the fumes. Well this man, put a new twist on this. He cut his wrists, then he put the hose into his car, and then he started driving round the city.
8. AC: °((laugh))°
9. PC: Yeah! So again, it it's sad but it's it's part of, part of policing you know. Erm, there are always these strange and wonderful things that you see. Erm, but of course the bombing was something else. That was a huge event, which a lot of officers became involved in, um, after the event, so erm they've all got their memories of it. You know I've got (.) some wonderful memories of it which, um, I can relate to people with a great deal of humour. Um (.) I can remember being down on the cordon one evening. A very dapper, well dressed gentleman came up to me and he said um
10. AC: °((laughing))°  
:  
: [tells story]  
:  
11. PC: ((laugh)) But as I say you know there are these there are these memories and everybody involved will have their own personal memories of what was an incredible event.
12. AC: Hmm.
13. PC: So, erm, it's still it's it's part of the culture here now.

This is immediately reminiscent of the “occupational wisdom” and “streetcraft” of traffic wardens (Richman 1983:111 *et seq.*): a “mythology” had been established, with story-telling sessions at the end of each day (“post-5.00p.m.”) among the traffic wardens, where (sometimes possibly apocryphal) tales of “street administration” were told to each other. However, rather than bringing a corpus of sociological material for thematic analysis, I shall consider this extract in its own right. This is a procedure following Sharrock and Anderson, who, outlining the approach to transcripts in Conversation Analysis, say

“Each transcript is inspected for what it contains, what its structures are, and how its features can be made visible and analysable.”

(Sharrock and Anderson 1987:247)

An interesting feature of this extract, in illustrating the professional domain, is how the Police Chief establishes this domain, which is categorially-determined. He achieves this through the use of reference terms, or referents. Ordinary referents provide a linguistic basis to suggest that there is an overlap between the professional and personal realms of experience. (This overlap is only part of the problem of interviews and their analysis.) The use of these referent terms are not isolated events.

Consider, at the beginning of the extract, the Police Chief’s demarcation of incumbents in the following bloc of utterances:

PC: Erm (.) we we see so many things you know, policing is a unique job, um, no day is the same, um, you see all sorts of strange and wonderful things, um, which are sort of part of the police mythology.

“We”, (as in “we see so many things” and, in the first extract, “just this last weekend we had a man attempt suicide”) as used by the Police Chief, refers to members of the Police Force; likewise “you” is used as a referent to members of the Police Force (“you see all sorts of strange and wonderful things”, “there are always these strange and wonderful things that *you* see”). The Police Chief can refer to an all-inclusive “anyone” in the manner which a potential suicide might refer to there being “no-one” to approach about their difficulties (Sacks 1967)—his utterance “the anecdotes *people* can tell about the *human race* are quite startling” shows how, in categorial terms, he is able to collect and refer to members of the Police Force as “people” and non-members of the Police Force as “the human race.”

The use of these reference terms—the usage of reference terms *per se*—is an important but neglected feature of interviews in social inquiry. This feature can be seen in the *selection* of reference terms by this interviewee, the Police Chief. Consider the use of a number of other potential reference terms: for example, rather than “we”, the Police Chief could have referred to WPC Mills and PC Marshall. The selection of hypothetical but categorially-implicative reference terms (we and you, people) is predicated on the interviewee’s assessment or inference of the state of knowledge of the interviewer. That is, the interviewee tailors responses to the interviewer’s state of knowledge. This assessment and accommodation of an interlocutor’s state of knowledge is known as “recipient design” (Sacks and Schegloff 1979:16). The Police Chief *could* have selected WPC Mills and PC Marshall as “recognitionals”, which are defined as

"such reference forms as invite and allows a recipient to find, from some 'this-referrer's-use-of-a-reference-form' on some 'this-occasion-for-use', who, that recipient knows, is being referred to."

(Sacks and Schegloff 1979:17)

Quite apart from any considerations of anonymity, not using people's names, the Police Chief (correctly) inferred that the interviewer (me) would not recognise WPC Mills and PC Marshall. As Sacks and Schegloff (*ibid.*) go on to say, the use of recognitionals is contingent upon the case that "recipient may be supposed to know the one being referred to". He designed his reference terms accordingly, and in terms of the conversational *preference* for minimal reference:

"nonrecognitional forms (and indeed minimised recognitional forms—e.g. "someone") are available to any speaker for any recipient about any referent."

(Sacks and Schegloff 1979:17)

The Police Chief's use of non-recognitional forms contrasts with the use of recognitional forms, i.e. people's names, by another "interviewee". In sociological investigations, the researcher requires contacts with informants, and may rely on informants to identify other potential informants and other categories who should be approached in the ethnographic enterprise (Gold 1997:390-393). That an informant freely provides the names of people for the ethnographer to approach in the furtherance of their endeavours, is surely a bonus?

### ***Transcribing Interviews: Categorisation and Confidentiality***

Compare the interview above, with the Police Chief, with the extended extract from an interview with Frank, the Rail Chief, however, who *does* refer to people by name, and

by rank. I have reproduced an extended extract in order to demonstrate that the use of names and ranks is commonplace. This is intended also to counter suggestions of the selectivity of extracts, which may be leveled at the presentation of individual occurrences of identifying details. An extended bloc of talk shows how names and ranks are used pervasively throughout the talk. Ranks are identifying details as well as names, because there may only be a limited number of incumbents of a particular rank, and only one incumbent of other ranks. The potentially-identifying details have been deleted, and the placements or local occurrences of these details within the talk are preserved *via* shading.

1. FRANK: I can tell you from my side of the story it was a, it was a Saturday—I don't recall the date now because it's twelve months ago—erm, I was actually in the signing-on point which is at the far end of this building, as you walk towards the platforms
2. AC: Aha.
3. FRANK: and then turn left down the platform at the far end and it's known as ((inaudible)), and on that particular day I was erm, I was in that, I was in the signing-on point, I had two of my [rank] with me [forename—surname, forename—surname] and the [rank] by the name of [forename—surname]. Erm, my first recollection before the bomb actually went off was that I had a driver by the name of [forename—surname] come to the window and apologise for being late, because he he'd been un unable to get past the police cordon
4. AC: All right.
5. FRANK: so I said “what police cordon?”
6. AC: °((laugh))°
7. FRANK: he said there's a police cordon outside. They've put they they've taped everywhere off. And he didn't know why. So I rang er TOC control, and I said is there a security alert in the city, because I've just had a driver

roll up late for work who tells me that the place is taped off. So they said yeah, we believe there is, but it won't affect you. So I said then, okay

8. AC: Right °((laugh))°
9. FRANK: Anyway, my [rank], [forename] who went, left the station and came down to the bottom there, where this building is now
10. AC: Yeah
11. FRANK: and was actually out front because there was several hundred people on ((inaudible)) there, adjacent Chethams. You know Chethams there, behind behind the - in front of the station there
12. AC: Yeah
13. FRANK: if you go downstairs and go across, you'll see there's a grass bank, and it's just grass all the way round. Chethams's School of Music's just over there.
14. AC: Right.
15. FRANK: An' there was about, I don't know (.) two, three, maybe four hundred people sat on the grass that had been moved back by the police as they'd moved people back. So I saw them out of the window, you can see from the signing-on point's kitchen window. You can actually
16. AC: Aha.
17. FRANK: see. Anyway it was like that. And erm, I thought I'd better advise my [rank]. So I rung him up, cos he was on call, and I said we have a security alert. And I believe it's a bomb. And funny enough I said to him I said I'll let you know if anything goes bang.
18. AC: °((laugh))°
19. FRANK: And I put the phone down. And by that time it'd be what, maybe about eleven o'clock. And could hear the police helicopter circling as well. So we never took any more notice because we had things to do, we were getting on with things. Anyway, about five past eleven or so, maybe, I don't know, give a couple of minutes, there's this almighty bang. The building shakes, about fifty to sixty years' worth of dust lands on our head, in the S.O.P. Although there was no structural, no real structural damage to the office we were stood in

20. AC: Yeah.
21. FRANK: erm, all the messroom windows where the drivers were having their break came in. the false ceiling in the drivers' lobby which is immediately outside the office came in. there was bits of material, false ceiling, bits of aluminium that hold the stuff up, the windows come in, and then of course (.) er there was a great (.) there was the usual. You get the bang, and there's a bit of silence when everybody sort of recollects what's taken place, and then and then there was erm the sounds of people who've been hurt, screaming and things, you know. I went into the messroom (.) to see what the damage was, and I'd one or two drivers and the like that'd been injured, so I rendered first aid. One of my drivers by the name of [forename] (.) [forename—surname], and he, he was very concerned about his wife because she worked in the city
22. AC: Right.
23. FRANK: because although he was bleeding he wasn't, he wasn't greatly injured but he had superficial cuts to the head
24. AC: Hmm.
25. FRANK: an' arms and things. And we wanted him to go to hospital and have a rest because by that time things started to get going. A lot of my staff (.) er, [rank] [rank] (.) were taking the first aid kits from the offices and going onto the grass where there's people
26. AC: Right.
27. FRANK: Funny enough it was one of the [rank] who is actually here, next door, [forename—surname], I recollect seeing [forename] pick a woman up bodily and run. We were using the tram(s?) to ferry people away from the site and we come outside you see we'd actually left the office by this time after rendering first aid to those in the office. I'd advised the Control of course what had happened and that and that we had suffered damage. Started my [rank] off in seeing what train crew were around and who should have been there, were there any missing, because we'd get the normal enquiries after the bomb where's my husband, where's my girlfriend, that kind of thing. And then I went outside. And outside it was like (.) well it was carnage, you know, there was people who'd been injured erm, people lying on the grass, people being picked up, er there were policemen (.) both BT and the Metrolink policemen (.) they'd commandeered a tram and I think they must have loaded I don't know, maybe a hundred (.) hundred fifty people

28. AC: °((exhale))°

29. FRANK: and trams took them away to Crumpsall Hospital and the like. And then remem, remarkably soon (.) really, considering the fact of the bomb, erm, the station was deserted. Of course the damage to the station was was that a lot of the glass and the glass roof had come down showered down onto the concourse outside

30. AC: Ah, right

31. FRANK: er, all the windows on the side of the, from the side of the bomb had come in, every single window had had virtually come in, next door in the office where where the track- that's where [rank] comes upstairs (.) and he came in and as he came he dropped his pen he bent down to pick it up and as he as he bent down the bomb went the glass and windows all came in and blew him back into the (.) back into the cabinets, and he's actually finished work with it now, he's no longer at work. (.) He has been given early voluntary retirement, but he's erm (.) he actually went off sick and it he he, well he's, I couldn't I don't know whether he's recovered from it or whether he still suffers from it but he's a, he's on a walking stick which he wasn't, and I think a lot of that was attributed to the fact of the bomb. Er, he was very lucky. Had he been stood up the glass would've taken him with it and it would have (.) It was [forename–surname] that was er the [rank] who'd er been injured erm. (.) Yet remarkably on the day he didn't take much notice of it, he was too busy helping other people. Er, as was er [forename–surname] the [rank] that was on. I had a [rank] by the name of [forename–surname] who incidentally is also on duty at the moment (.) he was up here taking rules, and he had actually come out of the building and gone downstairs otherwise he'd have been in the same room that ((inhale)) when all the glass came in. And you can look out there you can just see out there where the partition has been blown back as well. Doesn't really say. What else happened? As I say all these people were involved in like myself helping people onto trams and getting them away and within about (.) I don't know (.) thirty minutes or so the place was deserted. Course the police had cordoned everywhere off and nobody was getting allowed in or out of the station. Er, the station wasn't fit to come into anyway. Er, the [rank] I er I got in contact with again, and advised him, and he came, he actually came to work then, er found it difficult getting in but e- he eventually got in. We had staff turn up for work (.) We had staff ringing in to see what was going on so we, we, we (.) manned the signing-on point although it was a bit (.) dusty and slightly damaged, we manned that and we ad we kept instructing our drivers where to go because we still had a service to run.

For the purposes of doing ethnography, such conversations are vital. Some informants can provide the ethnographer with names of people with whom further interviews should be conducted (Gold *op cit.*). However, for transcription purposes as well as considerations of confidentiality, this—the occurrence of people's names—is a methodological inconvenience. This is discussed in the chapter on the “practical work of pseudonymisation”.

With an analytic attitude which tries to preserve the phenomenological intactness of the interview, the transcription attempts to preserve the “nominal intactness”, or the interactional features of names, e.g. the sequential and categorial properties of names in talk. The excision of identifying features such as names and ranks occludes appreciation of how names and ranks are used in talk, e.g. diminutives of names and ranks, and the routinisation of distinctions (duty clerks, operating managers, supervisors, traction inspectors, trains inspectors, etc.). The transcriber concerned with the productional particulars of members' accounts has to locate suitable pseudonyms (Carlin 1997). It also highlights a dilemma with the use of pseudonyms, and a contradiction in the Larimer Street studies. The real names of certain individuals are retained in terms of “moral credit”—people who are seen to be doing a good job (Rose 1965:3); whereas the names of the men on the street are pseudonymised.

So here I am relating the work of Rose, interviews, confidentiality and membership categorisation. Elsewhere I report the concatenated organisation of categories by members—that categories are “duplicatively organised” (Sacks 1974a:220-221)—and

that this duplicative organisation has consequences for the unequivocal identification of any individuals. These consequences are twofold: first, by unequivocally identifying one individual, it may be possible to identify another; second, that identifying one individual in terms of their “positive contribution” is implicative for the individual that is not identified. As I said before, in Chapter Six on “The practical work of pseudonymisation”, the asymmetric treatment of anonymity and “pseudonymity” *apropos* category-incumbents is, in Sacks’ terms, “inferentially rich”. In the case of the Larimer Street inquiries, the identification of some category-incumbents rather than others reflects negatively on those whose identities are disguised.

In light of the above observations, for the purposes of transcription some interviews are more problematic than others. The Police Chief’s decision to proceed “without naming names”—as we discussed in relation to cooperation and non-cooperation with the evacuation phase—is “less arduous” for the transcriber concerned with productional particulars of talk-in-interaction. Whilst the blanket policy of changing all names is desirable, such ethical guidelines for research do not make the task any easier. As Rose found, there are individuals whose contribution to events should not go unrecognised (a moral judgement by the analyst is being made here). The report on the inquiry may not be the place to show such recognition, however. Read the transcripts of talk about a real-life event, *viz.* the Manchester bomb; call some people real-life heroes.

There is another set of people who are uniquely identifiable. For example, there is only one Chairman of the Millennium Committee, and only one Leader of the Council.

Some of the interviewees are *potentially* identifiable. Identifications such as “police chief”, “fire chief”, “rail chief”, “theatre manager”, etc., are functionalised glosses of interviewee’s identities (see the section on functionalised pseudonyms in Chapter Six). There are a series of rankings within each agency which may be considered as “chiefs”:

I am therefore using the line-management of agencies (see the section on “Organisational Activities: Confidentiality and Knowledge” in Chapter Four) as a resource for disguising identities.

Care must be exercised with this procedure. Consider the following utterances, from which names and ranks have been deleted:

PC: I'm [ ] and I am the [ ] for Manchester.  
And um, I am responsible for policing the city centre area um and I've got  
[ ] and [ ] staff for that purpose.

Even without the provision of rank, it may be deduced from detailing the number of staff. For similar problematics of transcription consider the following extracts:

DAVID: I'm um one of the um [ ] residential people here in the [ ],  
and I'm a [ ], and my role is to (.) um principally be the  
[ ] of the place and that's why I'm titled the [ ] (.)  
but I'm also the [ ] (.) and um [ ] person here too.

JS: My name's [ ]  
AC: Okay, and you sell The Big Issue  
JS: Yeah, at [ ]

MONA: I'm a [ ] based in the [ ] here at the, the [ ] which is where I'm working in [ ].

The provision of productional particulars, i.e. referred to by interviewees, and which constitute self-identifying characteristics is a methodological problem as well as an ethical problem for the project.

Further, care must be taken with the provision of *a priori* categories by the transcriber. The extracts from the interview transcript quoted above identify the speaker as Police Officer; and the discussions of the extracts identify the speaker as "the Police Chief". The provision of categorial identities is problematic in that it works to provide background resources for the interpretation of turns at talk (Watson and Sharrock 1991). The provision of categorial identities in the thesis is difficult to overcome; it may be easier—that is, reasoned—through my considerations of transcription. A possible form of presentation of transcripts is more anonymous, that is, less categorially-implicative, by identifying my questions/comments only. Nevertheless, the categorial identification would remain a problematic feature of the discussions of the transcripts.

Another difficulty arises in regarding (and presenting) people as one-dimensional, e.g. the Police Chief as solely a police chief. It would be erroneous and fallacious to

suggest that respondents' orientations to Manchester and the bombing of the city-centre are somehow "overwritten" by their official or professional identities. The Fire Chief, for one, socialises in the city. The bomb also had implications for attention to professional duties. The librarian and the rail chief discuss how the bomb—and the cordon set up after the explosion—affected people both in physically getting to work and in moving between their workplaces. Without suggesting the existence of a simple dualism in identity-categories, I shall refer to "professional" and "personal" identities.

### ***Interviews: Categories, Referents and "Ownership"***

Within the interviews, and taking the Police Chief as the example, there are "professional" as well as "personal" aspects or versions of accounts. The authorisation of the professional account is coincident with the personal account, whereby the Police Chief moves from the "we" referent to "I" ("I can remember"), introducing a personal-professional (or personal-as-professional) contour to his account.

In making moves between talking about the bomb from personal and professional experience, a respondent "owns" the personal experience. By this metaphorical use of "ownership" (Sharrock 1974:52) I mean to imply that the personal testimony of personal experience provided to an interviewer is incontrovertible, in the sense that the respondent is in possession of the authoritative version of events from their own standpoint. Elsewhere (Carlin 1998:3) I liken the incontrovertibility and methodological irony in analysts' first-hand formulations as the "Mildred Pierce phenomenon"—the assertion that the analyst's account is the only account worthy of

consideration. This is an analogous case, except that reporting procedures which regard members' first-hand formulations as controvertible or as being somehow deficient involve a methodological irony. This culturally-methodic feature of authorising accounts—"I was there and you weren't"—is also discussed by D.R. Watson (1990:270-271). As D.R. Watson goes on to say, however, the ““experience-licensed” nature of the claims” is not the necessary commonsense criterion of veracity in some institutional contexts (see the discussion of stories, below).

Here I am not invoking a methodologically ironic notion of epistemological standpoints *apropos* of homogenous analytic categories, e.g. race, gender, class. As an analyst I do not have the power to redefine words with pre-existing meanings. Instead I refer to members' standpoints as evidenced by domains of professional expertise, e.g. as a police chief, and as witnesses of an event. Although on occasion, and *in situ*, it may provide a useful interviewing technique—"playing Devil's advocate"—the interviewer is not in a position to subvert this personal standpoint; analytically, the *post hoc* subversion of accounts introduces a level of methodological irony.

This error is made by Dorothy Smith (1978) in proposing an alternative interpretation of an account of mental illness-type behaviour. Cuff (1980) criticises Smith for abandoning the policy of ethnomethodological indifference, that her proposed interpretation was motivated with the intention of undermining psychiatric definitions of mental illness. In a later study (eventually published in 1990), Smith correctly leaves the downgrading and dismissal, and upgrading and authorisation of versions to

members themselves, rather than entering into such methodologically-ironic competition herself. The competing versions of accounts in her explicative study are published letters concerning a student riot in Berkeley, California, by a professor who witnessed the riot (and, by virtue of his witnessing, claims ownership of his version) and the Mayor's rejoinder.

Smith may have been interested in a collection of cases (Baruch and Treacher 1978:107-219) where patients and their spouses are interviewed. Rather than subverting Angela's account of K's mental illness-type behaviours and suggesting alternative interpretations of the behaviours Angela presents, Smith could have compared the versions of the same events given by patients and their spouses, who offered competing versions. A methodologically non-ironic study could be produced comparing the versions of events, and showing how the authors of the collection themselves ironicise versions through the selectivity and co-categorisation of extracts. Importantly, the reader does not know, and has no means of knowing, whether the extracts are decontextualised or remain properly situated within the context of their production/occurrence.

Until it is publicly displayed, i.e. talked about, the ownership of personal experience is among the *imponderabilia* of ethnographic research. As sociologists we are unable to turn mental phenomena into ethnographic "findings"; we may speculate upon people's motives in the mentalistic sense, but such speculation is analytically senseless except as

a topic for analysis: “there is no reason to look under the skull since nothing of interest is to be found there but brains” (Garfinkel 1963:190).

### ***Displaying Recipiency: The Interviewer in the Interview***

“Although the participation of the speaker in a turn is apparent, the role of the hearer has not received much attention.”

(Goodwin 1989:91)

Following Rose’s advice on talking with people for purposes of worldly inquiries, I attempt to minimise the interference or direction I give to the talk. In preserving the turn-by-turn character of the talk in interviews, however, it is observable that I do produce talk. Some of my talk can be formulated as “Rose’s gloss”, referred to in the intellectual profile of Edward Rose.

Within news interviews it is incumbent upon the interviewer to ask questions of the interviewee. Over the last decade a growing corpus of literature which focuses on news interviews as distinctive speech-exchange systems has proliferated (Carlin and Slack 2000). Within the sociological interview, it is incumbent upon the interviewer to ascertain information from the interviewee. It is crucial to the “internal validity” of the interview setting that the talk is preserved *in toto*, to account for the interaction which constitutes the interview.

By “internal validity” I do not refer to issues of “validity” and “reliability” but to the “once only” and “first time through” (Garfinkel, Lynch and Livingston 1981:132) character of interviews, and the integrity of representations. The preservation of the

internal validity of the interview means not only the complete transcription of the interviewee's talk, rather than abbreviated renderings, and consequentially methodologically ironic presentations of interviewee's responses; it is also essential to preserve the interviewer's contributions in an equally exhaustive manner. This presupposes a recording enterprise (in the transcription chapter, I refer to recording as the "analytic prior" of transcription) which preserves as much of the setting's phenomena as possible, including "pre-interview" phenomena. This recording enterprise captures features which are hitherto not considered important features of sociological study. For example,

[Atkinson, Cuff and Lee]

1. ((general background noise))
2. C:<sup>1</sup> Right-e:r-
3. ((general background noise))
4. ((pause ca. 4.00)) ((general background noise))
5. C: -Are we ready to go again now?
6. ((general background noise))
7. ((pause ca. 3.00)) ((general background noise))
8. R: Yes
9. ((general background noise))
10. C: Good, Ray's ready – e:r can I just mention um . . just–
11. just mention one more thing before I go round the table
12. and then I really have got a batch of (other points).
13. Ray has – ((background noise ceases))
14. C: –just reminded me might as well bring this one up as well
15. C: just to mention it . .

The extract above is reprinted from a study by Atkinson, Cuff and Lee (1978). The "background noise" (lines 1, 3-4, 6-7, 9, 13) to which they refer (1978:135) is the noise of a coffee-break within a meeting, with its attendant clinking of crockery and the production of conversations by different cohorts of members within the same

interactional setting, *viz.* a coffee-break during a meeting. They found that an utterance—“Right-e:r—” (line 2)—was audible above “background noise”, therefore available for transcription, and oriented to by participants in the setting, i.e. it was interactionally significant. This utterance, produced within a meeting by an incumbent of the category “chair of meeting”—so categorised by the hearers of this utterance as being relevant to them—is an “attention-attractor” (1978:137).

Rigorous transcription of the interviewer’s utterances ensures that the “context of elicitation” of interviewee’s utterances is available for scrutiny, rather than, e.g. Parker (1985), being edited or systematically “screened out” of the final version.<sup>3</sup>

Interviews are interactional encounters, between the interviewer and interviewee(s). As such, the interviewer has to display recipiency of talk within the interaction. A task of the interviewer is then to display recipiency throughout the interview without, or at least minimising, the extent of direction and ratification of responses. By “direction and ratification” I am suggesting the influence of the interviewer on the trajectory of interviewee’s turns, through the conversational activities of asking questions; and by providing “continuers” or “acknowledgment tokens”—what Emanuel Schegloff (1972a:404) refers to as “assent terms” and Douglas Maynard (1980) calls “solicits”—which encourage a particular trajectory at the expense of another possible trajectory of talk. Examples of acknowledgment tokens include “yeah”, “mm-hm”, and “uh-huh”.

In the interviews presented in this thesis, it is interactionally visible that I produce such solicits or acknowledgment tokens as, *inter alia*, “right”, “aha”, “mmm”, “hmm”, a questioning “hmm?”, “hm” and the quieter “hm”. Maynard (1980:270) refers to these productional particulars as solicits because they “demonstrate recipient attention and invite further related talk.” D.R. Watson (1998:14) notes that the term “continuer” is problematic because this description does not attend the instance-by-instance work, i.e. the haecceity of such an utterance: an utterance “achieves the status of ‘continuer’ by its active employment as a member of the ‘yes’/‘no’ set”.

Jefferson (1993a) observes that acknowledgment tokens are not necessarily coterminous, or interchangeable. Her observation is based upon empirical data of therapy talk, which suggest that different forms of acknowledgment token accomplish different interactional work. Fitted together with her paper on “transcriptional stereotyping” (1996 *op. cit.*), which I refer to in the transcription chapter, Jefferson’s observations on pronunciational particulars and acknowledgment tokens encourage analysts to transcribe on an “instance-by-instance” (1996:162) basis. This more careful procedure does not traduce members’ productions, i.e. a source of methodological irony, and preserves the phenomenological intactness of interactional settings.

Elsewhere, Jefferson (1993b:11) notes how some acknowledgment tokens may be “thoroughly disattentive” to the ensuing talk, however. That the recipient’s responses may be “thoroughly misfitted” (*ibid.*) to talk is illustrated by the following extract<sup>4</sup> from the parenthood-counselling sessions:

Extract 57 (5)

1      CR1    so what about you  
 2                what can you remember about your childhood  
 3      CD3    erm  
 4      ((pause))  
 5                I didn't used to like sitting in the car outside the pub  
 6                while my mum and dad were in there getting pissed  
 7      CR1    ah right  
 → 8                that that's a really good one isn't it  
 9                so  
 10     CD1    ((laugh))  
 11     [  
 12     CD2    ((laugh))  
 13     [  
 12     CD3    ((smile))  
 13     CR1    we could say  
 14     would you be doing that with yours  
 15     CD3    no well I don't drink anyway  
 16     CR1    right

Some continuers, e.g. “yes”, “right”, or “great”, have the potential to display affiliation with particular responses. Within the context of the interview setting, continuers may suggest the interviewer’s agreement with the interviewee, which may be problematic not just in terms of the integrity of the inquiry but could also have ethical or political ramifications. (The interviewer could unwittingly (or wittingly) find themselves condoning or endorsing particular positions, for example.)

In order to “limit the collusion” with talk as it is produced, the interviewer may use more “neutral” information-elicitation techniques. That is, to use such tokens of recipiency which do not in themselves affiliate or disaffiliate the interviewer from the ongoing talk.

### **"Stories" and Story-Formats: Linguistic Activities – Interview Activities**

Although anthropology has not topicalised stories *per se*, with its tradition of studying folklore and oral traditions, it has looked at stories. Stories have not been a particular concern of sociological investigation, however, excepts as conduits to e.g. autobiographical objects. This is unfortunate as in this section I intend to show the relevance of stories to methods of sociological inquiry, particularly interviews.

Harvey Sacks identifies two different “types” of stories in conversation: volunteered, i.e. teller-initiated stories; and invited, i.e. recipient-initiated stories. For Sacks, stories typically take more than one utterance to do. Since this is the case, the turn-taking system has to be suspended by the interlocutors so that the story-teller is able (potentially, at least) to complete the story. This can be a practical problem in a sociological interview, which largely occur on a question-answer basis.

Another formal speech-exchange system is the murder interrogation. Why should we look at murder interrogations? If this seems somewhat “left-field”, I intend to show how features of murder interrogations may inform and elaborate our considerations of interviews.

As D.R. Watson (1979, 1990) shows, stories, i.e. blocs of talk of more than one utterance are told during murder interrogations. The parties to the murder interrogation have certain rights and responsibilities: the suspect’s rights are enshrined in the Miranda-Escobedo ruling; and police officer’s rights and responsibilities are “mandated

by” the pre-allocation of turns in murder interrogations, whereby the police officer asks questions and may interrupt the suspect. Although the police officer has these certain rights, however, if the suspect is telling a story the police officer has to cede the ‘floor’ and remain silent, refraining from asking questions.

This may seem counter-intuitive, that the police officer provides conversational ‘space’ for the suspect to tell a story. However, it is important for the admissibility of confessions in court that the suspect does more than make a claim to have committed the crime. That the suspect makes a claim, e.g. “I did it”, is unsatisfactory for the legal requirements of confessions. In the murder interrogation as a formal speech-exchange system it is not enough to claim responsibility, say “I did it”, nor is it enough to authorise any claim to responsibility, say “I was there and you weren’t” (the “Mildred Pierce phenomenon” again). The suspect has to *display* that they committed the crime, and such displays are commonly exhibited during stories.

The “incriminating” story is one which is conjunctive with or “fits” the evidence, and its conjuncture is adjudged by the police officer. The police officer uses the Documentary Method of Interpretation in their assessment of the suspect’s confession, which enables them to

“assemble coherence to a set of particular documentary evidences [i.e. the police officer’s knowledge about the facts of the crime and the suspect’s displays of knowledge about the facts of the crime] by reference to an underlying pattern [i.e. the claim to responsibility for the crime]”

(D.R. Watson 1990:271; my brackets)

D.R. Watson's analyses of murder interrogations point up a feature of stories which has relevance to our considerations of the sociological interview, *viz.* a distinction between teller-initiated, i.e. "volunteered" stories and recipient-initiated, i.e. "invited" stories.

Typically the volunteered story opens with an adjacency pair, a "story preface" of the following structure:

A:	I saw something terrible in college today
B:	Oh really?
A:	This girl was walking in front of me when she fell down the MacRobert steps... (story continues)

In the first pair part, A offers to tell a story. In the second pair part, B gives or declines permission to tell the story; whereupon the second pair part determines the trajectory of the story (whether it is told or not). (This structure was observed earlier, in Rodriguez and Ryave's (*op. cit.*) study of secret-telling encounters.) Prospective teller has to give the 'floor' away in order to get permission to tell the story (Sacks). The opening format of the volunteered story can be seen in this extract from the telling of a joke (Sacks 1974b:338):

- |           |  |
|-----------|--|
| 1. KEN:   | You wanna hear muh-eh my sister told me a story  |
| 2.        | last night                                       |
| 3. ROGER: | I don't wanna hear it. But if you must,<br>(1.0) |

Sacks' extract, rather than my fictional example,<sup>5</sup> shows the adjacency pair format of the story preface: Ken offers to tell a story; the recipient, Roger, gives Ken permission

to tell the story, whereupon Ken has conversational ‘space’ to proceed. That is, when Roger or any recipient accedes to the speaker’s volunteering to tell a story, the speaker may take a long bloc of talk without major interruption from the recipient; i.e. there is a temporary suspension of the turn-taking system. Stories are, therefore, collaborative productions in that the recipient agrees to listen (or at least agrees not to interrupt).

The story preface acts as a set of instructions for recipients to hear the ensuing talk, whilst, at the same time, acting as a set of constraints for the teller.

As an illustration of recipient instructions, this next extract takes place within a “round” of jokes in the hotrodders session. In that stories are fitted to each other—a story can be followed by a “second story”, and such story selection and story-telling may proceed indefinitely—Al infers from Ken’s opening sequence that the story is a joke. Knowing only that Ken’s sister is twelve years old informs Al’s subsequent conversational actions:

“[Al] uses the mentioning of the source [of the joke] to employ what he knows about the sister, that she is twelve years old, to occasion telling a joke which can be delivered as a guess by being the sort of joke such persons tell”  
 (Sacks 1974b:344).

So again we are looking at category-membership.<sup>6</sup> As such, before Ken is able to proceed with the story, Al (lines 4-5) guesses at the joke Ken’s sister might have told:

4. AL:           What's purple an' an island. Grape— Britain.  
 5.               That's what iz sis//ter —  
 6. KEN:          No. To stun me she says uh there was these three  
 7.               girls an' they just got married?

Ken's corrective of Al's candidate "type" of joke at line 6 instructs the recipients that the type of joke is not one which might be associated with the category "twelve year old girls" and is fitted to the jokes in the round. Al's guess "reoccasions the originally intended telling" (Sacks 1974b:344), and where the fit of the story, i.e. as a "dirty" joke is provided for by Ken's utterance "No. To stun me she says ..." (line 6). Ken's utterance acts as an instruction for hearing the story.

In the story-preface above, "I saw something terrible in college today", A is providing a set of instructions to B: that if B accepts A's request to tell this story, the story contains "something terrible".<sup>7</sup> It also works to constrain A to produce a story which contains "something terrible". One feature of stories in ordinary conversation is that the recipient must listen to the story until "something terrible" is produced; and the story is not completed until "something terrible" is produced. In this sense, the story preface sets the relevance for the telling and for the hearing of the story.

The production of the reason for the story, e.g. "something terrible", indicates the imminent closing of the story to the recipient, who typically produces a "story appreciation utterance". Story appreciation utterances acknowledge that the story has finished, and confirm to the teller that the recipient has understood the story as it was intended, e.g. as a story about "something terrible" rather than a story about "something

hilarious". (At this point, consider again the story-telling extract, between lines 10-11, from the interview with the Police Chief reprinted earlier in this chapter. The "analytic mentality" of ethnomethodological programmes is such that the content of the story itself is not relevant, at least as the ultimate point of reference; it is indifferent towards specific stories and specific accounts.) The story appreciation utterance also provides for the telling of second stories, i.e. stories that have features in common with the first. If the first story is about "something terrible" the second story is about "something terrible". Second stories also display the understanding of the first story: if the first story is about "something terrible" and the recipient of the first story tells a second story which is about "something hilarious", then the teller of the first story has warrant to infer that the teller of the second story failed to understand the first story.

Teller-initiated stories contrast with recipient-initiated stories. In the murder interrogation, the police officer has a preference for recipient-initiated or invited stories because they have more control over the production and relevance of particulars. One of these particulars, as I mentioned above, is the satisfaction of legal requirements. And the invitation of stories, the pre-allocation of turns and elicitation of information within the murder interrogation, is relevant to the sociological interview.

In the murder interrogation, the police officer invites the suspect to commence a storytelling (D.R. Watson 1990:292):

[Watson]

- 28 Officer: al::right (.) were: investigating the death (.) of a  
 29 gir::l (.7) at two o one Patterson  
 30 Strawson: two ten:  
 31 (1.2)  
 32 Officer: two ten (.2) Patterson: (.7) whose body waz foun::d:  
 33 (1.0) disseminated: (.2) in the: tub at: that address.  
 34 (.6)  
 35 Officer: er: will you tell me? (.6) in your own words: (.2)  
 36 what you know:: about this homicide.  
 37 (.9)  
 38 Strawson: duyou want me to star:t from the beginning?  
 39 (.7)  
 40 Officer: yers would you please?  
 41 (1.2)  
 42 Strawson: I dropped off my girl:friend, (1.0) an:d: (1.2)  
 43 [continues with story]

The invitation sequence has the following structure: the police officer P invites the suspect S to tell the story, then S accepts the invitation. S could accept or decline the invitation (again, see the Miranda-Escobeda rules). Invited stories have a variant structure to volunteered stories: if the invitation to tell a story is accepted, the storyteller must provide the recipient with the information the recipient requests. If certain details are requested in the invitation, and if the invitation is accepted, the storyteller must provide those details.

In interviews, there is a “pre-allocation” of turns, and of rights and responsibilities, and this pre-allocation is manifested in question and answer sequences. That is, the “turn-taking mechanism” departs from the simplest systematics” model of ordinary conversation outlined by Sacks, Schegloff and Jefferson (1974). As I outlined in the section on Conversation Analysis, the parties to talk determine the order of speakers

themselves, *via* the orientation to speakers' turns and the next transition relevance place. These procedural rules *apropos* allocation provide for, e.g. current speaker selects next speaker, or next-speaker self-selects.

However, there is a variation on this local orientation to turn allocation in research interviews; both the interviewer and interviewee orientate to an allocation of turns which is “pre-given”. Turns in interviews are thus “pre-allocated”. The interviewer asks questions which the interviewee answers, and the interviewer can request further information on any answer given by the interviewee. The sociological interview requires that the turn-taking system be suspended by common agreement so the interviewee can (potentially, at least) take a long bloc at talk without major interruption.

The pre-allocation of turns in the murder interrogation, wherein the “police officer” asks questions and the “suspect” provides answers to questions, has a resemblance with the sociological interview. The pre-allocation of turns in the sociological interview, wherein the “sociologist” asks the questions and the “lay person” gives the answers, returns us to the central theme of membership categorisation analysis:

“an orientation to the distribution of identities amongst co-participants to whom activities or turns can consequently be ‘tied’”

(D.R. Watson 1979:11)

We can also see how the membership categories in both pre-allocation systems are “duplicatively organised”: where there is an interviewer there is an interviewee (respectively, police officer and suspect, sociologist and lay person).

## **Conclusion**

In this chapter I have brought together such resources from linguistic settings that bear upon the analysis of sociological interviews. Stories, like interviews, are collaboratively, interactionally produced: either the teller requests the ‘conversational floor’ in order to produce an uninterrupted bloc of talk (“volunteered” stories), or is invited to do so (“invited” stories); in each case, the recipient of the story agrees to give the ‘conversational floor’ to the teller in order that the story can be produced.

I have continued the ongoing themes of categorisation and confidentiality and how they are oriented to by members, *viz.* the transcriber, and the *in situ* orientation to confidentiality by interviewer and interviewees. This is also evident in a bloc of talk produced by the Fire Chief (reprinted in the chapter “Observation: Categories and Appearances in Public Space”):

FC: A few things happened there which, from which, from a national ((laugh)) security point I would I won’t say

AC: °((laugh))°

The analysis of interviews as linguistic settings accounts for both the interviewer and interviewee, in that the interview is a collaborative accomplishment of their practical methods of sense-assembly.

Interviews are collaborative, linguistic activities; as D.R. Watson (1998:8) says, an interview is “a conjoint production”. The presentation of derivations from interviews,

i.e. data obtained from interview-type procedures, do not reflect this collaborative, linguistic work which is constitutive of the interview.

The thematic organisation of data obtained *via* interviewing practices encourages a level of methodological irony which is not sanctioned by individual interviews, nor by individual member's responses. The thematic organisation of interview products constitutes an "instructed reading"; as Lee (1984 *op. cit.*) refers to newspaper headlines, thematisation "can provide instructions as to how to read" the interview-products so thematised. The work of writing-up data derived from interview-type procedures constitute methods by which sociologists

"decontextualise and recontextualise the 'data' from the interview-specific, collaborative practices through which those data were produced."  
(D.R. Watson 1998:3)

The work of identifying themes is an analyst's work, and the thematic organisation suggested by the analyst is an (ironic) imposition upon the data. The thematisation of data is a feature of an approach to interviews as transparent conduits or "windows on the world" (D.R. Watson *op. cit.*) through which phenomena can be discerned independently of the interview. That is, talk which is ostensibly "of" a phenomenon instead of linguistic activity, occasioned for the practical purposes of an interview. As D.R. Watson states,

"we must speak [of phenomena] not only as embedded in this linguistic transaction called 'an interview', but '[phenomena] *for* the interview',

[phenomena] as local, *in situ* productions embedded in the local practices of the interview – practices such as sequencing and categorisation.”

(D.R. Watson 1998:7-8; emphasis supplied, brackets added)

This chapter does not regard the interviews conducted as part of a project *re* the Manchester city-centre bombing to be interviews “on” the bomb. To use “the Manchester bombing” as an ostensive definition of these conversations glosses the nature of these conversations as series of accounts and assemblages of linguistic activities produced by the interviewer and interviewees.

Chapter Eleven applies concepts drawn from Conversation Analysis to look at the presentation of talk in textual outcomes of research projects. Before that, however, the next chapter reports upon membership categorisation activities “in flight”. In so doing, it provides analytic purchase on the notion of observation in and for sociology: that reports of “sociological observations” are *post hoc* reconstructions of *in situ* activities of membership categorisation.

<sup>1</sup> The use of capitals designates the individual respondent to whom I refer. This procedure follows Sacks, who capitalised the word Member to designate individual members as opposed to courses of action.

<sup>2</sup> See Appendix for transcript notation used in extracts.

<sup>3</sup> Parker’s presentation in *The People of Providence* is used as an instantiation in the following chapter.

<sup>4</sup> This extract is analysed elsewhere (Carlin 1994:197).

<sup>5</sup> I use the expression “rather than” to highlight that the recorded extract from *actually happened*; this links to considerations of presentations of speech *versus* naturally occurring talk in the earlier chapter on recording/transcription and the following chapter on the textual presentation of linguistic practices.

<sup>6</sup> And, at the same time, implicitly furnishing support for D.R. Watson's arguments on the unwarranted bifurcation of sequential and categorial analysis; and in particular, his rejection of the view that Sacks abandoned his concerns with categorial analysis in favour of sequential analysis (D.R. Watson 1994a:181-182; 1994b:151).

<sup>7</sup> Note that the “MacRobert steps” example is an invented sequence of talk.

## **Membership Categorisation Analysis: Observation and Recognising “Normal Appearances” in Public Space**

Continuing the explication of research practices as linguistic activities, this chapter turns towards the generic and routine activity of observation. In examining the activities of observation this thesis is moving towards the procedural basis or “the how” of social analysis. It deals with unexplicated categorial order, taking visual appearances as a topicalised phenomenon.

A single-case vignette is presented so to make available the practices of observation. In line with the methodological policy of “ethnomethodological indifference”, this chapter remarks on the activities of observation whilst remaining neutral about what is being observed.

Also, in accordance with the procedural policy of “ethnomethodological indifference”, the vignette itself is available to analysis and interpretation. That is, this chapter does not seek to provide a “preferred viewing” of ethnographic observations. Therefore, this chapter resists an ethnographic “stipulation” of the categorial order in favour of the attempt to present data derived from observations as transparently as possible; hence, other researchers can “retrieve” the cultural methods used in the analysis, and suggest alternatives. In the previous chapter, I referred to the taking of a position which stipulates interpretations as the “Mildred Pierce phenomenon”, whereby “findings” cannot be criticised or disputed: a writer can reserve the right to uphold their preferred

interpretation because they alone were witness to events (Carlin 1998). Whilst “a solution” is presented before “the puzzle”, this solution is a candidate solution rather than “*the solution*” to the puzzle. Topicalising the linguistic nature of sociological research practices, “the how” of social analysis, explicates how a solution was arrived whilst

“abstaining from all judgments of [a solution’s] adequacy, value, importance, necessity, practicality, success, or consequentiality.”  
(Garfinkel and Sacks 1970:345)

The visual order is a linguistic order, in that members categorise each other ongoingly. The ongoing activities of categorisation produce a visual scene which is linguistically constituted and linguistically organised from within. This chapter notes how the categorial order is realised *via* observation of the visual scene.

Such considerations have relevance for sociology because sociologists approach visual settings as members of the self-same society they study, using the same common-sense methods as other society-members use. The sociologist is a member of society *imprimis*, in the first place.

### ***“Waiting for the World to Walk By”***

As mentioned in the intellectual profile, Rose conjointly taught on a course with Fred Templeton on field methods, held at Uni-Hill Elementary School. In his paper “Waiting for the world to walk by” (1994; see items #138 and #144 of the analytic bibliography), Rose discusses how “looking” is a routine activity. The resources for

this paper derived from observations he made as his eyesight was undergoing rapid deterioration. Even with the limited vision he had left, Rose elaborates upon Sacks' notion of the "viewer's maxim", which Sacks defined as follows:

"If a member sees a category-bound activity being done, then, if one can see it being done by a member of a category to which the activity is bound, then: see it that way."

(Sacks 1974a:225)

That is, *via* his use of the viewer's maxim, which holds even with limited vision, Rose was able to recognise how the visual scene was constituted. As Rose reports in his paper, the scene consisted of "an adult" walking past the railing upon which he was seated, i.e. "towards" the elementary school, and "a child" circling around the adult. Being walked to school is a category-bound activity, in which it is inferred that when seeing an adult and child walking towards a school on school-days first thing in the morning, that there is, recognisably, a parent walking a child to school; further, that a parent walks their child to school, rather than somebody else's child; and that a child walks to school with their parent, rather than another child's parent. In this visual scene, Rose recognised the adult as a parent of the child who skipped around them, and that the child attended the elementary school that they were approaching. Thus, Rose "saw" a parent escorting their child to school.

An alignment was suggested in the aforementioned intellectual profile *apropos* of Rose's watching a parent with their child whilst he was seated on an iron railing next to a school; and his early fieldwork encounter, watching Paul Radin talking with a tribal

chief whilst they were seated on a timber fence beside the Russian River. The significance of this event, for Rose, was the thoroughly linguistic nature of fieldwork activities. Indeed this is the connection between these events: the *aperçu* that the world is linguistically constituted. The routine, natural language practices involved in making sense of a scene—watching people, talking with people, listening to people's accounts—are done by lay people as well as sociologists. Looking (or “observing”) is not inert: members (as this thesis emphasises, lay people and sociologists) are active observers of the world, and actively attend to an intersubjective, visual order. Whilst Rose's vision was problematic, the inferences that the scene made available to him *via* the viewer's maxim were available to anyone witnessing the same scene.

### ***Observation as Analysis***

The chapter on transcription suggests that transcribing as a practical activity involves a form of “proto-analysis”. That is, the beginnings of future analyses are made during the activity of transcribing. Research is not episodic, then, as if analysis only begins when transcription stops. In similar fashion, observation may be regarded as generating analysis:

“it is one legitimate and fruitful way to approach materials, for the initial observations themselves, and in that that sort of sophisticated lay observation of a scene is one way that you come to find items that can be extracted and developed quite independently of the observations one initially made”

(Sacks 1992b:271)

A provisional analysis may be an epiphenomenon, then, of *in situ* observations. To reiterate, observations are made using ordinary, sense-making practices. So in Rose's

terms, a “natural” sociological account precedes the professional sociological account. When an observer makes observations, some observations are regarded as more/less relevant than others, whilst some observations may be regarded as irrelevant; some “observations” might not be “observed” at all, i.e. “seen but unnoticed” (Garfinkel 1967:36). The observer may make connections with the visual scene, e.g. as instantiations of activities, or previously identified phenomena, or that observations fit into an analytic framework.

Although this chapter contains a sequence of fieldnotes, it is concerned with issues somewhat different from the chapter on fieldwork, and is presented separately. The *caveats* on fieldnotes are applicable to this chapter. Some base-notes were made on the evening of the reported observations in mainland Europe, and were finished on the afternoon of my return to England.

There follows an ethnographic vignette, which constitutes an account of a series of observations through the course of one evening in Brussels.<sup>1</sup> That night in Brussels occasioned reflections on the nature of observation for sociology. The source and outcome of these reflections is the recognition that in becoming cognisant with a visual scene, observations *qua* ordinary member are made before observations (and analysis) *qua* sociologist. That is, ordinary, common-sense activities in the interpretation of a visual order occur and are arrived at prior to sociological interpretations.

**Vignette: An Evening in Brussels**

I was in Brussels for one night. I wanted to do some exploring but for the moment I could not go too far from the hotel: first, I had to find somewhere to eat and then cash some travellers cheques to pay the concierge. Once this was out of the way I resumed the exploring; from time to time I returned to the Grand Plâce to see its illuminations at night.

I took some photographs. As I replaced the camera in its pouch a man—he was wearing a yellow sleeveless jumper—put his arm out and stepped off the pavement directly in front of me. I had to stop abruptly otherwise we would have collided. I have no idea whether this was intentional. At the time it seemed quite a threatening gesture and anticipated a confrontation. I was in a strange town whilst he was a Bruxellesçois. Instead, however, he moved off with two other men so I relaxed. I relaxed because they spoke English: so he wasn't a Bruxellesçois after all. If there was going to be any disagreement I would prefer it to be in English rather than in French.

I walked behind them around side streets. The other two men had Northern (Yorkshire) accents, and they were talking about “Shearer and Sheringham”. Alan Shearer and Teddy Sheringham play football for different teams in the English Premier League, but play together only in the England side. So I inferred that they were talking about the national team. But I could not place the

game they were talking about. I moved up behind them to listen more closely for news on the England side.

As I accelerated to overhear better, the man in the yellow tank top turned away. So I suddenly realised that he *wasn't* with these two men; although from behind, the "tie signs" and spatial alignments indicated that they were all together. Indeed, that he had been close beside them all the time didn't appear to have been noticed by the two men.

This was all split-second stuff. The Euro '96 Championships were being held in England, and I was more interested in listening to a conversation in order to catch up on the results. The men were not particularly informative, and I stopped to watch the towering arrangements of fresh fruit and live crustaceans that were being set out on stalls in the narrow street.

As I passed a doorway, I caught sight of a television inside; some people were sitting below it, looking up at the screen: a football match, between players wearing white and orange shirts, which was available "at a glance" as an England *versus* Holland match. So *this* was the match the men had been talking about. I went into the café which was showing the England *versus* Holland match. It was rapidly getting darker, and neon signs were lighting up. At 4-0 up I settled my bill and set off to the Grand Plâce again.

This was to be my penultimate night-time visit there: the outdoor cafés had not as yet packed up their tables and chairs; there were still some people finishing their drinks. There were a lot of young men sitting on the museum wall, watching the tourists: people craning their necks looking up at magnificent buildings, cameras round their necks or at the ready, groups of people shuffling together whilst one of their number squinted through their camera's viewfinder, people carrying maps and looking at guidebooks.

Having paid the concierge I entered the Grand Plâce from the Central Station/Rue Des Eperonnier side. As I was standing with my back towards the museum I realised that the two elderly women next to me were speaking in English. They were a curious couple, standing in one corner of the Grand Plâce, facing each other, animatedly discussing a neighbour from back home in the UK.

Then I noticed—and again this is split-second—that the man wearing a yellow tank top was standing just beside one of them (the nearest one to me, with her back towards the direction of Central Station) with his head held back, looking diagonally across and towards the spires above us. He was standing as any other tourist would stand.

Well now I knew who he was—or rather *what* he was—and therefore what he was (or was not) doing. Of course what he was not was a tourist; what he was

not doing was admiring the view. He was eyeing-up these women's pockets, or bags, with a "pickpocket gaze": where would he find money or valuables, an assessment made whilst affecting or mimicking the activities associated with being a tourist.

I was genuinely fascinated by his passing as a tourist, but gripped in a dilemma: there are two elderly women (i.e. categorisable as "vulnerable to pickpockets"), who speak English (therefore they would understand any warning that I might cry out) and I hoped to stop this man from taking advantage of them. Maybe he noticed me staring: he walked away. Perhaps he had stolen something as he walked away; I could not tell and shall never know.

I went for another walk. There were a lot of tourists taking photographs at the bottom of a street, so something of interest was happening. It was the Manneken-Pis. I returned to the Grand Plâce to watch the illuminations come on. I started to get bored and became more interested in the shutting down of the cafés: people being asked to drink up and move on, taking the furniture inside the cafés leaving customers isolated, etc. (Here I made a note about the waiters requesting customers to leave: I recognised this was the gist of the exchanges even though I did not understand the spoken language.) Whilst watching plastic tables and chairs being removed from the café alongside the museum, I noticed the yellow tank top. It was the pickpocket again.

He was standing with, among, a group of Asian tourists. It did strike me as odd that he had the audacity to manœuvre himself *into* a group of people. His manner was of following the flow of conversation—he even seemed to laugh as they laughed rather than standing adjacent to them. Suddenly, the group, or a member of the group, made an exclamation: they all ran towards a brightly-lit window (it looked like jewellery from my position). “Ran” is the wrong word: they moved less than ten yards, very fast. They huddled together, staring at the glittering stuff, and I watched the man in the yellow tank top rush with them. His trajectory was slightly different though; they rushed headlong, whilst he arrived at the same place by moving in an arc, finishing up nearest to me, staring into the window. If I had not known otherwise, I would have thought he was in their group, a friend or relative. At a glance he was in their group. He passed, or attempted to pass, as being with them.

He turned away and looked at me. At this point I do not know whether has stolen anything or not. I was looking directly at him (I had been staring at him for some time) and had a notebook and pencil in my hand. I was in the process of putting the book into my jacket pocket (it was catching on the pocket-lining so I had to take some time over it): I held the flap of the pocket up so that the red cover was visible. I knew I was smirking at him: he began walking away, “hugging” the museum wall, looking at my uplifted pocket flap. He caught my eye again and disappeared. By that I mean I turned to my left, where the women were still talking, but did not see him.

### ***Observations on Pickpockets***

Before discussing this vignette, this section draws together some extant considerations of pickpockets. From previous ethnography's we learn about "jack-rolling" and the practices involved in jack-rolling, e.g. finding a suitable target or "live one", luring the target into a trap—"supposedly for [whatever] purposes *but really to rob*" (Shaw 1966:97; brackets and emphasis added).

We can see how pickpockets operate in teams, in terms of "mob work" (Maurer 1964:56-102; Sutherland 1937:44-48). Maurer and Sutherland provide detailed explanations and glossaries of the pickpockets' argot. They both supply descriptions of pickpocketing activities, the roles of each member of the cannon mob (where "the term "cannon" is used to designate the pickpocket and also the racket of picking pockets"—Sutherland 1937:44).

The visual order is oriented to by pickpockets; "suitable" targets are those who are likely to be carrying money, as well as being unfamiliar with their surroundings. Suitable targets are "profitable" targets:

"Professionals become quite adept at selecting such victims [those carrying sizable amounts of money] *by sight*, then following up their selection with a rapid and deft manual exploration of the person to ascertain where the money or wallet may be carried"

(Maurer 1964:57-58; brackets and emphasis added)

This preliminary activity, known as “fanning”, may not be necessary if the pickpocket sees the wallet being put in a pocket, or by warning signs of criminal activity: notices such as “Pickpockets operate in this area” or

“‘Beware of Pickpockets’ [are] helpful, for whenever a sucker sees th[ese] sign[s] he feels the pocket in which his money is located to discover whether his pocketbook is still there, thus relieving the mob of the necessity of fanning him.”

(Sutherland 1937:44-45)

In Kirkgate Market, in Leeds, there are regular Tannoy announcements such as “*There are pickpockets at work in the market. Keep all purses and personal belongings in a safe place.*” Perhaps Chic Conwell, the thief in Sutherland’s study quoted above, would have appreciated this technological advance.

Maurer and Sutherland, though Maurer in particular, discuss the subtleties of picking a pocket, and how the mob skillfully manoeuvre their target into position. Pickpockets are adept at causing confusion or distraction, e.g. falling or “feigning drunkenness” (Sutherland 1937:45). Another sort of distraction occurs in the crowded streets of Bombay, where one of the gang-members pretends that he has been pick-pocketed; he causes a disturbance in order to raise (and distract) the attention of others around him. Meanwhile, during the commotion (diversion), another gang-member helps himself to a bystander’s wallet.<sup>2</sup> Pickpockets may also use a concealment device so that the target momentarily focuses away from their pocket and attends to the “throw” (Sutherland 1937:45) used to “shade” (Maurer 1964:67) the hand which picks the pocket, e.g. a coat, or a newspaper.

We know that we may be more at risk from a pickpocket in different places, that pickpockets are in “certain localities” or “*working right spots*” (Maurer 1964:59-60; emphasis supplied); or are in attendance at certain events. These include the London Underground, Camden Market, the Notting Hill Carnival, the Glastonbury Festival, the markets in the Old City of Jerusalem. Pilgrims’ guides (human as well as textual) are unequivocal:

“Handbags, like shoulder bags, should have a strap and be kept zipped. The crowded markets are a happy hunting ground for bag snatchers and pickpockets—some of them fellow pilgrims.”

(Richards 1994:20)

It should be noted here, however, that pickpockets do not always operate in crowds. According to Maurer (1964:174), this is a popular misconception. Public spaces are worked by pickpockets, whether they are crowded or not.

In his vivid description of a ‘shooting gallery’ in East Harlem, a safe-place where drug-users can go to inject, Phillip Bourgois recounts conversations with Doc, the manager of the shooting-gallery. Doc’s recollection of Malcolm X does not concern Malcolm X himself, nor his teachings, but is instead based upon his activities as a pickpocket. Doc is sure that he heard Malcolm X because Malcolm X had been a speaker on 125<sup>th</sup> Street, which was where Doc “used to ‘work’ sidewalk crowds rapt in attention before charismatic speakers” (Bourgois 1998a:60).

Denver airport has a particular problem with pickpockets. This may not be a surprise when we see tourists pass through Denver International Airport, a gateway to the Rocky Mountains and skiing holidays: skiing is an expensive sport, there is a lot of money to be stolen from the crowd. Frederic Thrasher, in his early ethnography of 1313 street gangs in Chicago, notes that

“In the heart of the Ghetto is the crowded Maxwell Street market, one of the liveliest and most picturesque spots in Chicago. Some of the Jewish gangs, like Itchkie’s ‘Black Hand society’, a pickpocket ‘outfit’ find excellent opportunity for sport and prey along this thronging Rialto; nor is it overlooked by gangs of other regions.”

(Thrasher 1927:12)<sup>3</sup>

Activities of pickpockets are amenable to Membership Categorisation Analysis. Sharrock’s (1974) paper on ownership can be brought to the study of observations on pickpockets, such as Maurer’s observations that the pickpocket

“has a certain proprietorship in his district or street; there is even a sense of pride about it. He may tell you frankly that he *owns* it.”

(Maurer 1964:173; emphasis supplied)

Sue Scott provides a vignette which illustrates how the observation of visual scenes is categorially or linguistically organised. On a ferry crossing, Scott (1997:24) reports how she witnessed a man approach a woman, who was asleep on the deck. Without waking her, he unzipped her bumbag and removed a personal stereo. For Scott, there was a degree of equivocation *apropos* of the nature of this observation: “theft or intimacy?” (Scott *ibid.*). *Via* her use of the “viewer’s maxim” (Sacks 1974a:225), Scott saw it as being an act of intimacy between lovers. This explanation is preferred by

Scott, because it appeals to the notion of a summertime-cruise-around-the-Greek-islands being a category-related activity—where categories are “duplicatively organised” (Sacks 1974a:220), i.e. partner-partner. However, Scott suggests that there may be an alternative explanation, that it may have been an act of theft instead. It is not the correctness of one version of events rather than another version that is a methodological concern. Rather it is how members arrive at realisations—regardless of their adequacy—and the explication of realisations *via* members’ social-cultural practices that is of concern.

### ***Recognisability: Observation and Membership Categorisation Activities***

I did not see this person pick anybody’s pocket in the Grand Plâce. Nevertheless, I recognised him to be a pickpocket. Here is a sociological problem, then: without any evidence to confirm his being a pickpocket, *I saw him for what he was*. I did not see him engage in *any* criminal activity. Yet still I claim to have *seen* a pickpocket. This was the first pickpocket I have ever recognised as a pickpocket. But without any previous experience of pickpockets, how did I realise that he was a pickpocket?

I am emphasising the visibility aspect because, I presume, pickpockets should blend into a crowd—surely pickpockets rely upon their invisibility?<sup>4</sup> I do not possess any unusual insight; I have not received any special training or been through an education programme which taught me to spot criminal activity; nor am I a phrenologist, looking at the size and shape of people’s heads as “clues” to the possession of criminal

tendencies. Nor could I tell whether anybody was a pickpocket: I take it that pickpockets depend upon our very inability to spot them in order to steal from us.

Now I should say that in the light of this observation I did not set out to find pickpockets as a subject of study. From what I have just said I think we may regard pickpockets and their activities to be an elusive subject of study. The reason I am talking about this pickpocket is because I happened to be there, and I happened to see him. Whereas the ornithologist may yearn to observe a certain species of rare bird, it never occurred to me that I would have the opportunity to “spot” a pickpocket. I recognised rarity value of “seeing” a pickpocket; thus I “wrote-up” fieldnotes made at the scene and in the hours which followed. I do not expect to have the opportunity of “spotting” a pickpocket ever again.

I also recognise, however, that this may be quite dissatisfying for readers. After all, I am talking about a series of observations: there are no photographs, and no video data. (I do not imagine that one could, either, for I assume that any self-respecting pickpocket would not allow themselves to be caught on camera.) In Sacks’ terms, this chapter presents readers with

“an altogether informal, unproved, perhaps unproveable, perhaps irrelevant to prove it, characterisation of what took place.”  
(Sacks 1992b:271)

I cannot prove anything, and I am asking you to take my word for things: that this actually happened and that this actually happened *this way*. However, in this thesis I

am not interested in *proving* that I saw a pickpocket. After all, I could have been completely wrong. It is of interest that I can *legitimately* claim to have seen a pickpocket, that I did indeed see things *this way*. *Because* I didn't see him pick anyone's pocket, perhaps the question is not whether he *was* actually a pickpocket. I take it that the topic of inquiry is *how* I saw things, that I saw things one way rather than another. These considerations of the observation of a pickpocket have been made as an ordinary member; inferences drawn from observations are warranted by my membership in society. Although the "data" for this chapter are not retrievable, such as transcripts or recordings, the inferential and categorisational work done is made as transparent as possible for readers to adjudge the warrant of observations.

Sometimes we recognise individual people as being representatives of certain type of person. That is, we categorise them. As mentioned above, I have not been trained to "spot" criminal activity; however, we can learn to categorise people and we can be taught to categorise people. From hospital settings, David Hughes (1977) outlines some of the features of recognising and categorising patients, that categorisation is not necessarily based on medical expertise but also on common sense judgements; Lynn Sbaih (1995) notes how "triage" nurses recognise and differentiate between urgent and not-so-urgent patients as they enter the Accident and Emergency Room. The triage nurses "prioritise" patients; they adapt the queue of patients as it may be necessary when new cases present themselves for treatment. Their professional competence enables them to know "what is different" (Sbaih 1995:63). So the A&E queue is a "special case": whereas queues display the coincidence of sequence and category (D.R.

Watson *op. cit.*), so that “sequential turns” in the queue can be seen as “categorial turns” (such as next in line, next-but-one, etc.), to be “next in line” in the A&E queue is contingent upon the arrival of emergencies categorised as “more urgent” medical emergencies.

Following Sacks, we find that some people are trained and experienced at “seeing” what is different; however, it is not just trained personnel who utilise as an observational method the “incongruity procedure” (Sacks 1972a:283). By “incongruity procedure” he meant that by being able to recognise who and what was “normal” at a certain place, anyone or anything different or out of place would become noticeable. This is exemplified by White men *en route* to a corner to buy heroin in a Black neighbourhood:

“Our pace was perhaps just a little too fast; our heads were bent a bit too low; and our arms were swinging just a little too fast and wide; but we tried to act like normal white pedestrians strolling innocently through East Harlem at midnight under a freezing December drizzle.”

(Bourgois 1998a:38)

From the above quotation, normality varies with time as well as place. What is normal on a weekday may not be normal at the weekend; what is normal during the day may not be normal at night.

In the Grand Place, I did not recognise and categorise this man as a pickpocket by looking at him in a “passing glance” (Sudnow 1972). Recognising and categorising his

activities took time; I came to the realisation that he was a pickpocket, rather than “instantly” recognising him as a pickpocket. Garfinkel formalised a family of members’ methods as the Documentary Method of Interpretation: with the historicoprospective unfolding of events, i.e. the mutual elaboration and revision of underlying patterns and indexical particulars, I recognised a man to be e.g. Bruxellesçois, English, then Bruxellesçois again. Over a course of time, I began to understand what he was doing:

“As the police are oriented to using appearances as evidence of criminality, so criminals are oriented to using appearances as fronts, i.e. as hindrances to recognition.”

(Sacks 1972a:284)

Upon closer inspection, I recognised his activities to be recognisably “tourist” activities, i.e. he was engaged in activities which any new visitor to the city would be involved in. However I had previously observed him in different areas of the Grand Place and in streets surrounding the Grand Place—remembering how I had been wary of him because I recognised him to be a habitué, familiar with the place, whilst I was the visitor. And I recognised him to be a habitué even though I had never seen him before in my life, and knew nothing of his personal biography. In terms of ownership, I knew that this was *his* “home”.

This was not based on professional competence, nor intimate knowledge of the normal appearances of the setting, nor of persons within that setting—I am not a police officer and this was my first visit to Brussels and the Grand Place—I saw this man presenting

himself for all practical purposes as “a tourist” even though I knew he was not a tourist. I recognise that

“tourists’ comprise visible kinds of persons, how their presence, appearance, accoutrements, ways of talking show who they are and make plain the purpose of their activities to anyone who should witness them.”

(Watson and Sharrock 1991b:6)

In another context I learned how to distinguish between “tourists” and “pilgrims”. I do not consider that “expert” or “inside” knowledge in order to see someone presenting themselves as a tourist when they were a habitué, however, as tourists and pilgrims engaged in the same activities, e.g. looking at guide-books, reading maps, taking photographs, visiting “sites of interest”. For what reasons would he be doing this?

Some researchers have found that people in “marginalised” or “stigmatised” groups engage in the work of passing. Harald Eidheim discussed a small population of Lapps living in northern Norway. He found that the stigmatised Lapps restricted their use of their own language:

“Lappish must be regarded as a secret language or code, regularly used only in situations where trusted Lappish identities are involved.”

(Eidheim 1969:45)

This is an instance of what Erving Goffman (1968:31-45) called “the own” and “the wise”: the wise are those people who know a person’s true identity, with no need for any façade. The own are those people who are unaware of a person’s true identity, and from whom their true identity will be concealed.

In order to consistently pass as having an identity different from your real identity, the building up of forms of evidence is necessary. Graham Watson (1970 *op. cit.*) showed Black South Africans in Colander, under the Apartheid regime, who were “passing for White”. Only White people were eligible to perform certain kinds of work. Even if they did not appear to be White, if a person performed such tasks, it was assumed that they could not be Black. Poverty was associated with being Black rather than White. Living in a Whites-only area was also taken as evidence of a “White” identity; so too were the language spoken (Afrikaans was associated with poverty, which was associated with being Black) and membership of a tennis club. If suspicions were raised about a person’s ethnic identity, a pass-White could reply *“I do a Whites-only job, I live in a Whites-only area, my children go to a Whites-only school, I’m a member of a Whites-only tennis club, and I don’t speak Afrikaans.”* A combination of these is taken as evidence that a person is not-Black.

Harold Garfinkel (1967 *op. cit.*) gives an account of Agnes, an inter-sexed person. Agnes was born male, but when Garfinkel met her she had been living as a female for two years. Within that time she had been learning how to be seen as a woman rather than as a man. At the time of Garfinkel’s study, a person was seen *either* as a woman or a man. Like the pass-Whites, not being seen as one meant that a person was seen as the other. Agnes knew that if people didn’t see her as male then she would be seen as she wanted, as female. Garfinkel learned from Agnes how people are seen at first

glance and over time to be a certain sex; and how the knowledge of sexual comportment can be exploited in order to pass as a member of a different sex.

There is also the problem of establishing a particular shared cultural identity. For example, how do you make sure that you will be recognised as an American prisoner of war rather than a Vietnamese guard, and that the person you are communicating with is also an American prisoner of war (Butler 1977:403). Part of this involves knowing how to pass as a member of a particular category, it involves certain cultural knowledge.

In Butler's case, he was talking about a "pass-code"; something which Americans would know and they could rely upon their Vietnamese guards *not* knowing. Here, what this thesis is referring to is something cross-cultural: things that tourists normally do and habitués do not.

So the same common-sense practices with which we recognise someone as being female, or White, can be relied upon by someone who wishes to pass as female or White. As Sacks said,

"Persons may exploit an ability to present appearances to which they are not entitled."

(Sacks 1972a:284)

Also, these same interpretative methods can be clues for pickpockets when they are "taking in the sights", or, in other words, looking for 'live ones'" (Shaw *ibid.*). As mentioned above, the visual order is an intersubjective order. Members, on a routine

basis, see what other people see; in his “police paper”, Sacks (1972a) elucidates how different finite provinces of meaning open up different ways of observing the visual order. Similarly, Goodwin reports that

“An archaeologist and a farmer see quite different phenomena in the same patch of dirt (for example, soil that will support particular kinds of crops *versus* stains, features, and artifacts that provide evidence for earlier human activity at this spot.”

(Goodwin 1994:606)

Elsewhere, this thesis reports how the police see everyday objects “in terms of favorite misuses”, e.g. that “[g]arbage cans are places in which dead babies are thrown” (Sacks 1972a:292). Sacks did not limit the activity “doing looking” to the police, however:

“for a bunch of people on a train, each of them may have a different way of seeing who else is there. That can be suggested in a fairly obvious way. One guy on the train could see the others as ‘whites’, one guy may see the others as ‘marks (e.g. if he’s a pickpocket), and various other such ways of seeing what the others are.”

(Sacks 1992b:6)

This thesis suggests that pickpockets, looking for potential targets—see “live ones” as those people likely to be carrying money (rather than no money) in the same way that people in Colander see someone to be White (rather than Black), or female (rather than male), or as a tourist (rather than as a pilgrim, or a pickpocket).

### ***The Manchester Bomb: A Case-Study in Normal Appearances***

This section looks at how teams from the Emergency Services found a bomb before its detonation in the city-centre of Manchester. Schegloff (1999:27) emphasises

Goffman's influence on Sacks' paper on police work and normal appearances, that Sacks used manuals of police practice rather than observational study. This section follows Sacks' procedure in his "police paper", in that it relies upon textual materials (oral accounts of looking for a bomb or "security sweeps" are presented in Chapter Eight). The collaborative nature of interviews, using extracts from conversations about the Manchester bombing, is considered in Chapter Nine.

On Saturday 15<sup>th</sup> June 1996, Manchester was especially crowded. It was the Saturday before Father's Day, and the city centre was filled with more shoppers than is usual. England were hosts of the European Championships, Euro '96. So the numbers of people in the city centre were also swelled by football supporters from Russia, Germany, France and Spain, for matches at Elland Road in Leeds, and at Old Trafford the next day.

What happened in Manchester, over 12 months later, has been pieced together. First, a short bloc of talk with the "Fire Chief" is presented; following this account, the reader is provided with a countdown to the explosion. This sequence of events has been compiled using chronologies prepared by Greater Manchester County Fire Service (Rigby *et al.* 1996), Paul N. Williams of Manchester City Council; and from conversations with members of the Emergency Services who were involved. First, an elicited account from the "Fire Chief" (FC).

FC: Euro Ninety Six was there so we had up, a lot of foreign visitors in the city as well (.) erm and (.) we found a, the white vehicle (.) it was a Ford cargo van seven point five ton erm parked on Corporation Street just near M and S (.) well down on its axles (.) er and at that juncture because they just said there was a bomb within the city now A we didn't know whether we've had smaller bombs we've had sent little incendiary devices we didn't know what we were looking for. Was it was it small was it large

AC: There've been bombs in litter bins haven't there?

FC: absolutely it could have been absolutely anywhere. So nobody really knew it was like looking for a needle in a haystack really. Erm, but "you know" this this vehicle was found and then as I say because it was down °((laugh))° on its axles and when you looked at it it was you could see what was going to happen. So then there was a mammoth task of evacuating eighty thousand people from the city, a tremendous number of people, er from the city centre (.) and you know when people, you you you're doing cordons and then people doing limbo dances under the tape you know

AC: Hm.

FC: to get in "I just want to change this" and "I won't be long" you know "I've just come from Northwich can I just change this" you know. I don't er, "you know" that's the psychology of people I, I, I, I can understand to a certain degree but erm (.) that was quite a mammoth task so ourselves in conjunction with the police embarked on the, the evacuation. Er, the (.) the bomb disposal team were er notified and they were coming from the Liverpool area erm (.) and they, they came on site. A few things happened there which, from which, from a national ((laugh)) security point I would I won't say

Secondly, here is a "chronology of events" from the morning of the explosion. The summary items compiled from textual materials (itemised above) manifest how members of the Emergency Services attend to the "normal appearances" of objects and settings:

09:22: "The vehicle carrying the bomb, a white van, is parked and abandoned on Corporation Street, outside Marks & Spencer. A traffic warden gives it a parking ticket."<sup>5</sup>

09:43: A number of coded telephone warnings are received, by Granada Studios and a hospital in North Manchester. The caller is a man with an Irish accent. The calls give the general but not the precise location of the bomb threat. There are further calls to Salford University, Sky TV and the Headquarters of the Garda in Dublin, also using recognised codewords. Greater Manchester Police's emergency procedure is activated.

The calls warn that it is a "big bomb".

10:02: "The van is located by P.C. Wendy McCormick".<sup>6</sup> It is a 7.5 tonne lorry, with its hazard warning lights flashing. Police Control receives a report that "there are wires coming out of something plastic."<sup>7</sup>

The evacuation of the city centre begins.

The "Fire Chief" tells us that they suddenly understood why the warnings said that it was going to be a "big" bomb. It is in a truck, which has the capacity to hold a lot of explosive material. And the truck is found "well down on its axles".

The "Police Chief" tells us that "the fact that it was in a lorry gives you a pretty good indication that it's something special."

10.13: A bomb disposal team is dispatched from Liverpool; police helicopter equipped with 'Skyshout' public address system airborne.<sup>8</sup>

"[A] full-scale evacuation of the 80-90,000 people in the city centre begins. Over the next hour the immediate vicinity is successfully cleared but many shoppers remain at the edges of the danger zone assuming that they are at a safe distance or that it is a false alarm."<sup>9</sup>

The "Police Chief" tells us that the football championships actually helped the evacuation. He tells us that the police had set up a large presence in the city. If the police had not already been mobilised, it may have been very different.

The Fire Service were unaware that at these initial stages, buildings are being evacuated using Automatic Fire Alarms. This results in the mobilisation of crews into unsafe areas: some crews are unaware that they are traveling towards a bomb.<sup>10</sup>

11:00: The bomb squad arrives. A cordon of 400m is placed around the vehicle, with the bomb squad working at a distance of 200m.

11.09: Other possible devices are being reported to Control.<sup>11</sup>

11.12: A remote controlled “wheelbarrow” is in position, and in three minutes carries out a controlled explosion.<sup>12</sup>

11:14: A warning is shouted to get clear.<sup>13</sup> A small explosion is heard.<sup>14</sup>

11:16: The bomb explodes. This is approximately 30 minutes earlier than the telephone calls had warned.

Seeing the size of the explosion, a “major incident” is declared.<sup>15</sup>

So following Sacks (*op. cit.*), we find that some people are trained and experienced at “seeing” what is different; yet it is not just trained personnel who utilise the “incongruity procedure” (Sacks 1972a:283) as an observational method. For example, I have not received any formal training in spotting a pickpocket operating in a public place. However, through my use of the incongruity procedure, I was able to recognise and observe the activities of a person as a pickpocket’s activities. The incongruity procedure is tacitly employed during “security sweeps”: based upon their knowledge of “normal appearances” (Sacks 1972a:284) of a setting, people are able to recognise incongruous/suspicious persons, e.g. someone who may be planting an explosive

device, and incongruous/suspicious objects, e.g. a bomb which may be disguised as something which appears “normal” in that setting.

The Emergency Services in Greater Manchester had to locate a bomb<sup>16</sup> within the city centre, in a limited period of time: they may not know what the bomb looks like, but, should they see it they recognise it for what it is—a bomb. A large, unmarked van is parked in the city centre. There is nothing unusual about this. However we have to consider that the Emergency Services were looking for a bomb, and the coded telephone messages had warned that it was a “big” bomb. As reported in the chapter on interviews, the Police Chief tells us that “the fact that it was in a lorry gives you a pretty good indication that it’s something special.” A van can hold a lot of explosive materials. It was parked on double-yellow lines. This is a traffic violation. The van was parked illegally; it shouldn’t have been there. Its hazard warning lights were flashing. This is suspicious.

However, it is suspicious *because* they are looking for something suspicious, a bomb: a van, parked on a double-yellow line with its hazard warning lights flashing, (perhaps its driver is, for example, collecting photographs from Boots or withdrawing cash from an Automatic Teller Machine?), is not a suspicious object in a busy city centre *per se*. This can be highlighted with reference to the differential treatment afforded to the van by the traffic warden and the Emergency Services: before the bomb warning, the van was illegally parked;<sup>17</sup> after the bomb warnings, the Fire Chief tells us that the van is found “well down on its axles”—the van is heavily loaded. So this appears to be the

bomb and it looks big. Furthermore, a report is received that “there are wires coming out of something plastic.” (Rigby *et al.* 1996:2) The officer who files this report recognises the normal appearances of a van, and also recognises that “something plastic” with wires attached is not a normal appearance of a van.

### ***Connections of Research: Belgium and the Manchester Bomb***

The vignette reported in this chapter is an account of my final night in Belgium. The following morning I would fly back to Manchester Airport. As it so happens, I had flown out from Manchester to Brussels on the Saturday morning previous to this. My hosts were pleased to see me when I arrived: I did not know it then but a bomb had exploded in Manchester that morning. From radio reports, they could not ascertain the extent of the explosion, nor the “chaos” it occasioned; whether my journey to the airport might have been held up by the emergency services closing-off the city-centre, or whether flights were interrupted.

Just over two years later, I would return to the site of the explosion and meet with people who provided oral accounts of that Saturday, and how their lives had been subsequently affected. I was informed how the chaos mentioned above was managed by the emergency services. However, for operational reasons, that is, “from a policing operational perspective”, the relevant bloc of talk is cut short. Hence, below is an edited, elicited account of the management of that evacuation (PC = “Police Chief”).

1. AC: Could you tell me about coming into work for the first time after the bomb?
2. PC: It was um ((cough)) I suppose it was like the aftermath of a nuclear war really. Because the city centre was(.) er, a ghost town. Because of our security cordon, you could walk through the city centre of Manchester and it was it was though some sort of horrible plague had wiped everybody out. There was er devastation everywhere(.) um and(.) you had(.) everywhere you could hear security alarms burglar alarms because of course they'd been triggered(.) um and you were walking around through all this devastation and all you could see(.) was damage(.) debris but no people.(.) It really was quite eerie. Um, and it was certainly quite eerie actually going in to the area where the lorry bomb actually was and and and and looking at the extent of the damage. Er
3. AC: °Right.°
4. PC: And it was quite a, a scary feeling(.) because on that Saturday morning(.) there'd been over eighty thousand people in the vicinity of that area. It was the it was Father's Day weekend there were a lot of shoppers in the main shopping area, the heart of Manchester. And erm, you've got to ask the question that if we hadn't found it before it went off, what would have happened.
5. AC: °Mm.°
6. PC: I I suggest there would have been thousands of deaths.
7. AC: Euro 96 as well
8. PC: Euro 96 we had the all the visiting football fans as well it. It would have been erm, a catastrophe, an absolute catastrophe
9. AC: And Euro 96 would have caused its own policing problems
10. PC: Well you, it, yes yes and no. Euro 96 was probably responsible(.) for the success of our operation that day, because erm(.) you you've got to sort of remember that the Euro 96 programme was ongoing ((cough)) and because of the large number of visiting fans and the, the public order potential of football matches, we had an operation running and on that day in question we had an emergency control(.) up and running in this building, staffed with trained senior officers(.) all the IT support, and we had extra officers on for the city. So we had in place a lot of additional resources, we had an emergency control, up and running(.) to deal with football but of course on that day it had to deal with other things.

11. AC: "Mm."

12. PC: So if we hadn't had that emergency control up and running it would have been a different story altogether.

### ***Conclusion***

Considerations of a pickpocket within this chapter have been speculative. They involves, to some extent, "guesswork". After all, I did not see any crime committed. This is instead a report of what "street ethnography" might usefully consist—the methods we use to make sense of appearances of public space.

Fundamentally, it moves towards the explication of observation as a linguistic, categorial activity, a gloss for an assemblage of members' methods such as categorisation, inference-making and the Documentary Method of Interpretation. At the heart of observation is the issue of *recognisability*: how visual scenes are recognised, and made recognisable, by members.

<sup>1</sup> The following vignette was presented as an addendum to a paper given in Belgium (Carlin 1998).

<sup>2</sup> I wish to thank Anjalee Tarapore (personal communication, 11 May 1998) for this observation.

<sup>3</sup> Maxwell Street market is the site of the wager between Mr. Cohen and Mr. Goldberg, adjudicated by "Charlie the Policeman" (Wirth 1928:236), which is reported for its texture of membership categories in the prior chapter on "The practical work of pseudonymisation". One gang-member elaborates the use of membership categories at this site, saying "Gang excursions to "Jewtown" [Maxwell Street] for purposes of robbery were frequent" (Thrasher 1927:142; brackets supplied).

<sup>4</sup> Maurer observes that this is a naïve presumption: "Because the public has been impressed with the fact that pickpockets work in crowds, there is a widespread belief that they cannot work anywhere else. This is not true" (1964:174).

<sup>5</sup> Paul N. Williams.

<sup>6</sup> Paul N. Williams.

<sup>7</sup> Rigby *et al.* (1996:2). Quoted with permission.

<sup>8</sup> Rigby *et al.* (1996:2).

<sup>9</sup> Paul N. Williams.

<sup>10</sup> Rigby *et al.* (1996:5).

<sup>11</sup> Rigby *et al.* (1996:2).

<sup>12</sup> Rigby *et al.* (1996:2).

<sup>13</sup> Paul N. Williams.

<sup>14</sup> Rigby *et al.* (1996:2).

<sup>15</sup> Rigby *et al.* (1996:3).

<sup>16</sup> The Emergency Services cannot legislate for the placement or location of bombs. In September 1997, a series of bombs in apartment blocks in Moscow caused widespread panic. On September 13, the city's mayor, Yuri Luzhkov, ordered all the residential blocks in the capital to be searched by security forces. Hours after a nine-storey residential apartment block was searched for explosives in the southern Russian town of Volgodonsk, near the borders with the Caucuses, a lorry-bomb was parked outside it.

<sup>17</sup> From Richman's (1983) ethnographic study of traffic wardens and their "street-craft", it is feasible that the traffic warden had observed the location of the van earlier during their "beat", and noted the position of air-valves on the wheels to check whether it was moved temporarily to avoid incurring a parking ticket on their next "round" (1983:69). However, the van was parked at the crossroads between the dual carriage-ways Cross Street/Corporation Street and St. Mary's Gate/Market Street; perhaps Richman's traffic wardens would have adjudged this junction too busy to allow for a "second sighting" before giving the van a ticket?

## Writing Up (1): A Textual Presentation of Linguistic Practices

Previous chapters have looked at the linguistic constitution of, *inter alia*, observations and the interview, that is, the interview as an interactional, endogenous accomplishment of the interview by the parties to the interview (both interviewer and interviewee). Chapter Nine showed that the interview was internally and linguistically organised—a series of linguistic activities produced by both parties.<sup>1</sup> The linguistic constitution of the interview is made visible by the ethnomethodological programme of Conversation Analysis, which highlights the collaborative nature of members' interactional products.

In this chapter, attention is focused upon the writing-up of linguistic activities occasioned "in the field". These linguistic activities would include, *inter alia*, talk with informants, and "extra-research" activities (that is, assembling or "encountering", "pre-interview" and "post-interview" activities),<sup>2</sup> as well as interviews *per se*, etc.

### ***Conversation Analysis and the Analysis of Texts***

Unlike McHoul (1987), I do not wish to aver a conversation-analytic treatment of virtual, by which I mean *ersatz*, non-verifiable or fictive discourse. McHoul attempts to subject fictional materials to conversation-analytic inquiry, which he claims already have "equivalents for a deal of transcription conventions" (1987:89). These "equivalents", it turns out, are no more than authors' descriptions of verbal and non-verbal actions by fictional characters.<sup>3</sup> Even if we concede that fictional works contain "identifiable" structures of talk—and such structures are surely the result of writers' familiarity with conversational structures by virtue of their membership in society, i.e.

some writers happen to better observers of social life than others—McHoul seems to have his line of argument the wrong way around. The *raison d'être* of Conversation Analysis is to identify phenomena of order in social-interactional settings, which requires records (in audio/visual and transcript formats) of talk. Hence, McHoul's reasoning should not be a form of literary criticism, i.e. to subject stretches of dialogue in novels and plays to conversation-analytic study, but how Conversation Analysis can inform the reading of talk. That is, how the ethnomethodological programme of Conversation Analysis makes presentations of talk available as linguistic, social-cultural accomplishments.

In this chapter I bring the 'machinery' of Conversation Analysis to a passage in Tony Parker's book *The People of Providence*. By such means this chapter examines critically the status of presentations of people's talk. It seeks to demonstrate that the presentation of research activities edits out the linguistic, interactional nature of these activities.

Throughout this chapter, I shall introduce certain provisos *apropos* the analytic status of the critique presented herein. A naturalistic analysis of materials is precluded by the unavailability of naturally occurring phenomena, which have been written out of the account (see below). Claims are illustrative, and explicitly so.

As noted earlier (in the section "The Visibility of Notices of Confidentiality" of Chapter Five), "'Providence' is [...] a fictitious name" (Parker 1985:373).

### ***Conversation Analysis and the Analysis of a Text***

The Introduction (Parker 1985:11-13) contains various person's reactions to Parker's announced intention to write a book about the "Providence" housing estate, including a caretaker, a young woman, an elderly man and a teenage boy—these categories are supplied by Parker (1985:11-12). It can be inferred that each respondent's utterances are compressed into an amorphous bloc of talk: whilst preserving the vernacular talk of the respondents, Parker has sanitised that talk through practices of redaction, i.e. textual arrangements or editing procedures deployed in the compression of talk.<sup>4</sup>

It can be inferred further, from the first (the mobile caretaker) respondent's talk, that he is answering questions; e.g. the passage

"In one word? If you asked me to sum up the estate for you in one word what'd I say? Well, I don't think I know really, it's very hard isn't it, just one word?"  
 (Parker 1985:11)

This passage can be seen as an answer to a question or gloss-elicitation that was possibly worded "*If you could sum up the Providence Estate in one word, what would you say?*" or such like.

A further circumstance or particular that points towards an edited interview format is the use of the caretaker's gloss—"mixed"—as a prompt in the subsequent extracts or blocs of talk. The warrant for the assertion of this particular—that "mixed" becomes a prompt—is supported by the young woman's elliptical remark "*Well, if I could think of*

*one word to tell someone what a place is like... ”* (Parker 1985:12) is positioned prior to the question “*Mixed?*”, which is surrounded by quotation/diacritic marks.

The diacritic markings suggest that “*mixed*” is a repeat of a “candidate answer” (Pomerantz 1988) furnished by Parker. Pomerantz suggests that “one of the ways that we elicit information is by incorporating a candidate answer in a query” (1988:360). So the linguistic practices that conversation-analytic researchers are “sensitised” to suggest that Parker’s prompt was a candidate answer. This observation is warranted, along with the further inference that Parker engages in clarifying work even though intervening clarifiers are absent from, i.e. have not been preserved in the text, by the comment

“Well yes, that’s one word for it, I think that’s about right that is, ‘mixed’. ‘Mixed’ – how do I think he meant?”  
 (Parker 1985:12)

This final utterance warrants the suggestion that a clarifier may have been of the order “*What do you think he meant by that [‘mixed’]?*” The teenage boy also queries the candidate answer with a gloss-ungloss pattern: “*Mixed?*? *What did they mean, ‘mixed’ how, what sort of way?*” (Parker 1985:13).

At first glance the elderly man confirms the edited Question-Answer interview format, which could be usefully subjected to the notions of “adjacency pairing” (Schegloff and Sacks 1974a), and “insertion sequences” (Schegloff 1972b). The bloc of talk presented by Parker is as follows:

"It would be extraordinarily difficult for me to try and summarise a place such as Providence Estate in a hundred or a thousand words, so it would be totally impossible to do it in one.

Certainly if somebody has already said to you 'mixed' I would say that was an appropriate word, certainly. I couldn't say precisely what they might have meant, but I should have thought a moment's glance round would have made it clear because it is instantly visible, isn't it, how mixed it is."

(Parker 1985:12-13)

Adjacency pairs are pairs of conversational actions that typically occur next to each other. They are sequentially produced actions, in that one speaker produces a first part and the second speaker produces the second part. Further, the second pair-part is conditional on the first pair-part: the second speaker is required to produce an utterance that is tied or connected to the first pair-part.

The 'machinery' of Conversation Analysis enables us to appreciate the adjacently-paired format of ordinary talk. In this way, the Question-Answer format, *viz.* Parker's question followed by his respondent's answer, becomes Q-A-Q-A, whereby Parker's question is followed by a question upon that question by the respondent, i.e. a request for clarification. The respondent's question is answered by Parker (we can refer to this answer to the respondent's question as a "clarifier"), which in turn is followed by the answer to the original question.<sup>5</sup>

We can present this sequence in the following way:

**Q<sub>1</sub>:** "If you could sum up Providence Estate in one word, what would you say?"

**A<sub>1</sub>:** "It would be extraordinarily difficult for me to try and summarise a place such as Providence Estate in a hundred or a thousand words, so it would be totally impossible to do it in one."

**Q<sub>2</sub>:** "One respondent has described the estate as 'mixed.'"

**A<sub>2</sub>:** "Certainly if somebody has already said to you 'mixed' I would say that was an appropriate word, certainly."

As members who share the same culture as Parker and the elderly man with whom he talked, we can see that Q<sub>1</sub> (*"If you could sum up Providence Estate in one word, what would you say?"*) is edited out, and that the edited-out question is indeed this question by attending to the utterance A<sub>1</sub> (*"It would be extraordinarily difficult for me to try and summarise a place such as Providence Estate in a hundred or a thousand words, so it would be totally impossible to do it in one."*) The second pair-part in the sequence is available from Parker's candidate response to A<sub>1</sub>, designated above as Q<sub>2</sub>, which in this interview presumably takes the variant form "*One respondent has described the estate as 'mixed.'*" This interpretation of the closure of the omitted sequence is suggested by A<sub>1</sub> "*Certainly if somebody has already said to you 'mixed' I would say that was an appropriate word, certainly.*"

The clause "*if somebody has already said*" lends itself unproblematically to the suggestion that Parker's framing of questions conforms in some measure to the speculative accounting for questions suggested here.

A speaker may produce another utterance-type prior to producing the second pair-part. This is known as an “insertion sequence”. Insertion sequences take the form of adjacency pairs, which are typically of the form “question”-“answer”, where the first pair part (question) of the insertion sequence is given by the prospective answerer of the base sequence. Schegloff provides the following illustration:

A: Are you coming tonight?  
 B: Can I bring a guest?  
 A: Sure.  
 B: I'll be there.  
 (Schegloff 1972b:78)

Between the first adjacency pair or Question-Answer, a second adjacency pair—an occasioned, topical or relevantly positioned Question-Answer exchange—is inserted.

Parker gives a more extended piece of talk with “the young woman”. This is similarly amenable to inspection as an example of one-sided presentation; the point of this exercise is to demonstrate how the presentation is one-sided, that the interaction has been eviscerated.

1. Q<sub>b</sub>: *If you could sum up Providence Estate in one word, what would you say?*
2. Q<sub>i</sub>: “Mm?”
3. A<sub>i</sub>: *If you could sum up Providence Estate in one word, what would you say?*
4. A<sub>b</sub>: “Well, if I could think of one word to tell someone what a place is like ...”
5. Q<sub>ii</sub>: *Would you describe it as ‘mixed’?*

6. Q<sub>iii</sub>: ““Mixed””?
7. A<sub>iii</sub>: *One respondent has described the estate as being ‘mixed.’*
8. A<sub>ii</sub>: “Well yes, that’s one word for it, I think that’s about right that is, ‘mixed’.
9. Q<sub>iv</sub>: *What do you think he meant by the word ‘mixed’?*
10. Q<sub>v</sub>: ““Mixed” – how do I think he meant?”
11. A<sub>v</sub>: *In what way is the Providence estate ‘mixed’?*
12. A<sub>iv</sub>: “Well you know ... I mean, there’s all sorts of people here all together, isn’t there? I should think that’s what he meant.”

Following Schegloff’s conventions (Schegloff 1972b:78), the subscript character “b” indicates the “base” and numerals indicate components of (possible) insertions. Possible or candidate insertions are presented in italics; direct quotations from Parker’s text appear in regular font.

At line 2, we see that the first insertion is a “meta-question” (Schegloff 1972b:79), asking Parker to repeat the original question, which is repeated at line 3. Note here that the nominal descriptors Q and A do not indicate the type of utterance, e.g. a question or an answer, but rather component or paired parts. In the first insertion sequence, lines 2-3, A<sub>i</sub> is a repetition of the original question but is also a response to the question or first pair part Q<sub>i</sub>. The foregoing points towards the inherent difficulties involved in coding, and with issues of context. A<sub>i</sub> is both a question and an answer: it can be labelled as Q<sub>b</sub>, as a repetition of the base utterance, or as A<sub>i</sub>, a response or second part to the request for repetition at line 2. The ‘machinery’ of conversation analysis, in this case the notion of insertion sequences, is sensitive to such interactional phenomena as

answering a question with a question. The accessibility of turn-taking in conversation reveals that such responses as questions do happen, and relevantly happen in the next turn. The beginning of the answer to the base question,  $A_b$ , is positioned at line 4.

In proposing the omission of turns at talk during these conversations I am not suggesting that my italicised insertions are “valid” insertions; I am not making any analytic claims for them. These italicised turns are inventions. It is not the adequacy of the italicised turns that is at issue. Rather, it is that the ‘machinery’ of Conversation Analysis can be brought to bear upon the *presentation* of talk. In this manner, we can see how the interactional nature of interviews has been disregarded. The findings of early Conversation Analysis reveal that Parker’s contributions to conversations have been edited out, undercutting claims to *allow* people to “speak for themselves”. This aspect of Parker’s work challenges the ingenuousness of the talk. Elsewhere, in a series of interviews with convicted sex offenders, there is more transparency concerning the status of the unobtrusive and non-directive interviewer, and the editing of transcriptions:

“I have omitted most of my questions and allowed what they said to stand in the way that they said it...”  
(Parker 1969:11)

In one of his papers on directions and direction-giving in conversation, George Psathas (1991) explicitly omits questions and answers from a direction-giving sequence in order to display the directions themselves:

15. A: u:m (0.4) Two A will take ya right *across* Mass  
 avenoo an ya just stay on two A (0.6) uh until  
 ya get to *Lowell Street*. (1.4)  
 (Psathas 1991:207)
21. A: An ya turn right on *Lowell Street* (1.2) an its about  
 (.) quarter to a half a mile (0.4) um (.) take another  
 right on *Bartlett avenoo*. (1.0)  
 (Psathas 1991:208)

Is such a procedure feasible? Is eliminating or omitting talk-as-it-happened a sustainable analytic practice? The omission of talk-as-it-happens removes the blocs of talk from the context of their production. That is, it separates the talk from how it was occasioned. Psathas (*ibid.*) edits an insertion sequence from a transcript in order to illustrate more clearly the iteration of directions, but does so without making any claim aside from clarity of presentation, which is itself hypothetical—"If we were to eliminate the inserted questions and answers..." (Psathas 1991:207).

So, in the light of the above, we see that Parker's presentation of talk is flawed in the following ways: it (deliberately) does not account for talk as a collaborative, interactional accomplishment.

At line 5, we see how Q<sub>ii</sub> would prompt the further response "by incorporating a candidate answer in a query" (Pomerantz 1988:360). Prior to any further response is another insertion, turns Q<sub>iii</sub>: and A<sub>iii</sub> at lines 6-7. Having furnished the source of the candidate answer, Parker's respondent produces an utterance A<sub>ii</sub> that concurs with the candidate answer and is seen as a closing to Q<sub>ii</sub>; this pairs lines 5 and 8.

Parker then asks another question,  $Q_{iv}$ , in an attempt to elicit further information on the description of the estate. Between  $Q_{iv}$  and  $A_{iv}$  (lines 9 and 12) there is another insertion, lines 10-11. The question  $Q_{iv}$  is followed by another question,  $Q_v$ , requesting repetition and clarification of  $Q_{iv}$ . Clarification (relevantly) happens next; it is provided in the second pair part of the insertion,  $A_v$ , which occurs prior to the answering of the question (remembering that the italicised utterances are invented on the basis of talk presented by Parker). Henceforth, the speculative wording of utterances at lines 9 and 11 overlap each other.  $A_{iv}$ , at line 12, is thus shown as a probable response to its first pair part  $Q_{iv}$ , and as a probable response to the second pair part of the insertion sequence,  $A_v$ .

In this speculative manner we see that Parker asks three questions, the second and third of which are premised on each other and constitute further attempts to elicit information on the first. Within this question and answer sequence there are a series of insertions. The legend may be clearer if questions and answers, and insertions between question-answer sequences, are labeled thus:

(line 1)	$Q_1$	
(line 2)		$Q_i$
(line 3)		$A_i$
(line 4)	$A_1$	
(line 5)	$Q_2$	
(line 6)		$Q_{ii}$
(line 7)		$A_{ii}$
(line 8)	$A_2$	

(line 9)	$Q_3$
(line 10)	$Q_{iii}$
(line 11)	$A_{iii}$
(line 12)	$A_3$

This presents the Question-Answer sequence or pairs as, e.g.,  $Q_1A_1$  or  $Q_2A_2$ , i.e.  $Q_{n+1}-A_{n+1}$ . Insertion sequences between the Question-Answer pairs are designated according to their placement, e.g.,  $Q_iA_i$  or  $Q_{ii}A_{ii}$ , or, i.e.  $Q_{n+i}-A_{n+i}$ .

From the format of the presentation in Parker's Introduction, "pre-interview" and "post-interview" activities would seem to be asking people on the street if they would answer a (or a series of) question(s). These interview activities would involve approaching people ("Sorry love but if it's insurance we've got more than enough thanks"), informing people of his intentions ("Going to what? Bloody hell, write a book..."; "A book? About Providence Estate?"), and closings ("No, don't mention it mate, nice talking to you..."; "You're welcome sir, good afternoon").

### ***Conclusion***

What we see, then, is that Parker's presentation of talk is missing utterances, some of which are suggested as possibilities above. It is important to note that these suggestions can only be speculative: the very point of Parker's presentation is that it prevents us from appreciating how the actual, original sequence of talk was constituted. This is a problem of ethnographic presentation, and is avoided by the reproduction of "retrievable data".<sup>6</sup>

The ‘machinery’ of CA identifies *lacunae* in social reporting instruments, e.g. interviews. Vitally, the practices of writing-up in the Introduction to *The People of Providence* are *post hoc* renderings of linguistic activities, which edit out the collaborative nature of research activities.

The following chapter moves on from this examination of the practices of writing-up talk, to the writing-up or textual presentation of arguments. The textual presentation of research activities—the publication of the research—may be an end-point along a continuum of these activities.

The overall theme of the next chapter is methodological irony. Its specific relevance to this thesis on the linguistic constitution of research practices is how methodological irony is actually done, i.e. that methodological irony is a routine research activity. I will suggest that writers upgrade their own activities of argumentation at the expense of competing lines of argumentation. Methodological irony, and the upgrading/downgrading of argumentation are, *in toto*, linguistic activities.

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<sup>1</sup> In Chapter Nine (in the section ““Stories” and Story-Formats: Linguistic Activities – Interview Activities”), I refer to the duplicative organisation of interviews; that is, the interviewer-interviewee pair. A cognate position, *mutatis mutandis*, identifies interviewer and interviewee as “complement members” of the interview (Turner 1972:373). This formulation is applicable to focus groups, i.e. interviewer and a local cohort of interviewees, as linguistically constituted research practices.

<sup>2</sup> See extant studies for discussions of such linguistic activities as “assembling” and “beginning” (Turner 1972), “closing” (Schegloff and Sacks 1974) and “recommencing” (Atkinson, Cuff and Lee 1978).

<sup>3</sup> McHoul cites Samuel Beckett’s play *Waiting for Godot* as an example of this, wherein Beckett writes “All three take off their hats simultaneously, press their hands to their foreheads, concentrate.” (Quoted in McHoul 1987:89).

<sup>4</sup> In Chapters Seven and Nine, however, I draw attention to Jefferson's (1996) observations on "transcriptional stereotyping". The use of standard orthography or caricature is a linguistic matter. As emphasised in Chapter Seven, transcripts that caricature "pronunciation particulars" or that preserve the intactness of setting via "instance-by-instance transcribing" (Jefferson 1996:162), are linguistically constituted.

<sup>5</sup> The designations of speakers' turns, *qua* the categorial identification of speakers in transcripts (see Chapter Seven), are analysts' achievements. The methodologically non-ironic realisation of members answering questions with questions is provided by the notion of "formulations" (Heritage and Watson 1980).

It is possible to identify within the answer to the question (lines 2-4) a formulation [f] (line 4):

- |    |    |   |
|----|----|---|
| 1. | Q  | If you could sum up Providence Estate in one word, what would you say?    |
| 2. | A  | It would be extraordinarily difficult for me to try and summarise a place |
| 3. |    | such as Providence Estate in a hundred or a thousand words                |
| 4. | f→ | so it would be totally impossible to do it in one                         |

If the textually available and inferentially available utterances are arranged into a sequence, the (textually available) formulation (line 4) is followed by a further (inferentially available) formulation by Parker (line 5). This could also be glossed as "interviewing by comment" (Snow, Zurcher and Sjoberg 1982) This formulation pair receives assent [d+] (line 7):

- |    |    |   |
|----|----|---|
| 1. | Q  | If you could sum up Providence Estate in one word, what would you say?    |
| 2. | A  | It would be extraordinarily difficult for me to try and summarise a place |
| 3. |    | such as Providence Estate in a hundred or a thousand words                |
| 4. | f→ | so it would be totally impossible to do it in one                         |
| 5. | f→ | One respondent has described the estate as 'mixed.'                       |
| 6. |    | Certainly if somebody has already said to you 'mixed'                     |
| 7. | d+ | I would say that was an appropriate word, certainly                       |

<sup>6</sup> *Vide* the sections "The 'Taped' Record", "Producing a Transcript" and "Practices of Transcription" in Chapter Seven.

## Writing Up (2): Linguistic Practices and Methodological Irony

The previous chapter looked at how the techniques of Conversation Analysis, which were outlined at the beginning of the thesis, can explicate the inscription of ordinary language activities. This chapter moves on from the textualisation of linguistic activities in sociological write-ups to argumentation—the presentation of lines of argument *via* linguistic activities.

The central points of this chapter are “methodological irony”, the setting up of a sociological theory which is competitive with members’ understandings of the world; and how methodologically ironies are linguistic activities. Setting up methodological irony works to upgrade a particular standpoint, used by sociologists, at the expense of others. Setting up methodological irony is a culturally methodic procedure *via* the linguistic presentation of argumentation, which does not feature in sociology textbooks.

According to Anderson and Sharrock, methodological irony is accomplished *via*

“the contrast of the world as it is *allegedly* seen by the members of society with the world as it really is (which, conveniently, is assumed to be the same as it appears to the sociologist).”

(Anderson and Sharrock 1983:569; emphasis supplied)

“Methodological irony” occurs when a sociologist presents an account of the world that departs from the accounts given by the world itself. In terms of a “commentary machine” Sacks (1963), the sociologist intends to suggest that whatever the world is

showing or telling, the case is otherwise. The sociologist relies upon members' understandings in accomplishing the study, but the presentation of the study stands as a rival to those understandings and points of view; it impugns and traduces members' methods. Methodologically ironic sociology sets itself up, therefore, as being "unaccountable" to members' points of view. After attempting to discover how the world is, methodologically ironic sociology proceeds to tell the world how it *ought* to be.

Methodological ironic sociology is characterised by the claims of what Rose (*op. cit.*) calls "The Great Sociologist" who has the insight, some privileged access, to what is *really* going on. The methodologically ironic sociologist sets up a mythical "straw society", inhabited by "straw members", and this setting-up is idealised to the sociologist's advantage, for the practical purposes of doing methodological irony. The methodologically ironic sociologist must achieve this set-up; it takes work to accomplish the reading of the reality of the straw society as the sociologist intends, because it is only through this set-up or (mis)characterisation that the sociologist can present his/her findings successfully. In contradistinction to the joke "One million lemmings can't be wrong", the methodologically-ironic sociologist has readers believe that entire cohorts of members—including the reader of the sociologist's text—are wrong, are operating under a misapprehension, or are somehow deluded about the real nature of the society in which they live.

Sacks explains how theorists, in the very act of theorising, set up ironic characterisations of the world in which people live. The production of an ironised

version of the world is a feature of the act of theorising; the act of theorising presupposes ironic formulations about the world, and irony is a requisite for such theorising. As Sacks remarked,

“You find that [all ironic projects] begin by saying something like this, ‘About the thing I’m going to talk about, people think they know, but they don’t. Furthermore, if you tell them it doesn’t change anything. They still walk around like they know although they are walking in a dreamworld’. Darwin begins this way. Freud begins in a similar way. Bloomfield’s analysis of language begins in a similar way, and I could provide a much larger list”

(Sacks, in Hill and Crittenden 1968:13)

Like Sacks (*ibid.*), Goodwin (1989) argues that Chomsky too, in his early theories of transformational grammar, ironicises the way that people actually do things by comparing members’ practices according to a standard that he himself sets. Goodwin argues that Chomsky’s transformational grammar model is methodologically ironic in presenting speakers’ actual utterances as degenerate forms; for Goodwin, Chomsky’s methodologically ironic position *apropos* actual talk warrants the analysis of retrievable data.

This chapter shows that Chomsky’s ironic project continued in his political analysis too.<sup>1</sup> This exposition is indifferent to the merits of Chomsky’s analysis. Instead, this chapter turns on how Chomsky makes his analysis plausible: how does Chomsky, through textual, i.e. linguistic devices, upgrade or persuade people to believe his version at the expense of competing, common-sense versions?

The following sections of this chapter argue that Chomsky has clung to his methodologically ironic projects in his political polemics; in his “institutional analysis”<sup>2</sup> he claims that people are ignorant and unaware of the pernicious effect of the media, which he argues are preventing people from becoming cognisant with the actualities of power structures in the United States. Instead of being governed by the elected governing party, power is actually wielded by large corporations; these corporations have “parent” corporations, which themselves have parent corporations, so actually power is controlled by a select few. Chomsky “distills” the list of power-brokers down to fourteen multinational corporations.

The following section or interlude may, at first glance, be seen as an awkward digression which takes the reader away from the world which Chomsky concerns himself with. The film-of-the-books, *qua* documentary, is presented as redressing an imbalance, providing a platform for the insights which are so radical that Chomsky has been effectively marginalised and excluded from media coverage. However, this particular example of the medium, and the methodological ironies of the message which it carries, briefly requires a “second viewing”. A methodologically ironic circularity inherent in theorising society is that the theorist theorises a problem so to present the solution: further, they have the insight to discern the problem and theirs’ is the only solution. In this way, Chomsky’s later work is a paradigm exemplar of methodological irony.

**Noam Chomsky: Methodological Irony and the Media**

The echoes of methodologically ironic Marxist arguments, e.g. “false consciousness”, clearly reverberate within Chomsky’s work. Chomsky argues that the media are (sometimes unwitting)<sup>3</sup> accomplices or co-conspirators with the corporations, by passively and actively working to maintain these hidden power structures. Chomsky is engaged in a process of messenger-shooting: at one and the same time he blames the media in a “democratic society” for allowing un-elected, unaccountable bodies (the heads of the multinational corporations) to exercise power in their own interests unchallenged; and for “manufacturing consent” among the population to provide a mandate for the multinationals’ *via* argument through silence. In not speaking out against the power that is, *de facto*, vested in the corporations, the corporations are effectively handed a mandate by silence. In the manufacture and reflection of the corporate ideologies, the media failed to make people aware of the situation, and people were deluded into imagining that they had some autonomy within their everyday lives.

Even the language that Chomsky uses to formulate his arguments is methodologically ironic, then. To say that consent is somehow “manufactured” vests in it an equivalence with the term “construction”, which precludes recognition of members’ ongoing, common-sense, collaborative practices. Terms such as “manufacture” and “construct” pejoratively downgrade and ironicise members’ methods, endorsing the representation of members as “cultural dopes” (Garfinkel 1967a:68). The treatment of members as dopes, devoid of the capabilities for rule-use, finds clear expression in the work of Durkheim, Marx and Parsons, as well as later, “rule-governing” approaches such as

Feminist Social Theory. Garfinkel (1980) advocated a respecification to emphasise the accomplishment of activities by members, and which reflected the exigencies of the *prima facie* minutiae of lived experience. For Garfinkel, the use of the word “production” (rather than “trivialising” terms such as “construction”) more accurately demonstrates how members’ achievements constitute societal constraints, and how these constraints are managed and methodised by members themselves through the daily work of everyday life.

Chomsky not only tells people that they are prevented from understanding the actuality and extent of media manipulation, they *do not* understand what is going on; if only people *could* understand that through *their* inaction *they* have become complicit in allowing a cabalistic cohort to wield ultimate power. In Chomsky’s view, the media and the audience are not entirely at fault; however, they must share some of the “blame”.

Throughout this chapter on the linguistic constitution of analysts’ written reports, the routine, textual devices used by Chomsky are highlighted. The explication of ordinary linguistic activities shows that *via* his upgrading his own versions at the expense of competing versions, Chomsky is enabled to elevate himself above propaganda. Through his use of ordinary language activities, Chomsky is able to present himself as being immune to the manipulation of corporations. A reading of his political writings suggests that thought control in a democratic society does not apply to Chomsky; only Chomsky himself has been exempt from the corporate brainwashing of “ordinary

people”, only Chomsky can see through the media-manipulating structures and only Chomsky can make people realise the “true” nature of society and how they are being subtly manipulated for corporate designs.

The notion of the manipulation being “subtle” is crucial to Chomsky’s project: “subtle” manipulation stands in contrast to “obvious” manipulation. If manipulation were “obvious” then Chomsky would expose himself to insulting people as being credulous. ‘*Sure, baseball is fun, but that’s the way the corporations want it because it keeps the attention of men away from their being manipulated.*’ (Again, think of Marx’s notion of “false consciousness”.) ‘*Sure, shopping is fun, but that’s the way the corporations want it because it discourages women from realising in-built inequities within society.*’ In so doing, Chomsky (1992) hits “easy targets” through a duplex ironicism: he characterises women as shoppers, more interested in designer clothes and make-up than politics; and men as boors who are content to sit in front of the ball-game with a can of Budweiser. Chomsky then takes these ironised caricatures to show how *he is* aware of the manipulative nature of corporate control.

For example, in his “propaganda model”, Chomsky argues that *The New York Times* is so highly respected, even revered, that it has become an “agenda-setting” medium. Newspapers and TV stations, with an attitude which regards items in *The New York Times* to be important, or “newsworthy” (if a story is in *The New York Times* it must be news worth repeating) consult *The New York Times* for news reports, whereby items that appear in *The New York Times* are “newsworthy” items. Conversely, however,

items that do not appear in *The New York Times* are not “newsworthy”, that is, they do not appear as news in other outlets. Editorial decisions as to “what makes the paper” at *The New York Times* dictates the material that becomes stored in *The New York Times’* archives: axiomatically, an item that doesn’t appear in the paper cannot be placed in the archive. So US history is history according to the clippings in *The New York Times’* archives. Chomsky attributes the lack of outrage of the American people to e.g. US involvement in the genocidal conflict in East Timor to *The New York Times’* “failure” to carry the story. If the editors at *The New York Times* had covered the conflict at all, let alone consistently, Chomsky argues, other media would have followed the story up because it had appeared in *The New York Times*. Under these circumstances, the American public would have been made aware of the atrocities carried out with US-supplied weapons, by US-trained soldiers, and for US corporate interests (the continued demand for US arms-shipments and increasingly sophisticated, i.e. lucrative weapons and weapons-grade material).

Chomsky’s film, *Manufacturing Consent: Noam Chomsky and the Media*, stands as an exemplary ‘text’ of setting up, *mutatis mutandis*, a “preferred viewing” (D.R. Watson 1992). Purposeful editing, interviews which give Chomsky platforms to explicate his arguments unchallenged, the selectivity of extracts (what *wasn’t* included in the film?), the ridiculing of accepted versions, i.e. versions that differ from Chomsky’s analyses of US media, all work to upgrade the preferred, *viz.* Chomsky’s own version.

A bombardment of images and sounds including graphics, illustrations, voice-overs, the use of archive footage, and the juxtapositioning of Chomsky's arguments with extraeneous images works to produce the desired impression of Chomsky as the tireless, dynamic, unpretentious intellectual, who disregards his eminent professorial status to give unpatronising responses to schoolchildren as a guest on their school radio station. Not only is Chomsky presented as a quasi-Weberian "Great Man" who has been marginalised by the US media; he is a great man with a great message, and his message is indisputable.

For example, during a filmed debate, the film juxtaposes sequences of a boxing contest as the debators (one of whom, of course, is Chomsky) "exchange" arguments. The debate is edited to appear as occurring in an argument-counter format; with the interstitial images of the boxing contest, the viewer sees the debate as being comparable to two boxers slugging it out with each other, debators-cum-pugilists delivering punches and counter-punches. A ringside bell sounds every time a speaker finishes his turn; grunts, cheers and the sound of gloved fists hitting the mark punctuate (Chomsky's) turns. Towards the end of the debate the viewer sees the "opponent" on the podium reduced to silence as Chomsky produces a combination of punches—all of which hit their intended target—and lands a right hook: cut to boxing ring, where the referee looks down at a prostrate, unconscious figure as the audience cheers, the time-keeper's bell signals the end of the contest, and the triumphant contestant is carried aloft on the shoulders of his seconds.

My apparent conflation of debate and boxing contest is intentional: the metaphor is utilised so laboriously, with the editing rendering the boxing almost subliminal, that the film *intends the viewer to see it this way*.

So far this chapter has outlined some features of methodologically ironic sociology by presenting a preliminary demonstration of the methodologically ironic project of Noam Chomsky, and how Chomsky seeks to authorise and legitimate his ironic version against common-sense versions. Alternative versions are excluded at the expense of the version of which the methodologically ironic sociologist is a proponent.

Chomsky (1992) argues that “ordinary people” (in the United States) are ignorant of the identities of the continent’s most powerful forces, political and financial, and that this knowledge is deliberately kept from people by the newspapers and television news channels. The media are manipulated by and acquiesce to the demands of powerful multi-national corporations: they are controlled by the interests of external, clandestine forces.

This section has established that the *presentation* of Chomsky’s commentary is not extraordinary, nor revolutionary; rather it is an assemblage of exoteric common-sense devices, purposefully arranged in order to discredit the “conventional” way of looking at the media. Through the very act of discrediting or downgrading one “version” an alternative, contrasting or oppositional version is upgraded. It is noted that the upgrading and discrediting activities of argumentation included a re-wording to counter

the accusation that Chomsky was advancing a “conspiracy theory” So by ironicising the conventional viewpoint he affords definitional primacy to his own “anti-establishment” thesis.

There is an important feature of the approach or “analytic mentality” (Schenkein 1978) taken in this thesis. The methodological position herein does not disqualify, or seek to disagree (or agree) with Chomsky’s central argument. It is not the message but the methods used in its presentation that are the focus of inquiry. Whether Chomsky is correct is not at issue: the rest of this chapter addresses how Chomsky seeks to show his version is more convincing than other, competing versions.

This is an advantageous feature of writing up research. The analyst must work towards a written-up version of their research which is not just persuasive but, at the same time, discounts alternative versions. The inscribed, linguistic activities of research outcomes are explored more fully in the next section.

### ***Authorising Accounts and Obtaining Sympathetic Readership***

Let us consider an introductory statement from his co-authored book *Manufacturing Consent* (Herman and Chomsky 1988):

“Perhaps this is an obvious point, but the democratic postulate is that the media are independent and committed to discovering and reporting the truth, and that they do not merely reflect the world as powerful groups wish it to be perceived. Leaders of the media claim that their news choices rest on unbiased professional and objective criteria, and they have support for this contention in the intellectual community. If, however, the powerful are able to fix the premises of discourse, to decide what the general populace is allowed to see, hear, and think

about, and to “manage” public opinion by regular propaganda campaigns, the standard view of how the system works is at serious odds with reality.”  
(Herman and Chomsky 1988:ix)

What can we notice from these prefatory remarks? Let us immediately turn to some literary devices that Herman and Chomsky use in setting up the thrust of their book.

Consider “Perhaps this is an obvious point, but ...” (Herman and Chomsky 1988:ix). This is a simple device in which Herman and Chomsky reveal how they will authorise their (“unconventional”) argument. That a point is “obvious” tells the reader that they should know it already, or at least immediately recognise its veracity. An “obvious” point is a point which the reader is being instructed to accept and endorse. “Perhaps” works to ameliorate or minimise the force of any potential condescension, to recognise that the reader might not know it, in an attempt to avoid being (or seeming to be) patronising from the preface of the book onwards; although it also works to set out their definition of the role of the media according to, or contrastive with, the democratic postulate. A crucial move that is being made here is that Herman and Chomsky authorise their claim by aligning themselves with an already recognised, accepted and authorised version of the democratic postulate. In effect, they are saying that “this is so, but it’s not just us that say it is so”.

Even if the reader accepts Herman and Chomsky’s definition of the media’s role in a democratic society, an anachronism is built into their definition (“that they [the media] do not merely reflect the world as powerful groups wish it to be perceived”), whereby a

contrast is outlined for future, i.e. the following arguments, reference. In Schützian terms, Herman and Chomsky orientate towards a “future project”, the realisation of their thesis; and in order to realise their thesis they must set out the logical moves which, if (and only if) they are accepted by readers, produce a feasible, plausible and sufficient account. For Herman and Chomsky, the media in a democratic society are *independent* (of something; that something we find out to be state and private interest); the media have a *commitment*, and that commitment is to *discovering* and *reporting* the *truth*.

So the reader sees what type of media characterises a democratic society; a media that does not fit this type is not characteristic of a democratic society. A question this raises is, if a media does not fit this type, does it indicate that the media is uncharacteristic or is it the society that is undemocratic?

Readers do not have their answer immediately. Herman and Chomsky require another stage in setting the foundations for their ironicised version, which is found in the subsequent sentence. I have italicised details of this argument which seem particularly salient for its production.

Consider “*Leaders of the media claim* that their news choices rest on *unbiased professional* and *objective* criteria ...” (Herman and Chomsky 1988:ix; my italics).

This may be seen to have a triplex meaning: that out of the mouths of the media leaders is spoken the truth; or that media leaders are unwitting propagandists; or that media leaders are complicit propagandists. “Leaders” is used to denote straw figures: the leaders of the media possess the authorised versions, although there exist circumventions of authority in accounts from studies of professions, whereby the leaders know some but not all that is going on. For example, doctors may regard themselves as the final arbiters of diagnostic and treatment decision-making; however, nursing and ancillary staff are able to keep details from doctors, and have their own ways of working which sometimes conflict with doctors’ orders (Strauss *et al.* 1971). Thus, Herman and Chomsky subscribe to the notion of an omniscient, all-knowing, vertical hierarchy of organisational knowledge which I critiqued in the chapter on fieldwork activities.

That the media leaders “claim” qualities for their decisions is seen as ironicising, trivialising and downgrading of their decision-making activities, *qua* “Well, they would say that wouldn’t they?”: it engenders scepticism in the reader as to the validity of the claim. Herman and Chomsky then rest on the inferential capacities of readers to underscore specific contrasts. That the qualities “unbiased, professional and objective” are *claimed* by the leaders of the media invites readers to infer that choices are *not* based upon unbiased, professional, and objective criteria, and are therefore *not* unbiased, professional or objective choices.

They move on to articulate the implication and upshot of news media controlled by a minority in society, and its ramification for political awareness in the wider majority:

“If, however, the powerful are able to fix the premises of discourse, to decide what the general populace is allowed to see, hear, and think about, and to “manage” public opinion by regular propaganda campaigns, the standard view of how the system works is at serious odds with reality.”

(Herman and Chomsky *ibid.*)

Herman and Chomsky refer to the “standard view”. This is a gloss for existing theories of media management, rather than a consistent, consensual approach. The “standard view” can be regarded as a “placeholder” for approaches within media studies that are not consonant with their arguments on media management and corporate control. Furthermore, it sets the “standard view” of media studies in opposition with the actuality of media control by unseen hands on the “tiller”. So the premise of this sentence is organised in such a way that their thesis is upgraded to a certain status; that is, examining media and society through the “Propaganda Model” (see below) reveals what is *really* going on. Media commentators, and the “general populace” (Herman and Chomsky *ibid.*) are experiencing some form of collective “delusion” if they are not appreciative of corporate interference with the messages broadcast by and received from the media.

We see a *gestalt* switch or “figure-ground” effect in use: from one point of view, a picture is interpreted in a certain way. Seen from a different view, however, the picture changes entirely. Seen from the “standard view”, the operations of the media are “at serious odds with reality.” The standard view is to be regarded as the wrong view, then.

If contact with reality is questioned, the view and conclusions derived from this view will not seem credible. Seen through the prism of their “Propaganda Model”, the *gestalt*, “reality” returns thereby sensible discussions may begin.

In the first paragraph of their book, Herman and Chomsky ask us to accept the premises of their exordial discussion of the “propaganda model”:

“The mass media serve as a system for communicating messages and symbols to the general populace. It is their function to amuse, entertain, and inform, and to inculcate individuals with the values, beliefs, and codes of behaviour that will integrate them into the institutional structures of the larger society. In a world of concentrated wealth and major conflicts of class interest, to fulfill this role requires systematic propaganda”

(Herman and Chomsky 1988:1)

What makes this a plausible opening paragraph? How is it seen as a sensible opening? These are critical questions, for if their introduction is regarded in any way as being defeasible, their subsequent arguments may be undermined, even disregarded. And, as already noted, it is incumbent upon Chomsky to present his thesis in a believable, plausible, rational manner because his radical standpoints on the “independence” of the “independent media” do not receive air-time.

Chomsky attempts and needs to show that independence from outside interference is a “necessary illusion” (Chomsky 1989) intended to cajole the entire society that the media are “committed to discovering and reporting the truth” (Herman and Chomsky *ibid.*).

One thing to be noted is that Herman and Chomsky rely upon readers' unquestioning reading: the first sentence tells readers what purpose the mass media has in "larger society". The paragraph as a whole displays the conservatism inherent in teleological, consensual and structural functional explanations. The mass media is presented as constituting a system within a larger system. We are not cajoled into thinking otherwise by the establishment of forthcoming conflict argumentation, presaged by the line

"In a world of concentrated wealth and major conflicts of class interest, to fulfill this role requires systematic propaganda".

(Herman and Chomsky *ibid.*)

Furthermore, the media have "functions" ("to amuse, entertain, and inform"); and following their list of (benign?) functions Herman and Chomsky attempt to "smuggle in" their characteristically malign, dysfunctional element: functional for the propagandists, that is, but dysfunctional for the (unwitting) recipients of the propaganda. The attempt is made *via* a commonsensically conjoined, serialised item in the list ("*and to ...*"). This item in the list, the most important for Herman and Chomsky's argument, is a neatly packaged series of items in itself. It is a tripartite consensus-structuralist statement:

"to inculcate individuals with the values, beliefs, and codes of behaviour that will integrate them into the institutional structures of the larger society"

(Herman and Chomsky *ibid.*)

Respectively, the purchase of each of these items bears upon: the separation between individual and society; the need to integrate the individual into society; and the

institutional/structural notion of society, which is subsequently given a conflict-structuralist gloss by the final statement:

“In a world of concentrated wealth and major conflicts of class interest, to fulfill this role requires systematic propaganda”.

(Herman and Chomsky *ibid.*)

As such it sets up a contrast, whereby what the media *ought not to be doing* is exactly what it *is doing*.

In terms of the Documentary Method of Interpretation (Garfinkel 1967), Herman and Chomsky are concertedly involved in setting up a pattern through which their evidence is to be read. We have seen that the pattern is established in their preface, and in the opening paragraphs of their first chapter. This pattern is then reprised, or continually re-established, throughout the book. Herman and Chomsky then provide copious, detailed examples to illustrate their arguments, to elaborate the pattern that they have established. Indeed, the sheer detail and volume of their examples works as further support for their arguments. So the pattern or schema for interpretation is provided, and persistently presented; and the evidence is to be interpreted through Herman and Chomsky's desired pattern of interpretation. The evidence, in turn, provides coherence to the pattern, i.e. the pattern and evidence are mutually elaborative.

As I said earlier, it is not the task of this chapter to subvert Herman and Chomsky's presentation, nor to propose an alternative interpretative schema through which their

evidence may be appraised. Such an enterprise is itself methodologically ironic. Instead, a principled analytic neutrality, or “ethnomethodological indifference” (Garfinkel and Sacks 1970:345-346), provides access to the multiplicity of common-sense devices employed by Herman and Chomsky to realise their version, and how they authorise their version as the preferred version.

Herman and Chomsky have not exhausted the potential for setting up a preferred version, however. The ordinary use of a contrastive device, *viz.* between democratic and undemocratic societies, brings their institutional analysis, and their use of methodological irony, into greater relief:

“In countries where the levers of power are in the hands of a state bureaucracy, the monopolistic control over the media, often supplemented by official censorship, makes it clear that the media serve the ends of a dominant elite. It is much more difficult to see a propaganda system at work where the media are private and formal censorship is absent. This is especially true where the media actively compete, periodically attack and expose corporate and governmental malfeasance, and aggressively portray themselves as spokesmen for free speech and the general community interest. What is not evident (and remains undiscussed in the media) is the limited nature of such critiques, as well as the huge inequality in command of resources, and its effect both on access to a private media system and on its behaviour and performance.”

(Herman and Chomsky 1988:1-2)

Notice here how Herman and Chomsky require readers to make the logical leap from the condition of a society to media manipulation through the phrase “makes it clear”; for as it stands, the “equation” {(power of a state bureaucracy) + (monopolistic control of the media) + (official censorship)} = media serves the interest of the dominant elite}

is a *non sequitur*. Herman and Chomsky elaborate upon their position by arguing that this is not at all an obvious state of affairs: in arguing that

“It is much more difficult to see a propaganda system at work where the media are private and formal censorship is absent”,

Herman and Chomsky (*ibid.*) are claiming possession of privileged insights into the behind-the-scenes workings of society which are commonly unavailable to casual observers. Indeed, Herman and Chomsky are far from casual observers; they are “experts” in their field who teach us that all is not as it seems. Herman and Chomsky further upgrade their roles as “white knights” (Herman and Chomsky 1988:8) by telling us that *we* should not feel compromised by *our inability to see through* the rhetoric of investigative journalism and freedom of the press:

“*This [the hidden nature of the propaganda system] is especially true* where the media actively compete, periodically attack and expose corporate and governmental malfeasance, and aggressively portray themselves as spokesmen for free speech and the general community interest”

(Herman and Chomsky 1988:1; my emphasis)

“What is not evident (and remains undiscussed in the media) is the limited nature of such critiques...” (Herman and Chomsky 1988:1-2). It is only by being able to “see” the (or rather *their*) critical point of view, namely the Propaganda Model, that one can become aware of its marginality. Through the self-referral to “their” critiques, Herman and Chomsky further upgrade their own claims.

The illustrations as textual exegesis in this chapter demonstrate how the process of downgrading existing arguments is accomplished, a process that is concomitant with upgrading competitive arguments. Moreover, it shows how these parallel processes are not the esoteric preserve of members of the “intellectual community” (Herman and Chomsky *ibid.*); instead they are readily available, common sense means of organising words-on-the-page.

The means or devices used, i.e. common-sense members’ methods used, invoked and relied upon by Herman and Chomsky to authorise their version, to the exclusion of possible alternative versions include: the serial ordering of premises; contrast structures; setting up *gestalt* switches and appealing for readers to use an alternate, competing paradigm to adjudge the credibility of versions; devices for discrediting opposing viewpoints and authorising a preferred view; relying on readers’ use of the Documentary Method of Interpretation through the process of reading, which, for an American-English readership, involves attending to words-on-a-page in a left to right, top to bottom manner, to discover for themselves the veracity of arguments in the gradual unfolding of supporting evidence.

The concerted undermining of alternate versions affords the opportunity of upgrading a preferred version. The common-sense methods listed in the above paragraph are among the valuable resources for providing one version of events at the exclusion other “sides”. As the aforementioned considerations show, it is not a precondition to “get one’s oar in first”: rather, it is useful to provide summary outlines of alternate versions,

thereby to emplace competing theoretical positions for unfavourable comparison, to treat as a “coconut shy”.

### **Conclusion**

This chapter shows how the work of Noam Chomsky, in his role as media commentator, is of necessity methodologically ironic. The active use of ordinary, natural methods to authorise a preferred view, precluding potentially rival views, or discrediting existing views, is not unique to Noam Chomsky. Therefore, this chapter constitutes a move in the identification of formal properties of realising lines of argument, through the upgrading of the preferred view whilst discrediting any possible alternative interpretations.

In this chapter I do not attend to the adequacy of Chomsky’s theories, to which I remain indifferent for the purposes of analysis. Instead, this chapter is concerned with the linguistic constitution of his argumentation. However convincing or unconvincing Chomsky’s lines of argument are, however much one wishes to agree or disagree with his theories of media manipulation and his “propaganda model”, his arguments are advanced through ordinary, textual work, where textual work is, *in toto, linguistic* work.

Setting up preferred versions is textual work. Introducing forms of methodological irony are, therefore, linguistic activities. In order to disseminate and publicise research, the researcher has to use their own natural language practices, and rely on readers to use

the self-same methods in order to assess the contribution of their research *apropos* extant literature.

This is the unifying theme of the thesis. *From the inception to the conclusion of research projects, researchers are ongoingly and unrelievedly involved in ordinary language practices.* These ordinary language practices are constitutive of research practices, including writing up the research. In writing-up their research, the researcher is engaged in the activities of claiming that extant work is (at least minimally) contestable. In itself, the practical activities involved in establishing the contestable nature of extant work claim a warrant for the researcher's project, *via* the elaboration, clarification or refutation of extant literature.

In this chapter I have been concerned with the routine, textual activities in the establishment of a “preferred” version or argument at the expense of alternative, competing arguments. The final chapter continues to treat textual materials as topics of inquiry in their own right. It moves on to look at the project-specific issues of relevance and visibility of literature corpora, i.e. how literature is located, and the relevance of literature to researchers’ projects is established. The loci of these considerations are commonplace features of research projects, *viz.* bibliographies and literature surveys.

Bibliographies and literature surveys are linguistically organised, linguistically constituted aspects of research projects. The following considerations of the textual

inscription of bibliographies and literature surveys shows how, and to what extent, these research practices incorporate members' practical reasoning.

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<sup>1</sup> To anticipate one of the concerns of the next chapter, *viz.* the relevance of materials, it should be noted that the work of Chomsky – whether associated with linguistics or with political science – is relevant to the considerations of wording up research as a set of culturally methodic practices. That is, the relation of Chomsky's work to sociological work is not at issue; and in any case, as I go on to say, this thesis is indifferent to any relation of Chomsky's work to sociological work.

<sup>2</sup> In *Manufacturing Consent: Noam Chomsky and the Media*, (1992) Chomsky uses this term to answer critics who argue that he is offering "conspiracy theories".

<sup>3</sup> I wish to thank Jennifer Blackman, Department of Film and Media at the University of Stirling, for discussing this point with me.

## Constituting Bibliographies: Linguistic Activities and Literature Surveys

This chapter provides observations on features of “the research process” that are regarded as standard and unremarkable features of research projects, *viz.* bibliographies and literature reviews. Reading and writing are ordinary cultural activities in that they involve the interpretation of natural language. This means that the practices of research projects—*inter se*, taking and writing-up fieldnotes, observations, interviews, transcribing recordings—as well as reading and writing, are conducted through language. Again, I bring to the reader’s attention Rose’s witnessing of his ethnology tutor, Paul Radin, “conducting an interview” beside the Russian River. Rose saw his teacher “in the field”, where fieldwork activities were ordinary language activities. Research projects incorporate natural language activities *via* the preparatory and ongoing reading of books and journal articles, the drafting and writing-up of the research.

### ***Bibliographic Practices as Topics for Inquiry***

If scholarly endeavour—and this thesis takes the case of sociology—is regarded as a “bibliographic enterprise”,<sup>1</sup> the practices of bibliography of and within the discipline can be taken as a “topic”. A twofold sense of topic is implicated here: first, attention to a particular issue or subject-matter, *viz.* the bibliography; secondly, the treatment of a particular issue, *viz.* the bibliography, as a natural language phenomenon. Hence, this chapter takes the “linguistic turn” in the analysis of bibliographies and literature surveys. The subject matter of sociology is linguistically organised. So too are “the

procedures used to report, and thus to constitute, boundaries between” (G. Watson 1984:357) sociological specialties. Explicating bibliographies as topics of analysis in their own right, rather than taking them as unremarkable and un-remarked-upon resources for use within studies, provides sociological purchase on routine phenomena.

Treating bibliographies as topics makes available for analysis the linguistic constitution of sociological inquiries. The topic *versus* resource modality discussed in previous chapters facilitates the description of routine features of research—in this current chapter, the production of literature surveys and assembling of bibliographies—as research methods and practices *per se*. Whilst other chapters in the thesis, *via* the principles of “ethnomethodological indifference” and topicalising phenomena, consider (*inter se*) the attribution of pseudonyms, presentation of sociological reasoning, discrediting alternative analyses, recording (and, to a lesser extent, transcription) as missing sections in sociology textbooks, this current chapter turns analytic attention towards “‘seen but unnoticed’, expected, background features” (Garfinkel 1967:36) of sociological reports. That is, this chapter continues the overall task of the thesis in explicating the culturally-methodic, i.e. linguistic activities involved in the accomplishment of research projects. This chapter takes bibliography as an instantiation of, *mutatis mutandis*,

“scholarly activity that requires investigation as a phenomenon in its own right”  
(G. Watson 1984:361)

What does this attitude towards bibliography amount to? The analytic gain of treating bibliographies at topics allows the fine-grained study of bibliographies as texts in their own right, i.e. a thoroughgoing, praxiological treatment of bibliographies. Studying bibliographies as texts—the practices of reading and compiling bibliographies—demonstrates that a bibliography *informs and is informed by* the text it accompanies (or, put otherwise, the text that accompanies the bibliography).

### ***Literature Surveys: Temporal and Textual Organisation***

Although bibliographies appear at the end of work, the compilation of bibliographies is an ongoing part of the research project as each work is referenced. Individual researchers, according to their own preferences and styles of working, may insert each new reference into a specific file, wherein the bibliography is compiled cumulatively; others may compile the bibliography after the work is finished.

Similarly, literature reviews appear towards the beginning of work, e.g. in chapters one or two, the first or second section. The early placement of a literature review does not necessarily mean that it was completed before the study itself commenced, however. Further works may be added to the literature review *ex post facto*, i.e. in the duration or on completion of the project, which may be recent publications or works located as the project progressed. So a review of the literature is, *pro re nata*, subject to change. Literature reviews enable the researcher to contrast their work with anterior works in a given area or on a given topic, by pointing towards deficiencies, *lacunae* and methodological flaws contained within them. The researcher can also upgrade the

rationale of their project by reacting to an extant literature, e.g. the time elapsed since a similar study, a novel approach to a familiar problem, the paucity of literature that studies [a certain topic] or [a certain topic in a certain place], etc. This may be of necessity in locating or applying a methodologically non-ironic, analytic or conceptual approach to previously under-analysed phenomena or practices, e.g. the teaching of English for Academic Purposes (EAP) (Alzamora 1999);<sup>2</sup> the linguistic constitution of moral order in a school staff meeting (Baker 1997);<sup>3</sup> evaluation of parenthood training courses (Carlin 1994);<sup>4</sup> the analysis of conversations in what Rose refers to as “managed” languages, *viz.* Dutch and English (Haegeman 1996).

Bibliographies, like literature reviews, are also subject to change. The changeable status of bibliographies is attested to and exemplified by an anthropological study of a small rural community (Friedrich 1977). First published in 1970, reprinted in 1977, the second edition appends a “Supplementary Bibliography” to the original text:

“The works listed [in the supplementary bibliography] appeared since *Agrarian Revolt in a Mexican Village* was first written. The author provides them for those interested in continuing their study of agrarian reform and agrarian politics.”

(Friedrich 1977:159)

The addition of a supplementary bibliography illustrates the “work in progress” rather than fixed nature of bibliographies. This feature is alluded to in the introduction of a bibliography of British society:

“The present edition is up-to-date to the spring of 1974, as far as practicable, with some forthcoming titles included. We hope that it may be possible to produce further editions, possibly through the publication of supplements.”

(Westergaard, Weyman and Wiles 1974:5)<sup>5</sup>

Further posthumous publications by Harvey Sacks have also impacted upon the “snapshot” nature of his bibliographies.<sup>6</sup> A “provisional” bibliography of Sacks, compiled by Schegloff (Sacks 1985:22-23), was itself superseded by a later listing of Sacks’ lectures in a Special Issue of *Human Studies* (Psathas 1989).<sup>7</sup>

As mentioned just above, bibliographies tend usually to appear at the end of articles and books. That is, they are part of an article-book “closing unit”.<sup>8</sup> There are notable exceptions to this placement, of course. For his treatise *Suicide*, Émile Durkheim distinguishes between “general” and “special” bibliographies; this distinction is affected in the placement of references:

“Wherever necessary, the special bibliography of the particular questions treated will be found at the beginning of each chapter.”

(Durkheim 1970:52)

At the end of each chapter in his *The Method of Sociology*, Florian Znaniecki (1934) provides annotated bibliographies on topics considered therein. Johnson’s (1960) comprehensive overview of sociology contains an innovative feature of special interest, in that it provides a “Bibliographical Index” (Johnson 1960:650-675).<sup>9</sup> A biographical sketch by his daughter (Marwick 1964) precedes a bibliography of Louis Wirth (Wirth 1964:341-350). Similarly, Florian Znaniecki’s daughter compiled a bibliography of her

father's work (Lopata 1969). These are the last items in the collections. A *festschrift* for Pitirim A. Sorokin is concluded with a bibliography of his work and commentaries on his work (Hallen and Prasad 1972:355-371). With Sorokin's continued publication post-1972, a bibliography attempting to list works by Sorokin himself and secondary readers on his work becomes dated.<sup>10</sup>

Whilst this may seem to be an obvious point, it bears reiterating; for it serves as a reminder that, aside from topical and methodological entailments bestowed by alternate analytic commitments,<sup>11</sup> bibliographies are textual records of relevances which capture a temporally defined situation, a point in time. It is self-evident that "current trends" reports and "state-of-the-art" reviews (e.g. Button 1988/89; Heritage and Watson 1976) can only survey literature available at the time of submission to the journal. For the temporal, cumulative nature of conversation-analytic bibliographies, cf. Heritage and Watson (1976) with Heritage (1985) and (1989). While the latter paper "is a revised, shortened and thoroughly updated version" of the paper published in 1985, the earlier paper "contains a considerably larger bibliography of Conversation Analysis." (Heritage 1989:37). The 1985 version of Heritage's paper contains an updated bibliography from its publication in 1984.<sup>12</sup> It may even be that, as Westergaard *et al.* (*ibid.*) demonstrate, bibliographies are presented as being explicitly oriented towards the temporality of academic work, i.e. that more work within the parameters of the original bibliography is subsequently published. Such a cumulative approach is provided by dedicated journals such as *Contents Pages in Education* and *The Philosopher's Index: TPI* is a periodical

which abstracts recent publications (articles and books) in philosophy. Here too, editorial decisions are made for the inclusion of articles:

“The following factors are weighed in selecting journals to be indexed: 1) the purpose of the journal, 2) its circulation, and 3) recommendations from members of the philosophic community. *Articles in interdisciplinary journals are indexed only if they are related to philosophy.*”

(*The Philosopher's Index* 1998:iii; emphasis added)<sup>13</sup>

Titles indicating that a bibliography is not exhaustive, “selective” or “select”, instruct the reader to approach the bibliography as a non-exhaustive, “selective” bibliography (Singer 1965) of a particular topic or subject-area. *TPI* (1998:11) also lists “A Definitive Bibliography” among the recent publications it had received. Hence, the authorisation *via* linguistic practices or “wording up” of bibliographies: compare claims to “provisional” status, e.g. Schegloff (in Sacks 1985; also in the notes to Rose's bibliography) and “impromptu” status, e.g. Sacks (1989); with “This bibliography is, it is hoped, complete” (Lukes 1988a) and claims to “comprehensiveness” (Lukes 1988b). The sections “Making Connections: A Problem of Relevance” and “Establishing Relevance” below will consider the implications of titles more extensively.

### ***Locating Literature***

When embarking along a line of sociological inquiry, it is regarded as incumbent upon the researcher to explore the extant literature pertaining to the topic. This accepted practice within sociology acknowledges works recognised to be authoritative works on particular topics, and widely-cited sources. As a substantive example,

"No one who writes on or studies surnames can proceed without the help of his predecessors in the subject, even if he is merely disagreeing with them."  
 (Pine 1969:134)

Literature reviews provide, then, a summary section of the *loci classici* on a particular topic (Carlin and Slack 2000). Of course, this assumes that such literature exists.<sup>14</sup> This may not be an unproblematic process, as the relevant literature may be of a highly exiguous nature.<sup>15</sup> Keyword searches in journal indexes may have to be systematically broadened to include supposedly relevant subject-headings (Paton 1988:260); and, in the process of a review of sociological literature on humour, trawls through *Sociological Abstracts* throw up some curious anomalies (Paton 1988:268).

After expanding his keyword search to include words associated with humour (including jokes and laughter), Paton cites articles by Gail Jefferson (1979)<sup>16</sup> and Harvey Sacks (1974b) as "ethnomethodological studies of humour" (Paton 1988:261).<sup>17</sup>

This is a questionable corpus title, given that Paton makes reference to conversation-analytic papers, or alternatively, papers by authors associated with Conversation Analysis rather than Ethnomethodology. However, ethnomethodological/conversation-analytic studies "on jokes" have an equivocal status *apropos* of the corpus of literature on humour because the joke is not the topic<sup>18</sup> of study in an essentialist sense. Rather, the analytic or explicative attention is given to the interactional work done by members in telling and recognising a joke (Sacks 1974b, 1978). This is not to imply that differences in approach to topics should be exempted from consideration as part of the literature. What counts is how the analyst uses particular items.<sup>19</sup>

Writers may select or delimit the corpus of literature *via* a literature review. This practice differs from providing assistance to other analysts, *qua* Florian Znaniecki (*ibid.*), by highlighting works that are “quite widely cited in this area”<sup>20</sup> for future academic debate. The wording up or linguistic constitution of literature reviews, e.g. “little systematic or scientific study of the subject” (Pine 1969:134)—serves to derogate and downgrade works or corpora of work for the reader:

“From time to time numerous works on surnames have appeared, but many of these have been of an ephemeral nature and only a small number of books are worth studying on this matter.”

(Pine 1969:134)

An example of an author’s provisional delimiting a corpus of literature *via* a bibliography, rather than *via* literature reviews as discussed above, is found reading the work of Charles H. Page. At the end of his chapter “Sociology as a Teaching Enterprise” (*op cit.*), Page annotates items in a bibliography that he introduces thus:

“To my knowledge, there are no large-scale studies of sociology as a teaching enterprise. *The following are a few of the many volumes that might be consulted in undertaking such a study.*”

(Page 1959:597; emphasis added)

So the subtitle above for this section, “Locating literature”, can be seen to have a double meaning: actually finding literature on a topic, and the linguistic placement (or displacement) of a work within a corpus of literature. This is implicated by the phrase “corpus-status” (Garfinkel 1996:5). Corpus-status is the identification and recognition of a work as a significant work within the boundaries of a particular field or discipline.

For instance, what may be a marginal text within a discipline in its entirety, may be acknowledged to be a crucial contribution to a field within the discipline. Not all works within a corpus have the same status, e.g. some possess a “marginal” status while others are accorded a “founding” status. How different works may be afforded different statuses within the corpus may be subjected to scrutiny. Although this latter question is addressed only elliptically, this chapter returns to considerations of “corpus-status” throughout its discussions.

As stated at the beginning of this section, the onus is on future researchers to locate literature that is considered to be “relevant” to their current topic of investigation (considering the relevance of literature is explored further below). In this manner, Page’s own chapter becomes a contribution to be consulted in any future study of sociology as a teaching enterprise.

The analyst may judge that the extant literature is inadequate. The textual presentation of such arguments serve to upgrade the analyst’s current research at the expense of previous research, as in an ethnography of poolroom hustlers:

“A bibliographic check reveals no decent research on poolroom hustling, sociological or otherwise. Apart from an occasional work of fiction in which hustling figures, there are merely a few impressionistic accounts in newspapers and popular magazines. With a couple of exceptions, each article is based on interviews with only one or two hustlers. No article analyses hustling on any but the most superficial level or provides a well-rounded description. The fullest survey of the subject not only omits much that is vital, but contains numerous errors of fact and interpretation.”

(Polsky 1969:33)

Enumerating particular articles, Polksy criticises a contribution published in *Esquire*, saying that it

“contains a few worthwhile observations, but it is sketchy, ill-balanced, and suffers much from editorial garbling, all of which makes it both confusing and misleading for the uninitiated.”

(Polksy 1969:33)

This critique downgrades the particular research whilst claiming special inside knowledge of the poolroom scene, having the authority to write without “misleading the uninitiated”. The possession of competency or expertise in a certain topic area—whether this is claimed or displayed as a feature of the literature review—was explored in Chapter Twelve, *apropos* Noam Chomsky’s analyses of corporate influence on news media.

To follow Polksy’s *modus operandi*, a sociologist interested in studying, say, olfaction might proceed by citing various fictional works pertaining to smells and smelling, e.g. Salman Rushdie’s *Midnight’s Children*, Patrick Süskind’s *Perfume*; and introduce sociological approaches via Georg Simmel. Less well-known contributions (e.g. Largey and Watson 1972) could then be cited as precedents and legitimation for the study of the olfactory senses, i.e. other sociologists have taken the sense of smell as a serious topic of study also as justification for the current analysis, before introducing their own work. As it happens, a striking feature of Largey and Watson’s paper is its originality. Without a sociological corpus of literature to react against, they have no option but to source anthropological, psychoanalytic, scientific and fictional literature. An analytic

gain of Largey and Watson's work is that in finding themselves without a sociological corpus of literature, they are not constrained by "discipline-specific literatures" (Garfinkel 1996:13). In a very real sense, it can be seen as the foundation piece of the *sociology of odors*.<sup>21</sup>

It follows from the example of Largey and Watson (*ibid.*) that if analysts are interested in a substantive topic of inquiry, they should not desist from citing or using non-sociological literature where such literature complements, informs or appertains to the topic in question. For the practical purposes of initiating inquiries upon a phenomenon, sources from different disciplinary areas can identify a phenomenon, include insights on, and provide perspicuous observations on phenomena. As Sacks said, for instance,

"the relevance of the Chicago sociologists is that they do contain a lot of information about this and that. And this-and-that is what the world is made up of."

(Sacks 1989:254)

The sociologist should attempt to take precedents into account, and ask whether the precedents for the current study are sensitive enough to render the phenomenon, whether or not the phenomenon is analysed in a sociological manner. Precedents for the current study can then be used to situate the current contribution or relate it to a wider field.

Throughout this thesis, then, references are used that are not solely sociological; rather, non-sociological references are pressed into the service of sociology. More important

to the phenomena or topics at hand than being “critical of”<sup>22</sup> or impugning the disciplinary provenance of references, are the issues of relevance and sensitivity to the phenomena. In the perspicuous case of Largey and Watson (*ibid.*), without any available sociological research on odors they had to determinate the horizon of relevance for themselves.<sup>23</sup>

### ***Making Connections: A Problem of Relevance***

Researchers, as members, during the course of a research project, i.e. from its inception onwards,<sup>24</sup> use their own practical reasoning procedures in recognising the relevance of sources, *via* the recognition of names (Schegloff 1979), titles, keywords and subject headings. With a book or journal article to hand, relevance may be discerned through subject-headings within the text.

Interesting variants of intra-textual subject headings are provided by the “Larimer Street” reports. The transcripts of interviews published as “supplementary materials” to *The Unattached Society* (Rose 1965) are formatted to preserve the speech-exchange nature of the conversations between members of the research team and people on or affected by Skid Row.<sup>25</sup> Blocs of talk between interviewer and interviewee are separated by headings; whilst the placement of headings is an analyst’s achievement, the headings themselves are derived from the strips of talk that follow. In illustration, I have reprinted the following stretch of talk with permission:

#### **Larimer Street Isn’t Very Cheap**

BUCHANAN: One of the first things that we discovered about Larimer Street is that it isn’t really very cheap. The meals were not much less than they would be

in a small restaurant uptown, and, of course, the price of drinks is comparable. And surprisingly, they get a fairly high rent for many of their rooms down there.

#### It Might Be Less Expensive In The Long Run

GORMAN: It is interesting, this idea you mentioned about the economics. It seems that the prices on Larimer and uptown are not that different. And yet on the other hand, as you mentioned too, one is not obliged necessarily to keep up in other ways. This is why it might be less expensive in the long run.

#### It Did Hold A Certain Attraction

BUCHANAN: For these reasons, we were surprised to find that anyone would actually choose to live on Larimer Street until we had been there about a week. Then we found that strangely enough it did hold a certain attraction. People didn't judge you very strictly or very harshly, they didn't ask you many questions. By and large, the people who lived there were a friendly bunch of people.

(Gorman and Buchanan 1965:7)

This extract is reprinted from a conversation between one of the research team, Anthony Gorman, and John Buchanan, a journalist from *The Denver Post*. Like Gorman sixteen years after him, Buchanan had, with one of his colleagues, lived on Larimer Street for the purpose of research into the experiences of men on Skid Row. The conversation took place after Gorman had finished his two-month period of observation living "as a bum" on Larimer Street: Rose refers to Buchanan and Gorman as "intruders", who engaged in "intrusions on Larimer" (Gorman and Buchanan 1965:2).

The Larimer Street reports are cited throughout this thesis, for different analytic purposes. In Chapter Nine, on the collaborative nature of interviews, for instance, extracts are quoted in illustration of the frequency of identifying characteristics, which are occasioned by research practices such as interviews, and the problems identifying

characteristics such as names cause for the maintenance of confidentiality. In referring back to Chapter Six on the practical work of pseudonymisation, it was noted that not all names of participants in the research were pseudonymised. Following Sacks (1972b), the presentation of participants in the Larimer Street inquiries can be divided into collections of categories, *viz.* incumbents of categories who are pseudonymised and incumbents of categories who remain identifiable. For the purposes of illustration, we may call these collections “Collection P” and “Collection I” respectively. Members of the latter, Collection I, include members of professional categories, e.g. the police, journalism. Buchanan is one such member of Collection I.

However, in reproducing the section above from the Larimer Street reports, I am drawing attention to the way in which Rose inserted subheadings into the transcripts. Rose’s subheadings reflect his commitment to a naturalistic, non-ironic form of inquiry. This manner of subheading was not artifice but emergent from people’s own experiences as iterated in talk. The extract taken from “Intrusion on Larimer”, reprinted above, displays how titles are located within the text: e.g. the title *“Larimer Street Isn’t Very Cheap”* is a diminution of the phrase *“One of the first things that we discovered about Larimer Street is that it isn’t really very cheap”*; whilst the subheadings *“It Might Be Less Expensive In The Long Run”* and *“It Did Hold A Certain Attraction”* are direct quotes from the text. As mentioned previously in the section on elective (methodological) affinities between Rose and Sacks, in Chapter Seven, a radically different reprinting of one of the reports replaces the headers within the text with numeric divisions (see Item #157 in the bibliography, Chapter Three).

To quote D.R. Watson (1997c:80), whilst there is an “extraordinary diversity” of texts, there remain certain stable features.<sup>26</sup> The use of subheadings is such a stable feature. Subheadings are included in varieties of texts, e.g. “adult” magazines, credit-card applications, encyclopedia, ESRC forms, insurance forms, journal articles, newspaper columns, operating manuals for electrical equipment, to list but a few. Textual materials associated with the activities of particular categories, such as those categories collected in “Collection K” (Sacks 1972b:37),<sup>27</sup> e.g. various medical practitioners, also contain subheadings. So subheadings can be addressed as constituent features of textual materials produced by members through their practical work at e.g. Suicide Prevention Centers.

Coroner’s reports are produced by investigations into the cause of death. Investigations are carried out in cases of sudden death where some doubt exists *re* the cause of death, i.e. determining whether the cause of death was suicide. In studying coroner’s reports, how the reports were produced with the available evidence to hand, Garfinkel points towards the reflexivity of, *mutatis mutandis*, subject-headings where the subject-heading is a gloss for its “accompanying text”, and the accompanying text explicates, makes sensible or provides the relevance of the subject-heading (Garfinkel 1967b:175-176).<sup>28</sup> The reader of the coroner’s report encounters the title and subheadings; the sense of titles and subheadings are assembled by the text below (or surrounding) them. At the same time, the text is informed by the title, i.e. the reading of the text is oriented *via* the title.

Titles can be used in order to achieve relevance. As demonstrated in the section on pseudonyms and Membership Categorisation Analysis, categories that are *prima facie* unconnected can be collected together *apropos* their relationship to a topic. Subsequently, *via* the selection of “appropriate” titles, sections are made relevant to the topic and made relevant to each other. Compare “*Rachel is a user*”, with “*Jan has never used drugs*” (MacFarlane *et al.* 1991:1, 11). In this case, what do a drug-user and someone who has never taken drugs got in common? The common relevance is provided by being included in a series of accounts *re* drugs and drug-use, i.e. through a categorial relationship *apropos* drugs (*viz.* user *versus* non-user). That is, their alignments to drugs, albeit oppositional, warrant their being collected together within a set of accounts on drug-use. The relevance of these two accounts to each other is emphasised by their adjacently positioned in the book. Whereas Simon is a “ratified” interviewee, a *bona fide* drug user (“*Simon occasionally smokes cannabis*”), Liz is co-categorised with Simon through the invoking of the Membership Categorisation Device “family”: “*Liz, Simon’s mother*.” It is through the practical use of membership categorisations that the non-user Liz is established as a ratified interviewee; Liz becomes “news”, she becomes relevant to the business-at-hand of drug use. With a pattern which works to combine the person with drugs, the presence of each person (e.g. “*John is a drug counsellor*”) is accomplished through membership categorisations and recipient design, shown by the journalist’s designing of a newspaper headline to the relevances of the constituents of potential readers within the locality (Lee 1984:69).

In this way, relevance can be regarded as an occasioned matter. Relevance is not necessarily “out there” but is assembled through members’ linguistic, sense-making practices. Bibliographies can have a coherence, where this coherent is a member’s achievement.<sup>29</sup> In illustration of this, consider a problem for members, *viz.* earning money. A musician iterates his frustration at having to play “mainstream” music, music that makes him hirable to play in public houses and small venues, brings him to the attention of agents, and—potentially—secures him a recording contract. The end-goal is attainable, but his frustration is caused by the means to that end: the music that *he* wants to play is not salable, does not “have” an audience.<sup>30</sup> In order to earn money, the musician has to “sell out” and play music for which, according to those who can provide him with a recording contract, people will pay to listen. Can we bring a sociological literature to the dilemma facing this musician?

Howard Becker played as an amateur pianist with jazz musicians. He (Becker 1963) observed that the jazz musicians were disgruntled with their audiences, and with which forms of music audiences would pay to listen. For practical, economic reasons, the jazz musicians were forced to compromise, to suppress their preference for improvised jazz to please the “squares”. Becker’s studies, being concerned with musicians, are directly relevant to the concerns of the Mancunian musician above. However, another source, from another setting, may also have relevance to this musician’s dilemma. Mason Griff (1960) studied the “destinations” of fine-art students, i.e. what fine-art students do following graduation. One career trajectory was that of the “commercial artist”, which required a different attitude to painting from the fine-art approach. Griff’s interest was

in whether and how “artists resolve this conflict” (Griff 1960:219)—between painting for the outlet of creativity and the necessity to make an income, *viz.* painting to the demands of “clients”.

Griff’s chapter was brought to my attention several years’ previous to this encounter. Another commonality of Griff’s study of a commercial artist with Becker’s jazz-musicians studies is what Garfinkel calls the “Howard Becker phenomenon”, i.e. the omission of the defining feature of the member’s work (in Griff’s case, the production of art).<sup>31</sup> The “Howard Becker phenomenon”, or the “missing what”, is a characteristic of sociological studies that fail to preserve the constitutive activities of occupations. In Becker’s case (*op. cit.*), the criticism refers to the presentation of “peripheral” features of professional dance-band members, e.g. argot, tensions with audiences, their personal relationships and relationships with other musicians, etc. However, the identifying details of being jazz musicians, *viz.* playing jazz music together, is absent from Becker’s account. In both the aforementioned articles, by Becker (*ibid.*) and Griff (*op. cit.*), there are fulsome accounts of the tension between staying “independent”—*via* jazz and fine art respectively—and “going commercial”. However, in both articles, the constitutive activities of the professions under consideration are noticeably absent.

Briefly, then, relevance is occasioned and may be brought to the phenomenon at hand. What Garfinkel (1996) refers to as the “corpus-status” of these works, e.g. art, music, careers, “deviant occupations”, professions, does not suggest that they have relevance to the phenomenon; nor do the titles or subtitles. These articles do not possess *prima facie*

relevance, then; relevance to the phenomenon has to be discerned, and so bibliographies assembled, by members themselves. By making connections between the phenomenon of inquiry and considerations of cognate phenomena, a person's dilemma is amenable to sociological analysis.

Analysts, such as Largey and Watson (*ibid.*), are able to provide readers with illustrative examples from non-sociological sources, e.g. manuals on wine-tasting. Non-sociological references are not gratuitous, i.e. do not detract from the argument, as connections are made between the argument and the illustrations.

### ***Establishing Relevance***

Bibliographies may be presented as bibliographies of a specific topic or particular subject area. That is, bibliographies come with instructions for its reading, or "instructions-for-use". The "instructed reading" of bibliographies is situated in, e.g. titles and subtitles, as Garfinkel (1967 *ibid.*) highlights the reflexivity of a text and titles within the text, which mutually elaborate each other. The titles of bibliographies, e.g. "Bibliography of Animal Sociology" (Rose *et al.* 1954) and "Formal Theory in the Behavioral Sciences: A Bibliography" (Paris-Steffens *et al.* 1966) instruct readers to approach the bibliographies as bibliographies of "Animal Sociology" and "Formal Theory in the Behavioral Sciences" respectively. The earlier section on the "temporal and textual organisation" of bibliographies and literature reviews addressed the wording up of bibliographies, where it was noted that titles and subtitles provide instructions for reading bibliographies as authoritative or otherwise.

Also, bibliographies may be prefaced by notes which offer an “instructed reading” of the materials. For example, consult the “Notes on the presentation of materials” which precede the itemisation of works by Edward Rose, in Chapter Three. These notes explicate the editorial judgments *apropos* inclusion and exclusion of materials; the classification of materials; and the reasoning behind the chronological format of the bibliography. A bibliography circulated at a conference at the University of Stirling (“On the Margins: Social Exclusion and Social Work”) also delineates the criteria of inclusion within introductory notes (Beresford *et al.* 1997:1). Following Watson and Sharrock (*ibid.*), the specification of this listing is achieved more definitively in the introductory notes to, rather than the title of, the bibliography.<sup>32</sup> These notes tell the reader of the bibliography that the items included within the bibliography are produced by the very “subjects” of the books themselves. That is, to invoke the concerns of Chapter Eight, this bibliography is a list of works which are “owned” by members of categories: Beresford *et al.* rely upon, rather than explicate, the manner in which they perceive categorial incumbents to have a relationship of ownership over their experiences.

The categories chosen by the compilers of the bibliography are categories adjudged to be “socially excluded”. Although the introductory notes include a *caveat* on the definition of “social exclusion”—

“not everyone or indeed every group identified under the umbrella of ‘social exclusion’ may see themselves or interpret their experiences in these terms”  
(Beresford *et al.* 1997:1),

there remains an *ad hoc* stipulation by the compilers. This stipulation incurs that whilst people may not categorise themselves as “socially excluded”, their placement on the list results in such categorisation regardless. The contradiction this involves is that by producing a bibliography of works in which people are “speaking for themselves” they are actually denying people the opportunity to speak for themselves, or traducing what people actually say. So whilst purporting to produce a list of literature on experiences that are “owned” by particular categories, the compilers of the bibliography attempt to nullify this relationship between category and experience.

The system of classification instructs the reader how to approach items in the bibliography: these instructions are made explicit through subtitles. To refer to Westergaard, Weyman and Wiles (*ibid.*) again, the main title of their work is *Modern British Society: A Bibliography*. Headings and sub-headings, provided by the classifications, are related by the main title; e.g. “C31: Political power and elites” is a subdivision of “C30: Class structure”, which is a subdivision of “C: Class structure”, which is itself a sub-classification of “Modern British Society”. Titles inform and are informed by subtitles; hence, readers are implicitly instructed to approach “class structure” as “class structure in modern British society”. *Lacunae* in classified lists are implicated in readers’ frames of reference. *Lacunae* are “Gaps in the Literature”—to

borrow Garfinkel's phrase—only insofar as searches for literature extend to topics *other* than “modern British society”.

Garfinkel's use of this phrase pertains to judgments as to the “eligibility” of literature, or “literature as an eligible phenomenon” (Garfinkel and Sudnow 1972:20). Garfinkel himself is referring to the availability of literature on lecturing (Garfinkel and Sudnow 1972) science, scientific work and the work of scientists (Garfinkel, Lynch and Livingston 1981; Garfinkel and Wieder 1992). The “Gap in the Literature” (Garfinkel, Livingston and Lynch 1981:133) obtains to the vast amount of literature *apropos* occupations, whereas only a limited number of inquiries study the practical activities of work *per se*. (This limited number of inquiries is addressed and comprised by the ethnomethodological program.) The Gap in the Literature is a phenomenon of inquiry itself, in that theoretical and methodological options make available—or unavailable—the study of “work” as members’ praxis. That is, without taking account of members’ natural-language practices, i.e. by relying upon membership activities as a resource, studies of work “lose the phenomenon”. (An example mentioned above is Becker’s studies of jazz musicians; Garfinkel cited Becker’s jazz musicians in his explications of lecturing-as-work and the discovering sciences.) Also, the ethnomethodological program identified the phenomena of study, *viz.* membership activities, until when the phenomenon of natural language was not recognised to be a phenomenon of inquiry.

The “instructed reading” of bibliographies is also located in and by sociology’s texts, i.e. as discussed above, its placement at the end of an article or monograph on a particular topic or particular subject-area.

The “Methodological Note” in *The Polish Peasant in Europe and America* also bears upon members’ location of relevance in (and connections between) literature. It should be remembered that Thomas and Znaniecki wanted to access people’s interpretations *re* cultural acculturation and assimilation; Thomas elected that such access was available through letters immigrants sent home and received from their family. With this *caveat* on different forms of inquiry, Thomas and Znaniecki’s considerations on what should be the topic of study may, *mutatis mutandis*, be brought to the activities of literature searching:

“We have to limit ourselves to certain theoretically important data, but we must know how to distinguish the data which are important.”

(Thomas and Znaniecki 1958a:19)

However, this may perhaps introduce a form of methodological irony. According to Thomas and Znaniecki’s position, theory that is “grounded” in data is, nevertheless, grounded in data that is adjudged to be “more important” than other data. The methodological irony this involves is avoided by the *modus operandi* “ethnomethodological indifference”, by

“abstaining from all judgments of [a datum’s] adequacy, value, importance, necessity, practicality, success, or consequentiality.”  
(Garfinkel and Sacks 1970:345)

Furthermore, distinguishing the “important” data are analysts’ achievements. The sociologist’s work of deciding that some data are more important than other data, or possess an importance *a priori*, does not account for members’ interpretations of the significance (or otherwise) of events which the sociologist treats as data. For our present considerations on bibliographic and literature searching, Thomas and Znaniecki’s delimiting the scope of data analyses bears upon the retrieval of information through catalogue searches, where, *mutatis mutandis*, members, through their interpretive practices, use

“criteria as enable [them] to distinguish materials belonging to [their] sphere of interest from those which do not belong there.”

(Thomas and Znaniecki 1958a:19)

The “analytic mentality” of this thesis, on the linguistic constitution of research and research practices, thus identifies a commonality between doing bibliography and doing transcription. Chapter Seven showed how transcripts were comprised both by the transcript and the work of transcription. Compiling and bibliographies constitute a “*Lebenswelt* pair” (Bjelic 1994). The notion of *Lebenswelt* pairs refers to the textual outcomes or *post hoc* accounts of practical activities. In Chapter One, I noted that Bjelic demonstrates how there is (1) a scientific experiment; and (2) an account of a scientific experiment. However, the “lived work” of the scientific experiment cannot be recovered from the account of the experiment.

Bringing the notion of *Lebenswelt* pairs to bibliographies, it is possible to see that the bibliography is both, and at the same time, (1) a list of references pertaining to the topic-at-hand (the first pair-part); and (2) the procedural, working knowledge used to recognise the relevance of the references to the topic (the second pair-part). These two parts, that make up a *Lebenswelt* pair, are identifiable and inseparable. The bibliography is not an inert list of references. Rather, the bibliography is constituted by the list of references and the recognition of the bibliography as a *particular* bibliography. This leads us, in an ethnomethodological manner, to suggest the realisation of the identifying characteristics of a bibliography.

Sometimes the work of discerning relevance is already done for analysts, by the editors of chapter collections or special issues of journals. That is, there is an “achieved unity” between the papers. A variant form of achieved unity may be found in electronic journals, where the technology allows readers to browse purportedly “thematic collections”. Thematic collections are online papers which have appeared in different volumes and issues of the electronic journal. Unlike paper-based journals, however, the editors of electronic journals may sort articles together that address particular themes or topics—a “virtual collocation”—and provide readers with hotlinks between them. The decision to thematise papers is a *post hoc* editorial decision. A perceived lack of unity between contributions to an edited collection is an accountable matter, to which the editor of the collection may be held in reviews in professional journals.

The next section explores further the problem of relevance for individual researchers, i.e. relevance which is not mediated by editorial practices, taking the visibility of literature *apropos* begging as an example. Without reifying begging as a substantive topic, the following section elaborates on the notion of corpus-status in pursuance of the location of “core” and “canonical” texts.

### ***“Doing Begging”: Literature Surveys and “A Gap in the Literature”***

In March 1998, I was part of a team of researchers involved in a research project on public space and on observable street phenomena. Some exploratory themes from this project are presented elsewhere (Carlin, Evergeti and Murtagh 1999). Due to the visible presence and prevalence of beggars at the research site, the team engaged in a period of observation of beggars and begging activities as part of public spaces. The phenomena were not difficult to find; indeed, very often the phenomena found us.

Following this initial period of observation, I embarked upon the task of collecting and producing a literature review of sociological materials. I do not intend here to remark upon the technical aspects of literature reviews, such as trawling through bibliographies from work in a particular subject area—“seeking out the works of reputed authorities in the field and working backwards through bibliographies” (David and Zeitlyn 1996:6.5); nor to comment on bibliographic technologies (e.g. 5" x 3" record cards, Endnotes and ProCite), reference/style conventions (e.g. American Psychological Association, Chicago, Harvard), nor formats.

Literature reviews are familiar parts of research projects. The noticeable feature of doing a literature review on begging was the shortage of material that was available to me. A search of electronic databases, such as BIDS (*Bath Information Data Services*), using the keywords “beggar” and “begging”, is instructive in this regard. Keywords need to be synonyms, e.g. “panhandling”, or broader subject-headings, e.g. “homeless” or “homelessness”.

The use of an adequate synonym is not a guarantee of adequate results on a search: due to the temporal parameters of electronic databases, and the criteria for inclusion within databases, e.g. the keyword “panhandling”, rich sources on panhandling are excluded (see Cavan 1972, especially chapters 2 and 8). Similarly, keywords can be misleading in journals. An article with the keywords list “street homelessness, social space, begging” (Wardhaugh 1996:701) is less helpful to a research project on public space and begging than the keywords suggest.

Obtaining “relevant” material for the project on begging was problematic: not all commentaries, which were purportedly “about” begging, actually looked at begging in its particulars; i.e. there was “a Gap in the Literature”. Furthermore, disciplinary-specific literatures render unavailable relevant materials that are not necessarily recognised as relevant because of their disciplinary-specific nature. E.g. the “cotton bandits” of San Francisco, who sustain their heroin addiction by begging cottons, rather than panhandling for money; cottons are the absorbent materials (cigarette filters, cotton

wool, fragments of sponge) that act as filters in the process of drawing heroin-solution from the cooker into the syringe (Bourgois 1998b).<sup>33</sup>

I do not claim, in any way, that any considerations within sections of the thesis on particular topics are exhaustive; nor do I claim to have been as systematic as some might wish. Readers may note surprising omissions<sup>34</sup> from the references in discussions of topics. Remaining silent about certain texts does not indicate any “ideological” position towards the literature, nor towards any writers in particular, as one author contends:

“Scholars ... often manifest disapproval or disappropriation silently, by passing over unmentioned someone whose work or views one would normally expect to be taken into account.”

(Rescher 1998:92)

Omissions may be “noticeably” or “officially absent” (Schegloff 1972a:388), and analysts held accountable for such omission:<sup>35</sup>

“In his bibliography, Denzin slyly lists his own contribution to the [Agnes] debate but not the rejoinders of his EM/CA interlocutors.”

(D.R. Watson 1995b:217)

The central point of D.R. Watson’s review is to emphasise that if analysts are committed to dialogism, as D.R. Watson points out Denzin self-avowedly is in the book under review, then they have to enter into them and report them; however, Denzin refuses to engage in such dialogic debate. Candidate reasons why Denzin does not itemise the rejoinders to his work become inferentially available. The conversa-

analytic identification of, *mutatis mutandis*, “conditional relevance” (Schegloff 1972a:388 *et seq.*) stimulates the question *why* Denzin should remain silent about critiques of his work.

Inferences may not be limited in respect to the absence of particular authors, or authors’ works. The following takes cross-references as examples, i.e. those items listed in the “see also” closing-units of encyclopedia entries.

Gail Jefferson, in a comment on a special issue on Conversation Analysis (Jefferson 1989b), cautions against the recipient design of the conversation-analytic program for different audiences. In particular, she encourages conversation analysts to exercise exactitude in presenting Conversation Analysis as an approach or program of study which coheres with—or “makes it appear amenable to” (Jefferson 1989b:427)—other approaches. The recipient design of Paul Drew’s entry on Conversation Analysis in *The Encyclopedia of Language and Linguistics* is, perhaps, a case in point. Apart from a token reference, *viz.*

“Conversation Analysis is thereby a bridge between linguistic analysis (especially pragmatics) *and the sociological investigation of sociality ...*”  
 (Drew 1994:749; my emphasis),

the notion of Conversation Analysis as a form of sociology is noticeably absent. In Drew’s entry, the relevance of Conversation Analysis to linguistics is massively emphasised, at the expense of explicating Conversation Analysis as a sociological approach. Furthermore, whilst Drew (1994:749-750) provides a brief section on “The

Origins of Conversation Analysis”, the (ethnomethodological) provenance of Conversation Analysis has been edited out. This cutting-out is manifest in the list of “see also” items (Drew 1994:753), from which Ethnomethodology is noticeably absent. In contrast, the entry on Ethnomethodology in the same encyclopedia (Psathas 1994) displays no such alignments—where “alignments” are inferences warranted by the entry on Conversation Analysis. Indeed, the Ethnomethodology entry discusses Conversation Analysis, makes a cross-reference to Conversation Analysis, and lists works that are perceived to have corpus-status, i.e. derive from the conversation-analytic corpus of literature, in its bibliography.

To reiterate, the foregoing considerations are warranted by a particular reading of the entries. They are contestable, or subjectively problematic. That is, I would not expect all self-avowed ethnomethodologists or conversation analysts to agree. However, it is arguable that the omission of Ethnomethodology from Drew’s (*ibid.*) entry as a cross-reference is a *lacuna*, which is noticeably or officially absent. Noticeable absences are accountable matters; that is, possible “reasons” for an absence may be formulated, or inferred. Any inferences drawn are warranted by the absence, until information (or indexical particulars) to the contrary suggest otherwise, i.e. provide for a re-interpretation. This also goes to demonstrate the coherence of formal concepts, such as the “inference-making machine” and the “Documentary Method of Interpretation”, as an “overlapping famil[y] of practices” (Baker 1997:88).

In my considerations of begging (Carlin 1999), I use corpora of literature not usually brought together. If some of my sources seem rather eclectic, or idiosyncratic, it is because hitherto they have not been seen as contributions to the “literature” on e.g. begging. For example, a literature review on begging brings together references from my own long-standing interests in the ethnographic researches of the “Chicago School” of sociology and literature on religious pilgrimage.<sup>36</sup>

A *lacuna* in the extant literature on beggars and begging is the study of begging as a practical activity. It has been noted elsewhere (Garfinkel, Lynch and Livingston 1981:132) that practical activities are uncommon topics of research in their own right. In the case of discovering scientists, for example, there existed a crucial distinction between “studies *about* their work” and “studies *of* their work”. Furthermore, that there was “a gap in the literature” when referring to studies of practical activities (Garfinkel, Lynch and Livingston 1981:133; Garfinkel and Wieder 1992:188). That this was the case *apropos* of investigations into lecturing-as-work was itself a researchable phenomenon, or an inquirable matter, which was available for investigation:

“The phenomenon consists of the combined availability of endlessly numerous constructive analytic studies of occupations, in endless variety of detail, cost, and impact, accompanied by the practical absence of detailed, technically descriptive studies of work.”

(Garfinkel and Sudnow 1972:20)

Hitherto, begging has not been the focal topic of inquiry that a literature review might suggest. Begging as an activity and as a subject of study has been concealed (or at least subsumed) variously under other substantive subject areas.

The marginality of begging in society is reflected in the marginal status of the topic in social inquiries. The paucity of studies appears strange, considering the high profile of begging and strategic responses towards it, e.g. “Zero Tolerance” policing. Although begging is largely ignored as a topic of inquiry it is “recoverable” from classic sociological studies; to read about begging requires close examination of existing literature on other topics, and literature outwith sociology. Beggars and begging have an unwritten history in sociology. Begging is found to be subsumed under other, titular fields: alcoholism, homelessness, mental illness, skid rows, vagrancy. Begging is seen as being concomitant with particular circumstances, not a topic of study in itself; e.g. Bordreuil’s (1992/93) treatment of begging as homelessness. It is a matter of question as to why such ‘substantive’ topics receive attention whilst constitutive phenomena are glossed over. This assumes that begging is done, and that people know how to do it is taken for granted.

From identifying a gap in sociological literature we can move towards problematising the beggar in sociology. This is achieved by recommending the study of begging techniques (Carlin 1999), and—following Garfinkel’s study of Agnes—beggars as “practical sociologists” (Carlin, Evergeti and Murtagh 1999).

The concerns of this section have been to outline the problematic, i.e. practical matter of locating literature *apropos* sociological projects. Specifically, this involves locating

studies which engage with members activities rather than sociological presentations of topics, i.e. re-descriptions of members' practices.

### ***Ownership and the Visibility of Literature***

This section elaborates upon the aforementioned considerations of corpus-status and relevance in terms of the common-sense availability of inferences *apropos* the "ownership", or perceived ownership, of "disciplinary-specific" literature. First, I shall mention briefly a salient instance illustrating the intersection of ownership, methodological options and project-specific literatures.

As part of my professional activities, I received a call for research applications—"appel à projets de recherche"—an application-pack for multi-disciplinary, pan-European research teams to investigate issues of language and identity. Requirements for the application included a detailed proposal of research, copies of *curriculum vitae* of the nominated researchers, a timetable of research, a detailed budget, and proposed manifest of necessary equipment for the project. A further stipulation was for researchers to append a list of references germane to the proposed project: "une bibliographie et des références de publications des chercheurs impliqués (6 pages maximum)."

This leads to an important clarification, which is the reflexive relationship between the work and the references: the work implicates a set of relevances, and a set of references, i.e. a corpus of literature; the corpus of literature has implications for the work. As

mentioned earlier, the disciplinary-specific nature of literature delimits the inquiry, whilst the inquiry suggests and occludes corpora of literature. Adapting Wieder's (1980:76) phrase, this reflexive relationship between the work and the corpus of literature may be glossed as a "*bibliogenic ontology*". (The Greek suffix "-genic" indicates production, *viz.* "producing" or "that which produces".) The coinage "*bibliogenic*" implies the corpus of literature that becomes available through the election of theoretical and methodological options, and the theoretical and methodological options that are precluded by the corpus of literature.

Hence (as mentioned above), bibliographies are not necessarily inert texts, located towards the end of a book or article, but can be corpus-formative devices; e.g. Gillian Burrington (*ibid.*). The corpus-formative nature of bibliographies is emphasised by the attendant glosses provided by compilers of bibliographies; e.g. Charles H. Page (*ibid.*), Ned Polsky (*ibid.*). Further, the "fixed nature" of bibliographies (mentioned at the beginning of this chapter) involves a disciplinary (re)contextualisation, e.g. conversation-analytic studies in linguistics are embedded in linguistic, rather than sociological, bibliographies. According to Patricia Haegeman (1996:99), Conversation Analysis constitutes "a borderline field between sociology and linguistics". Applications of Conversation Analysis, as a methodological approach, may be "hidden" from sociological or linguistics communities *via* their administration to different "areas"<sup>37</sup> or *via* their placement in dedicated journals. To take an example, a sociological article on classroom interaction, *viz.* Mehan (1978) may be missed by sociologists because it appeared in a journal on education. It is not that sociologists

with a research interest in education are likely to remain ignorant about this study solely because it appeared in an “education” rather than a “sociology” journal; rather, I mean to suggest that the relevance and significance of Mehan’s arguments for sociologists *per se*—viz. the analysis of social settings and the accounting for all participants within the setting under investigation (rather than, in the case of classroom interaction, an asymmetrical focus upon teachers at the expense of pupils, or *vice versa*), may remain unappreciated due to its publication in a dedicated journal.

That is, some journals may not be seen to be within the purview or (to invoke the concerns of Chapter Eight) “ownership” of disciplines; consequently, articles published in such (extra-disciplinary) journals have a different visibility than “core” (as defined by members) journals. Adjudging a journal to be a “core” journal is a matter for members. The determination of a particular journal as a “core” journals is, to use John R. E. Lee’s term, “subjectively problematic”.

A complementary organisation of accounts is implicated by “subjectively problematic”. Although this felicitous phrase occasionally appears in published form, Lee’s students were the primary beneficiaries of its use.<sup>38</sup> Reference to the subjectively problematic nature of a phenomenon indicates the coherence of mutually elaborative features of a phenomenon. First, how the constitution and recognition of a phenomenon is a question or problem of definition, i.e. is open to interpretation; hence, definitions are “problematic”. The phrase also intends that such definitions are the accomplishment of society-members, i.e. definitions are “subjective”. As I have just mentioned, the

“problematic” and “subjective” features of definitions are complementary, or coincident. That is, they are not reified stages or processes but mutually inform each other.

Whilst the logical grammar of the term might suggest some form of individualist notion, Lee’s orientation is, *in toto*, praxiological; i.e. definitions are courses of action. It is to avoid the misinterpretation of the phrase in terms of individualism or agency that D.R. Watson (personal communication) prefers the phrase “*intersubjectively problematic*”. This clarification of the term enables readers to appreciate more clearly Lee’s sense of the term, *via* its relation to the sharing of a stock of common-sense knowledge: a natural sociological, cultural approach, rather than an individualistic approach.

I have introduced Lee’s notion at this point of the discussion to indicate how orientations towards journals are contingent, practical discriminations, i.e. are subjectively problematic. That is, a journal that may be a “core” journal in one discipline is not necessarily adjudged to be a core journal within another discipline; further, members of the same discipline would not necessarily accord the same journal “core” status.

This brings us back to the notion of “corpus-status”, which is discussed throughout this chapter; and practices of retrieving references, discussed in the earlier sections of this chapter. Locating a journal article may be contingent upon a journal being indexed

and/or abstracted in bibliographic databases, and in the bibliographic databases being searched, e.g. in the “social sciences” databases as opposed to the “arts and humanities” databases. As mentioned previously, if a journal is indexed or abstracted in, e.g. online bibliographic databases, retrieval of a particular article is deferred until a correspondence between keywords and search-parameters is met.

However, there are occasions when search-parameters are unable to capture or retrieve pertinent references. The journal *Semiotica* does not require its articles to be accompanied by abstracts or subject keywords. The possible relevance of an article in *Semiotica* will, therefore, be occasioned by familiarity of the author’s name (noted above in the section “Making connections: a problem of relevance”);<sup>39</sup> otherwise, relevance may be adduced *via* the title of the article. Whilst readers may associate the name “Douglas W. Maynard” with conversation-analytic research, researchers interested in silence as a topic would not immediately recognise one of his publications from *Semiotica* as a reference relevant to their research (Maynard 1980). For researchers interested in “silence”, the title, “Placement of topic changes in conversation”, is misleading. By “misleading” I do not intend to suggest that the title is a misnomer—it reflects the topic of the article, *viz.* how conversationalists achieve changes of topic within conversation and how topic-changes are occasioned; that is, how the organising or placing of topic-changes are accomplished by members themselves. Nor do I intend to suggest that researchers peruse the title, ponder a bit more, and move on; rather, the title does not contain any terms which may be picked up within search-parameters for a bibliographic search on silence, nor alternative terms for

use in secondary or tertiary searches, e.g. “absence”, or “pause”. Researchers would not have the opportunity to peruse and ponder the title, because it falls outside the search-parameters in the first place.

### ***Conclusion***

This chapter has explored a variety of overlapping issues pertaining to bibliographies and literature surveys. The analytic mentality of the thesis has taken bibliographies and literature reviews as topics of inquiry, as sites of commonsense, natural language practices. The topicalisation of bibliographies and literature surveys opens up a domain of research on hitherto unstudied features of research projects: bibliographies and literature surveys are accessible topics of future research in their own right.

The topicalisation of these taken-for-granted features of research projects makes formal properties of bibliographies and literature surveys available for analysis, e.g. bibliographies of an author’s works, bibliographies of particular topics, etc.; above and beyond substantive topics, e.g. begging, humour, personal names, poolroom hustling, silence, scientists’ work-site practices, etc.

The insights of Ethnomethodological programmes furnish empirical considerations of texts and textual particulars, e.g. titles and the relationship between titles and texts, corpora of literature and their relationship to corpora of knowledge.

As loci of practical reasoning, bibliographies and literature surveys are sites for the study of relevance, and how relevance is established by and for readers. The realisation of relevance is an assemblage of ordinary language activities, whereby researchers are oriented towards current and future courses of action *re* locating literature.

Bibliographies and literature surveys are standard features of research projects, which members assemble and produce using their common-sense, linguistic practices. Bibliographies and literature surveys are, *in toto*, linguistic entities.

<sup>1</sup> This is an adaptation of Charles H. Page's notion of sociology as a "teaching enterprise" (Page 1959), considered below. Page, among others, points towards Sociology having "border regions" (1959:589) as well as its own subdivisions. The substantive subdivisions of Sociology are manifest in the collection in which Page's article appears (Merton, Broom and Cottrell 1959), and criticised as "reifications" of sociological topics by Watson and Weinberg (1982:57).

<sup>2</sup> E.g. "Another approach, derived from the discipline of sociology, sees language data as socially organised and therefore involves a different view of classroom interaction." (Alzamora 1999:41).

<sup>3</sup> E.g. "In the staff meeting discussed here, [the staff] were engaged in the work of proposing and discussing rule changes. What is of interest in this paper is how that talk was done." (Baker 1997:77-78).

<sup>4</sup> E.g. "Ordinary talk is the fundamental basis for parenting-counselling sessions. Therefore, ordinary talk should be the fundamental basis for the evaluation of parenting-counselling sessions." (Carlin 1994:207).

<sup>5</sup> A second, updated edition of this bibliography was indeed published, by the same publishing house, in 1977.

<sup>6</sup> Viz. the publication of Sacks' *Lectures* (Sacks 1992a and b), and some of Sacks' early works (Sacks 1997, 1999).

<sup>7</sup> The same volume contained a Special Issue on Erving Goffman, with two bibliographies—of Goffman's works (Smith and Waksler 1989); and of secondary commentaries on Goffman (Waksler and Psathas 1989).

<sup>8</sup> Not all books contain bibliographies; e.g. references may be contained within footnotes.

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<sup>9</sup> This form of index is highly usable and useful: “References are listed alphabetically by date of publication. Boldface numbers following each entry identify the pages of this text on which the reference is cited.” (Johnson 1960:650)

<sup>10</sup> It is interesting to compare this bibliography of Sorokin’s work with a chronological listing in another reader on his work (Allen 1963:497-506). Cf. Stephen Lukes’ comment that “So many studies that concern Durkheim have been made recently that a complete up-to-date bibliography would itself be of book length.” (Lukes 1988b:597).

<sup>11</sup> Bibliographies manifest or announce such entailments, i.e. entailments are “instructably observable” (Garfinkel 1996:5), and are discussed throughout this chapter.

<sup>12</sup> E.g. Atkinson and Heritage’s edited collection, *Structures of Social Action*, had been published by the time of the 1985 version of his bibliography; articles slated for publication in a second volume of James Norton Schenkein’s *Studies in Conversational Interaction* are revised in the 1985 version, as forthcoming in the Multilingual Matters collection by Graham Button and John R. E. Lee; George Psathas’ *Interaction Competence* is originally referenced as a collection co-edited with Richard Frankel.

<sup>13</sup> *The Philosopher’s Index* (1998) 32 (3).

<sup>14</sup> I wrote an orientation paper on route-finding in Manchester for a research project (Carlin 1997), which was intended to provide visiting colleagues with an ecological context to the project. The intentions and scope of the paper notwithstanding, it was much later—and purely by chance—that I happened upon the very different but apposite (to the topic at hand, *viz.* vehicular motion in an urban setting) paper by Anthony F. Wallace (1965).

<sup>15</sup> Gillian Burrington argues that there is a paucity of literature *apropos* of the status of women in British librarianship (1984:61), yet manages to bring together an impressive corpus of literature on librarians and librarians’ work which reflects the gendered nature of librarianship as an occupation (1984:23-62).

<sup>16</sup> In an editorial note, George Psathas remarks of this study that “Prior to Jefferson, no studies of the organisation of laughter in interaction can be found in the literature.” (Psathas, in Jefferson 1979:79).

<sup>17</sup> Published in 1988, Paton’s search for “joke” and “laughter” misses various papers in the ethnomethodological/conversation-analytic plenum, which are within its temporal boundaries and nominally within its search parameters (see Fehr *et al.* 1990). Nor did Paton’s search expand to include Francophone sociology or Japanese sociology. Subsequent conversation-analytic research, outside the temporal boundaries of Paton’s literature search on laughter, has since been presented and published. As mentioned in the previous section, the continued publication of topical articles dates topical bibliographies.

<sup>18</sup> As Wieder (1988:454) notes, due to the consistency of topic within the ethnomethodological/conversation-analytic plenum, “a complete reference here tends to become a complete bibliography of ethnomethodology.”

<sup>19</sup> I am reminded that Garfinkel’s famous study of Agnes is cited in commentaries on gender and transsexuality. This appropriation into a substantive field of study is no mean achievement, considering that Garfinkel’s study constitutes a radical critique of these sociologies that reference it. I wish to thank Rhoda MacCrae for discussing this point with me.

<sup>20</sup> Helen Petrie, personal communication 11 February 1997, referring to J. W. McDavid and H. Harari (1966) "Stereotyping of names and popularity in grade-school children." *Child Development* 37 453-459.

<sup>21</sup> As Howarth's (1996) presentation shows, the "arrival" of substantive subject areas—in her case on death and dying—can be realised through the inception of dedicated interdisciplinary journals, e.g. *Death Studies* and *Mortality*. Extant works, e.g. Sudnow (1967) may then be afforded a new status within the boundaries of a substantive area. Sudnow's ethnographic study of sudden death may be appropriated and re-appropriated between and within sub-fields, e.g. Visual Sociology, Sociology of the Body, Sociology of Death, Human-Object Interaction.

<sup>22</sup> A critical approach—"criticism for criticism's sake"—which is *contra* Sacks' regard for classic ethnographies: "Instead of pushing aside the older ethnographic work in sociology, I would treat it as the only work worth criticising; where criticising is giving some dignity to something." (Sacks 1989:254). Sacks displayed a uniquely penetrating depth of scholarship, which is reflected in his approach to evaluating literature, e.g. "That conception has been criticised somewhat justly by Goodenough (I say 'somewhat' not that the criticism is only in part just, but because the concept can be subject to much more serious criticism) ..." (Sacks 1992a:382).

<sup>23</sup> An interesting case is provided by Schenkein's (1971) doctoral thesis, written during the development of Conversation Analysis by Harvey Sacks, which does not contain any references. This may be attributable to the prevailing culture of research Schenkein espoused: that is, to start with raw data rather than prior sociological theorising. Although Sacks had published little by this time (see Psathas 1989), the explication within Schenkein's thesis is clearly Sacksian. According to Schenkein's *curriculum vitae* (Schenkein 1971:vii), he read courses in ethnomethodology delivered by Aaron Cicourel, Harold Garfinkel and David Sudnow; and in Conversation Analysis with Harvey Sacks. Sacks had served on the examination Committee, along with Sudnow and Melvin Pollner. Sacks' own reaction to extant literature is discussed above, which demonstrates that Sacks' work was not reducible to Conversation Analysis *simpliciter* (again, cf. D.R. Watson 1999:1).

<sup>24</sup> The phrase "from its inception onwards" is deliberate, for even after a piece of research has reached its conclusion (for whatever reason, e.g. time, money, article accepted for publication and appearing in print) the analyst may encounter references and/or data which are relevant to the project in some manner.

<sup>25</sup> As noted in the section on elective affinities between Rose and Sacks, such formatting is edited out from the posthumous reprint of Tony Gorman's conversation with Anna Brown (Item #157 in the bibliography of Edward Rose, Chapter Three).

<sup>26</sup> Some resemblances between introductory textbooks are enumerated by Lynch and Bogen (1997). The central focus of their considerations is the positioning of sociology *apropos* science in textbooks; however, they also identify "conventions", commonalities of content and organisation, which are "arranged in an identical or near-identical sequence of chapters." (Lynch and Bogen 1997:485).

<sup>27</sup> The particular relevance of "Collection K" is Sacks' definition and application to topic, *viz.* "a collection constructed by reference to special distributions of knowledge existing about how to deal with some trouble (here, 'suicidalness')." (Sacks 1972b:37).

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<sup>28</sup> Here we may usefully consider the use and adequacy of subject-headings in e-mail communications, and their relation to the body of the messages.

<sup>29</sup> By “members” I am referring not just to the practical work of the compiler of a bibliography, but as a reader’s achievement also. Readers may approach bibliographies with different relevances—the reading of bibliographies receives more attention within this chapter below.

<sup>30</sup> The implication of “ownership” is different in its particulars from the musical forms and “communities of hearers” discussed by Hatch and Watson (1974). The musician referred to above has been made aware by [RECORD COMPANY] not that the music he composes and wishes to perform has *no* audience, but that the audience it *may* have is not sufficiently large enough to make his preferred musical style commercially viable.

<sup>31</sup> The work of painting may be found elsewhere, in retrospective comments by Rose (Garfinkel and Rose 1978) and commentaries on the artist’s work-in-progress (Driessen *et al.* 1979).

<sup>32</sup> Indeed, given the nature and title of their bibliography, it seems somewhat strange that Beresford *et al.* (1997) do not list Babuscio’s (1976) collection of accounts, *We Speak for Ourselves*; similarly, from the title of the conference, the first-person accounts edited by McCaghy *et al.* (1968), subtitled *Voices from the Margin*.

<sup>33</sup> I wish to thank Rhoda Macrae (University of Stirling) for bringing this ethnographic study to my attention.

<sup>34</sup> Cf. the bibliographical remarks at the end of a practical guide to doing research: “Inevitably, we will have ignored a text that some will think is crucial to the field. But we do not try to cover the entire territory, or even to map out all of its prominent features” (Booth, Colomb, Williams 1995:265).

<sup>35</sup> E.g. Janet Semple’s critique of Foucault’s incomplete presentation of Jeremy Bentham’s notion of the “panopticon” prison, in which each and every inmate could be under observation by prison warders at any time. Semple observes that the reception and acceptance of Foucault’s version of the panopticon is at odds with the level of scholarship expected of works which are afforded “corpus-status”. Among other *lacunae*, Semple writes that “[t]here is no index. The bibliography in the translation has been compiled from the footnotes in the original which may explain its oddly selective nature.” (Semple 1992:110).

<sup>36</sup> An autobiographical note: In 1992 I was among a privileged few students who participated in SY495, The Sociology of the Middle East, a final year course at the Department of Sociology, University of Manchester. This was the last course delivered by Dr. Ken Brown before his retirement. As some of the materials I refer to originally derive from that course, this is an opportunity to thank him for introducing me to unforgettable exotica such as Zaita, the cripple-maker, who demonstrates a talent for disfiguring people in order that their physical conditions—the moral insignia of “the trade”—incited greater sympathy—and therefore increased alms—from passers-by than able-bodied beggars:

“Can you imagine what this thin and meager body would suffer under the pounding of Zaita’s hands? A satanic smile played about Zaita’s faded lips...”  
(Mahfouz 1992:61).

<sup>37</sup> I wish to thank Sheena Murdoch (University of North London) for bringing this to my attention.

<sup>38</sup> I attended consecutive courses John Lee delivered: SY283, “Institutions and Social Processes”; and (with Liz Stanley) SY282, “Social Interaction”. It is of lasting regret for the sociological community that John Lee’s lectures were not more widely publicised and, tragically, were not recorded.

<sup>39</sup> As cited in this earlier section, see Schegloff’s (1979) detailed study of telephone openings, *re* the routinised use of names in recognising and identifying interlocutors, in talk between persons without the visual dimension being available to them.

## Conclusion

This thesis has attempted to establish that research projects involve practices which are taken-for-granted features of research and research methods, e.g. doing confidentiality, disguising identities, compiling a bibliography, assembling a literature survey, writing-up the research, etc. Furthermore, these routine practices, which are constitutive of research projects, are themselves linguistically constituted and linguistically organised. Research activities involve the iteration and inscription of natural language—a language that is known in common by researchers and participants of settings—an ordinary, commonsense language rather than a technical language upon which specialised vocabularies and disciplinary domains are established.

This thesis presents a series of studies in Membership Categorisation Analysis. Major, original, and unifying themes include (1) a coherent and consistent analytic mentality towards phenomena of inquiry; (2) the linguistic constitution of research activities; (3) the work of Edward Rose as explications of the linguistic nature of social life; (4) Membership Categorisation Analysis and the purchase it affords investigation of research practices; (5) confidentiality as a constituent feature of sociological reports. Confidentiality is addressed as a hitherto taken-for-granted, standard feature in sociology, and as part of the “lived work” of sociological research. The recognition of confidentiality as a product of members’ methods enables this thesis to pursue an investigation of confidential research and its difficulties: the sociologist must keep

information confidential, but the research site in turn withholds confidential information from the sociologist.

This thesis has presented an examination of the linguistic constitution of research practices. It has presented a reticulation of overlapping issues which are made available *via* the topicalisation of members' ordinary language practices. Original features of this project include the first analytic bibliography of Edward Rose, the explication of research practices as linguistic activities, the use of Membership Categorisation Analysis to explicate the culturally methodic practices researchers use as resources for their research projects, e.g. membership categorisations, ownership and possession of bodies of knowledge, category-related locations and category-related objects.

This methodological thesis has maintained a consistent attitude towards social inquiry which does not treat culturally methodic phenomena, i.e. the practices of inquiry, *as secondary to the textual presentation of the inquiry*. That is, it explicates ordinary, routine procedures associated with sociological research as loci of members' practical reasoning. The analytic mentality of the thesis enables it to resist the textual presentations of research as *fait accompli*, i.e. as incontestable, incorrigible and superordinate to alternate versions.

In this thesis, a variety of topics are raised and revisited. That is, topics are sustained and cumulative throughout the development of methodological arguments. However, the thesis should not be regarded as being internally diverse: such a reading of the thesis

is a product of the reification of topics. The reification of topics is an outcome of the synoptic reading and presentation of sociology. Instead, any *perceived* diversity of topics fails to recognise how topics are constituted in unifying, underlying ways. This thesis, then, de-reifies topics which are approached with a consistent analytic mentality. The explication of research practices as linguistic entities shows how the same methodological issues are realisable: how topics get to be linguistically constituted and identified by members and researchers.

Topicalising research practices means treating routine features of doing sociological research as topics of inquiry in their own right. Treating emergent properties as topics of inquiry enables the thesis to take phenomena, for example, secrets and secrecy as primordial, unmediated topics rather than using them in the service of a concept such as “impression management”. Hence, this marks a distinction between thesis and the work of Erving Goffman: normal appearances and secrecy are taken as topics rather than organising principles for conceptual elaboration.

Chapter One outlined the conceptual framework for an examination of the linguistic constitution of research practices. This framework was an introduction to themes in Ethnomethodology, Conversation Analysis and Membership Categorisation Analysis, as analytic resources for the thesis, which were brought to data derived from video-recordings and interview settings.

Chapters Two and Three followed on from the initial outlining of ethnomethodological programmes. This thesis presents an original and unique resource to the international ethnomethodological and sociological communities by assembling a bibliography of Rose's work. This is the first bibliography of Rose's work that has been compiled so far. Chapters Two and Three addressed the work of Edward Rose *via* an intellectual profile and an analytic bibliography respectively. These chapters accounted for the problematic use of intellectual biography in sociology as necessarily *post hoc*, linguistic activities. The analytic considerations of bibliographies initiated in Chapter Three were returned to in Chapter Thirteen.

Chapters Two and Three make available the linguistic nature of Rose's projects. Rose describes how he witnessed the famous ethnologist, Paul Radin, conducting a "fieldwork interview" with a Pomo Indian. Rose explicates how fieldwork, and sociology itself, are linguistically constituted activities. *This is the defining feature of this thesis: we conduct our research in and through language.*

This thesis examines the linguistic constitution of some ordinary activities that researchers engage in as a matter of routine. It demonstrates how these activities, *inter alia*, interviewing, observing, pseudonymising, transcribing, are constituted in members' ordinary-language practices and form linguistically organised features of research projects. The use of ethnomethodological programmes to highlight "seen-but-unnoticed" features of research projects shows how research projects—and the

constituent activities of research—are suffused with membership categorisation activities.

Confidentiality has been treated in discrete yet overlapping environments: the use of confidentiality by members in organisational settings, and the use of confidentiality by members doing sociological inquiry. Furthermore, it has been shown how issues of confidentiality are addressed in sociology, and how issues of confidentiality remain unexplicated features of the sociological enterprise. The organisation with which I participated brought the nature of confidentiality into sharper relief. The explication of members' common-sense devices to maintain confidentiality brings confidentiality into the purview of sociology. In so doing it elaborates upon the "fieldwork" observations reported in Chapter Four, reviews the visible impact of an important feature of social and ethnographic studies, and provides a conceptual basis for future ethnographic study of confidentiality in organisations.

Chapter Four discussed ethnographic observations made in an environment which was governed by confidentiality, where confidentiality was a "constituent feature" of the settings. From this research exercise it was observable that confidentiality has consequences for research, and consequences for participants. Ethnographic observations and case studies from sociological literature show how "confidential matters"—keeping matters confidential—is relevant and matters to people themselves.

The study of confidentiality developed within the thesis facilitates the study of the invocation of confidentiality, asymmetries of rights to invoke confidentiality (between the higher echelons, salaried staff, volunteers, offenders), the production of confidential materials, and the use of confidential materials in research. The thesis examines occasions of information disclosures by members. Organisations are collections of members, so the study of individual member's methods of disclosing and withholding information is necessary for an examination of organisations and confidentiality. Organisations cannot disclose or withhold confidential information; that is a member's accomplishment. Organisational activities are, therefore, members' activities.

Working with a particular organisation, Second Chance, has been instructive in the following way: issues of confidentiality have played a large part in this thesis because organisational activities were, in ways oriented to by members of the organisation, confidential activities. Whilst I do not claim that this thesis is a study *of* the organisation, *qua* ethnography as commonly conceived, a study of this particular organisation would necessarily require consideration of confidentiality. Confidentiality is *the organising principle* in an organisation that provides convicted prisoners with contact with “the outside world”. That is, a study of Second Chance, which did not account for confidentiality and how members of the organisation oriented towards confidential information, would only be a partial study. Such a study would, to borrow Garfinkel’s phrase again, exhibit the “Howard Becker phenomenon”; i.e. it would inform readers about aspects of the organisation without exploring the work of the organisation *per se*.

Chapters Five and Six address the tenet of maintaining confidentiality in social research. Rather than being iconoclastic or radical, challenging whether identities should be concealed, these chapters examine how confidentiality is practised by social researchers. This distinction is at the heart of the thesis itself: instead of accepting confidentiality as a feature or a resource to be used as part of the inquiries, confidentiality is treated as a topic for inquiry in and of itself.

An examination of the use of pseudonyms in social inquiry is perforce a study of names in social interaction. People's names are a commonplace yet receive little attention in sociology. A person's name makes certain information available: inferences can be made about a person just by knowing their name. The correctness of the inferences is not at issue: rather, how any inference—correct or otherwise—can be made is the topic of interest. We can infer e.g. ethnic origins from names such as O'Shea, De Boer, Krzyczewski. A survey of Hindu names from India in the first half of this century shows how much information about a person can be inferred by the name, and, potentially, from a pseudonym. Examples are provided to show how, in changing a name, the researcher can make unwarranted inferences available, thus compromising the analysis.

People themselves assume names, e.g. nicknames, stage names, pseudonyms; and are in turn assigned names by others, e.g. nicknames, pseudonyms to protect identities, or conveniences until a person's real name is established (think of a thus-far unidentified body in the mortuary). When sociologists get near to their "subject matter", watching

and listening to real people, they encounter those occasions where a person's name is spelled aloud. For analysts wishing to study interactional practices, this exacerbates the problematic task of replacing a person's name. It is not only people who possess names but organisations and places. These too are to be anonymised or pseudonymised. Chapters Five and Six show how sociologists endeavour to maintain confidentiality, to assign pseudonyms, whilst relying upon those ordinary, common-sense methods, e.g. the activities of Membership Categorisation, which sociology assumes to be of, and usually leaves to the lay person.

Chapter Five surveyed extant literature to examine the accepted practice in sociology of maintaining confidentiality, by providing anonymity to informants. That is, it looked at confidentiality as a sociological phenomenon. It observed the taken-for-granted nature of confidentiality, and the cursory notification of confidentiality as an extra-analytic matter in sociological reports. Therefore, it shows how confidentiality is an unexplicated yet oriented-to phenomenon by sociologists.

"The use of pseudonyms [...], which is common practice for sociologists to assure anonymity and confidentiality to their subjects, is consequential for the preservation of phenomena within and the phenomenological intactness of the setting."

(Carlin 1994:65)

In looking at the occasioned properties of names in talk-in-interaction, Chapter Six examined the consequences for research of changing identities. These consequences include obscuring phenomena for analysis and changing phenomena available for analysis. *Via* the recognition that the practices of pseudonymisation are culturally

methodic, linguistic activities, Chapter Six suggests how these consequences may be minimised, and concomitantly, how to minimise the violence or distortion to social settings and setting-talk.

Chapter Six demonstrated how sociologists endeavour to maintain confidentiality, to assign pseudonyms, whilst relying upon those ordinary, common-sense methods, e.g. the activities of membership categorisation, which sociologists assume to be of, and usually leave to the “lay” person. That is, confidentiality and the practices of pseudonymisation incorporate an extra-analytic, unacknowledged resource into the study. This chapter refers to the assemblage of these common-sense devices that are tacitly used by sociologists to protect persons’ identities as “the pseudonymisation ‘machine’”.

Chapters Eight, Nine and Ten show how “data” for study are problematic *apropos* the unexplicated cultural practices which “data” incorporate, e.g. ownership and membership categorisation activities. The suffusion of members’ practices in research “instruments” is manifest in “interview data”. A source of trouble for sociologists is treating the outcome of interview situations as data on substantive topics. Chapter Nine demonstrates how interviews and interview-products are collaborative outcomes of members’ conjoint, linguistic interchanges.

Chapter Ten, however, examines the recognisability of visible appearances of social phenomena, i.e. how the at-a-glance-availability of appearances is constituted *via*

members' culturally known-and-shared practices. The study of normal appearances links the considerations of observation and outcomes of interviews as collaborative events, e.g. security sweeps and looking for a bomb. The occasioned nature of personal names and identifying details in interview environments has implications for the researcher concerned with the productional particulars of members' linguistic activities. As a result, issues of confidentiality link issues of transcription of interviews and recordings of members' talk-in-interaction.

Chapter Eleven brings the 'machinery' of (early) Conversation Analysis to the analysis of a text. The empirical demonstrations of Conversation Analysis suggest how the textual presentation of talk in a sociological report has been edited. That is, the grossly observable facts of natural conversations, outlined by Sacks, Schegloff and Jefferson (1974) (see Chapter One), provide for candidate trajectories of interviewer-interviewee interactions. This is *not* a stipulative exercise, however; *via* the procedural policy of ethnomethodological indifference, I am neutral towards the interaction, and do not advance candidate trajectories as alternates or substitutes. The point is to highlight the routine, linguistic features of research practices such as interviewing, and writing-up interview products.

This theme is continued in Chapter Twelve, which examines the ordinary use of natural language practices in writing-up research. Taking the political and media commentaries of Noam Chomsky as exemplars, this chapter analyses how arguments are worded as plausible arguments, how arguments can be advanced and upgraded. Chomsky's use of

“methodological irony”, traducing members’ natural attitude orientations and substituting them with competitive orientations, enables him to upgrade his own arguments *at the expense of* alternative arguments. The work of writing up is textual, linguistically organised work.

Explication of the linguistic constitution of bibliographies, literature surveys and transcripts demonstrates that they are loci of practical reasoning on behalf of readers and compilers of bibliographies, and readers and producers of transcripts. Bibliographies are sites of interest for the analysis of the linguistic constitution of research practices for various reasons. Bibliographies and literature surveys are linguistically-organised textual artefacts, which are predicated upon the compiler’s relevances and practical reasoning.

In producing a literature survey of beggars, I found that there was a “Gap in the Literature” *apropos* the constitutive activities of “doing begging” (Carlin 1999). Literature on begging is found outwith sociology, and in sociology’s “forgotten canon”. Sociological studies which claim to study beggars overlook the activity of begging as an inquirable phenomenon; hence, studies of beggars also exhibit the “Howard Becker phenomenon”, i.e. by making aspects of the beggar’s social world available to readers, except the defining activity itself.<sup>1</sup>

The topicalisation of routine research practices such as bibliographies and literature surveys as linguistically constituted activities brings hitherto unnoticed phenomena

available for sociological analyses. As Chapter Thirteen shows, however, the considerations of topics herein are necessarily foreshortened by the constraints of writing a Ph.D. thesis. Explorations of research practices must, at some point, cease. Unfortunately, the effect of this is to allow salient features of research projects to remain under-analysed, even unnoticed. The considerations of topics in this thesis are certainly not exhaustive, the research practices explored are not exhausted, nor have I exhausted the potential of the sociological corpus. Readers will point to curious omissions within the thesis. Foremost of omissions, of course, is the work of Erving Goffman.

I recognise that Goffman may appear to be a noticeable absence for some readers, especially as he paid so much attention to issues discussed in this thesis, e.g. public space, visibility, normal appearances, secrecy, and beggars. It is important to note, however, that the thesis has presented a consistent and coherent methodological and analytic approach to inquirable phenomena.

I do not seek to trivialise or downgrade Goffman's achievements by excluding him from the thesis: it is a strength of this thesis, rather than a weakness, that considerations herein can be re-analysed from a Goffmanian perspective. For a strong example of this, consider the observations on a pickpocket in Chapter Ten.

That is, unlike the methodologically ironic device that I have sardonically described as the "Mildred Pierce phenomenon", the thesis does not cut out alternative explanations

*via* arguments by authority. As I said above, treating phenomena as topics in their own right enables researchers to explore the situated, linguistically constituted features of phenomena, rather than approaching ordinary language activities as subjacent to reifications of substantive sociological topics.

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<sup>1</sup> A report on begging as members' practical actions is presented in Carlin, Evergeti and Murtagh (1999).

## **Appendix**

This Appendix contains pseudonymised, specimen documents referred to in Chapter Four, “Confidentiality: Fieldwork and Linguistic Activities”.

The forms reprinted in this Appendix are used by members of the organisation in their administrative activities.

(1) The “Referral Form” is a constitutive document within the befriending process.

This form is a textualised interface between the voluntary organisation and the official workings of the Prison System.

(2) The “Report Form” or “Client Report Form” is used by ordinary members of the organisation (“buddies”) to update the area group of their prison-visiting activities.

The Report Form is, like the Referral Form, an administrative pro forma for the organisation. Unlike the Referral Form, the Report Form is outwith the initial process of allocation.

This Appendix also contains a legend of transcript symbols used in the thesis, for transcripts from video-recordings, interviews, and extracts of talk quoted from other sources.

**SECOND CHANCE**  
**REFERRAL FORM**  
**PRIVATE & CONFIDENTIAL**

HAS THE PRISONER GIVEN PERMISSION FOR INFORMATION TO BE DISCLOSED?	YES	NO
---	-----	----

SURNAME	FORENAME
PRISON	NUMBER
D.O.B.	HOME TOWN

SENTENCE	DATE OF SENTENCE	EDR
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ETHNIC ORIGIN (for monitoring purposes)

CURRENT OFFENCE(S)	PREVIOUS CONVICTIONS
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OTHER USEFUL INFORMATION:- family background - first language (if other than English) - hobbies - interests - skills etc., please continue overleaf if necessary

Signature of Prisoner:
Signature of Probation Officer: Name of Probation Officer (please print)
Date:

## **VISIT REPORT FORM: PRIVATE & CONFIDENTIAL**

**BUDDY ..... . . . . .**

### **DETAILS**

### **COMMENTS**

Client name ..... . . . . .  
Sentence ..... . . . . .  
Institution ..... . . . . .  
Letters ..... . . . . .  
Date of Visit(s) ..... . . . . .  
Mode of travel ..... . . . . .  
Miles (approx.) ..... . . . . .

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Client name ..... . . . . .  
Sentence ..... . . . . .  
Institution ..... . . . . .  
Letters ..... . . . . .  
Date of Visit(s) ..... . . . . .  
Mode of travel ..... . . . . .  
Miles (approx.) ..... . . . . .

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Client name ..... . . . . .  
Sentence ..... . . . . .  
Institution ..... . . . . .  
Letters ..... . . . . .  
Date of Visit(s) ..... . . . . .  
Mode of travel ..... . . . . .  
Miles (approx.) ..... . . . . .

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Client name ..... . . . . .  
Sentence ..... . . . . .  
Institution ..... . . . . .  
Letters ..... . . . . .  
Date of Visit(s) ..... . . . . .  
Mode of travel ..... . . . . .  
Miles (approx.) ..... . . . . .

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Client name ..... . . . . .  
Sentence ..... . . . . .  
Institution ..... . . . . .  
Letters ..... . . . . .  
Date of Visit(s) ..... . . . . .  
Mode of travel ..... . . . . .  
Miles (approx.) ..... . . . . .

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## **Transcript Symbols**

[ [	two utterances starting at same time
[	indicates the onset of overlap during a speaker's turn
Jeff wants us to go down the pub [what about ice cream	square bracket shows where "what about ice cream" begins during ongoing utterance
( )	word inaudible/unavailable for transcription
(word)	probable hearing of word, but not unequivocal
<u>word</u>	emphasis
°(word)°	utterance within degree signs is appreciably quieter than surrounding talk
=	'latching', indicating follow-on without break in talk
(1.0)	time of approximately 1 second silence
(.)	untimed 'micropause' (less than 0.1 secs)
(h)	laughter
(hhh)	outbreath
(.hhh)	inbreath
↑ ↓	sudden pitch shift upwards sudden pitch shift downwards
↑(word)↓	up/down arrows surround word(s) which increase in pitch and then return to 'normal'
↑ ↓	outline arrows up and down indicate where in sequence a kinesic action begins
,	location or occurrence of extraneous noises occasioned within the setting or from outside the setting, which participants (not the analyst) formulate as bearing upon the interaction
[  ]	shading indicates occurrence of name/identifying detail in sequence of talk

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