RE:READING WRITTEN DATA: on the interpretability of transcripts of talk about multiple sclerosis

A Thesis Submitted for the Degree of Doctor of Philosophy

by

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The thesis has as its consistent theme the questioning of the nature of empirically based sociological inquiry. Deriving its initial impetus from the Social Studies of Scientific Knowledge, Part I pursues the methodological implications of the problematic of reflexivity through an empirical research project - what counts for lay people, non-scientists, as knowledge - via the analysis of interviews with people for whom the irruption of Multiple Sclerosis has breached their belief in the nature and efficacy of medical science. The failed 'solution' of Part I's culminating chapter however - to embody reflexivity in the form of analytic writing - begins the turn of the thesis against its original grounds. In a pivotal central chapter, it is recognised that the transformative acts of recording and transcription means the transcripts (upon which the analysis has been based) are irrecoverably different from the original speech. The object of analysis is not talk, but texts.

Attention shifts from the question of analysis to the data as writing, texts whose value for research depends on their being different from either fiction or pure speculation (philosophy). Drawing then on contemporary work in philosophy and literary theory, Part II pursues this as a three-fold question of the datatexts' relation to their referents, re-reading them in turn through their bearing on the philosophical Question of the Self, against the quasi-literary form of autobiography and as dialogue, an event in (written) speech in which the knowledge was produced. As the work proceeds, it is recognised that their epistemological status derives, not from their ostensive referents, but in the work of reading against their form, producing particular interpretable meanings. Finally, engendered by re-reading the datatexts against Plato's Phaedrus qua dialogic inquiry, the thesis shifts from the question of referents towards the chiasmic relation between the form of the written and the figuraiity, the rhetoric of reading, which raises fundamental questions about the knowledge produced in/by sociological inquiry on the basis of the interpretability of data.

As itself a written text, the final form of the thesis - which has moved, not towards a forseen conclusion but by putting into question the grounds of each position as it is achieved yet consistently focused on 'the same' data - is of substantive theoretical import: essential to its questioning of what such knowledge 'is'.
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INTRODUCTION

The excitement of current work in the social studies of science is that it is struggling with a hundred years of tradition. Its significance lies not just in providing more or different news 'about science', but in its potential for reevaluating fundamental assumptions of modern thought.

Woolgar 1988a: 30

This extraordinarily bold claim by Woolgar in fact underplays the position. For as at least this thesis will suggest, the tradition which here becomes challenged has its roots in the fourth century BC, in Plato, in (what is held to be) the founding moment of western philosophy.

This thesis, written over the past six years, had as its impetus the position held within the social studies of science as it then was, the practitioners of which, unlike, 'most ethnomethodologists (who) tended to play down the scepticism in Garfinkel's writings, in favour of developing an empirical research programme' (Woolgar 1988a: 37, Fn 4) were committed to the endeavour of, as it were, pursuing an empirical research programme sceptically. This scepticism was manifested in a struggle with the phenomenon of reflexivity, conceived, after Garfinkel, as:

The relationship between document and underlying pattern (which) is reflexive. That is, there is an intimate inter-dependence between document and any associated underlying pattern such that the sense
of the former is elaborated by drawing on 'knowledge of' the latter; at the same time, the sense of the latter is elaborated by what is known about the former. The character of the document, as perceived by the actor, changes to accommodate the perceived nature of the underlying reality just as the latter simultaneously changes to accommodate the former. The establishment of a connection between document and underlying reality is thus a back-and-forth process. In Garfinkel's terms, "members' accounts are constituent features of the settings they make observable (1967)".

Woolgar 1981: 13

Almost uniquely, the adherents of this position in the social studies of science, amongst whom I counted myself, accepted that the problem of reflexivity applied not only to the knowledge production of others, the subjects of research, but must apply to their own (cf. for example Ashmore 1983, Latour & Woolgar 1979, Mulkay 1984, Walker 1984, Woolgar 1983). 2

From the beginning, then, the work was conceived to entail both a problem and a problematic: the problematic of reflexivity pursued through an empirical research problem.

The problem had as its immediate sociological context the observation that, whilst the sociology of science had studied the practices and institutions of science (Merton 1973), its social construction (Barnes 1977; Bloor 1984) and even had latterly turned to the constitution of scientific knowledge in the laboratory (Latour & Woolgar 1979), little attention had been afforded, in this 'age of science', to how lay people,
non-scientists, conceive of science. This problem was not posed as a question of whether they understood science properly, have accurate knowledge of scientific facts, know what science is. Rather the question was what, for lay people, counts as knowledge (as opposed to opinion or belief and so on), and what is the methodology by virtue of which that knowledge is held to have a special and perhaps exclusive ontological/phenomenal reliability. And how is this knowledge related to the concept of science as understood in the sociology of science?

The research project on which the thesis is based comprised a study of lay accounts of the chronic neurological condition multiple sclerosis (MS), given in interview by people who themselves suffered from the condition.

For those sociologists interested in knowledge as a social phenomenon, so the argument runs (cf Knorr-Cetina & Mulkay 1983: 4), a focus on science is especially fruitful because the production of knowledge and its epistemological status are topics for the practitioners, the scientists themselves, and thereby rendered unusually visible to the sociologist for analysis.

In that medicine operates at an interface between lay and scientific knowledge, in that medicine is perhaps the only science directly applied to and explicitly visible to non-scientists, it seemed a particularly appropriate field for my research.

Furthermore, within the domain of medicine, there are
features of the condition MS which made it an especially appropriate topic for the pursuit of both the problem and the problematic: for the nature of science - both as method and a body of knowledge - is immediately problematic for the participants in the research project, people who themselves have MS.

To put it briefly, although MS has been the object of concentrated medical research for more than a hundred years since it was first discovered by Charcot in the late 1800's (but see Chapter 2 and Appendix 2.1), according to the research literature, it is a condition which continues to defy the most concentrated scientific study:

For over a century, MS has intrigued workers in all the neural sciences, with perhaps more publications resulting than for any other neurological disorder. However we face today a situation little different from that of Charcot: it is still really a disease of unknown cause, inadequate treatment and unpredictable outcome.

Kurtzke 1980: 1970

For individuals, the experts' self-admitted lack of effective knowledge about a condition they can more or less define yet can do little about would seem to violate the common knowledge - if such there be - that science is both powerful and omniscient. MS would appear to induce a scepticism about science that would leave its victims with no reliable basis for knowledge. How, in that situation, did people go about making sense of their MS?

The pursuit of this problem was, as has been indicated, regarded from the beginning not as an end in itself but as an occasion for addressing the implications of reflexivity for research,
a position which involves the doubting of the model of scientific method as the paradigmatic basis for research. There was, therefore, a terrible resonance between the problem and the problematic.

This is how the thesis opens. As the work proceeds however the theoretical position and particularly the key problematic of reflexivity has to be reconceived, and is eventually discarded. The disciplinary context shifts away from sociology towards literary theory and continental philosophy; away from science and the problem of analysis towards the question of the interpretability of data. Beginning, then, with ethnomethodology and, to use Pollner's phrase, 'of necessity' "backing into Philosophy" (1987: xiii), the thesis ends with Plato's Phaedrus and the relation of language to inquiry. But although the work has indeed 'backed into philosophy', it does not do so in the sense of finding there, in that canonical discipline, in the authority of Plato, an ultimate resource, a sure ground upon which at last to be able to "stop doubting" (Blum 1971: 316). (Hence my assertion at the beginning that, if this thesis may properly be construed as belonging, at least in its origins, to "current work in the social studies of science", Woolgar is underestimating the gravity and the hubris of the challenge).
Although the order of concern is philosophical, the philosophical questions about the nature of knowledge and how that question may be addressed (in particular, the consequences of addressing it in particular ways) are produced, not by abstract speculation or introspection against the light of Reason and Logic and informed by the philosophical disciplinary corpus—but by a fierce and consistent concentration on a set of data, grounded—however problematically—'in' the world of the actual. The philosophical questions arise in the thesis from the practice of sociological research, in the attempt to carry out a concrete research project in accordance with the theoretical and methodological concomitants of its starting position. The thesis is thus the philosophic order treated sociologically.

Despite the shifting movement of the argument of the thesis (which will be described less cryptically below), it has a central and consistent theme—epistemology. I do not mean, however, the term in the sense referred to by Hindess as that generally understood: as "(A) discipline, bordering on philosophy, whose function is to examine the methods which are used or which should be used to produce valid knowledge" (Hindess 1977: 2-3), conceived, that is, on the basis of a specific relationship between philosophy and the (social) sciences in which the method is produced by philosophy and the (social) sciences, cleansed of philosophical incoherence, would be the realisation. Rather, and this is vital to the avoidance of misunderstanding, the term epistemology is used to denote the order.
the level of concern: with the relation between theorising and its grounds. As such, 'the thesis', as a representation of research, as itself "theorising", has to be included in that concern.

The final form of the written work is essential to its substantive matter, its structure essential to the resistance to the epistemology inherent in the thesis usually regarded as a virtual text, a mere description of research. Hence the retention in the final assembly of the chronological order in which the chapters were written, the refusal to 'up date' each chapter and the breaks between the chapters, the staccato effect, were risks taken as a way of making a substantive point. For the movement of which this thesis consists, from lay understanding of science to Plato was not conceived of from the beginning as the telos of the work. On the contrary each chapter was written in ignorance of where the work could lead. Each chapter of which it now consists represents a moment in the time of the work, a moment at which it appeared possible to move. Each chapter represents the next possible step, not towards a fore-gone conclusion but away from the previous one.

As it now stands, this work resists the unilinear, consistent model of a thesis, representing in its structure a kind of questioning that is open-ended rather than closed; that as a problem is addressed and some kind of response produced, takes up the grounds upon which the conception of the problem relied. Given the readiness to question the grounds of its
own statements, the movement is, rather convoluted, folding back upon itself, moving on from and returning back to, but always differently, to re-address sometimes implicitly sometimes explicitly, the 'same' questions, resonating through the work.

For the thesis is, above all, a work of writing, to be read not as a neutral report on a research project - a description of what occurred between the posing of a question and its resolution by means of appropriate method applied to data - for which 'the research' would be the object read about through the text, but which requires to be read for the constitutive effects of the form in which it is written, subject to a reading which is alert to the possibility that the form formulates the thesis.

However, in editing, in preparing the text for presentation, I have to an extent which I cannot but regret, restored a kind of mono-linear imperative: lost something of the blindness with which it was written.

This effect is apparent even more markedly in the summary proffered below in accordance with conventional requirements, where the limits imposed by the summary to dominate the polysemy of the text, to show how the argument moves from and is given rise to by one chapter to the next, to articulate the smooth connectedness of each part of the work, means the denial of much of what I hope nevertheless remains ambiguous and resonant in the text. In an important sense, the requirement to
summarise the work epitomises the very thing which the work is resisting: the conception of a thesis as a work which may be shortened or lengthened, for which writing comes after the inquiry, the meaning of which is closed and conclusive. It is such a view of writing, of the written, of the neutrality of language used sociologically to produce and describe knowledge that is eventually not merely doubted but directly challenged.

SUMMARY

The thesis as it now stands falls into two parts, the first driving towards chapter 4, the second, turning upon the pivotal fifth chapter, moving against the first.

PART ONE

RE:WRITING ANALYSIS

The original problem was, to reiterate, a question about lay understanding of science: what counts for lay people, non-scientists, as knowledge and what are the means by which it may be known to be such. It was through addressing this problem that the implications of the problematic of reflexivity for empirically-based research were to be worked out.

The first three chapters are concerned to explicate the grounds for the contention that MS, 'medical data', is an appropriate topic through which to theorise about knowledge, and to begin to elaborate on what is meant by that contention. Rather than
by direct description, the explication of the concerns is approached elliptically, for the difficulty from the beginning was in knowing how to proceed with the thesis given the problematic of reflexivity applicable, of necessity, not only to the analysis of the data but of the research literature, to the setting of the research in its disciplinary context.

The point of the first part of the thesis is to problematise the dichotomous and hierarchical categorisation of knowledge into 'expert' and 'lay'; the means, a disruption of the category of 'expert', explicitly of scientific medicine but implicitly also of that "disruption".

Chapter 1 is an extended objection to the acceptance in medical sociology of the facticity of biological science, both as fact and method, and to its complementary reliance on an unproblematic scientific methodology as the basis of its own research.

In the last part of the chapter, a further depiction of the sense in which reflexivity is being held as problematic is made via a consideration of the social constructivist perspective applied directly to medical knowledge.

In Chapter 2, I turn to the medical research literature on MS in order to substantiate the unexplicated basis of the critique advanced in Chapter 1, that the assumption (by medical sociologists) that medical knowledge arises simply from the
nature of disease is not tenable. This is done by showing in
detail how 'the nature of the disease,' MS as the thing it is
taken to be, is reflexively tied to the knowledge brought to
bear by experts on what are taken to be documents of it.

The 'document' in question is a lay text, a passage
from the Diary of the Comte d'Esta, written in 1822 before MS
was first identified but which, according to an authoritative
textbook on MS research (McAlpine et al 1972) is clearly the
first recorded case of the disease. The detailed knowledge of
current research findings and hypotheses which make it possible
to read the diary as an obvious case of MS is given, going
line-by-line through the diary extract.

The significance of the description of his condition
given by the Count has, it is shown, to be 'already known',
brought to the document. This allows me to show not only the
constituted nature of the 'objects' of medical research, of
'expert' knowledge, but to develop the theme of problematising
the assumption of hierarchical difference between expert and lay
knowledge of MS. Being readable as about MS only on the basis
of expert knowledge, the diary, d'Esta's account, is absorbed
into the body of knowledge about MS, which thus in turn
constitutes the 'layness', the non-expertness of lay accounts of
'the same thing'.

An important strategic function of this second chapter is
to introduce the current epidemiological and neurological
research into MS as to its nature and genesis; at the same time,
to link with Chapter 3 and to the thrust of the original research
problem, by showing how science has, in the case of MS, failed, despite concentrated scientific research effort, to produce either reliable or effective knowledge of what MS 'is' or means.

The salience of using lay accounts of MS as a way of approaching the question of lay theorising about knowledge is further substantiated.

This chapter is also, inter alia, a means of giving the details of MS - its accepted characteristics in terms of symptoms and prognoses (in all their ambiguity) and the peculiar problem attached to its diagnosis (in a way that is not mere description, but retains awareness of the problematic) which will be necessary in order to understand the data as it appears in the thesis (exemplifying nicely the chapter's point, of the necessity of bringing 'already knowledge' to the reading of the documents of MS, which then constitute, 'back-and-forth' the documents' meaning: the inescapability, for the thesis itself, of the documentary method).

3. Having in Chapter 1 placed the thesis' concerns against the relevant sociological literature and in the second against the literature on MS, in Chapter 3, following the formal structure of a thesis, I turn to elaborate on its theoretical auspices and the question of methodology.

However, given the previous chapter's demonstration of the problematic inherent in the description of what something 'is', the approach to ethnomethodology is made again elliptically,
through the criticisms made in the literature of 'it'. The meaning and implications of ethnomethodology both for theory and practice, and 'the troubles' with it identified by its many critics are, it is shown, not only different but incompatible. The object of their criticism, ethnomethodology, is constituted through the theoretical/sociological priorities which they (like me) bring to examples of it.

Although used at this stage to extend the points made about the scientific literature on MS to sociology rather than in themselves, many of the specific criticisms made of ethnomethodology re-emerge in the latter part of the thesis: especially the contentious question of the conception of the self, of consciousness, of 'the actor': the source of meaning, the conceived origin of 'members accounts'.

Having, although obliquely, fulfilled the requirement to describe the theoretical position of the thesis - the suitability of MS as an occasion for theorising is then made explicit, through its metaphoric resonance with what I take from 'ethnomethodology', Garfinkel's breaching experiments, designed as a means of rendering the taken-for-granted 'strange' and thus visible for analysis. In the case of MS, it is argued, the risk of the management of the methodological horrors being an analyst's artefact, rather than a feature of members' practical reasoning, is circumvented for the irruption of MS and the failure of science to provide valid and effective knowledge about it induces epistemological scepticism, not (just) for researchers but for the research participants.
The second half of the chapter turns to the question of method, of how to analyse the data, a set of interviews with people talking about their MS. Conversational analysis and the interpretive approach to talk data are considered and rejected in turn. Although both claim an indebtedness to ethnomethodology, both rest on an assumption of the independent facticity of the phenomena they reveal 'in' talk: its invariant structures for conversational analysis and the cognitive processes taken by the example of interpretivism that is being considered, to underly and to be revealed by people's talk. Neither method provides any assistance to the question of how to proceed to the data whilst remaining committed to the problematic of reflexivity.

There has nevertheless been some movement in the chapter. For through the consideration and rejection of these two methods of analysing talk, what has been demonstrated is the theoretical dependence of method. In that sense it is the theoretical problematic that still has to be pursued.

But the chapter ends finding no way forward. Despite pointing to the achieved nature of the account of the contextualising literature, both explicitly and implicitly in the three chapters so far written, to quote the final paragraph of chapter 3: "by analysis, the 'making strange' even of analysis to itself is depicted as over. The produced analysis cannot represent strangeness: all it may do, as here, is point towards its absence ...the claim (of privilege) is inherent in analysis." (100).
In the next chapter, this observation is refuted. For ironically that conclusion provided a possible purchase on the question of how to proceed whilst remaining faithful to the sceptical programme: that is, by including in analysis its own theoretical grounds, its constitutive nature qua an occasioned account. A kind of resolution emerges in the possibility of writing in such a way, of constructing a text in such a way, as to represent the reflexive relation in analysis itself.

The fourth chapter consists of three elements: an analysis of the data written according to normal form, with a problem, a method, and findings, concerning the research participants' accounts of how they came to the knowledge that they had MS and how and from what that knowledge was distinguished in terms of facticity.

Interleaved with this analysis, identified by an italic typeface, is a text which considers the matters suppressed by the third-person objective analysis, raising questions about its methodological and epistemological assumptions, its management of the 'methodological horrors', that it, of necessity, cannot attend to.

The third element consists of quotations from the data, the participants themselves accounting for their knowledge that what they had was MS. Whilst constituting the basis for the 'straight' analysis, this 'same' data constitutes the basis for the italic text's questioning of the grounds of the first.
Each of these three elements are accounts in themselves, in relation to each other and, as a whole text, an account of reflexivity.

This solution however ended in an aporia. For, despite the complexity of its structure as a piece of writing, despite its radical innovation as a form, my intentions in writing the chapter in that way as the only means by which to make the theoretical point, utterly failed to produce the intended effect (apart from on those readers already concerned with the problem of reflexivity). It failed to draw readers' attention to the constituted nature of analytic knowledge, to the epistemological horrors as a general phenomenon. It was, for example, read as an unusually candid description of the research process, 'warts and all'; as witness to a troublesome tendency to solipsism; and was even used as a teaching text for medical sociology students with the advice to 'ignore the bits in italics' (Pinch 1983). It was also, and this will come to have been of the greatest consequence, read as representing voices in dialogue; a kind of dispute between a naive and a sophisticated analyst over the data, whereas it was my intention that the three elements were the same voice: the voice, so to speak, of research.

This failure of the text's substantive and subversive radicality drove attention towards the question of the form of the written: to what it was about writing that resists an
author's intended meanings; to the relation between writing and author, the text and the origin, the transcripts and the speakers.

Written after a gap of more than a year, Chapter 5 marks a major shift in the work, a breaking away from the cumulative but aporetic first part, a shift which will in turn be turned away from as the work unfolds.

It is the pivot against which the movement of the remainder of the thesis turns. Its basis is the simple observation that what analysis works on is not talk but writing. To analyse the transcripts is not the same as to analyse talk, people in the unmediated self-presence of their speech.

Using extracts from the data, it traces the double trans-formation of the original talk, firstly in the active process of taping and second in the process of transcription, into texts-to-be-read. On the basis of this recognition of a crucial difference for research between talk and text, the data is now re-named as datatexts (dtexts), to denote what from this point is taken to be their critical characteristic.

Chapter 5 marks then a re-thinking of the assumption grounding the thesis so far, that the problem of reflexivity lies in the relation between analyst and analysed, for that was to take the written as transparent, took it for granted that to read
research literature was to have access to their author's intentions and assumptions, which took the transcripts as the talk of people with MS.

Broadly speaking, the last three chapters (6-8) of the thesis pursue the consequences of this reconception of the nature of the data for the research project.

PART TWO

RE:READING DATA

Throughout the first part of the thesis, the project has been concerned with 'getting at' the meanings the research population, the people with MS, attributed to kinds of knowledge. After the break in the fifth chapter, the barrier set up there against that possibility by the recognition of the data as texts, the concern has to shift to the meaning of the written, to the question of the interpretation of texts.

The conception of the problematic of analysis has also to shift, becoming rather a question of the difference made to research based not on 'the knowledge of the research population', but on texts.

What, then, are the consequences for research that the data are not talk but text, not speech but writing? In general this turns upon the difference held to exist between writing
and speech as media of knowledge. In particular, it turns upon the kind of texts the dtexts are, for they are not just any writing, but this writing in particular.

Fundamental to the possibility of (this) sociological research is that, even now, the data texts upon which it is based are different from the object texts of literary theory and of philosophy: they are not fiction nor pure speculation but in some crucially different way related to 'the world'.

As Part Two opens, the key to this essential difference is taken to lie in their relation to their referents, that which would confer upon them their epistemological status as knowledge, the sources of their authority as not-fiction, as not philosophy.

Providing a cohesive structure to Part Two, this possible relation is conceived of as being, perhaps uniquely, three-fold: their ostensive authority lying in their relation to the speakers whose words and knowledge they represent, to the lives they are about, and to the event, the dialogue through which the knowledge of which they consist was produced, in the original interviews.

Each chapter takes up one fold of this three-fold question of reference. At the same time, pushing at the difference data makes, the dtexts are re-read first against a philosophical question (the Question of the Self), against a comparable
literary genre (Autobiography) and finally against a philosophic text (Plato's Phaedrus) both written in and about dialogic inquiry. By re-reading the dttexts in terms of their similarities and differences to these other forms, I am trying to press on the problem of interpretation represented by the nature of the dttexts as an unprecedented form of text and its effects on the problem of their interpretation for sociological research.

The dttexts have however been read from the beginning as having a bearing on and being an instance of the problematising of knowledge for the speakers, the people with MS, induced by the irruption of their condition, the basis of their "terrible resonance" with the methodological problematic with which the thesis opened. In that sense the dttexts have been, from the beginning, readily interpretable - and with this meaning in particular. The question is not, therefore, how should the dttexts be interpreted, but how are they? What is it about them that had produced their ready interpretability; this meaning?

In Chapter 6, the dttexts are re-read for their contribution to a pressing problematic in contemporary philosophy, the putting into question of the ontological givenness of the self as a consequence of their own philosophical theorising, whose epistemological status is thereby undermined. A similar irresolvable dilemma was part of the epistemological scepticism interpreted as having been induced, not by abstract reasoning, but by the
disruption of the relation between the concrete embodiment of
the self - the body by MS - in the conception of MS as a
'natural breaching experiment' (see xvi above).

It is shown, however, through a sequence of readings,
each of which focuses on an aspect of the dtexts the one before
failed to regard as problematic (their relation to their
referents, the self-referential grammar of their speech, their
form as narrative about the self) that the contribution of the
dtexts to the Question of the Self, the presence or absence of
a questioning of the givenness of the self, depends upon which
aspect of the dtexts is being paid attention to, and what is
assumed to be the referential relation to their origins.

By the end of the chapter, each interpretation of the
dtexts' bearing on the Question of the Self that has been
advanced has been shown to be refutable. The 'real self',
ethnomethodology's 'practical reasoner' (and cf Woolgar 1981,
cited on pv above) and what it knows - the source of the
accounting practices traced in the first part of this thesis
and especially in Chapter 4 - as the origin of the dtexts and
key to their epistemological status is irrecoverable. The
abstract conception produced through these readings, produced
through reading, cannot be taken as the empirical origin of
speech, who can be known through research. 10

A philosophical question pursued on the basis of the
dtexts raises the question of the relation between the form
of the text and reading, in this case, their form as speech-
in-writing. As has been shown, their immediacy and authority as voice is disrupted by the fact that they are speech-in-writing which, in requiring interpretation, allows for difference in meaning. Reading mediates the meaning of the dtexts.11

In Chapter 7, breaking from regarding the dtexts as speech-in-writing, the question of the relation between form and reading is raised again, in terms of the second fold of the three-fold question of reference: to the life they are ostensibly about which, insofar as it is actual rather than imagined, appears to authorise the texts as not-fiction and to re-cohere the disintegration of the self-as-speaker into the author of the text, whose life it readably refers to.

Re-reading the dtexts as writing against the literary genre to which they are most nearly comparable, autobiography, raises the question, via literary theory, of the relation of author-to-text and of the differences held to exist between literal and literary forms of writing to which former category the dtexts seem most properly to belong.

Comparing the dtexts first in terms of their similarity to and then in terms of their difference from the characteristics of genuine examples of the genre, it is shown that 'the life' which appears to give rise to autobiography is rather produced as an effect of the form. But this works its way out in a very particular way in the case of the dtexts. As autobiographies,
the descriptions of their own lives by the authors of the dtexts can only be partial. This gulf between what is written and what it describes, inherent in the form, allows for, requires a reading which identifies and supplements that which is incomplete and self-interested in that description, making the dtexts, insofar as they are autobiographical in form, peculiarly vulnerable to sociological irony (Woolgar 1983). Thus the life of the author and therefore the author him/her self is irrecoverable from the text itself without the supplement of (rather than the mediation of, as shown in Chapter 6) reading.

According to traditional literary theory, the authority of autobiography as the genuine story of a life rests on and is identified by the author's proper name attached to the work. Because of the requirement to make the dtexts anonymous, it becomes the researcher's name, my name, that authorises them as genuine, the guarantee for readers of the thesis of the empirical status of the dtexts as not-fiction, and not my name as such, but rather as itself authorised by the tradition in/against which the dtexts are here being read. The tradition of sociology becomes the source of their authority, doubly de-facing the original author.

According to contemporary literary theory (de Man 1979) the defacement of the actual author and the difference between the text's description and the life it is about is a consequence of the figurality of the form of writing that is autobiography, a consequence definitive of and exclusive to literary language. It is shown in the chapter that the irrecoverability of the
original authors and their lives is a consequence of language used literally, the literal description of the life with MS of which the dtexes consist, read, that is, as literal by sociology and as a consequence of their being interpretable against autobiography. In being written texts that are not literature.

It is thus shown that the distinctions between literal and literary forms (of language and text) rest on the ontological status of the referents of each category; the referents which are in irrecoverable relation to the text 'itself' insofar as the form of autobiography is not itself neutral but produces that ontological status as an effect.

Put more broadly, in Chapter 6, it is the form of the dtexes as language - as speech that, in being written, requires interpretation and the mediation of reading, that at once confers their authority vis à vis the speaker-as-origin and undermines it. In Chapter 7, it is the dtexes' form as writing, as a particular written text, that requires a reading that supplements what is readably absent, that makes them peculiarly vulnerable to sociological reading, at once conferring their facticity and putting it into question.

These readings were produced in Chapter 6 by reading the dtexes against the form of a philosophical question, and in Chapter 7, by reading them against a comparable literary genre.
In the final Chapter 8, the dtexs are read against a specific text, Plato's Phaedrus, which concerns a philosophical question and is written in a particular literary form: the form of dialogue. It is this text which is held by philosophical tradition to be the authoritative source of the prioritisation of voice over writing - the basis of the phenomenological philosophy to which both ethnomethodology and the sociological problematic of reflexivity with which the thesis opened are, more or less wittingly, indebted. In re-reading the dtexs against the Phaedrus, the major themes of the thesis are set into play.

The chapter begins by continuing with the 3-fold question of reference: the dtexs to the event, the dialogue through which they came into being, re-addressing the theme of Chapter 7, the relation of form to interpretability, to readable meaning. Taking written dialogue as a form which produces a particular referent as its effect - a unique event, in speech, that takes time, and is about something (in both these cases, about knowledge) - the differences in how these effects occur in the dtexs and the Phaedrus are seen to produce particular interpretations of their meaning, which are specified in the chapter itself.

It is also shown, however, for the last time, that a text's referential status is produced in interpretation.

Dropping the approach to the nature of the dtexs through the question of their referential status, and following the chronology of the Phaedrus, attention breaks to the form of which the
dtexts(and the Phaedrus) consists and in and by which their substance -as an inquiry- is produced: to what is peculiar about knowledge produced in dialogue.

By the final section of the chapter, the reading of the Phaedrus as the source of 'Plato's opposition to writing' has become radically contested, the source of speech's authority itself undermined in/by the Phaedrus, through the convolutions of form against content in which Socrates' apparently plain statement of the first task of philosophy - without which philosophy cannot begin - to "know myself" are enfolded.

As written dialogue, the knowledge of MS of which the dtexts readably consist is similarly produced and undermined.

In the final section of the chapter, which is also the end of the thesis, and again against philosophic tradition, it is shown that far from the Phaedrus consisting of the denigration of rhetoric at the expense of dialectic as means of approaching the truth, true knowledge, as the Phaedrus itself exemplifies, in order for argument to move, dialectic requires rhetoric and that in order to persuade, rhetoric requires dialectic - argument against what went before. That they are in chiasmic relation which at once is essential to the movement of inquiry, the production of knowledge and at the same time its undoing.
The dtexsts themselves an inquiry, are, being argument in language, written in dialogue, involved in this figure of chiasm. They are, being dialogue, dialectic in form in that they are opened to question in the necessary activity of reading, and rhetorical in their effect so that their interpretability can only, by hard reading, be made problematic, their referential status not simply obvious: people present in their speech, talking about MS and producing knowledge of it that is recognisable as true.

Reading back over the three chapters making up the second half of the thesis after moving to the end of the Phaedrus, they can be re-interpreted as an anticipation of the elements of this final chiasmic model of inquiry. The first, Chapter 6, reading the dtexsts against a philosophical question, moves dialectically, putting each reading of the dtexsts on the Question of the Self into question. The second, Chapter 7, looking at the dtexsts in terms of the literary effects of their nearest equivalent genre, concerns their rhetoricity. And both occur in the first two parts of the final chapter, where it is the rhetoric of dialogue that is at first addressed, then the dtexsts/dialogue as argument, as an inquiry, and both in the final part, the text of the chapter itself moving in dialectic empowered by rhetoric.
The thesis as it ends turns from the question of its data back against itself - itself as a work of writing of a particular form and as a mode of inquiry that allowed, from the beginning, the movement against its own questions and solutions, chapter by chapter as it unfolds as a work. The movement of inquiry represented in/by the thesis is dialectical, its form as writing inexorably rhetorical.
INTRODUCTION : FOOTNOTES


2. To be contrasted with, for example, Collins (1981, 1983).

3. The risk is that of the accusation of solipsism and/or of a subjectivist personal history rather than a thesis. The effects of chronology discussed in Chapter 8.1 vis à vis the dtexts apply, however, by the same token, to the interpretability of the thesis.

4. Reading of 'the literature' at any point in the course of the work other than that at which it was done would, being in a different context, produce a different interpretation. (The most striking example of this in the thesis is the later recognition and return to the issues raised as 'Troubles with Ethno' in 3.1.1) (and see xvi, above). It will I hope become clear from reading the thesis itself why this would have destroyed the thesis.

5. There is from the first chapter a significant preference for citations from specified texts ie for 'empirically based' critique rather than one made on the basis of paraphrase or of general trends, perspectives and so on. This preference was used at the beginning as a means of eliminating at least some of the layers of 'accounting' involved in such practices; it becomes of increasing substantive consequence.

6. It is part of the commitment to 'empirical work' (see Fn 5) and its problematic that the medical sociology referred to is exclusively the research literature and not the theoretical.

7. The paper in first draft was presented in a number of research forums and pre-circulated to some of them: the Discourse Analysis Workshop, Oxford Polytechnic, September 1983; the George Santon Centennial, Ghent, 1984; a staff Research Seminar at Brunel University 1984 and at a meeting of the Graduate Research Forum at Brunel. The readers' responses referred to here were culled from those occasions and from the comments of other readers of the text to whom it was circulated.

8. See eg Mulkay 1985: 77, Fn 2.

9. The latter half of the thesis, so described, represents the work as an ordered and progressive sequence, as dealing with a question in the classical manner by breaking it down into its component parts ('three folds of reference')
which, separately addressed and then re-assembled, would represent the covering of the whole field of the question and a resolution of it. It will however be seen that this representation of inquiry is that which will be contested as the work proceeds.


11. This Chapter is also in effect a challenge to the concept of intentionality, fundamental to Austin’s Speech Act Theory and to Ricoeur’s hermeneutics applied to the social sciences (1981) (see Chapter 5, this text).

12. It seems right that I should at least acknowledge the intense controversy that has arisen over the past year concerning de Man’s war time relation with Nazism in his native Belgium and in particular, the matter of his - like Heidegger’s - subsequent silence. This raises in a particularly grave form the question of responsibility and writing discussed against the Phaedrus in Chapter 8 written before this controversy arose, having evoked again the anathema pronounced on deconstruction and all its works as involved not only theoretically but actually in the destruction of the human.

(See for example) J Hillis Miller writing in the TLS, June 17-23 1988, p676, and Derrida’s ‘Paul de Man’s War’ in the Spring 1988 issue of Critical Inquiry).
PART ONE

RE:WRITING ANALYSIS
CHAPTER ONE
LAY/EXPERT KNOWLEDGE IN THE SOCIOLOGY OF MEDICINE

***
As far as the writing of a thesis is concerned, the objective of a review of the literature is to demonstrate an expertise on the part of the author through her familiarity with relevant work and, at the same time, to delineate the lacunae in that work which it will be the task of the thesis to remedy.

Although reading the literature is basic to sociological (expert) practice, what is involved in such a reading and presentation of texts has rarely been attended to, reading and writing regarded as mundane, basic, technical skills. What follows is a review of the relevant literature from medical sociology. But it is the result of 'artfully managed' accounting procedures, and is as much constitutive of as constituted by 'the literature'.

***
As was described in the Introduction, the interest of MS for the thesis lies not in the condition as such nor in the 'people with MS' qua patients, but rather insofar as the ways in which people with MS make sense of the condition illuminates the broader issues of theorising with which this thesis is concerned. The medical sociology literature discussed in this chapter is approached, therefore, not in terms of its substance and findings but at the level
of its theoretical and methodological approach to data that is similar to mine, people talking about their condition.

The aim of this first chapter is to explicate those broader issues through a critique of the ways medical sociologists have treated the patients' perspective and conceived of its relation to expert scientific medicine.

My immediate theme is to trace a double absence: the absence of interest, despite appearances, in the knowledge either of patients or of scientific medicine, and the absence of interest in their own discipline as a knowledge producing practice. For the work which is to be discussed, medical science discovers and describes the physiological world, medical sociology discovers and describes the patients' perspective. The objects of research exist independently of the methods employed to discover and describe them. In the first section, this assertion is substantiated through a critique of work on the patients' perspective; in the second, it is argued that even in those instances in the literature where lay perspectives are considered from the perspective of the sociology of knowledge, the same applies. It is the unquestioned assumption of the facticity of science which accounts for the absence of concern with reflexivity: the possibility that research constitutes, through its theoretical assumptions and methodology, the objects of its study.

In the third section, a recent work which explicitly takes a social constructivist approach directly to medical knowledge is discussed. It is this latter theoretical approach which will be
considered in most depth since it most nearly approaches, and yet holds back from, the issue of reflexivity.

The literature specifically on MS will not be referred to until Chapter Two, where it will provide the opportunity for a substantive elaboration of the issues broached in Chapter One.

1.1

THE PATIENTS' PERSPECTIVE

There are now many detailed studies of lay perspectives in the literature of medical sociology. Much of the focus of the research is, however, on the analysis of the interactions between patients and GP's or other medical professionals. These studies pay attention to the problems to which these interactions give rise as communications, the majority of the research having the objective of enabling the medical profession to improve that communication which means, in practice, the improvement of the understanding by patients of doctors' instructions (See Becker 1974; Garfield 1982). There is also a strand within this area of work which considers the doctor-patient relationship in terms of the differential power statuses it exemplifies, and which seeks to speak 'on behalf of' the patient (Cf. Stimson & Webb 1975; Wadsworth & Robinson 1976: Corsaro 1982; Hughes 1982; Stimson 1982). It is generally the case that - as in most medical sociological research - the theoretical perspectives of that discipline, with their concomitant concepts and key issues, are applied to a specific area of social life. As the title of the
discipline makes clear, it is the sociology of medicine.

Of more direct relevance to this thesis are those studies which describe how lay people 'make sense' of their own conditions. These
tend to focus either on a single medically diagnosed condition - epilepsy (West 1979), heart attacks (Cowie 1976), rheumatism (Wiener 1975) for example; or concern lay perceptions about the nature or genesis of disease (for example Lederer 1952; Freidson 1961; Mabry 1964; Elder 1973; Snow 1974; Helman 1978; Blaxter 1979, Locker 1981; Pill & Stott 1982; and Pritchett 1982). Yet even though such studies do not directly focus on the doctor-patient relationship, and the data upon which they are based is most often gathered away from the formal medical setting, nevertheless their avowed purpose is to illuminate that relationship. Blaxter's study of the causal models of disease employed by working-class Aberdonian women, for example, compares those models with that of medical science in order to examine the implications of the "great potentiality for conflict" (1979: 161) she discovers in their difference, and therefore, "for [people's] behaviour as a patient" (1979: 154). Pill and Stott, studying working-class mothers' views on the aetiology of illnesses, likewise discuss their results:

(In relation to current health education policies, consultation behaviours in primary medical care and consumer attitudes to the services provided by their doctors.

1982: 43

In that sense, they differ little from the purpose of looking at patients' perspectives outlined some twenty years ago in what is still (along with Apple's 1960 study) one of the most ubiquitously cited research papers in this area, Baumann's 'Diversities in Conceptions of Health and Physical Fitness' (1961), in which she stated:
For the individual physician, an awareness of the differences in conceptions of health can produce another tool for the management of his patient, enabling him to interpret a medical regimen in the focus most meaningful to the patient's frame of reference and, thus, providing an additional incentive to cooperation in achieving their common goal.

1961: 46

- the "common goal" being, of course, that of the professional.

For society, Baumann describes the benefits as assisting, "persons interested in planning public health programs, or physical fitness campaigns" (46). Although the terminology may appear out-dated, the sentiments remain unchanged.

Even the work which directly addresses patients' perspectives on the concepts which medicine employs - of symptoms or of disease - retains this orientation towards the improvement of the practice of medicine. Jones et al's paper, 'On the perceived meaning of symptoms' (1981) for example addresses the question of patients' understanding of 'symptoms' but, to endorse the point just made, relate their findings to the literature on delay in seeking medical advice. It is significant that in this study, the 'symptoms' which were to be interpreted by the patients consisted of a list of 45, drawn up by the researchers, and rated on eight "semantic properties", similarly derived. What Jones et al were looking at in fact was patients' understandings of the doctor's perspective.

Cassell's much cited article on the concept of disease, 'Disease as an 'it'' (1976), although based on transcripts of doctor-patient consultations and thus dissimilar to my own data, is more directly relevant to my interests in that he is interested in the language in which disease is described, pace Jones et al, by lay
people themselves. He explains:

What the present study is about ultimately is a kind of 'mapping' of the body and its diseases as they are portrayed in the language of a person, in an attempt to better understand the relationship between mind and body...Questions of the relationship of person to body are crucial to many of the ethical and personal dilemmas of modern technical medicine. Anything that sheds light on these issues is of the utmost relevance.

1976: 145

As the latter part of the quotation shows, the relevance of this interesting approach to the patients' perspective is nevertheless still confined within the bounds of medicine; the patients' concepts of disease interesting only insofar as they need to be understood - by medicine.

This is not to say that there is nothing in this literature that touches on medical knowledge in a critical way. But what there is is criticism of, primarily, the uses to which medical knowledge is put - Illich (1975) being a prime example. There are also critiques in terms of its partiality, in the sense of its ideological assumptions and outcomes or in its denial of 'the whole person' as the relevant subject for its treatments.

A paper by Balogh (1981) weaves these criticisms together to argue that physicians regard medical knowledge as their own "private property", an ownership which entails the class subordination of the patient and the alienation of her/his self from the body that is medicine's sole object. It is the ownership of knowledge that is questioned, not its constitution as 'medical knowledge'.

There has also been a recent move towards the reconceptual-
isation of the discipline as the sociology of health and illness which, it is argued, will allow for the whole continuum to be studied. This could be seen also as a critique of medical knowledge in that it is a plea for 'holisticism', but the omission of the word 'disease' from this reformulation - reserved, as will be discussed below, for the physical conditions identified by medicine - is highly significant. By virtue of this re-characterisation of the interests of the discipline, new political issues (for example the relation of employment to health - cf. Brennan & Lancashire 1978; Harrington 1978; Townsend & Davidson 1980) can be addressed. The work on de-medicalisation of certain phenomena such as pregnancy and childbirth also come under this rubric, the argument being that these are 'natural' functions rather than pathological conditions and, as such, not the exclusive domain of medicine (Hall et al 1980; Oakley 1980; also Crawford 1980; Phillips & Rakusen 1978).

The research which has been looked at so far is, it has been suggested, interested in the way people who are ill make sense of their conditions only insofar as they present a problem for the practice of medicine. As the almost exclusive use of the term 'patient' indicates, it is interested in the perspectives of lay people only to the extent that they are patients: ie the objects of medical practice. The theory and practice of medicine, and of medical knowledge itself as a branch of science, are taken as given; a firm demarcation is thus able to be sustained between 'lay' and 'expert' knowledge - with only the former susceptible to sociological analysis. The marked absence of concern with how they themselves produce their knowledge of others is a concomitant of
the fundamental disinterest in matters of sociological theory
and their concentration firmly on its acceptance and application.
That people's sensemaking about illness may illuminate more general
questions is a possibility that does not appear to have arisen.

There are however two other strands within medical sociology,
broadly conceived, that purport a concern with knowledge about
medicine; the first from a sociology of knowledge point of view
which, often drawing on anthropology, looks at the patients'
perspective in terms of cultural variation; and the second, from that
of social constructivism, which examines the knowledge of the
professional discipline of medicine.

1.2 THE RELATIVISING OF MEANING

The thrust of medical anthropology seems to be the demonstration that,
far from being a universal system of practice and meaning, that of
the west is only one amongst many possible accounts of the social
organisation of sickness and health. This would seem to allow for
the opening up of the whole question of medical knowledge.

However, by the retention of a distinction between the notions
of 'illness' and of 'disease' - the former being subjective and
experiential and therefore subject to cultural relativism, the
latter being objective and empirical and therefore not so subject -
a universalism at the level of the physiological, that is an objective,
a-temporal, a-cultural biological reality is sustained. It is only
the different meanings attributed to this biological substrate, and the arrangements made by various societies to deal with its pathologies that, accordingly, can and do differ with time and geography (cf. Chapter 3.3).

The medical anthropologist Fabrega (1975) has summed up this distinction most unequivocally in his paper, 'The need for an ethno-medical science' (see also Idler 1979; Last 1981):

Terms such as 'diabetes', 'rheumatoid arthritis' or 'multiple sclerosis' seem deceptively simple. Careful analysis will disclose that they represent a complex set of physiological, chemical and structural facts. Furthermore, such diseases can implicate a host of social and psychological factors although, in a strict sense, they are not seen as necessary features of the disease.

Fabrega 1975: 960

The cultural variation in "what is considered as a biomedical disease" is accounted for in a lengthy footnote:

When viewed anatomically, biochemically, and physiologically, the human body is often seen as 'common' to Homo sapiens. This seems to imply that any disease which is framed in terms of the body's systems can be found in any social group and will possess a singular form and course. In fact, biomedical diseases are built out of negative deviations in the values of observed variables, and which of these deviate in a group, and in what fashion, reflect physical, social and cultural facts. Such diseases and their 'natural histories' are thus not invariable or universal. In a proximal sense, the functioning of the apparatus is affected by such things as climate, altitude, level of physical activity, and items which are ingested and serve as food and water. Many of the effects of these factors are readily affected by cultural influences. The genetic constitution of a population, which in a distal sense affects the 'constitution' of group members, also reflect social and cultural influences. Finally, the actual cutoff points which are used to mark deviations in physiologic variables should in principle reflect native conventions about well-being, health and adaptation. Otherwise the researcher runs the risk of applying his own standards indiscriminately, forgetting that they have been generated in quite a different social context. What is considered as a biomedical disease in a native group, then, to some extent will and should reflect attributes of the group considered as a social and cultural structure. (my emphasis)

1979: 975
But this complex account of variation in the 'human body', due directly or indirectly to "cultural" and "social" influences, maintains the non-variation of "the apparatus" - the "physical facts" - one of which is disease. Whilst diseases may be differently manifested and understood, nevertheless, as his discussion of the concept of 'ethnomedicine' makes clear, for Fabrega, "disease" exists as a universal substrate:

In focusing on fundamental properties of disease ethnomedicine can...help to clarify the effects and meanings of disease and thereby make its control more rational. (my emphases)

1979: 974

Furthermore, as the quotation suggests, not only the substance of western medical knowledge - the biological bottom line - but the epistemology (theory and concepts) upon which it is based are taken as unquestionable. The implication is that the production of knowledge of the "physical facts" is not itself subject to "cultural" or "social" influences. By his reference to the need for a more rational control of disease, the further implication is that the understandings of other societies is not only variant but irrational.

This sanctity of the substance and the method which produces western scientific medical knowledge of disease is maintained even by those researchers whose explicit theoretical paradigm is the sociology of knowledge. Idler, for example, in an article entitled 'Definitions of health and illness and medical sociology' (1979) states (in a way which echoes 'Mannheim's mistake' as identified by Bloor(1973) in the sociology of science): 2
Biomedical disease presents no data for sociological analysis, it reveals no social facts. Illness, the human experience of disease, is an explicitly social phenomenon with both an objective and a subjective reality. (my emphasis)

Idler 1979: 732

At the end of the article she, too, considers the objectives of her kind of approach to health and illness, explicitly placing the role of lay knowledge as a complement to and not in any way a challenge to, medical knowledge:

The Schutz and Douglas frameworks allow researchers to accept human health and illness in their own context and on their own terms, and they can reveal the subjective meaning of the experience of illness and healing and then recognise the knowledge of illness and healing as an important stock of knowledge that people have constructed for themselves from their cultures, histories, traditions and personal experience... (But most important of all, perhaps, is the fact that these frameworks allow researchers to organize and classify cross-cultural material in ways which may enrich the healing practices of both modern and traditional societies,

1979: 730

This same distinction between disease and illness, between fact and meaning, is maintained by Eisenberg who, like Idler, draws on anthropology in order to analyse 'Distinctions between professional and popular ideas of sickness' (1977): "(T)o state it flatly, patients suffer 'illnesses'; physicians diagnose and treat 'diseases'." In a revealing example drawn from the realm of 'psychiatric disorders', Eisenberg accepts that Charcot's patients (NB: Charcot is frequently attributed with the discovery of MS and the production of its first definitive symptomatology (see Chapter Two)) learnt to display what were to become accepted as the 'classic' symptoms of hysteria. But, he adds:
Let it be clear; the patients were ill before they saw Charcot: what changed was the patterning of symptoms; in a way, doctor and patient shared a folie-à-deux. He expected what they produced; they came to produce what he expected. It was the evolution of the clinical syndrome that had been altered, not its initiating pathogenesis.

Eisenberg 1977: 12

This observation of what, it could be argued, was an instance of the reflexive relation between document and underlying reality is here provided merely as further evidence of the presence of extra-biological components of disease. As Eisenberg explains:

(M)odels [of disease] are ways of constructing reality, ways of imposing meaning on the chaos of the phenomenal world. This is not to deny the independent reality of that world, but to emphasise that it does not present itself organised in the ways we come to view it. (my emphasis)

Eisenberg 1977: 18

Eisenberg, like Fabrega cited earlier, argues that medicine should incorporate into its practice the fact that diseases are not simply technical, physical pathologies, but also social/cultural phenomena. It is the mechanistic scientific paradigm of modern medicine that is to blame, according to Eisenberg, for disguising this important fact. As he concludes:

(O)nce medicine is reconceptualised as a disruption in an ongoing biological process, we will be less likely to pursue disease as a thing-in-itself.

Eisenberg 1977: 21

The argument is reformist in character: the patients' understandings of their condition are regarded as an essential complement to the identification and treatment of disease, their cultural variation relating only to the social conditions in which illness occurs and the meanings attributed to disease.
Thus, in spite of what might have been expected, the sociology of knowledge perspective on medicine, like all the works cited in the first two sections of this chapter, fails in practice to look at medical knowledge at all. Rather, medical knowledge is the ground upon which they stand to look at others.

1.3 SOCIAL CONSTRUCTIVISM AND MEDICAL KNOWLEDGE

From its title, Wright and Treacher's (1982) *The Problem of Medical Knowledge: Examining the social construction of medicine* would seem to be addressing this very topic. In a sense of course, that 'everything is socially constructed' is a socio-logical truism. How far this refers to a specific, coherent and discrete sociological position; quite what is meant by constructivism; and what it is taken to imply in terms of its concomitants for the analyst's own epistemology and practice are, as this edited collection both admits and demonstrates, as debateable with respect to medicine as it is to other areas of sociology.

In their Introduction, Wright and Treacher allow that the social constructivist perspective as far as medical sociology is concerned contains a variety of not always compatible themes and assumptions. Those whose work they have collected under that rubric are united, Wright and Treacher claim, by their common questioning of four assumptions, central to traditional historical and sociological approaches to medicine, viz:
1. That the identification of medicine or medical knowledge posed no difficulties; indeed it was self-evident...

2. (T)hat modern medical knowledge was distinctive because it was characterised by two particular features: it was built upon the findings of medical science, and it was effective...

3. (D)iseases, it was assumed, were natural objects which existed prior to and independent of their isolation or designation by doctors...

4. (S)ocial forces, whether much attention was given to them or not, were assumed to be self-evidently distinct from medicine.

Wright & Treacher 1982: 1-5

It would appear that much of the foregoing critique in the previous two sections concerning medical sociological research on patients' perspectives itself shares the constructivist position. However, it is this appearance of similarity that makes social constructivism so useful here as a way of delineating the approach I am in fact trying to take.

In addition to the shared questioning of traditional work, Wright and Treacher discern a unifying and distinctive theme amongst the contributions to their book: that is, a refusal:

(T)o regard medicine and technical knowledge as pre-given entities, separate from all other human activities. Instead, it is argued, medicine is to be seen as a highly specialised domain of social practice and discourse, the limits and contents of which are themselves set up by wider - but not separate - social practices.

Wright & Treacher 1982: 10

What is undertaken, then, is a search for those "social practices" which have "set up" the "limits and contents" of medical knowledge and practice. The social practices and discourses identified by the
authors of the various papers to be involved in the 'setting up'
of medicine may be summarised as: power, ideology, technical
developments, cultural change, social interests, and social relations
- acting alone or in combination.

It is difficult to ascertain, however, the precise role being
claimed for these 'social forces', whether they are intended to be
explanatory - causal in either a direct sense or in terms of necessary
and sufficient conditions for x to be as it is - or whether the claim
is weaker. According to Wright and Treacher, the latter is the case:

What is being proposed is not that medicine is unscientific
because it is permeated by social forces; but, in contrast,
that both medicine and science are essentially social
enterprises. (my emphasis)

Wright & Treacher 1982: 7

Precisely which phenomena the term 'medicine' is taken to encapsulate
is of course critical. The description of "both medicine and science"
as "social enterprises" suggests that what is open to sociological
analyses are only the persons, practices and institutions of medicine.
The phrase "permeated by" suggests that there is an a priori
independent phenomenon (of whichever conceptual type) which is in-
filtrated by social forces which pervert it into becoming something
it is not in itself. This suggests that there are, underneath the
patina of social forces, phenomena that are real, independent of
knowledge of them. The 'social forces' identified in the collection
seem to be operating on medicine, pushing it one way or the other
(and interestingly these forces are invariably characterised as
negative in the moral or evaluative sense). The modern western
account of the anatomy and physiology of the body and the phenomena
of disease remain inviolable. The body is the body is the body. Atemporal and universal.

The further relevance of the collection to this thesis has to do with the methodology used by social constructivists; in particular, their use of history as a way of making strange (cf. Chapter Three) Wright and Treacher account for the predominantly historical content of the collected papers in these terms:

(U)sing historical data is not an escape from the present but a way into it. The apparent strangeness and distance of the past enable one to discern features that are camouflaged in the present by the very taken for grantedness of everyday experience. We use the past to shake confidence in the 'obvious' appearance of medicine today, not in order to sanctify it as has so often happened in histories of medicine.

Wright & Treacher 1982: 2

The difference Wright and Treacher claim for social constructivism from the traditional histories of medicine is that previously, change was attributed predominantly to the activities of individuals - the genuises who made great discoveries. In addition, they explicitly reject what they call such "positivist" Whig histories according to which 'medicine now' is portrayed as the summit or at least the high ground of true knowledge, the culmination of centuries of onward and upward progress. Their own contention is that by a knowledge of past variations, paradigm shifts in medical theory and so on, present obviousness loses its solidity. Because medical knowledge had been different in the past; if - unless it is assumed that past notions of reasonableness were discontinuous with ours - that knowledge was once otherwise and sensibly so for people in
that past, then 'something' must be acting on the world of the physical - producing these differences.

The object (of medical knowledge) may appear to shift according to changes in theories, but this is because the point of view (literally) of the observer is held to shift vis à vis the object, each of whom thus sees a different aspect of the same thing, that 'seeing' a result of their location in history. If this something that changes is not a change in the physical (because that does not change), then it must be social in some sense. It is thus history which, according to Wright and Treacher, teaches us that the social world is in continual flux.

Whilst in one sense this is to challenge history, nevertheless it is to use historical data and, concomitantly, to regard the practice of history as itself a form of knowledge which is a-theoretical and therefore immune from those same social forces that they claim inexorably effect medicine as a theoretical practice. According to the theory of history which Wright and Treacher (and the authors in their collection) employ, 'the past' exists independent of analysis. For the method depends on looking at 'what actually happened' in the past.

Their study of the past appears to bring to analytic visibility the decisive and particular social forces which constructed medicine today (because one can now see what happened in the end; for example which theory of asthma won out (Gabbay 1982)), it is possible, they assume, to trace the story to which 'now' is the conclusion; to see, by that conclusion, out of the "melee of conflicting social forces" (11) acting in the past and in the present, the
structure and features of the plot which lead (was always leading) to things as they are now. It does not seem, at least in this collection, that social constructivism leads to any serious doubts about the independence of the analyses they themselves produce from the effects of 'the same' social forces.

Social constructivism in medical sociology, as in much of the sociology of science, falls short of reflexivity: reflexivity, that is, as conceived as the problematic in the social studies of scientific knowledge. 4

Wright and Treacher themselves use this term in reference to Gabbay's paper in their book. According to my reading, Gabbay uses the substantive topic of theories of asthma for an examination of the theoretical problematics entailed in the demonstration of the social construction of medical knowledge. His paper takes the form of proceeding to consider a number of possible constructivist approaches to the question in turn: the diachronic (theories of asthma over long periods of time; synchronic (theories of asthma held at the same period); and the single treatise on asthma. He employs each of these approaches considering them in terms of (i) their resolution of contrived paradoxes; (ii) the homologies between domains of thought (for example between concurrent political/medical theories); and (iii) in terms of the biographical approach (that is the life of a proponent of a particular theory of asthma). But, as he demonstrates, each approach, whilst offering "insights" into the question of theories of asthma, entails assumptions about that which it set out to examine. Having worked through these constructivist
approaches, as he puts it: "Our attempt at proof has, in short, left us in a position of historical paralysis." (1982: 42). Insofar as it is not only medical knowledge about asthma that is the product of and that produces "the social world", this "(mutatis mutandis)" also applies to historical knowledge. Whilst there appeared to be two choices offered as means to escape "paralysis", there is therefore, Gabbay argues, in fact no choice "but to act on the insights which have emerged, and develop the analysis further." (1982: 42). This, however, is the final sentence of his article, and there is no suggestion as to how this might be done. 5

The aporia which his demonstration has produced is, however, recharacterised by Wright and Treacher in such a way as to defuse the epistemological menace of its implicit reflexivity (a term which they, but not Gabbay himself, use). According to Wright and Treacher's paraphrase, Gabbay's argument is that:

It is more justifiable and more fruitful to try to relate past medical knowledge to the whole range of social circumstances in which it was generated and used. This however, he [Gabbay] insists is no simple or straightforward task for, as in medicine, so in historical analysis, the undertaking is rooted in our own social circumstances. We only understand such processes better if we develop a reflexive form of historical method not founded on scientistic notions of causal proof.

1982: 15

Apart from other indications, their use of the term "scientistic" allows for the untainted existence of 'scientific': an opposition which, I would argue, marks their non-recognition of the serious consequentiality of Gabbay's paper. Defused in this way, Gabbay's paper is made to merge with the rest of the collection and Wright and Treacher can continue unperturbed with their depiction of the
concrete and constructive purposes of social constructivism:

The search for a timeless medicine unaffected by its social contexts is, in our view, as misconceived and as misleading as the quest for the Philosopher's Stone. The way forward to forms of medicine which better serve the ends which their clients require of them is not, we would contend, through such a search, but through recognising the inherently social nature of medicine and through developing its structures, institutions and relationships in which forms of medicine that are considered desirable are most likely to grow. (my emphasis)

Wright & Treacher 1982: 15

As this quotation makes clear, the relation of medical knowledge to its object remains as given; it is not medical knowledge, despite the title of the collection, but the practices and institutions of medicine that require sociological analysis. Similarly, with the exception of Gabbay, the means by which social constructivists produce their own knowledge remains, for them, autonomous. For them, the term "reflexive analysis" operates as an antonym for 'scientism': a kind of sensitive self-reflection.

A Critique of Social Constructivism

The critique developed by Bury of social constructivism (1984) - upon which all of the works cited so far could be seen more or less explicitly to be premised - nicely encapsulates a common view of reflexivity: and the grounds upon which it is rejected. An examination of Bury's critique thus provides a timely reminder of the difference and difficulty of the path I am attempting to pursue in this thesis - that which might come after Gabbay's last sentence.

Whilst I share some of Bury's criticisms of social constructivism,
I differ fundamentally from the conclusions he draws about its value as a theoretical position.

Bury concedes that social constructivism is interesting in that it brings "normal, physical" medicine under the gaze of the sociological perspective (previously only done in regard to mental illness); and in that it regards it as inherently problematic. According to Bury, social constructivism challenges the notion of "disease" as an autonomous entity, arguing that medical knowledge is "deeply implicated" in social relations. It has gone beyond what he dubs as the "crude views" of medicine-as-sinister to develop certain marxist analyses in a more sophisticated form (citing Figlio 1982 as an example). Social constructivism is of interest because in addition it is "strongly counter-intuitive"; it makes the "mundane enigmatic"; and in any case it is influential (and here he cited Ian Kennedy's 1980 Reith Lectures 'The Unmasking of Medicine' and the work of Sontag (1979)).

However, in spite of these virtues, for Bury social constructivism remains deeply unsatisfactory. For, he argues, few of its practitioners have even recognised let alone attempted to tackle the fact that their work falls into the problem of "all such relativist approaches"; that is, that it is inevitably self-undercutting. If, as under its own logic it should, "scientific method becomes a myth", then all that remains possible for an analyst is a "radical relativism", a position from within which - as, according to Bury, everybody knows - anything and therefore nothing can be said with certainty.

This failure of recognition is made possible, Bury explains,
by social constructivists' simplistic misuse of their own claimed theoretical sources. They invoke Foucault for example yet completely ignore the strong influence of the nihilistic philosophy epitomised by the influence of Nietzsche evident in Foucault's later work. They invoke Kuhn as if he was, like them, "anti-science" whereas in fact, according to Bury, Kuhn was only against the "liberal-rational myth" of science, and indeed, quite on the contrary, "celebrated" science for its emphasis on notions of community, consensus and authority as its authenticating values. Furthermore Bury argues to embrace the constructivist perspective is to do in effect what is, ironically, conservative sociology, which "merely records" the status quo.

Even the work of Foucault itself, Bury remarks, contains an "ideological elusiveness" and not, as constructivists assume, the "clear radical commitment" which is his popular reputation.

Bury is equally sceptical of the emphasis in social constructivism on the ideological power of medicine. He surmises that research into lay knowledge would reveal that far from being the passive subjects constructivists presume, individuals are in fact far from being dominated or cowed by the single all-powerful paradigm of scientific medical ideology.

Bury also attacks social constructivism at the level of its outcomes. As with all perspectives derived from the sociology of knowledge, in its attempts to produce analysis at a generalised and universal level, social constructivism perforce places sociology at the "margins of social debate." By which he means that it quite
excludes the possibility of medical sociology's ever engaging in the collaborative work with other disciplines that is, Bury claims, essential for good and influential work (and thus sociological work at all).

This debate and the specific points that are attacked by Bury parallel some of those made within the sociology of science by critics of the social constructivism of researchers such as Collins (eg 1981) who are, as discussed in the Introduction, one of the formative influences for the beginning of this thesis.

Wright and Treacher themselves acknowledged such parallels:

> From the late 19th century until recently, medical knowledge lay behind a seemingly unbreachable conceptual barrier.

Wright & Treacher 1982: 1

In a footnote to this they added:

> So, for that matter, has the study of scientific knowledge. There are many parallels between the history/sociology of science and the history/sociology of medicine. In social studies of science, studies of scientific knowledge were extremely rare. Over the last ten years or so however, a body of writing has developed which devotes far more attention to the structure of knowledge.

Wright & Treacher 1982: 198

To be noted, however, is their reference to the "structure of knowledge" as a topic amenable to sociological analysis - yet at the same time, as has been argued in the previous section, the paradoxical immunity of biological/scientific fact from their critique. It is the legitimation of one theory and the de-legitimation of others that they see as subject to social forces: but the constitution of theory, how the knowledge is itself produced from 'evidence' that they fail to regard as amenable to constructivism's critique.
The crucial point as far as my own research is concerned is Bury's rejection of the 'relativism' of social constructivism on the grounds that it leads to 'radical relativism'. The grounds upon which this rejection is based are explicitly practical/strategic. That is, 'radical relativism' is banished not because Bury argues that there is anything inherently incorrect in it but in order that sociology may be continued.

This move, it seems to me, contradicts his own adherence to the canons/possibility of research; for it is, by his own account, the pursuit of knowledge of "normal physical medicine", by traditional sociological methods, that has inexorably produced the relativism of social constructivist theory (if not its practice). And thus, to follow the same logic on, it cannot be rejected on the grounds simply of its unwelcome implications.

What Bury has found and rejected in social constructivism is the problematic of reflexivity; paradoxically found through his critique of the failures of social constructivists to pursue the implications of their work to the bitter end.

***

One of the major factors enabling the problem of reflexivity to simply not arise is, as I have been arguing, the acceptance both by traditional medical sociology and social constructivism of the mundane facticity of science - both in its method and its theoretical basis as an objective means by which to discover and describe the world.

Part of that 'world' which medical science discovers and describes is the patient in terms of her physiology and diseases;
medical sociology discovers and describes the patients' perspectives. Both objects of research are deemed to have the status of objective fact. It is because science is regarded as simply factual that the patients' perspective, lay knowledge, is by definition subordinate to scientific medical knowledge and practice.

In the chapter which follows I turn to the medical research literature on MS - in order to substantiate, through the analysis of a single text, what has been implied throughout this one: that the assumption that medical knowledge arises simply from the nature of disease is not tenable. The 'nature of the disease' is constituted in reflexive relationship with the theorising practices brought to its study.
CHAPTER TWO
LAY/EXPERT KNOWLEDGE OF MULTIPLE SCLEROSIS*

THE CASE OF THE COMTE D'ESTÉ

In the previous chapter, the resort by social constructivism to history as one means of 'making strange' was opposed by the assertion that history is not merely a matter of letting documents of the past (or of the present) speak for themselves. The objective of this chapter is to substantiate that assertion, through the close reading of one such document, an entry in the diary of Augustus, Comte D'Este, dated 1822 (See Figure 2.1 overleaf).

The extract appears in McAlpine et al's Multiple Sclerosis: A Reappraisal (1972), one of the most frequently cited texts on MS (in 89 papers in 1982, according to the Science Citation Index). As shown in Figure 2.1, the Comte's diary is described by McAlpine as: "(T)he earliest description of multiple sclerosis" (1972: 112). However, since the Comte does not describe what is wrong with him as MS - the first (expert) naming and description of the disease not appearing until some thirty years later - in order to read the diary as a case of MS, it is necessary to be armed with substantial 'already knowns' about MS: to be an expert reader.

*For a summary of the non-medical literature on MS, see Appendix 2.1
The earliest description of multiple sclerosis is contained in a small diary bearing the title, the Case of Augustus d'Este, now in the library of the Royal College of Physicians in London, from which extracts were published by Firth in 1948. At the age of twenty-two, the first entry occurs:

In the month of December 1822 I travelled from Ramsgate to the Highlands of Scotland for the purpose of passing some days with a Relation for whom I had the affection of a son. On arrival I found him dead. I attended his funeral: there were many persons present and I struggled violently not to weep: I was however unable to prevent myself from doing so. Shortly after the funeral I was obliged to have my letters read to me, and their answers written for me, as my eyes were so attacked that when fixed upon minute objects indistinctness of vision was the consequence. Until I attempted to read, or to cut my pen, I was not aware of my eyes being in the least attacked. Soon after I went to Ireland, and without anything having been done to my eyes, they completely recovered their strength and distinctness of vision.

Subsequent entries in the diary leave no doubt about the diagnosis.

McAlpine 1972: 112
That the reading of a text requires what Barthes (1975) has called 'already knowledge' is not a privileged secret amongst cogniscenti. It is common knowledge, upon which McAlpine himself relies and presumes in other readers:

Doubtless owing to difficulties in evolving valid methods of assessing the effect of emotional factors on the onset and cause of MS, recent data is not available. Relevant material from our first book and quotations from case histories [of which the Comte's diary is one being discussed] while meaningful to those with experience of this ubiquitous disease, will for others merely underline the need for further studies. (my emphasis)

McAlpine 1972: 111

For McAlpine, the meaning of a case history depends on its reader being able to bring to the text a body of experience. By implication, the meaning of the text is there waiting to be discovered. It is in the light of this reading through experience that the diary can be understood as being...a case of MS; by drawing on that prior knowledge, this case history can be seen as having been a contribution, pathing towards that - same - knowledge of MS (Smith 1978).

In what follows, I will go through the extract quoted by McAlpine step-by-step (Barthes 1975), bringing out (bringing in) those features which enable me to see it as relevant to certain themes current in MS research; pointing out some of the significances I can now see lying behind the surface of the diary's text.

The point of this chapter is to demonstrate that medical knowledge is not simply a matter of the discovery of natural facts but is produced - and re-produced - by theorising practices; practices which, furthermore, operate on and engulf 'lay accounts'
in such a way as to render them precursors or 'merely documents' for medical knowledge. MS is itself constructed out of such artefacts as the Comte's diary.

But let me be clear. The contention in this chapter is not that people do not suffer, nor that neurologists, epidemiologists and clinicians are fools. It is that the object of research – in this case MS – is constituted in theorising practices as the thing it is held to be; an object which then has an indeterminable relation to what 'is': an object which is constituted in its particularities by the knowledge brought to bear upon documents taken to be 'of' MS (cf. Latour & Woolgar 1979).

***

"The earliest description of multiple sclerosis..." (line 1)

A description is always 'of' something: the concept of description entails an object: the object here is multiple sclerosis, a name for a disease, which is to say, a disease with a name. That is, a disease is taken to be an object with the capacity to be described. Even if current descriptions are in detail incomplete, as is the case with MS – see Kurtzke 1980a² – there are at least conceptualisable if not yet discovered boundaries between 'it' and everything else. The enterprise of research rests on the assumption that MS is a describable, discrete entity in some way, existing independent
of description: an assumption that is never questioned. Although some current researchers are exploring the notion that the term MS as hitherto used may have mistakenly embraced more than one disease (this being a possible explanation of the wide variety of symptoms, manifestations and prognoses currently and somewhat awkwardly having to be included under this name), that there is something to be properly called MS is not seen thereby to be challenged. The question merely concerns correcting or refining that which should and should not be included in the category, not whether MS is there waiting to be dis-cover ed by research (Stazio et al 1964; see also Bauer 1978; Broman et al 1981).

The concept of disease is consequential for knowledge about it: if there is a something (because it may be described), then it is not the same as, coterminous with, a normal body but is something in addition. This opens the way to particular hypotheses. 'It' may invade the body (cf. theories of MS as an infectious disease, or as an exogenous virus entering vulnerable bodies (Isager et al 1980; Poskanzer et al 1980a)); or be 'created' by the body as a reaction, for example by the body's normally protective auto-immune system turning against itself (Poskanzer et al 1980b). Certain effects are noted (as in the Comte's diary), taken to be the effects of something, and that something, the particular effects taken together and thus differentiated in that association from other diseases (with other groupings of effects) that are known to exist, constitute MS. How could you, it could be objected, seek for a description of something that did not exist, underly the documents which, as the Comte's diary bears witness, may be described? The
reflexivity inherent in this relation of document to underlying 'reality' (Woolgar 1981) is rendered invisible by the technical level at which the problem of what MS is are addressed, which is in turn entailed by the operative concept of disease. The task of research, then, is to identify the object: MS in its uniqueness.

The nosology of diseases rests - at least in principle - on the possibility of accurate description. Once this accuracy is deemed to have been achieved, then not only what is, but also maybe why and how it is will be illuminated. How then may something unknown be described?

One way to begin is on the basis of what is already known, by distinguishing it from other things or by drawing analogies with something similar in effect. MS is seen as being both like and unlike other diseases:

The symmetrical relationship between incidence, prevalence and age in MS is not shared by other such obscure diseases as rheumatoid arthritis which is said to be due to an auto-immune process...or ulcerative colitis.

McAlpine et al : 1972

Thus diseases which are currently described (and obscure?) are used as models for possible revelations about MS itself (cf. also Poskanzer et al 1963). What is unknown is sought through what is known, on the basis of assumed similarities and differences between critical features, like finding the size of an angle in a triangle given knowledge of two of them. However, as far as the example cited by McAlpine et al quoted above is concerned, as will be discussed below, none of the three 'angles' - incidence, prevalence
and age, and neither, therefore, the relationship between them - is known with any degree of certainty in the case of MS. Nevertheless, the ordering of knowledge, that assumes diseases conform to universal logical patterns provides for, links to, is possible to deduce from, what is not yet known. This ordering is regarded as a task in principal completeable.

A disease, MS, is taken not only to be a discrete, describeable phenomenon, but, although unnatural in the sense that it is conceived of as a pathology of the normal, it is assumed nonetheless to conform with one characteristic of the normal-natural: it will have a 'natural history', both as a disease and in the body of an individual, that may be traced.

The Comte's diary is cited by McAlpine in his chapter 'The Natural History of MS', a history that has a beginning. McAlpine says the diary is "the earliest" description of the disease; thus that there is a sequential story with a beginning (and therefore implicitly one which will have an end) of its discovery. But the beginning of MS is not unproblematic. 'The beginning' is identified by the date as 1822. There are however other versions of the "earliest description" of MS (and see Appendix 2.2).

Previous to the nineteenth century, MS was unknown. Knowledge concerning this disease began in development of pathology and it was an English student, Carswell, who first described plaques of MS in the human spinal cord, and Cruveillher, a French Neuropathologist, wrote the first important treatise to show the morphology. The clinical picture, recognition of the symptoms and the course of the disease emerged gradually within the next thirty or forty years, culminating in the classical descriptions of the French neurologist Charcot.

Bauer 1978: 3
which places Carswell in around the 1830's, Charcot's classical works being published in the late 1860's and 1870's (Charcot 1868; 1872; 1877).

McAlpine refers to the diary as a first description, noticeably not as 'the discovery'. This first description of MS comes from an account by a lay person, the Comte D'Esté; it is not the discovery. He did not discover MS, only experienced it. This point will be elaborated below (cf. also Chapter Four).

A 'natural history' though, it seems, can only begin with a written record: what is not written, preserved, cannot become history. Bauer declares in his account of its origins that "previous to the nineteenth century, MS was unknown" (my emphasis); but perhaps this is no more than saying it might well have existed but we (and this 'we' is not everyone) did not know about it. That is, its existence prior to the record does not count as, cannot count as, historical data. So 'the beginning' cannot be the beginning. Natural history begins, rather, with a particular kind of knowledge: the diary thus becomes an example of a pre-historical period, a kind of precursor to history proper. Let me take the next step, following the text.

"...is contained in a small diary..." (1-2)

The source of this "first description" is a "small diary", the private record of a non-doctor. By reading this phrase in conjunction with the next: "...now in the library of the Royal College
of Physicians in London" (3-4) and McAlpine's remark: "subsequent entries in the diary leave no doubt about the diagnosis" (22), we have an example of how lay knowledge - as was described in the previous chapter - is incorporated into the medical account in such a way as to deny it significance in its own right, a significance that might perhaps challenge the assumptions upon which the medical model is premised. It is by being included in the archives of the Royal College of Physicians that the Comte's amateur description of his condition has become source material, raw data, to be recreated by experts (physicians, McAlpine) as the first documented case of MS. It is this recreation that 'confirms' that D'Este's description was the product of ignorance, by its very difference from the physician's account. Thus, lay knowledge of disease, the Comte's selection and depiction of events in his body which seemed to him to be significant, are allowed for in expert knowledge: but it is by the admission of the diary into the library of the RCP that it becomes data, and only by the explication of its 'real' medical significance by those qualified to do diagnosis in retrospect (expert readers of the diary) that elevates it above mere subjective reminiscence. The expert readers are those, of course, who can recognise MS in the Comte's account.

"...a small diary..." (2)

One fundamental problem that concerns epidemiologists in respect of their research into MS is the difficulty of accurate data collection:
specifically relevant here is the oft-stated need for prospective rather than retrospective data, in order to identify accurately the point of onset of the disease in a patient (the beginning of the natural history of the disease in the individual). For what usually happens is that by the time a patient presents herself to a doctor, MS, it is assumed, will have been present in the body for some time. Given, however, that only one person in 20,000 it is said is likely to develop MS in any one year, even in those areas of the world where it is most prevalent (McAlpine et al 1972: 3), it is held to be impracticable to carry out prospective longitudinal studies on populations, the only form of study which would allow for the very earliest signs to be recorded before they were recognised to have been MS (see Kurtzke et al 1979).  

A diary is by definition written in the present rather than retrospectively (although the verb tenses plus the several weeks' events referred to in this single extract from the diary raises doubts about just how closely it was written to the events it records) - and thus could not have been influenced by what will later be known to have had significance. In that the Comte seems not to have known what was really the matter and, at the time of writing, (according to this extract at least), gives no sign of understanding his recovery to be temporary, his record of symptoms can thus be taken less problematically as recording the accurate point of onset of MS.

One could add however that this problem is not merely technical: for once named, MS is available to operate - like a prefatory statement - providing for the re-interpretation of not only the
present but the past as leading up to the present, the diagnosis of MS. Thus the identification of occurrences in that past as occurrences signifying the MS to come is not a matter of mere description but of reconstruction. This, one could also add, is not a problem confined to events in the past, but also to the accounts of those in 'the present'.

"...bearing the title, 'The Case of Augustus D'Este..." (2-3)

History depends on documents in another sense. One may speculate that the diary of a less well-connected person may not have found its way into the library of the Royal College of Physicians (Field mentions that Sir (sic) Augustus D'Este was in fact the illegitimate son of one of the sons of George III and an English lady he met in Göttingen (Field 1978: 31)), and consider the factors involved in any one piece of evidence being preserved qua evidence, or even ever being noticed at all; and of course the consequences for knowledge of what will thus always be indeterminantly absent from any collection of evidence.

The diary is described as a "case". It is noticeable that of the earliest published research papers on MS - as recorded in the Index Medicus, first published in 1877 - the case history was frequently used as the basis for medical research articles on MS. In 1879, 11 out of 25 articles (44%); in 1889 12 out of 21 (46%) and in 1899, 18 out of 42 (43%). (This may be an underestimate if some papers are based on cases but do not specifically say so in their titles).
By comparison with the titles of current research papers in the Index, it is clear that the case history has fallen into disuse as a research tool. The most recent articles on MS indexed under 'case histories' in the Index were in 1959 when there were 3 out of 105 (3%), although it remains in use at the anecdotal level (and is used in McAlpine et al in this way) and, of course as a basic tool for diagnosis.

This movement away from the case history is not without its critics. As Bauer remarked, in his talk to the Annual General Meeting of the Charity ARMS (Action for Research into Multiple Sclerosis) in 1978, in discussing the slow progress of MS research:

> [If you] sum up the amount of money that has been spent on animal models, on basic questions, on things that have been tagged with MS more or less optionally to count as basic research, I think that the amount that has been spent on MS proper has been very low. I made a rough calculation a few years ago, and found that less than twenty percent was spent on observations in patients. This is one of the important areas requiring expansion.

Bauer 1978: 6

For Bauer, research into "MS proper" means looking at patients. This involves, however, "observations" of them, as objects rather than participants in research. The clinician's 'case history' is elicited from a patient; unelicited cases, like that of the Comte, are not used for MS research.

One should note that, although not alluded to by McAlpine, Firth's original article includes a remarkably overt example of evaluative interpretation by a doctor of a patient's case history. Quoting from the final passages of the Comte's diary, Firth, himself an MD, comments:
Whatever [his] end, it can hardly be mourned, as the last picture [in his diary] is that of an afflicted man, prematurely aged, bald, becoming deaf, with a spasticity of such severity that locomotion was only possible with the aid of the shoulder of his servant, or his chair on wheels.

Firth 1948: 22

Whilst I am not suggesting that such a 'better dead' view of disability would be held today, nevertheless Firth's gloss on the case of the Comte highlights the fact that no patient's account can be interpreted, as it were, neutrally. Doctors, too, have to 'make sense' of the 'facts' of history.

"...now in the library of the Royal College of Physicians..." (3-4)

I have already commented on this elevation of lay knowledge to the status of 'raw data'. The statement that the diary is now deposited in this library may also be considered in the light of the fact that each discipline has its own special archive, consisting of particular documents picked out from all possible ones as especially being about, related to, informative about, the discourse of medicine as a practice, and discipline. They are commonly regarded as records of the history of the discipline; seldom as reflexively constitutive of the discipline itself, or of the knowledge upon which that discipline is grounded.

"...from which extracts were published by Firth in 1948..." (4-5)

That Firth's article is cited by McAlpine in McAlpine et al; that Field, who also cites McAlpine et al also referred to the
Comte; that I am citing their work and their citations, is a small loop in the strikingly incestuous web whereby certain components of 'the literature' on MS, and thus particular items of knowledge rather than others are perpetuated, whilst others never seem to be included in it. It may be one of the ways in which, in practice, that the almost exponentially growing mass (Figure 2.2) is reduced to manageable proportions, access to what counts as

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Figure 2.2: Entries re. 'Multiple Sclerosis' in the Permuterm Index; Science Citation Index 1971-1981

the literature made through citation indices such as the Index Medicus (which I have myself used) and via works already referred to by others (and see Wynne & Robinson 1983).

Whether the incestuousness is more marked in the MS research corpus than in that on other topics is difficult to say. But certainly particularly, although not exclusively, in the epidemiological literature, a mere handful of names are kept in circulation, by both quotation and reference.
A warrant for this repeated re-circulation of citation could be, as Kurtzke has suggested (1980a), that once the reputation of an individual researcher or research team is established, their name may be taken to guarantee a certain standard (conformity with desired standards) of practice, which in turn would guarantee their findings. But whatever the reason, one consequence is that whilst the number of publications increases, the core literature remains confined and, since it remains rooted in the unquestioned authority of a few key texts and authors, research into MS - ironically, given the admitted lack of progress in over a hundred years - appears to be working on the right lines; the lines that will, 'given more time and money', produce the answers to MS.

"..at the age of twenty-two..." (5)

As has been indicated, a problem acknowledged by epidemiologists working in MS is the difficulty of ascertaining the age of onset of MS in any particular person with any degree of accuracy; made more difficult because the knowledge is that there are early, prodromal symptoms which, to those who know, mark the onset of the disease but which may be so ordinary - pins and needles, fleetingly blurred vision (as in the Comte's case), headaches and so on (Kuroiwa 1975) that they would not be remarked sufficiently by a person to cause him to go to a doctor, where they would have been entered into the case notes, become part of that person's case history. The earliest signs have to be recalled retrospectively. Some of the problems arising from this necessity have already
been discussed. Why, however, is it deemed crucial to pin-point onset? And what, therefore, is the consequentiality of the problems of doing so?

According to the fundamental concept of disease, MS 'must' have a beginning, a natural history within the body of the patient. If it was possible to establish onset then the search for the causes of MS would be simplified, for it could be targeted on the period immediately prior to that point.

Yet at the same time, current knowledge of MS is that "the onset of MS [is] in young adulthood" (Ricarp 1981: 1290); this despite the acknowledged current impossibility of marking its beginnings in an individual and despite reports in the literature of its occurrence in children (Gall 1958; Brandt et al 1981 for example). Putting the latter to one side as aberrations, the Comte's age of twenty-two would be typical and, as such, another ground for the diagnosis of his condition as MS.

The age of onset is taken as significant also in terms of the prognosis for an individual, the later the onset the higher the probability of rapid deterioration or degree of severity of the disease (Kurtzke et al 1977; Herndon & Rudick 1983).

The matter of onset is an instance of what in one place in the literature appears an apparently full awareness of the problems of the data upon which the knowledge of MS has had to be based, and yet whose unreliability at another is put into the background, becoming rather the unquestioned basis for another element of the description of MS. 12
"...in the month of December, 1822..." (6)

If this is, as McAlpine avers, the first entry in the Comte's diary, that it was the state of his body at that time that provided the impetus for keeping a record, then December 1822 marked a noticeable event, what was, to him, a non-normal bodily occurrence. Notions of normality-abnormality, bodily health and bodily ill health, something wrong and nothing wrong, all too often go unexamined, as if everyone knows what they mean and how these antitheses mark out the parameters of what is possible.

Pins and needles may be - just pins and needles, or the start of MS. Signs are taken as symptoms of documents of the underlying reality of the condition of the body. The signs are however only significant in retrospect - or a projected retrospect which enables them to be ignored or brought to notice - and against the parameters of the normal - dependent on the meaning attributed to the signs.

Social anthropologists have noted the cultural specificity of meanings of bodily events: the tribe where the men feel the labour pains and the women briefly pause in their field work to give birth and so on. It is not events - the blurring of the vision - but the meaning attributed to them - as not-normal that constitutes them as 'events' in the first place. The identification of December 1822 as being the time of the event which began the Comte's MS, constitutes it as an event.
"...I travelled from Ramsgate to the Highlands of Scotland..." (6-7)

A significant fact for epidemiologists is the geographic distribution of MS, one thesis currently much in favour being that there are three distinct zones, the prevalence of MS differing between them, the higher the prevalence the more distant from the equator (Hyllested & Kurland 1966; Acheson 1972; Kurtzke 1980b; Baum & Rothschild 1981; Fischman 1982). In that the Comte lived in the zone furthest from the equator, where the incidence of MS is currently high (and, presumably, by extrapolation, then), that he would develop MS - and therefore that MS was what he was developing - is more likely than if he had lived in a low incidence zone.

The geographic distribution, taken to be increasingly confirmed as more studies are done (but see 32-33 above), opens the way for a number of causal hypotheses - specifically that environmental/social factors must play a part in the aetiology of the disease, which in turns opens up (determines a concentration on) particular lines of research. Alternatives being pursued are that MS is caused by an endemic virus, climatic or geophysical conditions, social environmental factors, racial specificity (although this theory is out of favour now) (Barlow 1960; Alter 1971; Russell 1971; Kurtzke et al 1979; Taylor et al 1980); or a combination of facilitating and precipitating factors - a genetic disposition, an endemic virus, and a precipitating factor such as 'stress'. The 'geographic distribution' has of course to be ascertained on the basis of counting cases; that, in turn, dependent on the occurrence of
diagnosis (which is not the same as the occurrence of onset) (and see Chapter Four, passim, on the lengthy process of the diagnosis of MS). There are in addition multiple technical problems - non-standardised record-keeping practices, no universally used diagnostic criteria and so on.

"...I struggled violently not to weep: I was however unable to prevent myself from doing so..." (11-12)

McAlpine cites the Comte's diary in his sub-section 'Emotional Stress', thus directing attention towards the record of the violent emotion the Comte vainly endeavoured to suppress at his Relative's graveside. It would be possible, however, to suggest, on the basis of common knowledge, that the physical stress a journey from Kent to Scotland would involve in the winter of 1822 as much as 'emotional' stress may have been the precipitating factor. Either interpretation would be plausible and how could they be adjudicated between? For both are similarly reliant on knowledge brought to rather than arising simply from the diary.

Certainly stress theories of various kinds have a common sense plausibility (and were offered to me in my own research by people with MS as reasons for the onset of their case of MS), and many linking emotion to onset occur in the literature (see Brickner & Simons 1950, and the extensive references cited by McAlpine 1972). But there is no theory which offers a precise explanation of the mechanisms that would be capable of transforming psychological events into physical effects.
The term stress is in any case so general that it would seem almost by definition always applicable - but not, by the same token, explanatory. McAlpine admits the limits of the Diary as a support of the hypothesis linking emotional stress to the onset of MS:

Striking though individual case reports may be, the part played by emotional stress can only be established by prospective studies.

McAlpine 1972: 114

This is however to settle the matter technically, regarding the weakness of case studies as one of numbers (and of course begs the problems inherent in carrying out prospective studies already discussed). It apparently omits any scepticism about either the concept of stress or about the causal relation between the mental and the physical. It is the context of the Comte's reference to his suppressed emotion - as immediately prior, the noticeable event immediately prior to the blurring of his sight - that constitutes the plausibility.

"...shortly after the funeral..." (12-13)

The events, the noticeable blurring of his sight, according to the Comte, occurred "shortly" after what, according to McAlpine, was an emotional trauma. The temporal proximity of one event with another is one way used to connect them causally. There are arguments about this in the literature, but these revolve around how long or short a gap between two events has to be in order to establish their connection; nothing about why this event and that
are 'obviously' connected as opposed to any number of other two or more events (Smith 1978; Woolgar 1981). What is seen to join them perhaps is their shared remarkable: extreme distress and then a strange symptom; an eruption of grief then blurred vision. Bob, one of the interviewees in my research group, said his daughter's MS was caused by a fall from a horse. It emerged only later in the interview that his daughter was now in her late thirties and had only recently had symptoms and been diagnosed with MS. The fall had been when she was about eighteen. This particular claim for a causal relation between two events appears unconvincing. But is it in essence any different than experts' connections? In any case, on what grounds is a short time—"shortly after the funeral"—more likely to indicate causality than a long one?

To read the Comte's case as "striking" evidence for the thesis that emotional stress is a precipitating factor for MS as claimed by McAlpine is to put aside these problems, or, more properly, not to recognise them as problems.

"...I was obliged to have my letters read to me and their answers written for me, as my eyes were so attacked that when I fixed upon minute objects indistinctness of vision was the consequence. Until I attempted to read, or cut my pen, I was not aware of my eyes being in the least attacked..." (13-18

According to the literature, optic neuritis or astygamous is frequently an initial indication of the onset of MS (Feasby & Ebers 1982; Isayama 1982). It is thus only with prior knowledge of what are held to be early symptoms of the disease ("indistinctness of vision") that
the Comte's unwitting report of what it really signified: the onset of MS, a signification which both endorses and is endorsed by the expert reading of the whole diary extract.

Given the wide variation included in the symptomatology of MS, it is likely that some putative symptoms (not only blurred vision) are never noticed as symptoms of MS. If, in this case, D'Este had not been literate, able and wanting to read and write, he may not have noticed anything wrong with his eyes and, for all practical purposes, these 'first symptoms' would not have existed.

Given the crucial role of onset in the general conceptualisation of MS and for research into its nature and cause, the possibility that first signs may be invisible should compound the uncertainty accorded to the data taken to mark onset. For it is not just the unlikelihood of experts being there when the first signs occur, but that the signs first noticed by an individual cannot be assumed to be really the first ones, nor, thus, to signal the real beginning of the disease.

"...Soon after I went to Ireland, and without anything having been done to my eyes, they completely recovered their strength and distinctness of vision..." (18-21)

The disappearance of his sight disturbance without treatment, taken with his age when it occurred and with the emotional stress he had endured shortly before, cumulatively add up to evidence for the presence of MS - given medical knowledge of what to look for and what things signify: an expert reading of the diary.
The report of the recovery of his sight without treatment further endorses this reading: the knowledge is that early attacks are frequently temporary. It is thus the very temporariness of the one recorded in the diary that adds to the likelihood of his condition being MS.

"...Subsequent entries in the diary leave no doubt about the diagnosis..." (22-23)

The establishment of a diagnosis is clearly the key foundation upon which all knowledge of MS must rest: it is desirable - crucial - that those who have MS may be distinguished from those who do not. Yet there is currently no laboratory or clinical test which will establish without doubt that MS is present in the body. Schumacher et al described, almost twenty years ago, a situation that has not substantially changed (Kurtzke 1980a):

(L)aboratory tests may lend support to the diagnosis or aid in ruling out other conditions but, for the present at least, are not of use in confirming the diagnosis.

Schumacher et al 1965: 554

Diagnoses are arrived at generally over a period of time (and see Chapter Four, passim) by a combination of clinical observations, tests and case histories. This situation is not, according to Lewis, uncommon in neurology:

As in other branches of medicine, the art of the neurologist consists of making a diagnosis from the patient's own account of his illness and from the physical examination; aided by appropriate radio-graphic or laboratory tests... For the neurologist, a complete and accurate history is essential. Very often, a precise diagnosis can be made from the history and examination is simply confirmatory; the converse, a physical examination which provides signs not predictable from the history tend to come as a surprise.

Lewis 1982: 13
In the light of this account, McAlpine's confidence in being able to diagnose the Comte's condition solely on the basis of his diary's report (read, that is, by an expert reader) is unremarkable, since the method by which he has arrived at the diagnosis of MS is not that different from current normal practice, a practice not confined to clinical situations. Kuroiwa et al (1975) for example report that for their epidemiological survey of the prevalence of MS in Japan, they examined the case records of 1,084 patients, assessing on that basis alone whether each person had had MS or not.

The method is clearly dependent ultimately on lay accounts, patients' descriptions of their symptoms - but "a complete and accurate history"? Such a thing has already been put into question.

The other element of diagnosis, after the noting of the case history, is the application of the appropriate disease category, which consists of those characteristics taken to be of MS and which thus constitute MS itself. There have been a number of diagnostic criteria developed for what is to count as MS, against which the patient's history and the neurologist's/physician's observations are compared. None of the several diagnostic criteria are used to the exclusion of all others, but Schumacher et al's (1965) remains one of the most ubiquitous and highly regarded. (It is worth noting the list of authors involved in the development of this criterion, and comparing them with that closed core of epidemiologists commented on earlier).

Figure 2.3 overleaf consists of a copy of the Schumacher criterion.
Figure 2.3

Diagnostic Criteria

Clinical diagnostic criteria: The diagnosis in most cases, even when termed "clinically definite" must remain one merely of high probability because of the lack of specific diagnostic tests. The familiar list of common symptoms and signs of the disease is of limited value since these may be caused by other diseases or be absent in multiple sclerosis. As emphasized below, laboratory tests may lend support to the diagnosis or aid in ruling out other conditions, but for the present at least, are not of use in confirming the diagnosis.

The following six criteria are deemed essential to characterize the disease state as clinically "definite multiple sclerosis."

a. There must be objective abnormalities on neurologic examination attributable to affection of the central nervous system. Symptoms alone, no matter how suggestive, cannot be accepted as diagnostic of multiple sclerosis.

b. On neurologic examination or by history there must be evidence of involvement of two or more separate parts of the central nervous system.

(c) Determining multiplicity of lesions, certain signs or combination of them require cautious interpretation. For example, pallor of the temporal half of the optic disc is pathologic only if beyond the average range and if supported by evidence of impairment of visual acuity and/or fields or by a history suggesting a previous attack of retrobulbar neuritis. It must also be kept in mind that "multiplicity of structural involvement" may occur in other central nervous system disease without fulfilling the requirements of "separateness," "multiplicity," or "dissemination" of lesions. Thus, if the involvement of several structures can be attributed to a single lesion at one locus, as is the case, for example, in tumor or infarct of the brainstem or compression of the spinal cord, such involvement cannot be considered as fulfilling the criterion of "multiplicity." Further, evidence reflecting the simultaneous and symmetric involvement of the lateral and posterior columns of the spinal cord, found commonly in disease of the central nervous system other than multiple sclerosis, cannot be interpreted as caused by multiple sclerosis in the absence of additional sites of involvement.

c. The objective neurologic evidence of central nervous system disease must reflect predominantly white matter involvement, i.e., fiber tract damage. Thus, signs must consist mainly of optic nerve, cerebral subcortical, corticobulbar, corticospinal, medial longitudinal fasciculus, cerebellar subcortical, spinocerebellar, and long sensory tract (especially posterior column) dysfunction. More than a minor proportion of signs of lower motor neuron (brainstem, spinal nuclear gray matter, or peripheral nerve) dysfunction will disqualify a subject as having multiple sclerosis for purposes of an experimental trial of therapy.

d. The involvement of the neuraxis must have occurred temporally in one or the other of the following patterns:

(1) In two or more episodes of worsening, separated by a period of one month or more, each episode lasting at least 24 hours.

(2) Slow or step-wise progression of signs and symptoms, over a period of at least six months. These arbitrary time-limits are necessary to exclude: (1) fluctuating or transitory neurologic impairment due to other causes (e.g., vascular), and (2) acute disseminated neurologic disease which is short-lived and nonrecurrent (such as encephalomyelitis).

e. The age of the patient at the onset of the disease must fall within the range of 10 to 50 years, inclusive.

f. The patient's signs and symptoms cannot be explained better by some other disease process; a decision which must be made by a physician competent in clinical neurology. It is realized there will be patients who do not meet precisely these criteria, yet have multiple sclerosis; conversely, there will be those who technically meet these criteria, yet will not be considered to have multiple sclerosis by the physician. Nevertheless, all these criteria should be met for inclusion of patients in a test series.

According to the authors, it consists of:

(S)ix criteria which are deemed essential to characterise the disease state as 'definite Multiple Sclerosis'.

Schumacher et al 1965: 552

It is noticeable that, for example, for criteria 'd' and 'e' - a particular pattern of remission-relapse, and a limited onset period (between the ages of 10 and 50) - are both 'characteristics of MS' disputed in/by the literature (and further problematised in this chapter). Yet they are taken as the distinguishing features characteristic of the very disease state which the criteria are intended to define ("definite MS"). That is, the nature of MS is presumed prior to its definition.(Cf. Hofstadter 1980).

Criterion 'f' on the other hand, almost suggests that MS is a residual category, simply the collection of those cases not otherwise accountable for by other diseases.

As can be seen, the Schumacher criteria depend not only on symptoms and on "neurological examination" but on a patient's "history". As the case of the Comte itself exemplifies however, a person's account of their condition - the origins and early occurrences of their MS - cannot be ascertained independent of a very specific set of 'already knowns'. A 'history' simply is not the ascertainment of the facts of the matter, and thus cannot, pace Schumacher et al, provide a third, independent, source of objective evidence.

The Schumacher criteria is not simply a means of establishing a diagnosis of MS for an individual but is the basis upon which knowledge about MS is both built and evaluated. Kurtzke - one of
those researchers dominant in the field of MS epidemiology (and co-author of the Schumacher paper) - in his "update" on the geographical distribution of MS, evaluates the validity and reliability of the various research findings on the basis of whether or not the researchers used the Schumacher criteria for definite MS as the basis for counting prevalence (Kurtzke 1980a). It is only those who did who are admitted into his 'Class A'; those papers whose results are to be the most trusted. It is the geographic distribution of MS which, as described earlier, provides for the theories that MS is at least partly socially/environmentally caused, a thesis that has been the source of a concentration of research effort into particular lines of enquiry. One can begin to see the consequentiality.

Yet according to other diagnostic criteria, for example Broman's, the only 'definite' cases of MS are those verified by autopsy.

Broman's system:

(W)as presented in preliminary form at the Gottenberg Symposium in 1972, and...on the whole corresponds to the proposals made by Helmut Bauer at the Gottingen Symposium in 1978.

Broman 1981: 69

He categorises the diagnosis of MS into a range of degrees of certainty, each defined on the basis of a matrix of symptoms/pattern so far experienced by the patient:

Category 1 = MS possible but improbable.
2 = MS possible but doubtful.
3 = MS probable but not quite convincing.
4 = MS probable beyond doubt.
5 = MS verified by autopsy.

Broman 1981: 30
Since Schumacher et al's criteria are intended to be the basis for the selection of subjects in clinical trials, one can see that Broman's category of verified MS would be, to say the least, unfruitful. The increased number of categories available under Broman's system make his version appear more capable however than Schumacher et al's of capturing the complexity of MS, incorporating the variation of its experience into its basic definition, thus paradoxically appearing to constitute a more precise description of the 'underlying realities' of MS. 14

Broman adds that there is of course no sharp delineation between these groups:

We are aware of the fundamental fact that...the clinically estimated onset of the disease often (always?) is illusory. (His query)

Broman 1981: 31

It appears that he is, unusually for authors in this literature, admitting a serious problem with the basic data on MS. But as his further comments make clear, these problems are conceived of as restricted, technical and temporary.

1. With the methods available today, no complete material of MS patients is conceivable.

2. (as quoted above)

3. The most ambitious follow-up study of MS patients could never be entirely prospective, unless considerable bias is allowed for the purpose.

4. The correct evaluation of neurological symptoms sometimes is impossible to accomplish (at least in cases with pronounced mental symptoms).

Broman 1981: 31 (My emphases)
It is because of the manner in which "the problems" are conceived that Broman is able to urge that, despite the difficulties:

(A)n attempt must be made [to diagnose MS] in order to come closer to the hidden realities.

Broman 1981: 31

That is, despite all the problems, even those admitted by Broman to be inherent in its diagnosis (let alone those I have been raising here) the independent reality of MS is never doubted. The imperative is to continue with the research rather than to go back to first principles.

**

It is not only the diary extract read, phrase by phrase, that constitutes it as documentary evidence of a case of MS but the extract read as a whole; the accumulation of indications - the symptoms, the circumstances in which they occurred, the age of the Comte and so on - which, taken together along with the "subsequent entries" (22) in the diary - that make it so.

It is also the context in which the diary is itself cited: in the authoritative text on MS (and see Figure 2.3 overleaf). Guided by McAlpine's (expert's) statement of what it means, any alternative interpretation of the diary is thereby rendered redundant.
Chapter 2 in 'Re:Reading Written Data' (Wynne 1989: 27-59)

'Lay/Expert Knowledge of MS'
(Wynne 1983)

Multiple Sclerosis: A Reappraisal
(McAlpine et al 1972)

'Some Aspects of the Natural History of Multiple Sclerosis: Precipitating and aggravating factors'
(McAlpine 1972)

'The Case of Augustus, Comte d'Este'
(Firth 1948)

'The Diary of the Comte d'Este'
(D'Este 1822)

Figure 2.4: The Embedding of the Text
In taking this passage from McAlpine step-by-step, I have been able to bring out themes within MS research that seem to be to be invokable by the text itself, relevances to the body of knowledge which constitutes (in both senses), is specific to, research into this disease but invokable only by those who already know what this body of research consists of and who thus may be defined as, included in the category of, experts-in-MS.

The relevance to my own theme concerns the way in which those who know about MS acknowledge certain methodological difficulties that, it would seem, are sufficient to throw doubt on the very bases for theory making and discovery, and yet continue researching in spite of them. For each theory relies on the existence of other already known features of MS, but each of these features is admitted elsewhere to be problematic. For example: to argue for a specific pattern of geographic distribution of MS requires that it be possible to select cases to count; detailed criteria for diagnosis lay down limitations in onset age although there is work in the literature that finds a much earlier onset age than ten, the borderline used by Schumacher et al (Landis 1898; Brandt et al 1981; Sheremata 1981); the 'significant' accumulation of specific symptoms into recognisable patterns of MS requires the possibility of an accurate diagnosis to confirm that the earlier symptoms were significantly patterned; and so on and so on. The whole edifice of 'knowledge of MS' seems like a house of cards, each layer resting on an equally fragile layer beneath, each member of each layer propping up its fellow.
And yet the method of research, its epistemological theory, remain inviolable; the ambiguities 'discovered' in respect of the facts about MS are taken as definitive of the disease itself, an awareness of the fragility of its foundations paradoxically construed as being the mark of expertise in the area.

The history of MS as a phenomenon looks to the origins of that history and finds that MS has been ever the same (emotional stress a precipitating factor, then as now). By that very similarity, the object of research, MS, comes off as universal and atemporal and as such, a phenomenon in the world 'out there'. Yet insofar as the Comte's diary is one example of the historical artefacts used for such a history, the analysis in this chapter at the very least has put into question the assumption that history is simply the recounting of what happened.

At the same time, insofar as the Comte's diary is an example of a lay account of early MS, it and accounts like it are basic both to an understanding of what MS is, and to all the research into it. The expert knowledge of MS thus depends on lay knowledge of it: but that lay knowledge is constituted as referring to MS by means of that very same expert knowledge. It cannot therefore be construed as independent of it. Contrary to what the medical sociologists of the social constructivist school assume, expert knowledge (of MS) is constituted by the accounting practices of experts. The relationship between expert and lay knowledge is reflexive rather than independent.
Chapter One showed that for medical sociology, the accounts of lay people, either of their own conditions or of the concepts involved in health and illness, are understood to be as it were by definition, different from medical knowledge of the same phenomena; the relation between lay accounts and medical knowledge viewed as essentially hierarchical. That is, lay knowledge is constituted as lay by the very assumption of its difference from expert knowledge, the latter being regarded as simply concerned with the facts of the matter.

However, these assumptions have been challenged in this chapter: the expert knowledge about MS ultimately depends on the accounts people with MS give of their condition and yet at the same time the understanding of those accounts as accounts of MS is only possible by bringing particular (expert) knowledge of MS to bear on them. Instead of a hierarchical relationship, there is a double reflexivity: between lay and expert knowledge of MS and between what is already known with what is discovered by MS research. It is not merely that accounts of MS, but MS itself which is constructed out of such artefacts as the Diary of the Comte, not because they necessarily give rise to a reading of MS but because there is activity upon the texts out of which MS emerges as the thing it is taken to be; a process that has as a crucial component, what is 'already known'.
CHAPTER THREE

TALKING ABOUT MS - AN OCCASION FOR THEORISING

The presentation in Chapter Two of current 'expert knowledge' about MS constitutes the context against which the respondents' reference to medical science in respect of their experience and understanding of their condition is to be placed.

What now has to be blocked in are the theoretical auspices (Blum 1971) of the thesis. For it is against that background that the next question, of how the data for the thesis - talk - can be analysed has to be taken up. This is a question however not merely of method but of methodology: one which appears almost intractable. For, unlike Gabbay (cf. Chapter One 19-20), I am (have been) trying to begin with the problem that, for him, came at the end of his analysis.

3.1 ETHNOMETHODOLOGY

Having argued at some length that the object of MS research is constituted by the research practices and assumptions of that which is 'already known' brought to bear on what are taken to be simply the documents of that object, it would be absurd to produce an account of the theoretical auspices of this thesis - ethnomethodology - as if that was immune from the same problematic.
Insofar as a thesis has to conform to form, however, some kind of account of ethnomethodology has to be given. What ethnomethodology essentially 'is' however is a question that cannot be addressed straightforwardly. For the deep difficulty is that the question of how such a question can be responded to, of what that very question assumes (in respect of its, of ethnomethodology and so on) is part of the very problematic that I am trying to address. As a way of conforming, then, I will take an elliptical approach to the question, focusing on the critics rather than on ethnomethodology 'itself'. For its critics, although, as will be shown, their answers to the question of what that theory/method is and entails for sociological research differs, that it may be described - and critiqued on the basis of those descriptions - is unproblematic. I am bound to take that assurance to be the very mark of their failure to comprehend what the theory of ethnomethodology entails: but in doing so, accept that my own critique is no more and no less assuredly grounded. What follows is, likewise, an account of accounts of ethnomethodology.

3.1.1 Some 'Troubles' with Ethnomethodology

For its critics, there are a number of 'troubles' with ethnomethodology, but, as will be seen, the troubles they each identify are tied to what each conceives ethnomethodology 'itself' to be.

(i) Locker (whose work will be discussed further in the next section) regards ethnomethodology as a limited but useful adjunct to sociological practice. In the Introduction to his research on
lay accounts of symptoms and illness (1981), he explains that, unlike other medical sociologists, he regards "social reality as a construct" (1981: x), an approach which allows in his view for the acceptance of ethnomethodology's methods despite a rejection of its theory. As a theory ethnomethodology, for Locker, fails to be able to account for the social action in which he is interested, for it "(S)ees man as a skilled cognitian but not...as an actor" (1981: 135). As a consequence, it "(W)ould take a description of the interpretive process [in interview talk] as all there is to say." (1981: 135) Therefore in his own work:

Some of the propositions of symbolic interactionism and labelling theory are used to demonstrate the centrality of meaning in social life, and the writings of Schutz and the ethnomethodologists are used to identify the cognitive processes involved in their production."

Locker 1981: x

(ii) For Maryl, however, the trouble with ethnomethodology is that, although the "(M)ajor appeal of this school is that it seems immediate and concretely human," it is not in fact "fully human" (1977: 277). It has its roots in phenomenological philosophy, from which it derives its focus on the "consciousness of the solitary individual" (1977: 277); but, neither being philosophers nor understanding the "basic implications of their position" (cf. Bury's attack on social constructivists in medical sociology, Chapter One), ethnomethodologists are simply ignorant of the fact that their position is premised on:
The one abstraction which is not justifiable. Unlike economic, political, religious and familial processes, individual consciousness cannot be concretized by being put back into a larger structural context of which it is a part.

Maryl 1977: 277

Citing Marx and Mead as his authorities, Maryl continues:

Social relationships are derivable (and explainable) from social relationships, and not individual consciousness. The only thing that can be derived from the solitary ego is the solitary ego.

Maryl 1977: 277

By its prioritisation of human consciousness over "history and the division of labour" ethnomethodology constitutes "an exercise in avoidance, like all idealism" (1977: 277), an avoidance derived from the "fundamental similarities between it and the hippie movement." (1977: 277). It is both mistaken and ideologically unsound.

(iii) On the basis of his reading of Garfinkel(1967) Gellner describes ethnomethodology as a "cult of subjectivity in the idiom of...scientism" (1975: 45), a concept of subjectivity that is a consequence of its having social/historical roots of a "very specific, volatile, unconstrained, fantaisiste, Californian kind" (1975 :63). Its tendency is to slip into having to argue that "only concepts ever constrain" individuals (1975: 57). Even if it usually manages to resist this extreme and absurd concomitant of its theoretical position, even so, ethnomethodology cannot account for the relationship between the concepts it identifies and the other constraints which, as according to Gellner, everybody knows exists: social forces.
(iv) Ingleby (in his article in Wright & Treacher 1982) argues that because it has its roots in linguistic philosophy, the trouble with ethnomethodology is that, like that philosophy, it is basically a-political. For:

In its concern to respect the logic of everyday practices, [it] rules out the possibility of criticising those practices: the root cause of this being its implicit presupposition of a social order free from contradiction.

Ingleby 1982: 124

Ingleby regards ethnomethodology as holding 'common sense' to be the "unadulterated core of human wisdom" (1982: 129).

The 'sense' which actions make is inherently a social construction, on which the common knowledge of social members is the ultimate authority; science can only shed light on the realm of human beliefs and conduct to the extent that it invokes this common sense. Deciding which actions and beliefs 'make sense' is essentially a lay activity.

Ingleby 1982: 129

It is clear from the context that this is pejorative. Because of this elevation of lay human consciousness, Ingleby, like Gellner and Maryl, regards it as unable to address the real questions of power relations. Crucially for Ingleby, its conception of the means by which to address such questions - (social) science - is clearly untenable insofar as it is regarded as itself dependent on 'common sense'; not because it is necessarily incorrect but (and cf. Bury in Chapter One) because it has undesirable consequences. Ethnomethodology is antithetical not only to the objectives of sociology but to the very discipline itself as a practice.

(v) Rogers contends that 'other sociologists', in attempting
to comprehend ethnomethodology, fail to do so because they are, on the whole, ignorant of literary structuralism and its de-authoring, de-constructing project. They have therefore failed to (be able to) see the dangers inherent in it to the idea of the human. For the trouble with ethnomethodology is that it ignores human consciousness. 

Pace the critics cited above:

(E)thnomethodologists make rare reference to and adopt no clear, explicit perspective on human consciousness.

Rogers 1984: 166

For ethnomethodologists, she contends, "a member is a course of activity" (1984: 171), people merely:

(F)unctionaries in social situations structured, first of all, by language and the rules of discourse.

Rogers 1984: 171

Such a depiction clearly and mistakenly denies the existence of that which patently society is composed: "person[s] with motives and subjectively meaningful experiences." (1984: 171). Therefore, despite Garfinkel's expressed desire to reject the traditional sociological treatment of the individual as a "judgemental dope", in the practice of ethnomethodology, "the human centre is missing" (Rogers 1984: 171).

(vi) Wolff quotes with approval Natanson's observation that, far from "challenging the involvement of man in the 'natural attitude'", Garfinkel in fact reinforces it, and thus fails to build on the work of Schutz, his claimed precursor (Wolff 1978: 540). But over and above this failure, for his own part, Wolff rejects ethnomethodology on the grounds of its double amorality:
Mn Garfinkel and most if not all ethnomethodology, 'ethnomethodological indifference' stands for indifference towards the very question of Is and Ought, including the question of a meaningful or good society.

Wolff 1978: 539

It is amoral not only in respect of society, but also of individuals:

If the traditional sociologist knows better than do the people whom he studies, the ethnomethodologist...has no relation whatever to his 'subjects' because, as we have already seen, they are exclusively cognitive; but this means they are not subjects at all, but agents - who, furthermore, 'achieve', 'accomplish', 'work at' establishing and maintaining social order.

Wolff 1978: 539-40

Thus ethnomethodology suffers from the "lack of a world" and is doomed to the triviality of its methods "at the expense of moral concern" (1978: 539).

(vii) And finally, an account which, by placing ethnomethodology in the context of the currently "intense ferment in the republic of letters" (Dallmayr & McCarthy 1977: 2-3), accounts for the very contradictions and differences in the depictions of ethnomethodology so far invoked. As they observe:

In a sense the same ambivalence [regarding the "respective weights of consciousness and intersubjectivity"] overshadows contemporary 'ethnomethodology'...Basically committed to the investigation of commonsense experience, or 'the practical activities of men in society as they make accountable to themselves and to others, their everyday affairs', ethnomethodologists tend to differ on whether ordinary life reflects invariant or transcendental cognitive structures or whether cognition is itself shaped by cultural contexts. In the first case, social reality is in danger of being dissolved into mental or 'ideal' properties; in the second case, cognition faces the hazard of historial and cultural contingency.

Dallmayr & McCarthy 1977: 10
Such differences and contradictions that have been remarked upon would be explicable, by this account, by the 'fact' that ethnomethodology is a divided discipline: the divisions themselves reflecting the "Crisis in Understanding" (the title of Dallmayr and McCarthy's essay), with its central quandary:

(R)egarding the relationship between science and understanding, knowledge and self-knowledge.

Dallmayr & McCarthy 1977: 10

Thus, rather than the 'troubles with ethnomethodology' being unique and specific to that discipline as a theory and practice, by this account, the 'trouble' with ethnomethodology is that it is itself the unwitting product and exemplification of 'troubles' in contemporary society.

**

In sum, the critics of ethnomethodology point towards a variety of 'troubles': its methods (but not its theory) are a useful addition to the sociological repertoire; it is utterly impracticable. Its focus is on the individual at the expense of the social; it ignores human consciousness. It is vilified as an absurd and idealist Californian cult; it is a-political, conservative, a-moral. It undercuts the possibility of science. It avoids contemporary crises in society; it is the very embodiment of the contemporary crisis.

By the juxtaposition of these accounts, the question of what "it", ethnomethodology, "is" or entails for sociological research is rendered problematic. But could the heterogeneity of the accounts be accounted for?
It would be possible to account for many of the differences and contradictions on technical grounds. For example to point out that each critic is taking a different set of texts as representative either of the practice or the programatics of ethnomethodology; that each critic is assuming a homogeneity within the 'school' that does not and has not been claimed to exist by its members. Mayrl in fact offers a nice hostage to this kind of account, when he declares that:

(T)aken on its own, Garfinkel's presentation of the objectives of ethnomethodology is virtually indecipherable.

Mayrl 1977: 265

but that:

(I)t can be clarified somewhat through consideration of the works of Cicourel, McHugh and Sacks.

Mayrl 1977: 265.

He then proceeds to base his critique of ethnomethodology on the texts of those latter authors. To admit to not only being unable to 'decipher' but to have settled for others' secondary texts, not in addition to but instead of Garfinkel's provides ample grounds for the dismissal of his characterisation and therefore of his refutation of ethnomethodology.

On the other hand, the critics of ethnomethodology could be countered on the grounds that they had seriously mis-interpreted the texts they cite: Garfinkel did not say that, intend that, and so on.

Alternatively one could point to the fact that the critics invoke different sets of theoretical precursors for ethnomethodology:
French structuralism, phenomenology, linguistic philosophy; arguing on that basis that therefore it of course appears differently in each one of these lights (putting to one side the re-complicating fact that the previous point applies to each of the attributed precursors too).

At a more trivial level, though not without consequentiality, one could argue that the depictions of ethnomethodology would differ because of chronology: Mayrl was writing in 1977, Wolff in 1978, Rogers in 1984 and so on, the extant corpus naturally different at each time point.

Or one could argue that, differing as they do in respect of their own theoretical positions and view of what sociological theory and/or practice ought to be - that matters of conflict, of morality, of consciousness or meaning - are both essential to sociology and absent from the works of ethnomethodology, it is unsurprising that their conclusions about it are unanimous only in their negative evaluation. According to this counter-argument, even had the critics each cited the same works and even had they cited the same extracts from them to substantiate their cases, their 'already knowns' about ethnomethodology's key absences would have been sufficient grounds for their depictions still to differ, and to contradict, each other.

All these possible accounts would be to argue, then, that the contradictions and differences are in the depictions of ethnomethodology, and not problems for 'the object' depicted, the character of ethnomethodology 'itself': that what is in operation
is (merely) the parallax effect. What is occurring is that whilst the object - ethnomethodology in its real character - is stable, it appears to move, due to the operation of the mechanics of vision, the differences in perspective from which it is viewed which make it appear to shift. Given different viewpoints, merely different aspects of the same phenomenon move in and out of visibility, in relation to the point from which it is viewed and to the different segments of the background - the theoretical auspices - against which it is seen: the contradictions the effects as it were literally of the mechanics of perspective.

Another observation of the differences in the depictions of ethnomethodology altogether however would be to notice that both the technical arguments produced above to account for the differences in the depictions of 'the trouble' with ethnomethodology and the critiques they seek to explain, share certain epistemological assumptions. Principally that the project, to argue about what ethnomethodology 'is' - may be done by taking certain works as documents which stand for their authors' intentions, motives, practices and so on: the critiques are then constituted in comparison with what each critic argues is - the meaning of the work and of the world to which the texts are taken to refer; a world which is presumed: in which "social forces", persons with "subjectively meaningful experiences" and so on unquestionably exist, ontologically given; against which ethnomethodology is compared and found wanting. They share, in short, a method of reading texts.
I do not propose to become involved in texts in that inconcludeable way. It is a crucial part of my own theorising auspices that the pursuit of the meaning of any document in that sense is misconceived; based on the very epistemology that I am trying to work out how to question; and what is taken as ontologically given is, in being fundamental to that epistemology, what also has to be questioned.

As has been both observed and practiced throughout this first Section of the thesis, 'the object', that which is critiqued, cannot be taken as independent of that critique. Reflexivity is in operation not just between analysis and analysed, researcher and researched but, as has been shown explicitly in the case of MS in Chapter Two and implicitly in Chapter One and now Three, what is taken to be the object of discourse - MS, medical sociology, ethnomethodology - is characterised by, constituted by the grounds of that discourse. 'The literature' is not the documentary archive of an independent object: the reading of 'the literature' cannot be immune from the problematic of the documentary method but is itself subject to the same reflexive constitution.

**

However, although the settling of the matter is, according - how else may one put this - to the theoretical auspices of ethnomethodology, in principle inconcludeable, the authors, the critics whom I have cited, have clearly managed to conceive of what ethnomethodology is, for the purposes of their critiques, both in terms of which works to
cite, their meaning and implications, their relevant theoretical precursors and so on. What is problematic in principle is managed for practical purposes. As I have managed it here. I want therefore to say that they have misread the texts; failed to comprehend what ethnomethodology is. But how far it is is possible to be said to distort a text, to stretch its meaning illegitimately is a critical and highly puzzling matter. Although I am contending that texts are not in principle documents of anything definitively deducible from them, I am not wanting to contend that they are infinitely indexical. How this paradox is possible is a matter to be considered throughout this thesis.

'Texts cannot be taken to mean anything at all! But it was this assumption of the implications of ethnomethodology's 'methodological horrors' that provided the grounds for Bury's contention, discussed in Chapter One, that social constructivism inevitably leads to an anarchy of meaning; and to Gellner's contention that the concomitant of ethnomethodology is that it no longer matters what is said by research; that its logical and only outcome is that there can be no standards, no criteria whatsoever by which either to speak, to practice, or to judge anything about the world. That, as Russell has characterised other's (mis)understanding of the project within the sociology of science to replace empiricism, it is assumed that therefore 'anything goes': which is taken in turn to mean that because nothing goes for sure, nothing can 'go' at all. Or rather, only 'anything' (1983).
Russell concludes her article with the observation that such issues are better addressed through sociological studies than through abstractions. My difference from her adherence to the social constructivist thesis notwithstanding, at this aporetic point is seems sensible advice to take. So let me move towards the substantive matter of this thesis, towards the analysis of the data: people talking about their MS.

3.2 MS: AN OCCASION FOR THEORISING

To recapitulate: the purpose of the research is to look at lay knowledge of MS, in particular at the question of whether and if so how people operate with a concept of science as a special category of knowledge in their own sense-making practices. It is to look at the taken for granted, at that which 'everybody knows' about science: the everybody who is not an expert in scientific medicine, people who themselves have MS. This would seem, then, to be a question about the difference between expert and lay knowledge (of MS). However, following what has emerged from the previous two chapters - the double reflexive relation between expert and lay knowledge and between the already knowns of expert knowledge and the objects it discovers - it is no longer possible to proceed by identifying lay knowledge in terms of its difference from experts.

How, then, may one proceed? Following Schutz and more recently Geertz, the notion of 'making strange' is currently seen as a key to the exploration of the mundane, the way to make visible the world of what everybody knows (Schutz 1954; Pollner 1974; Geertz 1975). But is it possible to make strange?
3.2.1 Making Strange

In the literature of the imagination, for example in science fiction, an alien is envisaged who enters the familiar world; by reading the world described in the text through its eyes, the reader is enabled to see the strangeness in the familiar.

However, as Oldman (1983) has pointed out in 'Making Aliens: Problems of description in science fiction and social science', even in the complete freedom of fiction, the author has to imagine both how an alien would 'see' our world and something not only of what the objects of such seeing would be like, but what that way of seeing would be. The inherent impossibility of doing this is demonstrated in Amis's recent novel Other People (1981), which is written in the first person of an amnesiac. Amis can have her find strange many of the things 'we' take for granted - the difference in light signifying night and day, what clouds are, what bodily sensations signify (those that indicate a need to defaecate, for example) - because she has forgotten their meaning. But the way, in the novel, she finds out what she has forgotten, the means by which knowledge of the unknown can be achieved, she has not and Amis cannot have her forget. Asimov et al's collection of robot stories, Machines that Think (1984) is similarly witness to the limits of the possibility of imagining strangeness, because of the essential requirement for fiction (and all texts) of intelligibility to the reader, predicated on familiarity.

Oldman however argues that some writers do achieve a:

(T)acit quality of alienness in their depictions of non-human or future human societies, just as some anthropologists achieve both explanation and a sense of wonder in their
accounts of other peoples and translators turn others' poetry into their poetry. They achieve, in other words, an apparent transcendence of their own ethnocentricity.

Oldman 1983: 49

That some think this is impossible in principle is due, according to Oldman, to their mistaken assumption that there is:

(A) stable hub or centre to one's own culture [when rather] culture and language should be seen for what they are: fluid, reflexive, and political...The dynamic quality of language is forgotten and its tendency to drift and develop independent of the way it is pinned to the world of events in its descriptive role is ignored. (my emphasis)

Oldman 1983: 63

Be that as it may, anthropologists are by their own definitions strangers to the worlds they observe: those worlds are obviously strange, and visible to them by virtue of that strangeness. But even so, as was argued in the case of medical anthropology in Chapter One, like science fiction's Martians, both that knowledge and the method by which 'natives' produce their knowledge is discerned by comparison with that which is not questioned at all: western epistemology.

In order to stand outside one's own culture, in order to see it more clearly, the social constructivists in medical sociology, also discussed in the first chapter, and challenged in Chapter Two, use the technique of invoking history in order to see the present taken-for-granted of medicine through the eyes of the past. This effort fails, as I have argued, because of the glossing over of the fact that it is necessarily the case that not only now, the present, but 'then', the past, can only be seen from now. That which becomes strange is constituted by what is taken for granted to be familiar.
This is not a problem to be solved. However, what I want to take from this is the fact that 'making strange' is, in all these cases, regarded as a problem for the analyst - the novelist, the anthropologist, the sociologist - attempting to render unfamiliar to herself the world that others, it is taken for granted, take for granted.

For the case of studying people with MS is profoundly different. What the subjects describe to me in the interviews about the experience of MS, what has been happening to them and how they make sense of it is, in effect, a world already made strange. To use Garfinkel's (1967) terminology, they are already involuntary participants in a naturally occurring breaching experiment, in which their ontological and epistemological taken-for-granteds about themselves, about knowledge, and about the world in which they live have been breached, force majeure, by MS. Thus, unlike in all those instances discussed above, the notion of strangeness is not mine, as analyst, but theirs. It is for them that the world has become strange and themselves strangers in it - and this strangeness is a problem with which they have daily to wrestle and work to make mundane again.

This is not to say that MS is a naturally occurring breaching experiment, but that thinking about it in that way may provide a way to pursue the question of lay theorising about science that does not banish the problem of reflexivity.
3.2.2 The Breaching Experiments

In his Studies in Ethnomethodology (1967), Garfinkel describes the experiments as "demonstrations", "aids to the sluggish imagination" (although, as has already been said, even the most actively unconstrained exercise of the imagination that is allowed in the form of fictional writing, cannot be totally boundless):

Procedurally, it is my preference to start with familiar scenes and ask what can be done to make trouble. The operations that one would have to perform in order to multiply the senseless features of perceived environments; to produce and sustain bewilderment, consternation and confusion; to produce the socially structured affects of anxiety, shame, guilt and indignation; and to produce disorganized interaction should tell us something about how the structures of everyday activities are ordinarily and routinely produced and maintained.

Garfinkel 1967: 37

The outcomes of the experiments were, according to Garfinkel, that:

(T)he members' real perceived environment on losing its known-in-common background, should become 'specifically senseless'. Ideally speaking, behaviours directed to such a senseless environment should be those of bewilderment, uncertainty, internal conflict, psycho-social isolation, acute and nameless anxiety along with various symptoms of acute depersonalisation. Structures of interaction should be correspondingly disorganised.

Garfinkel 1967: 54

These "outcomes" have much in common with what, in the literature on MS, are taken as symptoms of the disease (see Chapter Two, this thesis passim; and Appendix 3.1). Thus I can argue that 'ergo', MS is, for members, a naturally occurring breaching experiment... because it has these outcomes amongst its consequences.

But there is also an important difference, "procedurally". As Mehan and Wood state:
Its [breaching's] prime function, however, is to make the researchers aware of the way they normally do the world by requiring them to learn to do it otherwise. (my emphasis)

Mehan & Wood 1975: 233

In terms of that account, it is the members, the people with MS, who are equivalent to the "researchers", not myself as analyst.

There is a further not inconsequential difference. In all the breaching experiments in Garfinkel's book, the breaching is done by his students, at his direction, on their own everyday interactions with others. The description and analysis of what occurred is then written up by Garfinkel, who uses his students' reports as his data rather than material collected at the point of breaching by the analyst himself. In the extension of the breaching metaphor to my own data the procedure is different. However, it is not necessary to claim a total identity between MS or this thesis and the breaching demonstrations in order to explore what happens when the data is read in the light of this metaphoric resonance.

It has to be said, however, that Mehan and Wood, at least, identify certain limitations in the breaching procedures which raises another resonance with this thesis' concerns:

The linkage between any specific incongruity demonstration and a feature of social knowledge or interpretive procedure is obscure. The logic of the demonstration does not follow from the theory in any determinative way. One must not look to these breaching studies as a way of building a theory of the reality constructor that will be comparable to the truly experimental sciences.

The value of such demonstrations is great nevertheless.

Mehan & Wood 1975: 113 (my emphasis)
This caveat, that the procedures are "no way of building a theory" is telling in that it reveals how entrenched in (a version of) the scientific paradigm even the most 'idealistic' interpreters of ethnomethodology are. For it is not, as it could have been, from the very predicates of ethnomethodology ("that sweet poison" as they call it) that they regard the breaching studies as less than experiments, but from the retention of an unquestioned, privileged epistemological status accorded by them to the knowledge produced by the "truly experimental" sciences.

* 

The point of the breaching experiments is to attempt to make problematic and therefore visible to analysis some taken for granted methods of practical reasoning. I want to extend from 'methods' to theorising in a more fundamental sense.

The case of MS is possibly not unique amongst medical conditions; but it is particularly apposite for my purposes in that there is no certainty, no closure on what it means or signifies (cf. Chapter Two). It is a natural breaching experiment in which not only the methods but the substance of practical reasoning, by individuals about their lives, their bodies and their selves; the doing of knowledge which has become problematic, for members.

In a parallel with the argument that science is interesting to the sociology of knowledge in that it is a discipline wherein reasoning about theory, method and content are made explicit by its practitioners - is the practice of science - so with the occurrence of MS. The extent of the disruption is the mark of its richness as a resource for analysis. With the occurrence of MS,
the methodological horrors are not dismissible as an analyst's artefact - but are a matter of lived experience.

To substantiate that assertion it is necessary to turn to the data upon which this thesis is grounded - people talking about MS - to consider the question of how it may be analysed.

In the light of what has already been written, this can no longer be a question merely of selecting the most appropriate method from those available but is deeply problematic. For, to make explicit what has been perhaps implicit so far, there is a fundamental resonance between MS and the question of theorising. With the onset of ethnomethodology, the mundane practice of research is breached, sociology itself made strange.

3.3 THE ANALYSIS OF TALK

The data that is the basis for this thesis consists of the transcripts of twelve unstructured interviews about MS. Eight interviewees had been diagnosed by the medical profession as having MS; two were people under the process of being diagnosed; and two were relatives of those who had died from it. Appendix 3.2 gives details of the research group.

The data then is a collection of accounts of MS, from personal experience, amounting to some 25 hours of talk. They are closer to naturally occurring conversations in character than to anything that would have been produced by structured interview techniques; the objective was, as far as possible, to allow the people to speak for themselves free of the assumptions
that would have been inherent in any formal pre-scheduling of 'relevant questions'.

* 

In the first chapter, reference was made en passant to the ways in which medical sociologists have used talk as data; but their use of it is simply as a concomitant of their interest in patients' perspectives. The talk is treated unproblematically as simply the available evidence for patients' understandings of their conditions, of their relations with medical professionals and so on, an approach to (talk) data consistent with their generally realist approach to research practice.

However, given the theoretical position within which this thesis is being produced, the question of how to analyse talk cannot be taken as immune from the question of reflexivity. Its meaning cannot be taken as independent of the means by which it is analysed. But how then?

The most appropriate resource to turn to for thinking about this question must be those works which have approached the analysis of talk from the same theoretical framework as my own: ethnomethodology.

3.3.1 Conversational Analysis

Outside medical sociology (and sometimes within it, eg Corsaro 1982), the most highly developed technique applied to the analysis of talk is that of conversational analysis (CA). Although it claims its theoretical roots to lie in ethnomethodology\(^ {11} \) - particularly invoking the work of Sacks as well as Garfinkel himself - in many ways as will be shown, conversational analysis epitomises the failure of
practitioners of ethnomethodology to take seriously the epistemological problematic that at least my conception of Garfinkel's work involves.

By paying close attention to naturally occurring conversations (another relevance of their work to my data), CA has over the last twenty years or so been in the process of developing what might be described as a taxonomy of invariant features in the structure of talk - greetings, pauses, turn-taking and so on (see for example Sudnow 1972; Schegloff & Sacks 1973; Sacks et al 1974; Schenkein 1979. See also Leiter 1980; Benson & Hughes 1983).

Conversations are transcribed according to rigorous conventions. At its best - and the transcription work of Gail Jefferson (see Figure 3.1) is regarded as the sine qua non amongst CAs - what emerges is awesome in its exquisiteness, like the eyes of insects revealed under the microscope to consist of an utterly unfamiliar, perfectly ordered, beauty.

Such an analogy between CA and science is not fanciful: it is both claimed and runs through what is being practiced - the application of science to talk:

\[
\text{[CA] work [as] to explore the possibility of achieving a naturalistic observational discipline that could deal with the details of social action(s) rigorously, empirically and formally.}
\]

Schegloff & Sacks 1973: 233

Qua science, in this process of revelation through microscopy, what becomes observable and thus significant attains an independence of the methods through which it is brought to visibility. The arcane transcripts are taken to stand for their object - naturally
1. Conjunctional - Intra-Utterance Pause (ca. 109 'pickup' cases)

(1.1) [0:8815(A):20]
Andrea: - Mistuh Topp's in Munich is e?
Bette: - Uh:::m (0.8) He's either in Munich or Col:ogne now, ah
- f'goh- ah'v lost track. One or the o(h)ther, Uh:::hh (2.7)
I think he's::: ...

(1.2) [NB:IV:9:R:2]
Emma: - Honey ah'll come down after I had muh liddle bowl a'soup'n
salad'n ah'll call'm back to yuh uh'd love it.
(0.8)
Penny: - We:ll (0.7) okay

(1.3) [SBL:3:3:R:2]
Keith: ahh'll t-call {Te:d en (0.2) let im go over this agreement

(1.4) [NB:II:2:R:21]
Nancy: Assooming yihknow thot he'd be taking th'payment
b'k b'a:ck with im
Emma: - 'hhh hh So:: (0.7) 'tch he said Dad js didn't trust me ..

(1.5) [Fr:USI:4:R]
Mike: - She ex no use fer it but (0.5) she jis don't like the idear
'v im givin anything away b'fore'e essuh.

(1.6) [SBL:3:5:R:2]
Milly: - tahh'm gunna learn that it cert'ny is easy en all you haf
tuh do is tru:st en just sorta go alahng en I hhope it's
- this way becuz 'hhh lhis's gunnuh be: jis (!WOghNderful

(1.7) [TCI(b):9:5]
Linda: Yih don't have enough money tuh git Joshua a chew bo:ne?
- or (. ) or 'hhh miilk en e:gg's y'don'have enough money tih
git all that.

2. Conjunctional - 'Clean' Speaker Transition (ca. 59 'pickup' cases)

(2.1) [SBL:2:1:8:R:13]
Nora: but I c'n (. ) close muh bedroom o:ff it gets so hot'n
there.
(0.3)
Bea: - Yah.'t bhhhhhh'hhh Uh::m
(1.0)
Nora: - W'l who wouldju drum up.
( .)
Bea: - I don't know.

Figure 3.1 Jefferson's (1984) Transcript :"'Conjunctionals' as
Overlap-Vulnerable." (Extract)
occurring conversations - but, to continue the microscopic analogy - at a higher level of resolution, their natural transience fixed in order to enable close study.

This technique has been taken one step further. In the pursuit of a more fully captured phenomenon, using a concealed video camera, Heath, for example, has recorded medical consultations in vision as well as sound (Heath 1982; 1983). He has developed a technique for transcribing the gestures with which the interaction talk is embellished, and has produced transcripts of the interactions which are to be read like a musical score (Figure 3.2). What Heath has thereby produced is a demonstrable orchestration between voice and gesture on the part of patients, methods used by them to elicit the gaze of doctors. Work such as Heath's presumes that there is no in principle limit on the possibility of achieving a complete and accurate record of interactions.

The claim by CA is that the structures of conversation are discernible to analysis quite independent of both their content and context: and indeed by their own theory this has to be the case. However, an informal study of how the work is done demonstrated the difficulty of holding to that claim in practice. Attendance at a BSA Sociology of Language Group Conference in Plymouth (1984) which was predominantly concerned with/attended by CA's, provided me with the opportunity to participate in and to observe conversational analysis in practice.\textsuperscript{12} The discussion which follows is based on the fieldnotes made at the time. Although this was not in any way
Extract 6. Transcript 2.

gest. I had 4 (ug. gest 2).

P

Dr (writes)

1 2 3

Extract 7. Transcript 1.

mest initiation
side

1 2 3

tender: I ver(4) I was comin up the steps like this all the way up I felt - moderately slow (writes pr.)

1 2 3 (writes pr.)

Extract 8. Transcript 2.

gest. tell

of (uh) I can't breath (4) now (4) you know (4) owe (4) an I'm all troubl

(Dr reads)


gesture starts

gesture lands

1 2 3

I'm sorry I didn't getcher(4) least name:

start of 5's

pass shift

1 2 3

Extract 10. Transcript 1.

1.2.3. Sw...will you come our next visit the next time sir...you

1 2 3

Extract 11. Transcript 2.

1 2 3

I'm sorry I didn't hear what you said

1 2 3 you must (listen & talk to her)

Figure 3.2 Heath Transcript 1983
a formal study, nevertheless the issues about the practice of CA which were brought to light over the course of the conference are of direct relevance to the current discussion: by definition, the practice of conversational analysis cannot appear in its writings.

The conference took the form of a series of workshops, formal papers and seminars in which all the participants met together. It is the latter on which I shall concentrate. What was said in the final seminar is to be held in juxtaposition with comments made on a paper given earlier in the conference by Grimshaw (1984), which had been received very critically, on a ten-year and ongoing project in which a core piece of data—a dissertation defence and subsequent discussion about its merits amongst the examiners—was being worked on by a variety of sociologists, a variety which, however, did not include any CA.

The dissertation defense had been both stereo-taped and filmed. Each workshop group at Plymouth had been given a section of the audio tape, with its transcript, to work on, after Grimshaw's presentation. In the group of which I was a member, the workshop participants declared themselves unable to work "properly" on it, because CA transcript conventions had not been used (particularly concerning the marking of pauses and the layout of speakers' turns); but also because they felt the transcript was inaccurate in detail. That is, certain words heard by them on the tape as one thing appeared in Grimshaw's transcript as another. One of the workshop's conclusions had been that this showed, once again, that an accurate
data record was absolutely essential for CA analysis.

However, when it came to the analysis of a 'properly transcribed interaction, the transcripts alone proved (also) insufficient. In the final seminar, a new passage from a tape which had been worked upon separately in the groups was used, and people were invited to analyse it together.

Competing versions of what was going on in the transcribed interaction were put forward: what was most interesting were the grounds upon which proponents of each version claimed their own to be the most plausible. These grounds were, in essence, the invocation of 'common sense' - that 'in their experience' people meant this or that by what they said. Much of the discussion revolved around the personalities of and the weight of conflict between the two women in the interaction. That is, the character of the speakers was read off from their talk; they (the CA's) presumed they were 'able' to know just what each one of the women on the tape was/would be like, quite outside the brief taped exchange, and indeed needed to know that, in order to discern the structures of the talk.

There was a basic consensus: that when the interaction was broken down in detail, a great deal of conflict between the two women could be clearly seen; and yet, they said, they expected that the two women, if asked, would claim to be friends. (One member of the conference asked at this point whether the meeting thought that CA's were more inclined to discover conflict than consensus underlying talk. The Chair, with some asperity, asked whether he was proposing that the findings of CA analysis were an artefact of
the analysis rather than of what was really there: but the response to this challenge, and the topic, got lost amongst the discussion. Certainly this possibility did not re-appear in any of the subsequent debate, but it obviously underlies my description of the proceedings. It was remarked how fortunate it was that, because of the speed with which talk normally occurs, these conflicts get glossed over and, in the ongoing context of relationships, mostly get 'ironed out'.

The suggestion was then advanced that maybe the disputes in the seminar over the meaning of the interaction would be settled if it had been possible to appeal to the women themselves for their accounts of how they felt about each other, and asked for their version(s) of what was going on between them. As a response to this, someone replied that even if the women were asked this would not settle the matter at all, ordinary people being unable to reach the level of analysis that CA's are able to achieve. The only objection voiced to the original suggestion concerned, then, its political but not its theoretical, implication.14

Another participant suggested that a way out of the difficulty might be if there was some sort of machine which could record "tension" in a speaker. That record could be placed alongside the transcribed speech, enabling the analyst to have more and better information about the real feelings of the speakers, and therefore be able to produce a more accurate analysis; in response to which, descriptions of galvanic skin response machines were offered as just such a possible recorder (see Figure 3.3).15

No-one objected on the ground of theoretical principle.
Analysis of the sound of the spoken sentence "the very first showroom is used by young mums with handbags" — one of the contrived structures being used by IBM in speech synthesis studies because it contains many phonemes and makes it easy to hear the subtle differences. The acoustic structure (AFS) diagram below shows the frequency of the spoken sentence's sound wave, immediately below it, the symbolic realization transcription which will be used in speech synthesis. Below this the heavy black marks show the energy level of the voice — with its erratic pattern of breaks between words — while the base line is marked in tenths of a second.

Saying it like it is

Your all-talking all-thinking computer friend may still be far away, but he is already learning to read. Anthony Tucker tunes in to IBM's latest phonemes, as they probe the secrets of language.

Figure 3.3: Tucker (1985)
This biological sub-strata to talk had been invoked once before in the conference, when Jefferson, presenting a paper on the first day had tentatively suggested that according to her analysis of pauses and overlap - who begins after a pause and the relevance of the length of pause to what happened after - it seemed that there was a critical one-second period which is granted to other speakers as a reasonable reaction time, this minimum time corresponding to the time the brain needs to process reception and response (Jefferson 1984b). That is, pauses are as long as they are because of the nature of physiology: this, like the reference to skin tension referred to above had caused no remark except enthusiastic interest amongst her audience. Once again, reference was made to physiology as the basis for filling in the gaps in the explanations arrived at by analysis: the biological basis of behaviour acting as a residual category but regarded as the absolute bottom line of the structures of talk.

Grimshaw at one point raised the question of how disputed versions of the tapes could be settled - that is, as I heard it, asking the meeting to make explicit 'the rules' of their practice. But this appeared to be heard as a condemnation on his part of a lack of consensus in the discussion. His comment was responded to with some vehemence by a participant saying that they couldn't be expected to do fine analysis in workshops or meetings such as that one: that workshops were for 'being courageous', 'risking various ideas' which only in the solitude of writing afterwards
could become 'fine'. That is, such disputes as there were over the meaning of the taped interaction were only temporary, a result of the informal situation in which they were being discussed. This 'solitude' of 'writing' echoed the account Jefferson had given earlier of the process by which she analyses her data: that transcription involved listening to the tapes over and over, then reading and rereading the transcripts, far into the night (a description strongly reminiscent of certain accounts of scientific discovery, only achieved through the blood, sweat and tears of devoted and self-less scientists). The patterns that lie within talk are discoverable only through an arduously achieved familiarity with the data. Inexorably part of the technique, it is only this hard labour that will reveal the elaborately and minutely ordered structures of conversation.

This informal ethnography of the practice of CA at the conference also suggested something of a paradox: although CA posits itself upon 'scientific analysis' (with which the precision demanded of their transcription is consistent), the grounds upon which the disputed versions of a transcript were warranted came from the realm of "gut feeling", and "intuition" about their meaning, both concepts used explicitly and frequently in the various accounts of what was going on in the tape. At the same time, CA's relied upon a close attention to 'what was really going on' in the talk, although in the light of their debates as to the accuracy/usefulness of asking the speakers themselves to say what they had meant (i.e. acknowledging that there were still other possible versions that
could be produced), what sense that 'really going on' carries is not at all clear.

What was being analysed in practice was the interaction which gave rise to what was recorded, not only the records themselves. They are regarded merely as the 'traces' (Latour & Woolgar 1979) through which the phenomenon, talk, can be approached, the transcripts taken as accurate, fixed and detailed records, which merely preserved the talk that had taken place. But as has been discussed, in the event, the transcripts, however accurately reproduced, were not in themselves the sole basis for that analysis. They could not be deciphered without recourse to their meaning content.

The attraction of atomism is difficult to resist; that is, the conclusion that somehow the attention to all that fine detail will inexorably make a method fruitful, 'what is really going on' easier to discern. As Mehan and Wood have observed:

> CA's have been willing to ignore the indexical relations between talk and its context. They have treated talk as possessing finite meanings. In doing so, CA's have suspended consideration of the consequences of reflexivity that other researchers have treated as ethnomethodology's essential phenomenon.

> Mehan & Wood 1975: 135-6

Conversational Analysis is often, however, rejected simply on practical grounds rather than with a fundamental critique. Locker (whose analysis of talk will be discussed below) explains for example that it does not provide a practicable means of analysing his own data:
[What I am concerned with is] the analysis of interview talk about health and illness. This talk consists mainly of long descriptions of events by the respondents. CA tends to be undertaken in short exchanges between two participants in a conversation.

Locker 1981: 24

For critics such as Locker, CA is simply one among the sociologist's available repertoire of interchangeable and homogeneous methods, which are selected on the basis of their appropriateness to the analysts' intentions vis-à-vis her data. That CA can be considered on those practical grounds alone assumes that it constitutes no epistemological challenge to 'normal practice': that it is, on the contrary, a practice which shares the realist and scientific theory of knowledge which is the basis of traditional sociology - in fact is a high-class exemplar of it.

3.3.2 Interpretivism

It is true that after reading the work of conversational analysis, other ways of analysing talk, lacking its fine detail, appear crude and gross. The assumption that microscopy, atomism, brings one closer to the phenomenon is very seductive (analogous to the contemporary move in MS research to look for the disease beneath the skin, via NMR scanners, electron microscopy and so on). But it has to be rejected here. For that very reason I cannot use it to address my own concerns.

Even if one were to put aside the problematics involved in the practice of CA it would still be unsuitable (pace Locker) for my own data, for, in being concerned with 'lay conceptions of science', it is the meaning of the interview talk that I am bound to
address. And at the level of its theory, the 'meaning' of talk is in principle irrelevant to CA.

There is, however, an interpretive approach to the analysis of talk that lays claim to at least ethnomethodological influence, which is concerned with meaning, of which Locker (1981) is an exponent.

His research concerns the analysis of a number of interviews with six women, conducted at intervals over the period of a year, consisting of, "their accounts of their own experience of health and illness" (1981: 21) - data somewhat similar to mine in that by using only a "brief schedule" (1981:20) he too endeavoured to "make the interviews as much like a conversation as possible" (1981: 21).

As he explains in the introduction to Symptoms and Illness, unlike other medical sociologists, but 'like' ethnomethodologists, he regards, "social reality as a construct" (1981: x). His approach is however to reject ethnomethodological theory whilst accepting some of its 'findings':

Some of the propositions of symbolic interactionism and labelling theory are used to demonstrate the centrality of meaning in social life, and the writings of Schutz and the ethnomethodologists are used to identify the cognitive processes involved in their production.

Locker 1981: x

As a theory, ethnomethodology has to be rejected by him, for it fails to be able to account for the social action in which he is interested, for it, "(S)ees man as a skilled cognitian but not...as an actor" (1981: 135). It would, thus,
Take a description of the interpretive process in interview talk as all there is to say.

Locker 1981: 135

And for Locker there is much more to his data than that.

How, then, is talk analysed from such a position? At the end of his book, Locker discusses what he has been doing which is, by his account, to provide simply:

(A) relatively superficial explication of the practices and procedures integral to the cognitive organisation of one aspect of experience, that of health and illness.

Locker 1981: 177

But this 'superficiality' is meant almost literally:

(I) say superficial since every extract presented could be subject to a more detailed and extensive analysis. Much more is involved in the accounts I have analysed than has been described so far.

Locker 1981: 177

That is, there is a finite depth to the meanings of the interviews that could (have been) increasingly revealed with increasing levels of focus - a view that is clearly at odds with certain central tenets of ethnomethodology, for example Sacks on 'Sociological Descriptions' and its in-principle inconcludeability (1963), and clearly in conformity with the practice of CA (and, one might add, the latter clearly not in conformity with Sacks, the ethnomethodologist to whom they attribute their own position).

For Locker the talk that constitutes the interviews unproblematically represents simply what his respondents meant: the analyst's task is to read not only the words on their surfaces ("relatively superficially") but what the words are the documents
of, what lies behind them in the people themselves, viz: the underlying "cognitive organization of disorder", to quote the sub-title of his book.

Locker acknowledges that what the women said has to be recognised for "what it is", an acknowledgement that appears to be accepting an element of reflexivity in his analysis when he explains that:

Accounts given in interviews are to be read as such and not more or less adequate descriptions of some independent reality.

Locker 1981: 177

However, despite this caveat, they can, he continues be read as demonstrating that:

(It) was by means of the skillful use of common understandings that the women were able to display their status as moral persons and competent members.

Locker 1981: 177

Thus Locker specifically disclaims any necessary connection between what was said in the interviews and the external events to which they referred; he makes no such disclaimer however in respect of his conclusion that there was a relationship between the accounts which the women provided and the "cognitive organizations" revealed to be lying behind the document of their talk, in their minds, so to speak. For Locker, this relationship was not 'an account'.

For him, then, it is the women's talk about matters of health and illness that have to be understood as accounts, the product of their interpretive practices and the cognitive processes underlying them. The role of the analyst is to understand them as such, and pay attention to those practices rather than evaluating
them in terms of their accuracy, validity and so on.

The interpretive practices are, however, for Locker all on one side; his own analysis a straightforwardly achieved, accurate description of the women's meanings. That an analyst's description must, by the same token, be similarly constituted through interpretive practices is not something that appears to trouble him, despite the extensive discussion of his analysis that he appends as the final chapter of the book.

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This use of the documentary method in the analysis of talk is routine research practice, the ability of the analyst to see through the surface appearances to what lies beneath, that which analysis is taken to be. Analysts are analysts by virtue of the very fact that there is more to data/the phenomena of social life than appears to its participants. It is in making plain what is hidden that provides the possibility for knowing what is 'going on' in talk - both for CA and now for Locker. The careful application of correct method will represent that aspect of 'the world' in which analysts are interested. In that Garfinkel was himself cutting about this: "Ethnomethodological studies are(...) useless when they are done as ironies" (1967:vii), their persistence with 'instrumental irony' (Woolgar 1983) marks their work as, in practice, very little influenced by ethnomethodology.
What we have found is, once again, an assumed immunity from the 'horror' of reflexivity in the analysis of talk, at both extremes. By relying on the scientificity of its method, CA reproduces the metadiscourse of science - the topic and not the resource of this thesis. The interpretive approach, on the other hand, that examines the interpretive practices of talkers in producing meaning fails to address the possibility of the existence of 'the same' phenomenon in analysis itself. It appears from both that (expert) analysis uses some other kind of method than that/those used by its subjects. Despite their apparently diametrically different interests in talk - (only) in its structure, (only) in its meaning - they share at a more fundamental level a confidence that what is dis-covered about talk is there in the phenomenon; that method is merely a means of discovery of aspects of that phenomenon. To re-cite Blum, both are interested only in "the phenomena themselves" (1974: 2-3; Fn 1), looking only outwards to the world.

The selection of an appropriate method begins analysis: and marks the place where analysts have "stopped doubting" (Blum 1971:316). What is strange, remarkable, worth analysing, is what comes after, not before, that stopping.

Having thought about both the work of CA and of Locker in terms of the theoretical bases from which they begin, it appears that it is the analysis of talk as data and not merely data in relation to what the data is about that has to be considered in order to
continue doubting. Reflexivity is involved not only in the relation of data to that which it is taken to be data of but in the relation of analysis to data. But what of this chapter?

At the beginning, I argued that, in that MS was conceivable as a 'natural' breaching experiment, it is possible to say that in this instance, as the warrant for using MS as an occasion for theorising (about theorising), the methodological horrors are horrors already for the people whose talk is the concern of the thesis. In moving on from that assertion to discuss extant approaches to the analysis of 'similar' data - talk - as part of identifying an appropriate method with which to proceed, it is as if the question of what that talk concerns was not yet addressed.

And yet of course to assert that the 'methodological horrors' operate for the people with MS, that MS makes 'knowledge' strange, is to have already found a very specific meaning in that talk. An interpretation (analysis) of the data has already preceded the discussion of method and in fact has already constituted the impetus for the thesis.

In the ordering of the chapters of the thesis so far, in conforming to the proper procedures, in beginning with 'the question' and with relevant literature prior to addressing the selection of a method, what is written represents the process of analysis in a way that rhetorically re-presents scientific method - the independence of analysis from 'the data' (be it talk about MS or other analysts' analyses of talk): the immunity of the analysis
from 'the same' phenomena; inexorably covering over the fact
that the expertise of analysis is in itself an achieved account.
By analysis, the 'making strange' - even of analysis to itself
by means of ethnomethodological theorising - is depicted as
over. The produced analysis cannot represent strangeness: all
it may do is, as here, point towards its absence.
CHAPTER FOUR

ACCOUNTING FOR ACCOUNTS OF THE DIAGNOSIS OF MULTIPLE SCLEROSIS

The basic research question with which this thesis is concerned is with lay conceptions of science. But given what has already been written, the question is no longer addressable from a point of view that would, in resting on an acceptance of the givenness of expert knowledge (scientific medicine) then approach the depiction of lay theorising in terms of its difference. Rather the question has to be approached directly from the lay point of view, as far as possible looking at how people with MS themselves categorise knowledge of MS in terms of degrees of reliability, looking for what they count as knowledge.

The people in the study knew they had MS. In seeking to understand how they came to know that fact it may be possible to discern how and from what that was distinguished and thus, in effect, what counted for them as definitive, taking the point of diagnosis, for analytic purposes, as marking the achievement of correct knowledge, the taken-as-authoritative statement of what was wrong with them. This chapter therefore focuses on those sections of the interviews concerned with the process of achieving that diagnosis.
There is an irony for an analyst such as myself, whose theoretical auspices are, as evinced by what has been written so far in this thesis, an epistemological scepticism, in producing an analysis still based on data collected in the real world rather than on, say, imagined conversations between fictional characters.

One could of course side-step this irony by saying that I'm not claiming that this analysis is really true, that the talk is evidence of what these people actually think; that this analysis is not claiming any special status in virtue of the data's relationship to a 'real' world. I am, I might say, simply conforming to the usual requirement to base analysis on data collected in the field, complying with that convention merely in order to remain within the discourse of sociology: That there is no harm in going along with it.

But this instrumental conformity has profound epistemological consequences. For the data is very seductive: it has the feel for both researchers and readers of the stuff of life. Before you know where you are it has convinced you that it is reality, reality captured, preserved beyond the moment of its original existence for the remembrance of others who were not there. The people whose talk this is are so immediately recognisable as members, with us, of the everyday. They are describing a world we know to be like that. Ergo, the data not only is the reality of talk preserved on tape, but analysis based on it is about the world outside the talk.

This conclusion is deeply ironic in that it itself exemplifies how we reflexively create reality from what we take
to be the documents of it - and the talk of others is one such docu-
ment.

But this irony is intrinsic: it cannot be expelled from
analysis since it is itself the outcome of the very theorising
which has made it visible; it is rather a matter for 'celebration'
- even though how to express such rejoicing is tantalisingly
unclear.

The traditional form of writing sociology makes invisible
the role of the writer and the reader in creating a reading of a
text of analysis; it displays what is written as if disconnected
from the processes which made it possible. The form of writing
itself creates what can be said.²

What then counts, for the people in this study, as a correct
diagnosis? How, and from what is this distinguished? And what
does this have to say about their concept of science?

The interviews were designed to explore what having MS
meant to individuals and to allow them to tell their stories in
their own ways rather than in terms assumed relevant beforehand
by the researcher (myself). To this end I did not compose a prior
list of questions to be answered or topics to be covered. The
interviews were so unstructured that they might be better charac-
terised as 'conversations' (Dawe 1978).³

To suppose that each interview begins from the same point of
'strangeness' - in the Schutzian sense - for the interviewer is
to ignore as irrelevant the existence of the researcher as a
continuity behind and between each interview. Although the interview may have been the first occasion on which each interviewee represented their experience of MS in this particular form, the interviewer, I the researcher, am present in each of the interviews, cumulatively learning what constitutes 'talking about MS' from one interview to the next, feeding that knowledge back into each subsequent one as the then taken-for-granted assumptions grounding that talk: eliciting 'MS talk' whose appropriateness is self-confirmatory. Although the interviews were designed to be unstructured, what develops over the period of the fieldwork is an increasingly narrow theorising base rather than the presupposition-free situation I was supposing I was providing each time for each of the participants by virtue of planning not to have preformulated questions.

If an interviewer could be the passive recorder of subjects' talk about MS (the objective interviewer par excellence), then this would not matter. But interviews, these conversations, are inter-actions, mutually created by both speakers. What the interviewer cumulatively 'already knows' helps constitute what is said.

And yet to point out this researcher's learning of the MS culture and her active participation in its creation as a phenomenon through the talk matters, is to imply that this is a distortion of some other, more pure, way of researching: is to suppose that there could be a collection of data that was unmediated.

Mulkay and Gilbert (1982) found that when scientists talk about the truth and error of competing scientific findings, they employ a
flexible repertoire of social and psychological mediating factors. These factors are invoked by the scientists to account for what they deem to be errors, but they are not invoked when referring to what is considered to be correct knowledge. This asymmetrical accounting practice, Mulkay and Gilbert argue, allows for and continually re-creates the traditional conceptualisation of science as a body of knowledge which arises rationally and objectively simply from the unmediated discovery of the nature of the physical world.

Can the notion of asymmetrical accounting be used to discern what the people in this study view as differential statuses of knowledge about their condition? If this asymmetry should be found to be present in their talk, does it work similarly to preserve people's faith in the truth and power of scientific medicine; a faith which, from the long history of failure both in the case of the disease in general and their own cases in particular would seem to be fundamentally undermined?

For an analyst seeking to discern the accounting practices of individuals which may work to manage the challenge to their confidence in knowledge posed by the phenomenon of MS, it is also an occasion for reflecting upon how analysts settle these challenges for themselves. How is it possible for me, as analyst, to account for how people accounted for their knowledge of their condition?

Since this analysis is interested at a general level in lay conceptions of science, it begins by looking at how the people in the study initially accounted for their going to the doctor rather than anywhere else when they first thought there was 'something wrong'.

For to ask the general question pre-supposes that it is from doctors as experts that the most authoritative knowledge will be sought. The focus will then be on those sections of the interviews which relate the process of discovery of the diagnosis of MS. Examination of the data shows, however, that several distinguishable categories of incorrect and correct diagnoses were used by the respondents rather than Mulkay and Gilbert's simple dichotomy between correct and incorrect.

The analysis is organised around these categories, viz: Doctors' 'incorrect' diagnoses; patients' incorrect diagnoses; patients' 'correct' diagnoses; and doctors' correct diagnoses. The inverted commas are used as a textual device in order to indicate the difference between what were deemed apparent and actual instances of correct and incorrect.

4.1 GOING TO THE DOCTOR/ALTERNATIVE MEDICINE

Bearing in mind Mulkay and Gilbert's analytic device of asymmetrical accounting, are there differences in how the patients accounted for their recourse to medicine and to its alternatives? First, then, what was an interactionally sufficient way of introducing 'going to the doctor' as topic? The following extracts show how, typically, this was introduced.

Daniel: 4 [symptoms] and I didn't know what it was and the doctors.. first I went to an eye doctor because that's what was worst affected.

Anne: This [symptoms] lasted, went on for about two or three weeks. I thought well I'd better go and see the doctor.

Des: but [symptoms] so I went to the doctor and I saw the elder statesman of the practice and he...
George: [symptoms] just didn't make sense to me and I went to the doctor you know and..

Each account comprises a list of 'signs of something wrong', immediately followed by reference to "the doctor" and what the doctor said or did. The decision to go to the doctor - rather than anyone else - was simply reported. The reason lies implicit in the sequencing - first symptoms so then doctor. What is taken to be the most natural requires the least explication (Sacks 1974).

The symptoms were taken as signs of 'something wrong', the documents of an underlying reality whose nature was not known by the patient although its signs were discerned. The respondents thus had knowledge of 'the facts' but not what the facts meant. It was because "I didn't know what it was" (Daniel) and it "just didn't make sense to me" (George) that makes the hearable implication, with what is spoken of next - "the doctor" - that the doctor would obviously possess the knowledge that they lacked (Woolgar 1980).

The interactionally adequate reasonableness of the related course of action - symptoms followed by going to the doctor - relies also on members' intersubjective knowledge of the relation between a conceptual category 'symptoms' and the category of persons, 'doctors', with whom it is natural to consult.

That one will inevitably have to discard the vast majority of the data so painstakingly collected is an admonition commonly proffered to neophyte sociological researchers. The tapes used for this study consist of over twenty-five hours of talk. The quotations used here constitute only a fraction of that mass. The practice
of selective quotation is one way of coping with such over-abundance; and that selection is usually passed off as a solution to a merely technical problem. Yet what does the practice of selection assume? Firstly and crucially that an extract, a few lines extracted from their original site in the whole talk, can be read independently of that context; and second that they can adequately 'stand for' a set of 'similar instances' in the whole corpus of data.

At one level this is sheer delusion, as known by all afficianados of playbills which so judiciously quote from press reviews. But even granted the faith of the reader that I have not practiced quotation quite so creatively, the matter remains fundamentally problematic. For the in-principle indexicality of documents (here, extracts of talk) is (only?) managed in practice through reference to context, context and extract reflexively producing meaning through their juxtaposition (Garfinkel 1967). Therefore extracts divorced from the contexts in which they originated and inserted into another - the context of my analysis - would seem to be particularly vulnerable to a radical distortion of their original meaning.

But to make this objection is to propose that they have an original meaning which is fixable - even if fleetingly and reflexively-tied-to-context; that there is, potentially, a crucial difference in meaning between the same words in their original context and in another.

Taking each quotation from its original site in a person's whole talk and re-placing it together with others in a common
context, the context of analyst's topic, facilitates the interpretation that the quotations are, singly and together, about the topic: going to the doctor, for example.

But the in-principle flexibility of re-interpretation is not unlimited. For it would be difficult to select just any quotation and, by the same process of recontextualisation, make it work as relevant to any (analyst's) topic. The extract:

Meg: I do respond to cortisone very well so that I suppose my own sort of peculiar psychological defence is that every time I get it [attacks of MS] I'm going to have cortisone, I'm going to get better.

would not work as a convincing account of 'going to the doctor'.

Why not?

One way for an analyst to discern what may be a noticeable absence (here the absence of reasons provided for going to the doctor) is through comparison with similar yet different instances. Respondents referred in other parts of their talk to alternative medical practitioners, for example to acupuncturists and faith healers. Was a different kind of account given for going to these alternative resources?

The greater complexity of the accounting practices concerning recourse to 'alternative medicine' both requires and is demonstrated by the necessity for longer quotations from the data.

George: Someone said try faith healing, I tried him and he did nothing. Mind you I have no faith in anything before I went so you know...and basically being scientifically minded myself, I think that's the way it's [a cure] going to come. [-] I went to an acupuncturist, not an acupuncturist, a faith healer. He
moved me around and asked me you know he sort of twisted me in various directions and said what was the major problem and I told him and he said, sounds to me like MS. Oh, he was the one, thinking back now. Yea, I think in the meantime I had decided that's what I had

AW: So you'd seen the television programme

George: I'd seen the television programme, then I went to him and he really confirmed my beliefs.

* 

Meg: From my own point of view the most difficult thing about having an illness like this is that everybody has a cure for you [-] all of which sound fine but if you, I mean if you can picture a situation where you've come out of hospital, you're feeling fairly shattered, you you're not you definitely don't feel as strong as you used to feel and you get this barrage of advice and you must try my osteopath, you must try my this and and [-] and 'a' you find it difficult to resist it but 'b', which is much worse, is that - now now whether this is sort of conjectural or whether this is true - but I was left feeling that if you don't do it, then it's your fault that you're so ill [laughs]. So I went through a stage where I just thought, well, I had nothing to lose [-]

AW: And how did that end then?

Meg: Oh well because I was talking to my neurologist about it and I said that I found this quite a pressure and he said well fine, he said, well look he said, if you come across a faith healer or a homeopath or anybody else who can cure this please ask them to get in touch with me because believe me, he said, I'll go along with anybody who can cure you. And so then when people sort of wanted to do things I just ask them if they would mind talking to my neurologist, and that sort of stopped it all.

Meg provides an elaborate account for her resort to alternative medicine - the grounds of social pressure from others combined with her initial vulnerability to such pressures. By contrast, talking to her neurologist appears the normal thing to do, requiring no explanation. By appealing to 'proper' medicine, she eventually
managed to resist the pressure. Even though her neurologist was clearly prepared to listen to any genuine alternative ("anybody who can cure you"), Meg implied that the people who had pestered her were too afraid that their alternatives would fail to stand up to scientific scrutiny to take up his eminently open-minded offer. The implication is that should alternative medicine be endorsed by a medical professional, then Meg, like her neurologist, would have accepted it; but it was unlikely. Real medicine is the natural and final arbiter.

The data also included an account of why an individual did not go to alternative medicine:

Daniel: Yes the whole thing with that [the 'laying on of hands] is you believe in it. It may well have an effect for people who believe that whatever charlatan offers them something is going to work. Maybe it will. But since I think they're charlatans I'm not going to accept that they might work, and therefore they won't work for me. Which leaves me a rational loser in an irrational environment.

For both Daniel and George (quoted earlier), the efficacy of alternative medicine depends on a person's belief in its power. They both invoke 'science' as the reason for their own lack of such belief. George's comment suggests however that being "scientifically minded" is just one of his personal quirks, a personal rather than a universal faith. Certain treatments are not considered either credible or effective because they are grounded on an alternative knowledge (not-science). It is not that he regards science as an absolute truth about the world, just one that he 'happens' to believe in. Daniel's comment allows for a similar interpretation: that his own belief in rationality
which I am conflating with 'science' as a possible hearing)

paradoxically excludes him from what could be, had he a belief
in it, actually effective. Daniel spoke elsewhere of the power
of illusion in western medicine:

AW: I mean maybe that's got something to do with the
expectations of people in the medical profession

Daniel: oh yes

AW: that we tend to believe that they can do miracles
anyway

Daniel: and in some doctors it's certainly encouraged, that
sort of witch doctor attitude. Well, and again, the
witch-doctor thing, the best doctor that I had any
dealings with was the consultant in neurology at the
UCH, a man called G, and he's a real witch-doctor
man, full of shaman technique and so on, fostering
the illusion that he actually can do something for
you. Now is that really bad? Because as any witch-
doctor will tell you, the illusion is all. And
people are cured with illusions so...

This on the one hand implies a relativist view of medical knowledge
rather than a universal one; yet on the other, that unless the
person has faith in the illusion - or rather sees it as real
rather than as the illusion it really is, it is ineffective. So
although there is an acknowledgement that knowledge is relative -
that in principle there are many ways of knowing - the effectiveness
of knowledge is possible only when the grounds for that knowledge
in particular are also believed in: a belief that it is not possible
to create at will.

We can see then that 'going to the doctor' is a course of action
that is spoken of as simply arising from the fact of having
symptoms: doctors are the obvious place to go for knowledge of
what they signified, for expertise. By contrast, 'going to alternative medicine' was accounted for by the invocation of intervening factors - social pressures and temporary vulnerability. Although it was said that both alternative and scientific medicine required a person's faith in order for them to be effective, it must be noted that the necessity of having 'faith' in science was not invoked when people were speaking of the initial going to the doctor; only as part of accounting for their actions vis à vis medicine's alternatives.

None of the respondents claimed it would be sensible to have gone to alternative medicine to settle the matter of what was wrong with them. Alternative medicine was portrayed only as a source of possible alternative treatment, not of alternative diagnoses.

The distinctions between what was counted as medicine and what alternative medicine are themselves constituted through this analysis. That which is accounted for by people's invocation of the necessity of faith as a prior requisite for its effectiveness or by the invocation of intervening social or psychological factors is, by analyst's definition, what is countable as alternative medicine. That which is presented in the talk as requiring no elaboration beyond that implicit in the sequencing of symptoms-so-doctor becomes what alternative medicine is alternative to: scientific médecine. The possibility of distinguishing between two kinds of knowledge and the capabilities of those two kinds of knowledge are created by the process of this analysis:
and demonstrated because they have been demonstrable.

At the same time, the grounds for the analyst's proposition of the possibility of there being 'two kinds' of medicine relies on her member's knowledge of the possibility of this kind of distinction. She 'knows' that medicine means (in some sense unexplicated before the analysis but explicable through it) a coherent, identifiable, scientific body of knowledge and practices; and that there are alternative knowledges and practices which purport to be relevant to bodies and their conditions but which are premised on different, non-scientific, theoretical grounds. Thus the analysis and what is analysable - the phenomenon of scientific medicine and its alternatives - are reflexively bound together.

4.2

DOCTORS' 'INCORRECT' DIAGNOSES

The fact of MS, taken as the fact of the matter, is for these people a fact only (and obviously) to be sought through doctors' knowledge. Given the exclusive power granted to scientific medicine to classify accurately what is wrong, how did the respondents account for the fact that they also said their doctors had, in every case, initially made a diagnosis other than that of MS?

Des: I saw the elder statesman of the practice and he diagnosed iron deficiency

AW: Oh that's a good one! Southampton doctor was it?

Des: No it was a doctor up the road, Doctor G.

AW: So what did he give you for that?

[-]
Des: [-] I saw the other doctor, Doctor R, later on and he, he's got seventeen MS patients so he knows all about it.

* 

Jane: ..but they did lumbar punctures and tests like that and they were all clear, and they put it down to hysteria because [recounts her history of mental and physical stress at that time].

* 

George: He [the doctor] said that probably I'd just overdone it you know, too much walking. But I'm not the sort of person that overdoes exercise and I thought perhaps it was just me.

* 

Although his first doctor made an 'incorrect' diagnosis, this was accounted for by Des in terms of the fact that the doctor had lacked direct experience of MS. 'Lack of experience' works to limit the potential generalisability of this criticism to all doctors to just this one individual, Doctor G. With sufficient experience of MS (as "Doctor R" had had) doctors were capable of knowing what was not iron deficiency. Thus Doctor G's inexperience was confined to him as an individual; it was Doctor R's knowledge which was typical of doctors in general.

George also invoked experience as the grounds for his doctor's mistake, but in this case it was the doctor's insufficient experience of him, George, as a whole person, in terms of his habits of physical activity. Since the doctor did not (and could not have had, it is implied) have this whole picture, he could not appreciate that the grounds for his diagnosis - "overdoing things" - were unfounded. George recalled concluding that it was, then, probably "just him", which seems to mean that since his
symptoms could not have been the result of over-exertion, and since the doctor had offered no other reason than that for them, they "must" have been due not to disease but to some weakness in himself; the responsibility for the mistake comes to rest not with his doctor but with him.

In Jane's case, the 'incorrect' diagnosis of hysteria was even less of a mistake on the doctor's part since, according to her account, given the symptoms she was displaying at the time and her then recent history of stress, no doctor could have known that it was really MS. And "the tests" (the lumbar puncture being a commonly used indicator of the condition) had in any case been done but had showed nothing. In the fullness of time the doctors would know what had been wrong all along (as indeed had been proved to be the case three years later). Thus the diagnosis was not incorrect; but a reasonable conclusion given the circumstances.

Accounting for 'incorrect' diagnoses, then, introduced intervening factors to do with deficiencies of particular kinds: an individual doctor's lack of direct experience of other cases of MS; insufficient (but understandable) knowledge of the patient as an individual; the absence of unambiguous symptoms; an insufficient elapse of time for the disease to have developed clearly. These deficiencies are all presented as the upshot of mediating factors which - only temporarily - obscured the relation between the evidence and its true significance: they are not presented as grounds for regarding the diagnoses as really incorrect. And most critically, they are
either deficiencies in the knowledge of specified individual
doctors or deficiencies in the disease itself - in the sense
of its notorious ambiguity. The mistakes are accounted for in
a way that preserves the efficacy of doctors and of medical
knowledge per se.

The possibility of these diagnoses being erroneous comes from
the knowledge that what was really wrong was MS, both at the
time being recalled when the 'incorrect' diagnoses were being
made and at the time of the interview. I selected these people
for interview in the first place because they were 'people with
MS'. Consequently their talk was understandable in terms of
that basic fact. In the light of that knowledge, held by both
of us, any other illness was known at the time of the interview
not to have been the case. My response to Des, "Oh that's a
good one" speaks both to our knowledge that iron deficiency was
not the correct diagnosis and to a wealth of other 'incorrect'
diagnoses of which I had been told.

That there is 'something wrong'; that it is possible to
'know' what 'it' really is and therefore also that it is possible
to be wrong about it, are all assumed unproblematically by both
interviewer and interviewed - and now analyst. Arriving at a
diagnosis comes off in this talk as a technical matter of, as in
these extracts, achieving sufficiency of contextual knowledge to
understand 'the facts' (the physical signs of something wrong)
to make up for what was once lacking. The fact that, in the end,
the right diagnosis was made provides, as a prefatory statement,
for the possibility of this analysis which makes distinctions between 'incorrect' and incorrect, 'correct' and correct knowledge.

By means of their accounting practices, the respondents constituted doctors' mistaken diagnoses as not having been really incorrect. That category was reserved for their own attempts to find out what was wrong with them.

4.3 PATIENTS' INCORRECT DIAGNOSES

Daniel: I actually looked, somebody I've forgotten who, did it for me. It wasn't my wife, somebody else, read up something in a medical handbook thing. Some other mad disease. But I had a lot of the symptoms of it certainly and it was a very very nasty thing. And when I went to the doctors and asked if I had it they just laughed

AW: Did they say what you had then?
Daniel: Yes they said stop reading those books!

George: I mean I hadn't gone into other illnesses at all. I hadn't even thought about...the only thing I'd heard before this, I mean I'd heard of MS, was muscular dystrophy and so I thought well, I wonder if its muscular dystrophy. In my mind.

Meg: Oh no well I started off, I was absolutely, I knew it was a brain tumour, I mean absolutely convinced. [...] I was absolutely petrified. I mean I, and that sort of blocked everything else out because I was so sure I was going to have the, that awful brain scan, they were going to say yes, it's a tumour, because because of the paralysis.

Each of the incorrect diagnoses mentioned were of diseases which
the person now viewed as 'worse than' MS - a brain tumour, muscular dystrophy and even syphilis (extract not quoted) - all frightening and terminal diseases.

Accounts of their own incorrect diagnoses were often produced in the context of speaking of their relief on eventually hearing from the doctor that it was MS that they had. Thus their own attempts at self-diagnosis acquired from the context in which they were spoken of a kind of added frisson in that those imagined complaints had been more dire than the actual one - an exaggerated and now faintly silly over-dramatisation of what was really wrong. By this contrast, medical knowledge gained both credibility and a kind of benignity, for its diagnostic pronouncements had always turned out to be more optimistic than those the patients had thought they could reach by themselves.

Although the sources from which the respondents had drawn their versions of what was wrong were still regarded as reliable - it was not that medical dictionaries contained untrue facts - they were acknowledged to be, in hindsight, an inadequate substitute for real medical knowledge. Medical knowledge was thus held to consist of something in addition to 'the facts', a something by definition not available to themselves as proven non-experts. People said they had sought the name of a disease which would explain the whole range of their symptoms, a disease of which their symptoms could be understood as sensible documents, based on their knowledge - crucially flawed as it had turned out - of physiology. It was the grounds which link facts and explanation together, the theories behind medicine, that they had been unable
to supply and which had lead them - in spite of following the correct method - to draw their incorrect conclusions. The doctors' ability in the end to make the correct links - from the same facts - maintained and reinforced the interviewees' view of experts \textit{qua} experts, and of themselves as mere amateurs.

George gave this version of ignorance an added twist. In our interview he said he had very early on seen the letters "DS" written on his physiotherapy card, but he had not understood that the initials stood for Disseminated Sclerosis (an earlier and now almost entirely superseded term for MS). Now, however, he could see that the true diagnosis had been available to him long before he had finally known that he had MS. This demonstrates that knowing also entails a prior understanding of what that knowledge could be - in order to recognise it as knowledge.

Arriving at the correct relationship between their symptoms and the disease of which they were the manifestations required, as the respondents' accounts show, an underlying theoretical ground which would bind these two necessary but not sufficient elements of knowing. For the people, though, there was in the end an independent external criterion by which to adjudge the accuracy of diagnostic attempts - the correct fact of MS.

There is no equivalent external criterion in sociology with the facticity of 'having MS' to settle the supremacy of one theory/account over another. Thus for me as analyst there is no way of knowing for sure whether the links made in analysis between the documents - the data - and the features noted in the analysis
which have lead the analyst to draw the conclusions she does, are correct.

It is possible to refute the contention that this is a handicap unique to sociological analysis. One way would be to argue that "the disease MS" is not, actually, an external criterion independent of the discourse of its discovery but that it, too, is constituted by discourse. The 'fact' that the symptoms and the disease, when matched by an expert, make sense is to say no more than that experts count these symptoms as indicating MS and that MS is the name given to that which is constituted by those symptoms. If these symptoms are not co-present then ergo it is not MS but something else. The certainty bestowed by the existence of an external criterion depends, according to this line of argument, on an externality which is in fact spurious. To argue this way would be then to say that medicine - one discipline within science - can really be no better than sociology at achieving certainty of knowledge. We only appear to be less able to be certain because science claims to be more so. Demonstrate that medicine is no more firmly grounded and it will be stripped of its false facticity. A familiar argument.9

But what about the current analysis? I am using people's talk as raw data and, through the use of my expertise as analyst, demonstrating features not visible to the people themselves, but claiming that they are features which the people themselves rely on in order to produce their accounts. Only the analyst can 'see'
what lies behind. I am implying, if not explicitly claiming, a
special status for my own analysis.

But it is I who have proposed both that patients accord
themselves lacking in the expertise necessary to do accurate
diagnosis and that this 'expertise' is itself a closed reflexive
loop. Both the analysis and that which is to be analysed are
inescapably my creations through this text. Inescapable because
there is no other way of writing. I have to speak for both the
data and for myself.

Can I then also say that the patients themselves provide
their raw data and their analysis of it? Is it possible to say
anything? What cannot be said?

4.4

PATIENTS' 'CORRECT' DIAGNOSES

Although as I described earlier respondents admitted they had
made mistakes, they did not portray themselves as altogether
ignorant. They may have made incorrect diagnoses, but they were
also capable of arriving at the 'correct' one by themselves.

George: I found out - I was watching a television programme
and it wasn't a documentary or anything - and there
was something on about MS in it and the, I thought
these symptoms are very like mine. Afterwards in
fact I went to a book, a medical dictionary, looked
up to see what MS was and read all about it and thought
that's me. Definitely. So I went to my doctor and
said have I got MS? And he said yes.

Meg: The reason I switched to MS [as being that which
was wrong] was very simply I was on the private floor
at the National and in the next ward was a woman with
MS. And I wandered in to talk to her one day [-] and
she started to tell me about her disease and I thought
my god, that's what I've got. So that's why I asked
her [the GP] - if I had MS.
I saw one evening by chance I saw a programme on television and I saw these people I thought god, they look like me. And so it came to my mind that possibly I'd got this disease you see. So when I went to the hospital, confronted them with it, they said no, definitely not. They were really adamant about it in fact. [ ] This was about three years after I had had this lumbar puncture so they were adamant about it and I was, well what could I think other than the fact that it couldn't be, then. And then after two years they decided that I might have it.

In these instances, the interviewees' source of knowledge was a direct comparison between themselves and people who they knew had MS. But these striking similarities were not thought sufficient to count as knowledge that they themselves had MS. They each went to their doctors for confirmation. In all three examples, the doctor's response was presented as an essential pre-condition of the factual status of their suppositions; the people themselves could only know the epistemological status of what they thought they knew by virtue of medical arbitration. John's account strengthens this point. He first accepted from the doctors that he had guessed wrongly, then subsequently that he might after all have been right all along. But this did not mean that the doctors had been wrong; merely 'wrong'.

There is, it should be noted, a striking contrast in the elaborate accounts people gave for how they themselves arrived at their 'correct' diagnoses and the monosyllabic verdicts they said their doctors had produced: "and he said yes" (George); "they said no" (John): an asymmetry which itself works to bear witness to the stumbling and hesitant seeking after knowledge that people themselves managed and the precision of the doctor's verdict.
At one time I was very struck by a metaphoric resemblance between the idiosyncratic patterns of individual's figures of speech, the substance of their stories about their MS, and their personalities. I wondered if it would be possible to use such noticeable speech patterns as indicative evidence - good evidence because it was "unintended witness" (Marwick 1968) - of how each person dealt with their MS. The symmetry held convincingly in every case. For example George's constant use of the phrase "you know" - appealing to a hearer's understanding and approval of his statements - tied in nicely with his readiness to admit his own misunderstanding of what his symptoms might indicate ("I thought it was just me") even when he had had clear evidence of what was really wrong with him (the "DS" written on his medical card); and what seemed, throughout his talk, to be a particularly passive acceptance of his disease. I was able to produce for myself a highly detailed correspondence.

But now, given that my theoretical auspices have changed from an earlier 'interactionist' perspective to a concern with the "methodological horrors" with which all knowing is ineluctably involved (the in-principle reflexivity, indexicality, inconclude-ability and defeasibility of all statements (Garfinkel 1967; Woolgar 1981)), it is 'obvious' that I would have noticed this symmetry. After all I could find this symmetry because of the reflexive relationship between reality and what I took to be the documents of that reality. I was reading George's character from what he said, taking his talk as documentary evidence of what he, underlying those documents and giving rise to them, was like;
understanding what he said in the light of what I deduced his character to be; noticing what was idiosyncratic about his talk because it expressed and revealed what he was. There were/are no independent criteria; they were/are all reflexively constituted by me as hearer/analyst.

The auspices for my theorising when the symmetry first struck me and the different auspices for my theorising now created both what was then and is now 'obviously' discernible in the data.

My current theorising auspices also provide grounds to account for the seductive quality of people's talk as data. Part of its immediately recognisable 'reality' is that we take talk to be the documents of the selves of others. The people talking on tape seem real because we are practiced in the deduction of people from their talk. There may be other documents too - bodies, gestures, smells, textures - but no medium that is any more direct; no way unmediated. All we have to go on are the documents of selves - behind which to read the selves of others: and our own.

DOCTORS' CORRECT DIAGNOSES

By comparison with the accounts of the other categories, the eventual achievement of the correct diagnosis is recounted as curiously matter-of-fact by those who at the time of the interviews were in posession of what they regarded as a definitive diagnosis of MS. The fact of MS is as simply reported as 'going to the doctor' was in the first place.
George: So I went to my doctor and said have I got MS? And he said yes.

Des: But I think he his doctor sussed what it was quite quickly [-] I think actually he told me on my birthday what it was.

Anne: [I went to see] the doctor and he then arranged for me to go and see this consultant at UCH and by the time I got to see him, you know how long it takes to get an appointment, it [the symptoms] had gone. And he in fact diagnosed it then but he didn't tell me because you don't know whether it's going to come back again. And then about a couple of years later when I started getting symptoms and went back, and he told me.

The fact that the correct diagnosis was eventually made, and the facticity of what it was - MS - is merely reported rather than 'justified'. However, crucial distinctions between "doctors" (GP's) and "consultants" are being made here. The status of correct diagnosis was in every case in these interviews reserved for the pronouncements of specialists, made either face-to-face or conveyed via GP's to patients.

The only way in which "consultants" are criticised - and for which accounts are proffered - is over the question of their telling or withholding the diagnosis from the patient: that is, not their knowledge per se but what they did about it; a criticism made of consultants not in terms of their expertise but qua persons.

How the experts eventually decided that it was MS is not detailed here - but evidence of the expertise that decision involved is conveyed by recounting the length of time since the first symptoms;
the different stages that had to be gone through of hope and despair; the visits to GP's, hospitals and so on. The sheer arduousness of the pursuit contrasts with the laconic reporting of the final verdict. The efforts of both patients and the medical profession also work to reaffirm that diagnosis is an exclusively expert skill.

These factors of time and effort are also involved in accounting for the lack of arrival at a diagnosis, so far.

John: [I have atypical symptoms] which has been a bit of a complex really, bit of a red herring to the hospital, what with having the polio of course [in childhood]. So they weren't quite sure what was the matter with me. It took them quite five or six years before they came to the conclusion that it might be MS because they can't find anything else. And in fact I still have to go back into hospital within a week or two for other tests. Although they confirmed it was MS, they still think there's an outside chance it may not be.

Meg: And it's really only in the last, oh I should think in the last six months that I haven't cared that it doesn't have a name or that no-one wants to give it a name [-]. I mean because for me it just wasn't, it obviously is some sort of demyelination and I don't mind what they call it.

AW: So how do you feel about them not actually acknowledging that it is MS? I me-, are you a hundred percent sure that it is?

Meg: No, no, because I think that MS has become 'how long is a piece of string'? My own feeling is that in twenty year's time there won't be a diagnosis of MS. I mean I think it umbrella's a whole lot of things.

John's account provides for the re-appraisal of the validity of the diagnosis by invoking his atypical symptoms and the confusing residue of his childhood polio. The doctors in any case, he said, had only concluded it was MS "because they can't find anything else."
it was always a merely contingent diagnosis because of the intervening factors. So the re-consideration of his diagnosis due in a few weeks' time does not force him to characterise re-diagnosis as the correction of an error, a doctor's mistake: they had not been sure when they initially made it. Thus this is not a questioning of a correct diagnosis: the epistemological validity of diagnosis is not undermined by his account, nor is the expertise of doctors. They had always known enough to know that they didn't know for sure.

Meg invokes the problem of taxonomy to account for her own case. What the doctors had diagnosed - a "demyelinating disease" - was both sensible and sufficient and not incompatible with the possibility of it turning out in the end to have been MS all along whatever, she says, that means. Her scepticism is thus addressed to the question of the re-definition of boundaries of what is to count as MS. She does not fundamentally question the basic conceptualisation of the disease, nor the doctors' abilities to apply its taxonomy correctly.

These accounting practices portray what might have seemed to have been a fundamental confusion on the part of doctors as merely the result of peculiar, non-typical circumstances in their own cases and of the long time period that has to elapse before certainty can be achieved. The doctors' not knowing becomes the mark, not of ignorance, but another example of their expertise.

All the accounts concurred that the authoritative diagnosis of MS is merely the revelation of what truly is the matter. By means of their special expert abilities and knowledge, doctors
are uniquely able to see clearly through the documents of appearance to what lies beneath them - multiple sclerosis.

There are at least three ways in which 'MS' is working as a prefatory statement here. First, the respondents' accounts can be seen as pathing towards the known-at-the-time-of-the-talk but not-at-the-time-being-referred-to true fact of having MS (Smith 1978). The indexicality of what they say is contained by the eventual diagnosis, in the light of which everything else makes a particular sense and acquires a particular relevance from the tale of that pathing.

At the level of making sense during the interviews and on earlier re-hearing of the tapes, I as analyst knew I had made the same prefatory statement by my conducting the interviews with these people in particular, because I 'knew' them to have MS. Detailed attention to the data shows how much that could have been questioned on the grounds of its relevance to the story of having MS was passed over as interactionally, for the moment, adequate - because the talk by definition was 'about MS'.

At the level of this current analysis, the prefatory statement is available that this analysis is about people accounting for MS and for various versions of knowledge about what was wrong - in particular, knowing the diagnosis and differentiating it from guesses and mistakes.

This is additionally complicated by my references in the analysis to my recall of earlier, different, thoughts about this material - rememberings which cannot perhaps be 'counted' as themselves data for this chapter because they were not recorded - a
conventional prerequisite for what may be counted as data.\textsuperscript{11} And yet all that process too is an intrinsic component of how I am re-reading this material now; that and all my other knowledge about MS, medicine, sociology, research, the world — an inconcludeable list — and which are all unrecorded (invisible) yet constitutive grounds for my theorising now.

All these levels reverberate together and are only arbitrarily distinguishable on the basis of chronology — this reading, my analysis, the conversations, the times the interviewees were recalling, and so on — for the purposes of this current writing.

I'M NOT A DOCTOR

There were in the interviews occasions when people directly referred to the fact that they were 'not doctors', references that may be used to elucidate what being a doctor, and expert, was held to entail:

Daniel: I'm not a doctor so I'm not going to say anything about certain facts — but I think there is some reason to believe that ACTH [a steroid used to control attacks] is a good deal less than I was origianlly led to believe that it was, let's put it that way. [—] But I, I don't really know anything about medicine therefore my opinions are not really worth having.

Anne: I think I've really said all I know about it really [MS] I mean I'm not an expert in it by any means because I haven't plunged myself into it properly, to sort of delve into all the sort of research. 'Cos you know it's a major field in itself.

George: No I feel that I mean I'm not saying that I know as much about MS as doctors do, but I feel that I know enough to help myself you know — but there are some
doctors I have no faith in [-] a couple I've got no respect for whatsoever. Some of my friends have become doctors; if I was to walk into a surgery and find them sat the other side I'd think, cor, I remember what you were like when you were younger. How on earth could you treat people? [-] You can't convince [some doctors] that they could possibly be wrong. What they say is right. And what right have I got to - he's a doctor after all. That's his view. I didn't say it was my view. [-] OK so they know, alright they must know more about the illness than I do but they don't know more about me than I do I'm sure.

*  

Daniel's scepticism about the effectiveness of the steroid treatment which appears to be contesting medical facts is however limited by his phrase: "is a good deal less than I was lead to believe," an implication that it is a matter of belief rather than fact that is open to scepticism. George makes distinctions between doctors' knowledge - "about the illness" and doctor's opinions. His experience of doctors before they qualified introduces the notion that doctors become doctors after they are persons, and thus, behaviours such as stubbornly clinging to their own opinions is a quality of the person, not the doctor. It is their weaknesses as people that make them want to be right in all things, even in matters that, by George's account, are not within the domain of their medical expertise.

George claims a domain of expertise of his own - knowledge of himself as a unique individual. This has been George's continual theme, that doctors can't and don't know all relevant matters about him - matters which are relevant to the course of his disease but
which do not arise from it. This lack in what doctors can know gives him the right and freedom to act without their advice and to accept or reject what they say according to whether he thinks they are speaking on the basis of their legitimate medical knowledge or of their personal opinions. In the realm of opinion, doctors and patients are equally non-expert.

There is evidence from the transcripts that patients do make distinctions between themselves as lay and doctors as expert although these are couched in some telling caveats which delimit what 'not being a doctor' and 'being a doctor' entails - which allow them to make some authoritative statements themselves. But these do not work to undercut the expertise of medical experts, only to provide for the fact that not everything doctors say arises from that expertise. However, these distinctions are not divided between 'lay' and 'expert' persons; instead there are distinctions between kinds of knowledge and persons. 'Expert' and 'individual' are not co-terminous. Thus the patients give the domain of facts up to the doctor; but it appears that 'facts' are not co-terminous with 'knowledge' either (as was said in relation to the patients' accounts of their inability to make correct diagnoses out of valid facts and information). It also seems that 'facts' are not accorded an exclusively or necessarily higher status than other kinds of knowledge; facts have a contextually contingent status. It is notable that it is Anne who gives the most self-effacing grounds for not calling herself an expert in MS; she is herself a protozoologist by profession and thus, being a scientist, is
aware perhaps of the fine distinctions within science which would be insider's knowledge of what an expert in MS would have to know. (This is rather like the fact that it takes a sociologist to be aware of what it would entail to claim to be an expert in, say, conversational analysis).

It is not, then, that doctors are experts and lay people in-expert. The domain of expertise is delimited. Outside that domain doctors may be subject to or suspected of the weaknesses and pressures of society which impinge on all its members. The patient is himself an expert in the details of his own case of the illness - the atypical, idiosyncratic features of it, a domain not available to the doctors, even with their expertise in medicine. Correctness and incorrectness then are shifting, conditional, contingent categories for the interviewees: but they are categories which shift between persons - they are knowledges residing in, arising from the experience of individuals, be they doctors or patients.

4.7 BELIEF IN SCIENCE

But what of medicine beyond the point of diagnosis? There is no cure and precious little effective treatment available for MS. Yet in the teeth of what their own experiences revealed to them of the inabilities of both doctors and medicine to grapple with their condition effectively (even though, in the end, it had been named), the respondents retained a faith in a science which will save them. This is a faith in a knowledge uncontaminated by the exigencies of the personalities of doctors or by mediating
social factors: arising as a possibility through the a-personal, a-social power of pure scientific enquiry.

At least this was what I thought was in the data. Careful attention supplied something rather more complex.

AW: ..the scientific way of looking at the world, is that your belief? Is that how
Anne: I don't really know
AW: I mean do you feel it has a rational explanation, MS?
Anne: yes
AW: it's not random or
Anne: at the moment it's pretty random, but I feel that ultimately the, it will be put on a much firmer scientific basis when more is known about the cause of it and how to combat it.

Meg: and I do respond to cortisone very well so that I suppose that my own sort of psychological defense is that every time I get ill I'm going to have cortisone I'm going to get better and that'll go on I suppose until they find a cure - is probably the way my mind thinks about it.

George: I'm reasonably optimistic, not that I think they'll find a cure, I mean I'd like them to find a cure obviously but I don't think they'll find a cure but I do think they'll come up with something either to stop the progression of the illness or something in the next ten years or so. From what I've read, from what I've heard and the sort of general feeling, I mean, but you can't predict those sorts of things. I mean I could be wrong, completely up the creek.

Daniel: I'm quite confident that medical science will [find a cure] [-] Oh yes they'll find it, they'll find it. I'm sure they will. But for example there was a doctor [that we met] by absolute chance. He worked for among other places the Bethesda Naval Hospital
a big American and he was a specialist in all things MS [-] he later on in the evening said what his research was based on and he said that well I don't think that in our time - and he was fifty years old - we're likely to see a discovering of what is the cause of it. Various possibilities, but it's so difficult to work on because of the remissions and so on but anyway, he said, the most interesting one we're working on at the moment is this triact-something and I said what do you mean? And he said well your blood plasma [-] well he says we've had some striking results with that. Not yet, not that we could confidently suggest it for general use, but one of the problems is that our machine - and he laughed - blew up. One of the patients, killed him.

Really, we can joke now. Well for the doctors it's a joke of course but for a hypothetical patient, not so funny. But anyway, as far as the MS is concerned yes I think MS, that medical science will...whether the atom bomb gets there first I don't know.

It is only Anne, herself a scientist, whose response expresses complete confidence in the power of science to cure MS - "when more is known about the cause of it." For her it is just a matter of time.

None of the others are so unequivocal. Meg refers to her expectation of a cure being found as part of her psychological defence against full recognition of her situation, made possible to sustain because medicine seems to be in control of her attacks. George's hopes are modest, for control rather than cure, but he simultaneously implies his own lack of qualification to make such a prediction ("I may be completely up the creek"). Daniel's expectations of science are - with his story of the Bethesda experiment and his reference to the atom bomb - couched in the knowledge that what science can do may be too powerful: either at the expense of individuals or indeed of humanity.

Thus although they each declare a belief in the power of
science, they also produce accounts of why it may fail - but this is failure in the specific case of MS rather than an impotence of science per se. Yet the very faith they express in science is a faith grounded in their own hopes and their need to hope; and as such, their predictions are biased by their desire for them to be true.

It is in this way that 'science' enters the epistemology of the everyday. It is according to science that subjective desires contaminate the development of pure knowledge. In the light of what they take to be an axiomatic dictum of science, people are unable to say for sure that science can save them.

4.8 ACCOUNTING FOR THE DIAGNOSIS OF MS

These patients' accounts of the different categorisations into which knowledge may be divided are strikingly tautological. Beginning with the premise that what is wrong with them is indeed MS, a knowledge which is the pronouncement of scientific medicine, any knowledge that resulted either in a different conclusion or that came from a different source was by definition either incorrect or required expert confirmation. Doctors were the natural people from whom to seek correct knowledge; knowledge had thereby been gained and ergo it had been the right resource from which to seek it.

What appeared prima facie to have been doctors' mistakes were accounted for as reasonable given the circumstances, and in any case were limited to failures of individual, specified doctors rather than doctors per se. The respondents' own mistakes were
attributed to their own lack of knowledge — in particular, their tendency to leap to conclusions because of their fundamental lack of theoretical grounding which turns knowledge of individual facts into true knowledge. Doctors had been able, in the end, to deduce what was meant by those very same 'facts'. The patients found it possible to make intelligent guesses, but in every case, they needed expert confirmation before they could become accepted as the true fact of the matter. The experts were those who could make the correct diagnosis — of MS — the "specialists", the "consultants" and those who could, as in the cases of Meg and John, knowledgeably state that they did not (yet) know what was wrong.

The axiomatic distinction between expertise and non-expertise enabled the maintenance of faith in a medical science disembodied as it were from both practitioners and its subjects. Their scepticism was confined to their own abilities to be expert, either about themselves or about what science could do. Although researchers could "laugh" about the explosion of an experimental machine and be condemned, as persons, for so doing, the knowledge science was held capable of producing remained in these accounts both immune from mediating factors — and demonstrably potent.

For science, and only science, had been able to describe 'what is': another case of MS.

But how has this analysis come to this conclusion?

The data I am using is part of a discourse about a disease, MS, but a discourse between lay people and a sociologist, myself, not between scientists or between scientists and researchers, (as is more usual in the sociology of science).
A major interest in considering this data is, for me, how it may bear on researching the lay use of a concept of science. In looking for talk to use as data for that project - talk about science - how, since it is talk divorced from the persons and sites of scientific institutions, are certain sections of the talk discernible as being 'about' science? And what should that 'about' include - the methods, the data, the theories, the knowledge of science?

Although 'medical science' appears to feature strongly in the data, in the talk both of myself and of the people with whom I was speaking, how can instances of it be picked out as being certain talk's topic without pre-supposing what is to count as 'talk about medical science'? Our talk seems to provide for a hearing that our topic is substantively about knowledge: what is known and by whom and from whom knowledge may be sought. One could say that all along we are displaying the project of research into MS.

The interviews were once criticised for my clearly not having known what to ask, the grounds for that 'knowing' being proposed as something I should have already known, given my training as a sociologist, my expertise: that is, the straight application of sociological interview methods to yet another topic - MS - would have told me what I needed - and ought - to have asked. That is to say, keep the form the same and the content will take care of itself. Indeed, keeping the form 'the same' will produce...another sociology of... To get away from the
kind of product that is the result of the traditional form involves paying attention to what form itself does: to the inherently reflexive relationship between form and content, theory and substance, method and findings.

The question of how it might be possible to escape from a priori assumptions about what medical science, multiple sclerosis, are, is a question that requires an answer only if it is believed that such answers are both necessary and in-principle available. It is the pursuit of definitive answers that turns questions into problems. Questions though may be an occasion not for angst but for wonder.
Readers may wish to refer to the Introduction—xix—xx.
CHAPTER FIVE
THE TRANS-FORMATION OF TALK

Paul Ricoeur (1981) has argued that the human sciences may properly be regarded as appropriate for hermeneutical exegesis insofar as "meaningful human action" which, he says, is the object of their explanation and understanding, is in several fundamental ways analogous to texts:

(T)he human sciences may be said to be hermeneutical [. . ] inasmuch as their object displays some of the features constitutive of a text as text[. . ] and inasmuch as their methodology develops the same kind of procedures as those of Auslegung or text-interpre- tation.

Ricoeur 1981: 197

However, his argument is unnecessarily circuitous for while it may be agreed that "meaningful human action" is a substantive object of sociology (the human science in question here) - certainly in the kind of research project with which this thesis is concerned and arguably in (all) others - the data are not analogously but actually texts.

This is not to say that 'all human life is text'. Rather that in order to be worked upon, 'raw data', (talk, observations, human action) is always first transferred to the written form. And it is that written form that is analysed.
According to the literature, talk, speech, "orality", in that it is "fully natural to human beings" (Ong 1982: 82), is both historically and ontologically prior to writing. As such, it has special qualities of unmediated presence: to be present to people's talk is to be in the closest possible "proximity" to they themselves. Analyses based on talk, on speech, are assumed to inherit this proximity, to be dealing unmediatedly with its subjects. To analyse their speech is the same thing as to analyse the beliefs, actions, behaviours of the speakers.

The act of transcription of talk is generally taken for granted as a matter of pragmatics, the fixing in writing of what has occurred. But what is then also taken for granted is that the record and the original talk are the same thing:

I shall now provide some transcribed pieces of talk and employ them for a discussion of some elementary procedures for analysing activities. Although I still plan to travel a little further with Austin, I think the journey will be more eventful if it leads us to confront the concrete problems of dealing with actual bits of interaction. (my emphasis)

Turner 1970: 203

But as I propose to show, the act of recording and the subsequent act of transcription are not procedures which simply transfer talk to a convenient form but are acts of constitutive transformation which have profound consequences for analysis. For there are many ways in which, being fixed by recording, speech, talk, the raw data, becomes something other than talk, both in itself (whatever that could mean) and in terms of its place in analytic discourse.
In order to convey how strange the taken for granted isomorphism between talk and transcript is, an actual tape recording should be played here, followed by a transcription. But because this is a written text the tape 'itself', let alone 'the talk' cannot be present; the difference between them has to be conveyed through a single medium, through writing, through absence.

There are two aspects to the constitutive transformation to be considered here: one is to do with the tape record itself, the selection and fixing of a fragment of the world. The second is to do with the subsequent move from the auditory to the visual form: from the heard to the written.

5.1 TALK TO TAPE

What counts, what is allowed to count as data is only that talk which is recorded on tape. Thus talk that is authentic in that it has the essential quality of ephemerality (Ricoeur 1981: Ch 5) is by definition excluded from the body of data.

Although it is every researcher's experience that talk occurs between researcher and researched before and after what has been preserved, none of that, though part of the interaction, may count for analysis.

By being recorded, any possibility of further things that might yet be said, as more always can be, is closed off. Nothing can be added and nothing taken away from what is on tape. Further-
more, what remains, the substance of what can be re-heard not only is fixed but becomes shaped by that enclosure: shaped as narrative. To quote Ricoeur, at first quoting Aristotle:

'Now a thing is a whole if it has a beginning, a middle and an end'; but it is only in virtue of poetic composition that something counts as a beginning, middle or end. What defines the beginning is not the absence of some antecedent but the absence of necessity in the succession. (The ideas of beginning, middle and end) are not taken from experience. They are not features of some real action but the effects of the ordering of the poem. 

Ricoeur 1984: 38-9

Here it is not, "in virtue of poetic composition", but analogously, the fact of starting the tape, letting it run for so long, then switching it off, work to order that which would otherwise not have had that order: confers form. By having a beginning, a middle and an end, the talk is formed as if "a whole" and as such seems to consist of a completeness (here a sufficient account of MS) because the tape is complete.

Talk becomes reified by being fixed on tape and thus in another way unlike talk. For talk, if one draws on experience, is not, cannot be, accurately remembered - the precise words each person said and in what sequence. Rather what happens is that there is a rememberance, if it is thought about in retrospect at all as a discrete 'talk'; a wild paraphrase which does not distinguish between the actual words spoken from what each thought the other meant, interpretations of what lay behind the appearance of what was said perhaps; the other's intentions, deceptions, significant silences, things not said; interpretations made in the light of
what was already known about the other and revealed as a result of that conversation among others. Such a paraphrase, a loose translation based on recalling not the words but the understood meaning of what was 'really going on' would be closer than an exact record to a representation of the work of talk in the world.  

By being fixed on tape a further violence is done - this time to the instant death of all speech at the moment of its being said. By virtue of being recorded, interactions are lifted out of the instant of the present in which the talk is occurring into any time, all time; into the orbit of any hearers.

Methodological texts on interviewing techniques draw attention to the possible effects on the participants of knowing that their talk is being recorded, as a source of possible bias/influence that must be taken into account during analysis. But there is a record (sic) of a different kind of 'knowing' about the tape. Daniel has just said he thought that the first prodromal symptoms of his MS began when he had gone to Africa:

AW: Did you go there as an adult or did you go there as a child?
Daniel: No I was an adult. Went there at the age of twenty-one first and of course back then many times afterwards. I lived there I suppose in all two calendar years spent in Africa.

AW: Where were you?
Daniel: Senegal in West Africa, Ghana, the Congo as it used to be called. I first went there to join my father when he was in Katanga and then I got interested in Africa and wanted to study it and so
AW: I taught in Uganda for a while, I did VSO
Daniel: Where, where in Uganda?
AW: Kako, that's outside Masaka
Daniel: Gosh I don't know Uganda at all I
AW: It's only a village
Daniel: Where is it, in the north or
AW: No on the west coast, west coast of Lake Victoria
Daniel: of the lake, yea. Well we I guess shouldn't consume the tape's valuable time.
AW: No.
Daniel: but er gosh - it's terrible what you hear about that place now. Aaah. But you were there, what years...?

The words I have underlined say something not of tape-induced self-consciousness or inhibition - but of the public presence, a third party of possible hearers of the tape to whom the talk is directly addressed and for whom the talk is taking place; silent yet present participants in the talk, for whom a discussion of the political tragedy in Uganda is acknowledged by both to be irrelevant.

Not only is there this third presence actively present in the dialogue, there is also, in "the tape's valuable time" elliptical reference to the presence of the future; the speakers are speaking into the future beyond the temporality of the present in which the talk took place, the future embodied in the presence of the technological apparatus for the talk's preservation.
Already, then, by being fixed on tape, what has become fixed is not merely an accurate record which conveniently holds in place what has now passed, but in several crucial ways has thereby a form and therefore a content which is a consequence of that form. A form, that is, not selected by an author for its aesthetic effect (Aristotle's poetics) but a conferred structure that resonates for readers with, for example, narrative; stories which, furthermore, in that they are addressed beyond the other person present, confers on 'the original talk' one of the characteristics not of talk but of the written.

5.2 TAPE TO TRANSCRIPT

In the literature however speech and writing are conceived to be fundamentally different (Ong 1982; Ricoeur 1981 for example). The comparison is however as between two discrete phenomena: talk-intended-as-talk and writing-intended-as-writing. The practice of transcription however is the process of reproducing, replicating in writing what was once talk. The transcript is neither writing nor talk, but writing-standing-for-sound.

Perhaps musical notation is an illuminating analogy through which to pursue this difference. In the composition of music, what is fixed by the system of notation (notes, tempi, instrumentation, key, rests and so on) is, in performance, turned into sound. What is necessary on the score are sufficient instructions with sufficient detail to enable the piece to be played. In transcription from the sound of talk to its being written down, this process is reversed in that the music, the aural noise,
comes first and by the conventions of notation, pinned to the page: the written fixes the sound which comes before it. To pursue the musical analogy a little further, no score, however, replicates any performance of a work; a performance — and each performance is different — always contains an excess that does not lie in the score itself. The score is only as it were a set of basic instructions upon which a performance is based, which constitute the minimum definitive code for a performance of that work in particular. But the performance is, as it were, always underdetermined by what is written in the score although is dependent upon it.

Putting this round the other way, assigning signs to sounds is not simply a question of correspondence. To hear and to transcribe is to have to make decisions about what is to be recorded.

Whilst at the technical level it could be said that this is because there are insufficient notations — even with Gail Jefferson's exquisite schema (see Chapter Three; Figure 3.1) — for everything that is hearable, that is not the most consequential point. For in the same way that it has become accepted that perception, what is visually perceived, is inexorably theory-laden (Gregory 1977), so is what is hearable. An a priori sense of the structure of a language is necessary in order to be able to hear from a stream of sound words and spaces-between-words (which is not of course to say that it is necessary to be able to articulate the structural rules, syntactical rules, before hearing is possible), rather that which sounds could make up which words
has to be known before they can be heard (like they are). The hearing of a tape is a matter of making sense of the sounds it consists of, and what that sense could be is constituted by what is already known by the hearer as possible to hear, as individual words and as overall sense, each essential to the other.

An illustration of this is provided by a comparison between transcripts of the same tape, produced by different transcribers/hearers. One of the data tapes was transcribed for me first by someone else. The context of the extract, shown overleaf (Figure 5.1), concerns the effects George's incontinence has on his social life; specifically, on going to the pub.

As can be readily seen when they are laid out in this way, the second transcript includes more words than the first; at the end of lines 04 and 14 and at the beginning of line 11. Whilst one could argue that these omissions/additions do not alter the sense of what was being said in any strikingly significant way, the differences in line 14, where the first version omits/the second version adds reference to a wheelchair as the cause of George's shame/embarrassment, whilst the first includes/the second excludes reference to his "ignorance" are of more consequence, especially when such differences will in all probability occur throughout the transcript(s), subtly and not so subtly altering their readable meaning. Of most consequence is that, as written on the page, there is nothing to signal either transcript as being more or less correct than the other. For despite the conflict between the two records of what was said, each is internally consistent.
<table>
<thead>
<tr>
<th>First Transcription</th>
<th>Second Transcription</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>George</strong></td>
<td><strong>George</strong></td>
</tr>
<tr>
<td>01 .I shouldn't be embarrassed I know</td>
<td>01 .I shouldn't be embarrassed I know</td>
</tr>
<tr>
<td>02 but I do get embarrassed about</td>
<td>02 but I do get embarrassed about</td>
</tr>
<tr>
<td>03 having to go out to the loo.</td>
<td>03 having to go out to the loo.</td>
</tr>
<tr>
<td><strong>AW</strong></td>
<td><strong>AW</strong></td>
</tr>
<tr>
<td>04 Why shouldn't you be embarrassed?</td>
<td>04 Why shouldn't you be embarrassed? It's not something</td>
</tr>
<tr>
<td><strong>George</strong></td>
<td><strong>George</strong></td>
</tr>
<tr>
<td>05 'Don't worry about it old chap',</td>
<td>05 'Don't worry about it old chap',</td>
</tr>
<tr>
<td>06 you know, that sort of attitude.</td>
<td>06 you know, that sort of attitude.</td>
</tr>
<tr>
<td>07 I can't help it it's just something</td>
<td>07 I can't help it it's just something</td>
</tr>
<tr>
<td>08 that's in me and it's an embarrassing</td>
<td>08 that's in me and it's an embarrassing</td>
</tr>
<tr>
<td>09 thing having to say excuse me</td>
<td>09 thing having to say excuse me</td>
</tr>
<tr>
<td><strong>AW</strong></td>
<td><strong>AW</strong></td>
</tr>
<tr>
<td>10 I think it's a</td>
<td>10 Is it shaming?</td>
</tr>
<tr>
<td><strong>George</strong></td>
<td><strong>George</strong></td>
</tr>
<tr>
<td>11 in a way I'm embarrassed about my</td>
<td>11 Oh no not shaming. In a way I'm embarrassed about my</td>
</tr>
<tr>
<td>12 ignorance I suppose yes because</td>
<td>12 illness I suppose yes because</td>
</tr>
<tr>
<td>13 I've never really changed. The</td>
<td>13 I am in a wheelchair. The</td>
</tr>
<tr>
<td>14 first time I went anywhere it</td>
<td>14 first time I went anywhere in a wheelchair it</td>
</tr>
<tr>
<td>15 was a horrible experience.</td>
<td>15 was a horrible experience.</td>
</tr>
</tbody>
</table>

**Figure 5.1**
An even more striking example of this managed consistency comes at a later point in the same two transcripts of the tape:

FIRST TRANSCRIPTION

01 John yes, lose their balance, I've never lost my balance or indeed my powers of putting my finger where I want, where I can reach. I can't move myself easily.
04 That's what I can do.

05 AW They don't think

06 John They don't think so, no, the hospital doesn't, no. They don't think it's the polio, no. I saw a specialist six years ago and he thought it could be the polio.
09 He too didn't think it was MS.

10 AW He said it's very very rare

11 John It's very rare but it does happen, it does happen. That's what I can tell you. Otherwise I feel OK,
13 I feel weak

14 AW So your eyes aren't affected at all?

15 John No.

16 AW So you can move around?

17 John No my arms are affected, I can't walk, I can stand but I have to hold on to something, I'm not very strong.

SECOND TRANSCRIPTION

01 John Yes, lose their balance, I've never lost my balance or indeed my powers of putting my finger where I want, where I can reach. I can't move myself easily.
04 That's what I can do.

05 Tina They don't think

06 John They don't think so, the hospital doesn't, no, no

07 AW They don't think it's that
They don't think it's the polio, no. I saw a specialist oh six years ago and he thought it could be the polio. He too didn't think it was MS.

He said it's very very rare

It's very rare but it does happen, it does happen. That's what I can tell you. Otherwise I I feel OK, I feel weak.

So your eyes aren't affected at all or anything?

No.

The arms, you can't move the arms

No, my arms are affected

His legs, he can't walk

I can't walk, I can stand providing I'm holding on to something but I'm not very strong.

Most strikingly, the first version of the transcript records an exchange between two people, AW and John. In the second, there is a third participant, who enters at 05. However the first version reads as a script perfectly sensibly (although maybe more awkwardly once the anomaly is pointed out). The transcriber has managed to allocate the third person's utterances intelligibly between John and AW, with a few crucial adjustments here and there: I can't walk at 17 to he can't walk at 19 in the second, for example.

By referring to what I remembered had gone on and who had also been actually present (Tina, John's Italian wife), I was able myself to make the correct transcription. Although in this second example I have used it is implausible that my recollection of the numbers of speakers involved is the incorrect one, the conflicts in the record of the words of the utterances, relying as they do
on a less gross level of recall (and see above re. the remember-
ance of the meaning of talk rather than its words) are much less
easily resolvable by reference to my own memory. By the compari-
son of these two versions it is clear that the production of a
sensible transcription - in both cases - is indeed a production
and not the replication of 'pure sound'. Meaning has been (and
has to be) brought to the act of transcription in order to make
sense. Indeed, if transcription was merely a matter of carefully
notating what is heard independent of meaning then it ought to
be perfectly possible to transcribe tapes in for example Serbo
Croat or Swahili.

Sound and sign-for-sound are not simply co-terminous. Written
words are simply not the sounds that make them up. Pitman's
Shorthand provides an interesting example in that it is a sign
system which claims to be entirely based on phonetics and is in
everyday use - from Parliament to the ubiquitous office: provenly
practicable.

Shorthand is the art of representing spoken sounds
by written signs. Pitman Shorthand provides a way of
representing every sound heard in English words.
Ordinary longhand spelling is seldom phonetic. Pitman
Shorthand is phonetic; that is, words are generally written as they are sounded and not according to ordinary
longhand spelling. (My emphasis)

Pitman Shorthand New Course: viii

But this theoretical claim cannot be sustained in practice. For
to hear spoken words simply as combinations of sounds has to be
arduously learnt and it is as I know to my cost, excruciatingly
difficult not to be distracted by the visual images of words in taking dictation. 19

But even taking the rubric of Pitmans at its face value, that rubric assumes that the sets of sounds that will go together to make each word, and the spaces that will separate one group of sounds from another are unambiguously self-evident. By thinking of what I can only hear as a continuous undifferentiated flow of sound when overhearing people speaking Gudjerati, for instance, it is clear that they are not. Furthermore, which of 'the same' sounds turn back into which words has to be done by reference to the meaning contexts: where $\exists$ records here or hear, where $\bigwedge$ is to be transcribed as where or were or ware - and so on.

These are still, it could be objected, technicalities that only make transcription problematic if the object is to produce a precisely accurate record of talk, the object perhaps only of Conversational Analysis or linguistics which are concerned with structure; that generally speaking, in practice it is clear from the context which words are appropriate. But of course 'the context' is also that which has been/has to be transcribed.

Transcription operates not just with phonetics, the sounds of words, but with meaning, which has to be ascribed to the sounds. And in that ascription lies the space in which literal transcription becomes impossible. The a priori necessity of knowing what it would be sensible to have been said makes it clear that this is not merely a technical matter, but rather of consequential
significance. For, excepting CA and linguistics, it is the
meaning of talk, the speakers' meanings, that are the object of
analysis. It is not, thus, just that transcripts may be differently
interpreted to produce different meanings, but that between
the hearable 'data' and the written record, interpretation has
already taken place.

What about the translation now not into words but the trans-
formation of sounds into a visual form, signs, marks on a page?
Writing, the placing of words on a page, is by definition linear -
even if the lines go backwards vis à vis written English - and
sequential. Talk it may be said is also linear and sequential;
but the two media cannot precisely map each other. Conversational
analysis takes a close interest in the turn-taking procedures of
sequential speech, as has been referred to in Chapter Three. On
the page the basic form of, for example, question and answer
(adjacency pairs), interruptions and overlaps are indicated by
transcription conventions:

(4.a.2) [Her:01:7:5-6]
Barnaby:  Oka'y?
Merrie:  O: I'key?
Barnaby:  'hh (.) Fair enough?=
Merrie:  'h
Barnaby:  → =I Wul?
Merrie:  → The be st'v luck.
Barnaby:  ← Once again:
Merrie:  → 'hh h
Barnaby:  'thnk you?
Merrie:  I-

But the sound of overlaps cannot be exactly represented: the nearest one might get in the written form would be, perhaps:

Daniel: and when I went to the doctors and asked if I had that, they just laughed.

AW/Daniel: Did they say what they had then? reading those books.

which is unreadable. The space of a page is only two dimensional, a flat plane. The space of speech is three-dimensional - or rather only metaphorically space at all - in which simultaneity of sound is not a problem. 20

Daniel: and when I went to the doctors and asked if I had that, they just laughed.

AW: Did they say what you had then?

Daniel: yes they said stop reading those books.

Being transcribed onto a flat plane, speech appears ordered, consisting of an order that is an artefact of that visual space. In the scoring of music this space is ordered differently: the parallel lines of the staves running from left to right across the page mark the whole horizontal plane of the page as a simultaneous time-space. Sounds therefore can be represented as being made together in the same time; notations of the individual voices carried by each instrument or family of instruments produce music in the play of one sound or absence of sound against others. On transcripts, the simultaneity of voices or of spaces, the pauses of no-speech, cannot be represented, only indicated linearly and sequentially in a written text of speech. And in that difference, the ordering imposed by the conventions and limits of the visible imposes a particular form on talk that has consequences for its re-reading.
One consequence is that each speaker is assumed to be addressing the other: that each utterance, to use CA's term, is in response to the previous one and directed towards that other, with important consequences for interpretation. An example of the power of the illusion that this might be is Garfinkel's 'counselling' experiment, in which one turn-taker used only a randomly selected but binding response repertoire of "yes" or "no" in response to clients' questions (Garfinkel 1967: 76-94). Another might be the ELIZA programme (Weizenbaum 1966: cited in Boden 1977), the possible consequences of which assumptions are described with some relish in David Lodge's Small World (1984).

A further consequence is that, as the grouping-and-separation between groups of words on the page seem to mark, there is an assumption that there is more than one speaker present (and cf. the two versions of the tape transcript presented above, pp 11-12). To introduce an example not of transcription but from analytic writing, in Chapter Four, 'Accounting for Accounts of Multiple Sclerosis', I was trying to represent two different aspects of the same voice simultaneously. But, because of the linearity of the written, these had to be placed on the page sequentially. This has then been read as representing two (or three) separate voices in dialogue, equal but competing interlocutors (see Mulkay 1985: 77, fn 2), a fundamentally different reading than the one I had been trying to evoke of a single voice speaking and questioning the grounds of that speech at the same time.
The visual appearance of transcripts unavoidably embody a representation of time passing vertically down the lines of each page, the time of the talk which is not 'real time'; a stately unidirectional flow along the arrow of time. This works to make talk appear always to be moving onwards, away from, and towards, developing from what went before as in an Aristotelean cumulative progression from ignorance towards enlightenment (Blum 1974). Unlike speech as it is occurring in which anything may happen in the time-yet-to-come, the physical presence not only of the words being read at any moment but of all the words that are yet to come and have already passed, being within the field of vision and anticipated in the pages present and yet to be turned, remind readers that what is to come has already been fixed; what might have happened already has.

The point at which one is reading vis-à-vis the proportion of the pages already read and those yet to come lends to the words on any page a place in the "whole", if the whole is taken to be, as I argued earlier, resonant with the form of a narrative. First the tape and now the text confers form on 'talk'.

In talk, the identification of who is speaking which words is usually distinguishable by the sound of each individual voice (and in any case witnessed by the other's moving jaws). In tape recordings, this self-identifying presence and difference between speakers in the timbre of each voice remains (although they may be difficult always to distinguish). In both modes, presence is embodied in voice. But in transcription, notation has to be
brought in by the transcriber from as it were outside the interaction in order to indicate on the flat plane of the paper, in the margins of the page that lie outside the talk that is being represented, whose talk each utterance is.\textsuperscript{25} 

AW: Did you have any theories about 
Jon: Yes I thought I might have had a tumour on the, in the brain 

AW: Aaah. What gave you that idea? 
John: I don't know, it was the only thing I could think of. 

Tina: Your spine 
John: Not the spine, I didn't think of, I thought my brain actually, but they said I was clear, nothing at all, couldn't find nothing so I went back regularly every two weeks for check ups and I got progressively worse, very slowly, very very slowly.

This continual, necessary, externally applied allocation of words-to-speaker provides on the page a framework that distances the reader from 'presence', and constitutes a concrete reminder of an outside to the talk that interferes with the flow of hearing/reading as if they were one, placing the reader with the writing, outside, conscious of the frame that the written demands, its artefactuality. The practical solution to the absence of self-identifying voice in transcripts in effect mediates between the spoken and the read, the speaker and the hearer/reader.

This constant reminder - "AW", "John", "Tina" - of a writer who must be producing the written (giving instructions how to read), and of the flat visual plane of the text as other to the space of speech works something in the way that according to
Phillipson (1985), the written signature of the artist on the flat plane of a canvas:

(S)erved to remind us of what we already know very well - that the space was indeed a constructed space, the site of an illusion, and not a reproduction of the world as 'it really was' outside the painting. The signature faces us with the paradox that the painting's image lies behind or beneath the signature, while at the same time lies within that same image; in our act of reading the signature, this apparently mundane convention affirms that the painting is other to the world.

Phillipson 1985: 105–6

The form of the transcript, the signature of the writer on the page yet absent from the interaction reminds readers that the transcript is not the thing itself - the 'raw data' - but something that has been made of it.

Thinking of space now in another way in talk, the presence and absence of sound which constitutes the noise of talk, the spaces between the words cannot be replicated in writing. Although in Conversational Analysis for example the pauses in talk are measured and marked:

(4.a.3.) [GTS:1:2:57:R:1]
Roger:  
I: 
H:APPEN duh wea:r (.) bl:ue jeans constantly.  
(0.2)

( ):  
'ë.h
Ken:  
Well
(0.2)
Roger:  
Even i:n
Ken:  
so do I no:w,
(0.2)

Roger:  
formal occas:ions yihknow hheh'hhn

Jefferson 1984
(0.2) for example or (.), the tiny spaces that by definition must exist to mark one word from another are not. But in its representation, the time marks fill in the spaces with a measurement of time which becomes not the representation of the absence of sound but again a reminder of another presence, an intermittently appearing commentator or instructor how to read which again, like the notation of speakers, is brought into the text and reminds readers that the talk which the transcript appears to replicate is a managed construction.

In hearing live speech, variations in tempo, in volume, of absences of sound, all add to the richness of the interpretive repertoire. Although the spaces - the pauses - are marked and measured on CA scripts, the tempo of the words, the sound, is not marked. It would be possible to play around with the idea of this, perhaps by writing the transcriptions on paper graded with time spaces - a graph or a matrix in which each square could stand for an element of time - and maybe thus speech, or at least its tempo could be visually represented in its presence, absence and consequent harmonics. But still this would not be talk.

To again invoke Phillipson, it could be said that writing, the written, can never recover talk; that talk cannot be subdued by the written word but has to remain something other (1985:161). What that 'other' is is embodied in the means used to attempt that unachievable recovery.
One could remark, invoking Lyotard (1979) that transcription turns talk into a commodity, data to be sucked dry as it were of its use-value, as evidence for something else that method demands analysts say. If these scripts were not texts but merely data they would, once analysed, have nothing more to say, no possible or necessary re-readings (Nisbet 1977). The idea of data for use and then discarding rests, however, on the assumption of a single meaning and ignores the endless indexicality of texts. One could say, with Sharrat (1982) that the written always has a surplus value. It is always more, in excess of any reading of it. Data, re-recognised as datatexts, go on offering further occasions for thought, a possibility paradoxically opened up by speech's seeming violation by being fixed and trans-formed.

If the original talk cannot be isomorphically represented but undergoes a doube transformation, first by being fixed as a permanent record and then by being transcribed into the written, then what it is to 'analyse data' both demands and allows for a radical rethinking. For, as I said at the beginning of the Chapter, what is available for analysis is not talk but texts.

But it is not that much has been lost in the transformation, but that much may have been gained. For to reconceive of transcripts as texts is to open up profound possibilities; for it shifts the work of analysis into the discursive field of hermeneutics and of literary theory which may offer other ways
of thinking about the aporia produced by the concept of reflexivity which threatens, if recognised in its full ubiquitous seriousness, to reduce sociology to either silence or self-immolation.
PART TWO

RE: READING DATA
CHAPTER SIX

READING AGAINST FORM : OF THE QUESTION OF THE SELF

The assumption of readability, which is itself constitutive of language, cannot only no longer be taken for granted, but is found to be aberrant. There can be no writing without reading but all readings are in error because they assume their own readability. Everything written has to be read and every reading is susceptible to logical verification, but the logic that establishes the need for verification is itself unverifiable and therefore unfounded in its claim to truth.

de Man 1979: 202

But if the dtexts are texts, what kind of texts are they, and how does that affect their interpretability? Prima facie, they differ substantively from the 'object texts' and the concerns of both philosophy and literary theory. They are not fiction, if fiction is taken to mean writing which is primarily the product of the creative imagination, about invented lives, invented situations, where the concern is with literary effect. Neither are they philosophy. Although they may involve and have a bearing on philosophising, a concern with the nature of understanding and of knowledge, they are not philosophical texts, concerned with pure argument.

For sociology, here, the essential question of the dtexts concerns their epistemological status, their status as knowledge; and therefore their relation, qua texts, to their sources of authority, to the
knowledge of people with MS. For it is their epistemological status which will constrain the problem of their interpretability.

This may be pursued as a question of their relation to their ostensive referents, a question which is perhaps uniquely three-fold: to the selves whose speech they are, to the lives they are about, and to the event, the interview of which they are the record and in which the knowledge was produced (each relation already problematised in Chapter 5).

As a way of opening up the question of this three-fold problem of reference (which will also be a way of considering the difference data makes), the first fold of the question that will be addressed is their relation to the speakers whose speech the dtexts represent. For the status of the knowledge that the dtexts are taken to represent (the basis of their interpretability in its particulars) depends first of all on its relation to the knower, to their putative source, the self who is speaking about him/herself.

As a way of beginning, the dtexts will be read against what has been called the key question for contemporary philosophy or, to be more precise, that philosophy which in having taken its route via phenomenology, has had at least historically, an intersection with the route taken by ethnomethodological sociology: the Question of the Self.
6.1  

THE QUESTION OF THE SELF

In his Introduction to a collection of papers by a number of French philosophers (including Derrida and Bordieu, Lyotard and Ricoeur) which were written in response to his question of what it is to be a philosopher today, Alan Montefiore\(^2\) identifies the question of the self as one of the only "motifs" he was able to find common to all the responses. This question appeared not so much as the objective of philosophy, but rather as a problematic which was involved in the very possibility for the philosophers \textit{qua} philosophers, of describing their own work:

\[\text{If, for instance, the possibility of a subject or agent conscious of itself as such is bound up with that of its own self-expression or articulation through concepts: if the possibility of the formation and meaningful employment of concepts has to be understood as in effect the possibility of language: if language has to be seen as essentially social or public in terms of its own primary possibility: and if the meaning of what is said or thought at any one moment lies as much in the ways in which it differs from what else, within the whole open range of possibilities that language may provide or suggest, might have been said or thought as in the words that have in fact been pronounced in the thoughts that have in fact occurred: then the networks of relations, both social and conceptual, on which the practical and constitutive possibility of language depend, or in which they consist, must always precede in order of both logical and temporal priority the formation of any individual consciousness as such. The subject, and \textit{a fortiori} the subject as individual consciousness, is thus displaced from its role as source and author of meaning. It has to learn to look upon itself as a secondary or derivative phenomenon dependent for its own self-conscious existence on those networks of meaning that precede it – networks which are themselves, moreover, never completable, never closed, never definitively systematizable into assured and self-consistent unities.}\]

\textit{Montefiore, 1983: xvi}
For the rhetorician Paul de Man, literary criticism has a unique contribution to make to thought about this difficulty:

Because it implies a forgetting of the personal self for a transcendental type of self that speaks in the work, the act of criticism can acquire exemplary value...Literary criticism, in our century, has contributed to establishing this crucial distinction between an empirical and an ontological self; in that respect, it participates in some of the most audacious and advanced forms of contemporary thought.

de Man 1983: 49-50

The contemporary questioning of the concept of the self which Montefiore found arose from the consequences of theorising, a result of doing that philosophy which takes its own practice as that which also has to be questioned. For de Man, it emerges through the practice of the critique of literature and the development of literary theory.³

Philosophers, by their own definition, write at the level of the abstract, a hermetically sealed language game, rooted only in language and the force of reason and logic. Literary critics write about what has been written by others, about literary works. But in both their analyses, mundane speech or common sense understanding is constantly invoked as an essential buttress to their arguments: what 'people' would say or understand.⁴ It is rare for these discourses to draw on texts documenting the way people talk, away from the discourse of academic theorising. Rather, in these two disciplines, the question of the self is a question which is debated with free recourse to invented examples from 'real life', and otherwise confined to the writings of other philosophers, other critics. This constant invocation of "ordinary
discourse" (Martin 1982) and what everybody knows about it, stated as if it were irrefutable, is an argumentative manoeuvre which is highly irritating, driving one to want to counter-claim that such assumptions are unwarranted; to say that ordinary discourse is simply not like that: drives one back into a corner where resort to empirical data (transcripts of talk) would be produced in counter evidence, and thus back into making the claim for a special status for properly empirical data and therefore for analyses based upon it. Although I have discarded the possibility of taking talk data in such a way (Chapter 5), nevertheless there still remains, even having relinquished any claim for an exemplary faithfulness of data to its referent upon which empirical work is premised, a difference between that writing and that of the other two disciplines. But what difference? What would happen if one asked what 'the self' was contemporarily held to be through re-reading the dttexts, first person narratives, ordinary discourse that whatever else they may be, are not invented?

The dttexts are particularly appropriate for such a consideration because, as a place to start, one could propose that it is not only the ontological self which MS deconstructs, but the unity and order of the material body, that sine qua non of an empirically conceived individual, which is violently and involuntarily disrupted by the distortion or the numbing of the very medium of the empirical self - the Central Nervous System: that which according to modern medicine (and underlying much sociological
discourse) is the essence of sentience, the medium of self-awareness, of life at all.

According to Goudsblom:

The democratization of the humanist tradition has placed the 'nothing is true' theory within everyone's reach. Perhaps in earlier centuries the odd individual had similar problems. The psychological and social constellations which make people receptive to the problem may not be new: what is new is the existence of nihilism as a common cultural property, ready to be incorporated into society and personality in all kinds of ways.

Goudsblom 1980: 179

To read the dtexts now is to see clearly laid out all the essential ingredients which would make the irruption of MS and the failure of science to produce effective knowledge of it (See Chapters 3 & 5) the ultimate exemplar not only of ontological but of epistemological nihilism.

And yet there is in the dtexts no chaos, nothing that resembles the abyss. There is no pain: indeed everything is 'just normal, just life, you know' (George). The worst is always over, or somewhere else, never in the present, the present of the dtexts' time. Yet that remarkable absence of chaos is also 'in' the dtexts. This extraordinary absence of 'madness' of nihilism, is pace Goudsblom, paralleled in academia - although there too maybe the acutest sufferers have become silent.

And yet to 'use' the dtexts, talk about MS, is also utterly inappropriate. For academics, for those sociologists of science whose programmatic response to the methodological horrors is to write to 'celebrate' reflexivity, their writing, their analyses,
are something that they do. Both they and the subjects whose
talk they use (scientists) are voluntary practitioners of the
knowledge game. For philosophers,\(^7\) for literary critics, the
question of the self is literally academic. But to have MS is
not (pace Chapter 3) a breaching experiment, that is set up
and performed for an analytic purpose and terminable at the will
of the researcher or of the subject: it occurs by default not
decision. It is not academic. It is non-optional, terminable
only with death. Thus for people with MS (as for Nietzsche's
New Philosophers)\(^8\) theorising is not an intellectual pursuit
practiced always within the option of stopping, but something
in which the stakes are of the very highest. For what knowledge
could achieve for them if it were true would be literal life as
against untimely death. What is at stake for them is not an
aspiration to the Habermasian or Platonic "good and true life" but
life at all. With MS there is no option either of retiring from
the questioning or of resorting to silence.

In any case, silence is not a risk to be dared, but for
all the only certainty:

Let us beware of saying that death is the opposite
of life, the living being is only a species of the
dead, and a very rare species.

Nietzsche 1974: 109

To use the dtexts as the occasion, the excuse, for theorising, for
demonstrating the power of or the question of method would be
violently to appropriate them (Phillipson 1985: Chapter 6). Yet
these dtexts are the occasion, in the Heideggerian sense, for
theorising about the very possibility of theorising, sociologically
or at all. For if this kind of theorising is worth anything at all, then it has to be risked concerning such a topic which is not playful: which concerns the literal death of others not merely the metaphorical death of itself.

Having reached Moscow by a morning train, Levin went to stay at the house of his brother-in-law... The professor was engaged in a fierce polemic against the materialists... The question was a fashionable one, whether a definite line exists between psychological and physiological phenomena in human activity, and if so, where it lies...

He had seen in the papers the articles they were discussing and had read them because they interested him as a development of the bases of natural science - familiar to him as he had studied in that faculty at the university; but he had never connected these scientific deductions as to man's original origin, reflex actions, biology and sociology, with those questions concerning the meaning to himself of life and death, which had of late more and more occurred to him.

Tolstoy 1980: 23-24

6.2 THE RELATION OF BODY AND SELF

For Montefiore:

Not the least of the difficulties [of the question of the self] lies in the fact that the principle of individuation of persons as a plurality of inhabitants of a common world of experience would seem to have to lie in the body and its spatio-temporal history.

Montefiore 1983: xiii

Because of the bodily occurrence of MS, the problem of the self would thus most likely arise in the dtexts in terms of the relation between the self and the body: between the ontological and the empirical self, to use de Man's distinction.
A distinction between "ontological" and "empirical" selves is applauded by de Man as part of, "the most audacious" modern thought. But to ask, what is the self? is to ask a question that already conceals part of the problem about this distinction: the copula 'is'. And in so doing, grounds the possibility of seeking an answer to this question because of the metaphysical predicate of the verb which presumes being as the referent of 'is'. [9] Although it makes sense to speak of various levels of the self, simultaneously existing as it were in real time, a continuous sub-text, it, that is the self, cannot be anything but represented in language, read into language by language: and 'is' is part of what language can say. The self which is written down is literally embodied in writing, given a body, its form of existence, by language. What 'is' means, refers to, is unsayable in (except by) language.

According to the dtexts, there is no question but that there is a relationship between the body and the self: but with MS it is one full of contradictions and terrible implications. The overwhelming problematic appears to have to do with the body as the source of meaning of and for the self, with the threatened distortion of its representational function.

6.2.1 (Mis)representation

For according to the dtexts, what the body with MS does opens the self to misrepresentation, opens a gulf between appearance and actuality. What the body appears to be doing is misread by others (although what others then do, the actions of their bodies, is held to be more simply interpretable):
Meg One of the other things that happened to me was that I had something that they call drop attacks so, when I got back the use of my right leg and I was sort of walking OK they um kept sending me out for walks, and I used to get these drop attacks when you walk along and literally the next minute you're on the ground. And you can get up again but I suppose some message doesn't get through to the brain and bang, you go down. And that I found particularly terrifying because you had to do all sorts of things like, you can no longer just afford to run across a road when you see a gap, because if you have a drop attack in the middle of the road, the bus goes over you. Things like that. And when you're coming up and down stairs you sort of think, oh I'd better hold on

AW and all those things like being in public, I mean to actually fall down in public is such a

Meg well I I mean I remember the first time it happened was walking round Queen's Square outside the National Hospital and I could not actually believe that no-one helped me up. I mean everybody looked away I mean obviously through embarrassment but er I was absolutely shocked. I thought everyone would leap to my aid.

The appearance of the body may provoke fear in others:

AW Would you say though that the body of a man is more important to his self? I suppose that sounds contradictory in a sense.

Daniel Could be..

AW and that men are [...] I've just read somebody called Peter Hunt [11] who said that people were afraid of the disabled

Daniel yes

AW because they represented undeniably you know the existential dilemma, everybody's inevitable dissolution.

Daniel Yes. This is absolutely certain. I noticed that when I was, here on this street, kids pointing me out when I was walking around with the stick, when I was first walking around with the stick, it was with real fear.

AW But why that fear should be different for men and for women?
Daniel fear, fear and scorn both. Oh there are women who react similarly. Certainly there are. But all the same I think there's a general law - it's not a law - it's a generalisation that men are... presumably because they're more.. possibly this is complete rubbish but er

AW it feels like there's something there.

Daniel There's something there. There certainly is something there. How to get it into words I'm not sure. I don't know if it doesn't go back to er primitive times. Man the hunter whose body is

AW crucial

Daniel crucial, yes. And to whom aggression is central and that if, and yea, I don't know, and yet and yet. I mean the people that I'm thinking of, that have reacted so, in a in an embarrassed and unhelpful way really in my knowledge and presence, these are all university people - or if not university people but intellectuals of a sort. It's very odd I mean but if the survivors of the hunter-gatherer as it were - you'd not really expect to find in some of those people. But I suppose it could be there. It could.

AW Do you think they fear it?

Daniel I'm certain they do. Yes. Deep down. There's something really primitive there, I'm certain. And well when you get to know somebody really well, or see them a lot. it's forgotten. But with somebody you first meet...you can see it. In gestures, in looks, but yes it's there. In a way I suppose people who are not themselves disabled or obviously ill or seriously ill, people who are outside that there again might not recognise it so readily themselves. But being yourself very ill does sharpen the perception in that area, considerably.

And even in the self; of its own body:

AW Do you think that is one of the things you have to fight when you are yourself told of the diagnosis - that maybe you have that fear?

Daniel Sorry? How so?

AW I wonder whether part of that fear that you have to fight against, or that you experience when you are first diagnosed,
that is, you as a member of the general population, if we're suggesting that there is this very primitive fear of other people's sickness.

Daniel Yes, yes I suppose that's right

AW would also be directed towards your own sickness.

Daniel Yes. Yes that makes sense. I hadn't thought about it before, but you're right.

**

The body appears to represent the self, the person embodied, to others in ways which are untrue:

George I always scare people though when I get out of the wheelchair cos they don't think that you could possibly walk when you're in a wheelchair, so it shakes them.

AW Do you think that they feel you're mentally deficient?

George Oh yea I'm sure they do.

AW So the body and mind goes together. That seems to be a very primitive - I was going to say primitive, but unchallenged belief, really.

George Some people are so bad about it I really feel like doing something to scare them you know.

AW Like what? Something really mad?

George Yea, really bite their arm or something you know, just because of the way they're behaving.

But even when known to be untrue, those 'ways' threaten the idea of the self:

AW I suppose it very much depends whether you or see yourself, that is your own personal worth as diminished by having to go about in a wheelchair and all that symbolises, or whether you really feel that doesn't touch the inside person.
Anne: Well one tries not to. But it does, it does make you feel.....

AW: I mean do you not think about that side of it?

Anne: Oh you do think about it. But one tries not to think about it. One tries to forget it. I mean this is why I don't delve too thoroughly into the literature and why I don't um participate in any MS rallies or outings or anything like this because I don't want to admit that um that I am so different.

Yet the body may also represent the truest account of the state of inner feelings:

AW: ..how did you react when [the neurologist] told you? I mean had you an idea it might be

Des: Well we didn't know what it was. I mean MS was just words. We didn't know what it was at all.

AW: I mean were you terrified?

Des: Well I must have been worried about it. Um cos this headache which I had went on and on for quite a few weeks I think. And it shouldn't have done really. And er the other doctors said it was psychosomatic. I don't honestly know.

Although by definition, that is unknowable for certain:

AW: So did you, did you feel that what you had could be a bodily representation of something that was going on inside you?

Daniel: I don't think so - at a conscious level. There may have been such things lurking at the back of my mind.

AW: But she [a psychiatrist] wasn't interested in looking at that?

Daniel: No. No she wasn't. I asked her if it could be a psychosomatic business and she said, well anything can be psychosomatic but er I don't think in your case... And
I left it at that. I thought that—and obviously to believe that it had been psychosomatic would have been in a sense reassuring

AW Because reversible?

Daniel Sorry?

AW Because reversible.

Daniel Because reversible. Exactly.

Persisting within the body there remains the undamaged self, or at least an idea of it, a perception of a self independent of the body:

Jane I think too if you've been born with a disability you know about it and you've never known anything different. Whereas I wasn't born with this. You, every so often you think 'my god. Seven years ago I was getting on the bus in the little village in Derbyshire, six months pregnant, six bags over each arm, striding up the hill, and then you'd come back and go for long walks.' I mean you remember what you were able to do and you still, that person who was able to do those things is still there in you. And there may be a time when the memories fade and you get used to what you've now become. I don't know. I don't know whether you ever can. I mean I dream a lot about it, about running

AW Do you?

Jane Yes running along the beach and going for long walks you know. I mean really you know, you don't need any Freud to explain that! (..) I'm an active person mentally and I can't, my body doesn't do what I want it to you know. There's another me inside here struggling to get out almost.
The body thus may become an object - of the self's anger for instance:

AW But do you feel it attacks you very personally, having the disease? How you feel about yourself?

George Yes I suppose so, I mean I say to people I'm not bitter but I suppose I am. But I don't know who to take it out on so there's no point being bitter about it. Who can you take it out on? It's no-one's fault anyway. I know I could go up to someone and say it's your fault that I've got this illness, that's fair enough. But there's no-one. You get angry. I get very angry with myself not being able to do something. I probably take it out on me more than anyone.

AW But can you disassociate yourself from your body?

George Yes sometimes I forget. No. I'm not sure. I don't really forget I've got it.

Although a healthy body may remain as it were within the idea of the self:

AW Do you feel that your body doesn't represent you, your internal, private self?

George Yes I do, definitely do that, yes.

AW That it doesn't represent...?

George It doesn't represent me, no. Inside I'm very fit, healthy person.

AW Normal.

George Normal feelings and normal everything and doing what I attempt to do, so I get very angry inside because I can't do them.

AW So when you're getting angry are you getting angry with your body then really?

George Oh yeah.
AW Not your internal self?

George No. It's my body I get cross with really. I'd kick myself if I didn't think I'd miss, this sort of thing. But th that is true yes it is, my inside my internal self is very much fit and healthy.

**

But what may remain as the self is not a pale remnant of the self-that-once-was, but a self which has become refined, purified, the essence of self:

Daniel ..and also it gives you, I couldn't say, a more accurate sense of what to value in life. At least I think so.

AW And also what to value about yourself?

Daniel Yes.

AW essential self?

Daniel Yes, yes. Well for example, put it this way, er...when others around me are not overstrained and so on, I laugh much more readily than I used to - and less maliciously. That would be hard to work out exactly what that means. But I've noticed it. It just bubbles up...and then a self-mocking laughter. Yes.

AW What, at bits of yourself that are er remnants from the old days?

Daniel Probably (laughs)

AW I mean that are no longer so part of you.

Daniel Yes.

Or a self that remains as it was, although with all its characteristic faults and virtues exaggerated:

Jane ..but I'm sure you're going to meet people like that. But they'd be like it whatever comes along.
AW         Yes. Very true. It just seems to make the person more like they were before.

Jane       Yes. True, yes.

or which may have become more gross, more self-obsessed:

George     And meeting other people with MS as I have done recently, I hadn't done really until I went to this ARMS thing in Southampton - not because I was reluctant to, though I dare say in the back of my mind I was a bit reluctant to meet other disabled people; but I discovered that they're all very wrapped up in themselves and all very concerned about themselves.

or more brutal:

Daniel     But apart from that I think that there's er.. an increased precision in one's judgement of people

AW         really?

Daniel     oh yes. One has reason to: there's a need to form judgements of people in the sense of 'will they be any help to me or not?' That brutal sense. And scary. How quickly it can be ascertained.

AW         Do you feel it's made you become more egotistical then?

Daniel     No. No I don't. No I don't think so. No, that latter formulation but I don't think that ...er.. it has a bit, Yes of course it has.

AW         Perhaps I'm using egotistical in the wrong sense. I don't mean in a hubris sort of sense.

Daniel     Yes. No, there is something there. I mean, maybe egotistical isn't wrong.
Above all, reading the dtexts, the problematic of the self induced by MS is a question of representation. The body is as a text, with a structure, a syntax, a grammar, but most obviously, qua body, a text awaiting reading; a document which carries (embodies) meaning, that cannot be conceived of without (a) meaning. That which always has to be interpreted.

To state that MS forces the body to become an object, a text, to the self, assumes that the self and the body are normally presumed to be a unity: and at the same time, that there is always already a hair-line crack down which they can be divisible into two, given the right kind of pressure.

To speak thus assumes a priori that the unity is always already a spurious and fragile bonding of really-two components - the endemic possibility of 'a subject' and 'an object'. Since the body by this definition cannot be aware of itself cognitively (because cognition is the exclusive property of the self), which part may be the subject in the self-body pair and which the object is not itself open to variation. Thus the argument is already foretold, pre-formed by the epistemology involved in putting the proposition in the form of a binary dichotomy: self-body.

It is the reading, the interpretation and translation of the text, the body, which is the ever-present, the never-to-be-forgotten work of those who are embodied in this way.

But the body defeats confident reading. It is a text which, because it is known to have the potentiality (having MS),
to distort any faithful correspondence between that which seeks meaning and a meaning which is found, always defies reading, even of itself by its own(er) reader. Yet it might also be the most reliable document of things that are beyond sensation (the idea of psychosomatic illness where, for Daniel, the body is not itself read but as it were involuntarily acts for itself; that act then readable for the meaning which it alone makes manifest).

The body is understood to be a deceiving text, understood to be such against the idea of an ideal text in which all that has to be understood and explained can be: an ideal text which is itself conceivable only as an opposite to this present far-from-ideal one. But its difference from the ideal is incalculable, because the distorted body cannot itself be fully and stably known (interpretable), that being part of its definition, that which makes the body with MS different.

By extrapolation, if the body is an unreliable text to its inhabitor, then also must that body be to others:

What it seems to be doing, deceives others;
seems to be standing up and listening;
seems to have fallen down through drunkenness;
seems to be clumsy because it is ill-disciplined;
seems to be self-sufficient when it is desperate;
seems to be normal when it is utterly fatigued.

A surface which conceals, deludes, exaggerates, obliterates...

Seems, Madam? nay, it is, I know not 'seems'.
'Tis not alone my inky cloak, good mother,
Nor customary suits of solemn black,
Not for the fruitful river in the eye,
Nor the dejected 'haviour of the visage,
Together with all forms, moods, shapes of griefs
That can denote me truly. These indeed seem,
For they are actions that a man might play,
But I have that within which passes show,
These but the trappings and the suits of woe.

Hamlet Iii

What MS has done is to rip apart what seems and what is - the body and its meaning. The body ceases to be faithful to the idea of the self - that it first once made possible (before MS); the idea of unfaithfulness entailing not only its ceasing to be a true copy, but also the idea of betrayal, betraying the self precisely because it once seemed, or rather once was, faithful. For it is only possible to betray where once there was a mutual bond that made each what it was, a third, bonded, entity: the self-body.

The body has become faithless, adulterous, betraying in a perverted sense; not just as a surface appearance but as a medium of sensations that inform, orientate, carry, embody existence. Indeed the betrayal is often that the body - in the early stages of MS or between relapses - retains its form ("looks normal"), seems unchanged and yet is not the same.

Being different, its communications to the inner self have always to be interpreted, translated, its language having become
foreign, alien. So the self has to become a translator, a process that is always inadequate since one language is not the same as another: what is possible in one is never exactly expressible in another; languages cannot be faithfully translated. This applies both to the self understanding itself, its body, and those of others.

To betray is to turn away to other gods, other purposes. But for those diseased, the bond between the self and the body cannot be so severed, but remains, in its faithless mode. The relationship remains, a terrible perversion of what it once was: indissolubly bound. Because of the relation between the idea of the self and the body, the 'self' cannot be sufficiently distanced from the body, and thus has to be profoundly affected by what the betrayer has become. Distance can never be achieved, although always striven for. With MS, it is this that happens rather than a conclusion that the self was never discrete, always only ever constituted by its embodiment. Rather betrayal than self-annihilation. Thus as the later extracts cited show, the idea of the self remains, beneath the appearance: brutalised, refined or purified through the suffering of this betrayal; and yet in some sense utterly unchanged.

For what in spite of everything seems to be preserved is the abnormality of their experience. The body inhabited by MS may mark out what is normally, was once, taken for granted about the body
as the embodiment of the self. But that breach has not produced anything that is beyond understanding. In the dtexts there is no fall into nihilism. For all that has happened is a replication, albeit in an extreme form, which is an intensification or a reversal of what is already known about what bodies are in relation to selves. The knowability of the body, the givenness of the self is not undermined by MS, nor the knowledge of it, as knowledge of what is. There is no 'Question of the self'.

But this is to have read the dtexts as transparent, to have read the referents of the dtexts in the first fold as isomorphic with the text, to have read through them to the 'experience of MS'. I have slipped from my intention to raise the question of rather than assuming the relation of the texts to their referents.

How may I step back from this fall? What if the question of the self is re-considered in terms of the language with which the self who speaks is referred to?

6.3 SPEAKING OF THE SELF

Taking the first hundred lines of one of the dtexts as a basic text, and re-reading them against their form of self-reference, as the form of speech, what can be read of the self to which they refer? On what grounds does that self appear as the referent of the speech?
To be able to read the dtexts at all, certain things have to be held as stable, heuristically if not literally: the 'authors' whose words they are; the readers within the text - the parties who speak to each other (read each other's words as texts), who hold each other present as stable at that moment, but also in the past and in the future of which they also speak; the referents - the world of events, actions and actors who are invoked by the words.

Also, critically, at least the possibility of knowing (assuming) if not actually verifying the relationship between these things: and absolutely fundamentally, an underlying schema of logic/reason/epistemology which holds together in an ordered and thus decipherable way the documents and their relationships, the words and their meanings.

Also to be held as stable is the reader of the text whose reading it is; and the text which is read. Language which is spoken, written as having been spoken and then read, is the only available present document of what the dtexts are about: and yet

language can only be about something such as man (ie conceptual) but in being about man, it can never know whether it is about anything at all including itself, since it is precisely the aboutness, the referentiality, that is in question.

de Man 1979a: 161

To read the dtexts with the question of their referential status in mind is to hold already in mind a knowledge of what reference could be: what the criteria could be against which the substance and degree of the dtexts' aboutness could be evaluated. And to assume that their referential status is the question which is key to their proper reading: that which has to be addressed (first) in order to produce a sociological reading: before interpretation.
Daniel Well I think it would be best if you ask me a question and then

AW Well the question I start with is the beginning really

Daniel My work?

AW Your MS.

Daniel Oh the MS.

AW with perhaps a bit of your background.

Daniel The diagnosis..I'm not sure. I'm I was certain that I had something seriously wrong with me in 1969; suddenly it was, that the right leg as I recall, slightly weak, foot dragging and at the same time the eye and the hand on the same side or..

AW And how old were you then?

Daniel Pardon?

AW How old were you then?

Daniel I was well I was born 4th July 1941 and you can work it out from that, and I gather that's the prime time for catching MS so the doctors assured me.. But I didn't know that it was, what it was. I knew it was serious because obviously it had to be neurological if it was three things on the same side of the body which were otherwise far away from each other..and I didn't know what it was and the doctors..First I went to an eye doctor because that's what was worst affected.

AW And that was here in England?

Daniel That was here. And the eye doctor said neuritis. I didn't believe that but I realised that obviously you can't have optic neuritis that affects your leg and your arm. At the same time, I think, some people say, I certainly know other MS patients who say, that doctors are too shy of telling patients that they have the disease. And I can understand their being shy I mean from just a human point of view. It's a nasty thing to tell
somebody. But I also think that the patient's point of view is possibly to be taken into account there. I knew damn well that I had something serious and I knew damn well that the neuritis wasn't the whole story by a long way. If I'd really wanted to find out, I could have shouted at the doctors and got them to tell me.

So you think they diagnosed it at that time did they?

Daniel I'm pretty sure they did, yes. I remember when I was in Lambeth Hospital, I ended up at UCH but Lambeth Hospital was where I started - we lived in that part of London then. This was with the eye and so on. And they were doing knee tests and reflex tests and two of the doctors, the consultant and the registrar I remember were doing these tests with me I was, it was the knee reflex test at the same time as they'd done the hand test, the eye test and so on. And they just looked at each other, with that chilling way that doctors have and I knew there and then I mean that it was extremely serious and if I had had the desire to know the worst I could have found out. I was afraid. I didn't want to know the worst so..

Did you know

don't blame doctors for everything, for sure.

Did you know what it might be? I mean did you know about MS?

No, no I didn't. And I didn't want to know. I actually looked, somebody, I've forgotten who, did it for me. It wasn't my wife. Somebody else, read up something in a medical handbook thing. Some other mad disease. But I had a lot of the symptoms of it certainly and it was a very very nasty thing. And when I went to the doctors and asked if I had that they just laughed.

Did they say what you had then?

yes they said stop reading these books!

'Amateurs', yeah.

I didn't ask again! And I only found out by accident. I was in er X Hospital and it was just a routine, every six months visit sort of thing and they, there was the registrar there and a student in the corner. And as I was being, just passing the time of day with the registrar who I rather liked, he just mumbled into the corner of the room who I was and er what I had. And I thought my ears were deceiving me. I stopped and I said 'wait a minute. Did I hear you right? Was that what you said?'
And he said, 'you haven't been told?' And then he talked for an hour or so.

How long after was this then, I mean how long had you been going?

It was four years after the initial attack. Yeah.

And were you getting worse or

Oh I was... as I am now I'm considerably recovered from the - it got...you know the way the disease goes. But there were several, the first four years, five years there were a lot of attacks, mostly in the eye but also in the leg.

So what were the doctors telling you it was then?

Neuritis.

In the leg...? They didn't provide any explanation

No no actually I'm misleading you. The trouble was that the first attacks were all in the eye. It was only the very first attack, that one individual first attack that affected the leg and the hand.

Oh. I see.

But then there were a whole series and they were all in the eye and it wasn't until 1974 or '5 that I really had a very bad attack in the leg.

Most basic as a condition of possibility for the interpretation of this interchange is that it is possible to ask a subject about their life; that every subject has a life which may be asked about, spoken of, referred to: that the subject who speaks is the same as that whose life is spoken of: that this speech is self-referential.
What are the indicators in this passage of the self-reference of speech?

- The speaker is marked as a unique and empirical individual, as the referent of the I by the tying by calendar time - of his birth date - and his story to particular times and places: "England", "1974"; and so on.

- The speaker is the self of his own speech by virtue of his ability not only to describe what happened to him: "the diagnosis, "six monthly visits" and so on, but by virtue also of his ability to describe what he (the same 'I') had thought at those times in the past: "I didn't know what it was. I knew it was serious" (18-19), "I didn't believe that" (26-7), "I knew damn well that the neuritis wasn't the whole story" (37-8); but also the thoughts unexpressed by that past self: "and I thought my ears were deceiving me" (74-5) (and a rather clearer example in the next passage that will be cited (14-22).

- He, the speaker, is able also to re-interpret the actions of the past self: "If I'd really wanted to find out" (38-9), and can interrogate as it were any past self when requested to do so: "Did you know what it might be?" "No, no I didn't" (56, 58).

- Furthermore, the context of that past self can be, now, filled in, a context a particular past self was unaware of: "So you think they diagnosed it at that time, did they?" "I'm pretty sure they did, yes" (40-1).
And, as for example line 93, "No no actually I'm misleading you" indicates, there is a version that is incorrect of what happened, and therefore a record that is to be put straight that he, the speaker, knows and is concerned to have correctly (re)presented. He, the speaker, is the authoritative knower of, ergo, his own story.

But: fifteen pages on comes another reference to the events reported in lines 98-100.

Daniel I began to wonder what would be the limits of non-recovery. If the legs didn't recover and I had already had attacks several, and eyes and so on...clearly it could end up with paralysis, and how would I face that?

AW Did you fear the brain going too?

Daniel No. I hadn't really thought about that. I had been told by one of the doctors that treated me that the brain's internal functions were not broadly affected by it and that I...but all the same, with total paralysis... Very difficult thing to face. And I di-, I was..

AW Especially if the brain's alright

Daniel yes. And I was thinking er then of the, the weight that that put on my wife particularly, my family more generally and wondered whether whether life was worth living really. And that, seventy-four, seventy-five and whether I shouldn't just get a big bottle of sleeping pills and take them. I thought about that. That was the trough, seventy-four, five.

AW Did you talk about that to anybody?

Daniel No.

AW So you saw that as your decision to make?
Daniel Yeah. Clearly it was. I mentioned it much later to my wife, but long after I had decided against it, because then clearly, it's one thing to do it and another thing threatening to do - it's blackmail and even telling my wife long after I had decided not to do it she became extremely upset and I felt I just shouldn't have told her, kept it to myself. I never told anybody - no. But er

AW Did you begin to change your mind when your body was recovering?

Daniel Yeah. That, partly, but also my mind, and it was, it really was a decision whether... it could happen again, yes it could. If I got into the state of facing total paralysis I don't think suicide would be an illogical thing to do. But that's something I'll just have to face when I come to it. Won't make any preordained plans. On the basis of past experience, all I can say is, you just don't know what you'll do if put in great, severe situations. You can't predict. You may say when you're able bodied that it would be better to be dead than totally paralysed; but you mightn't feel like that when you are totally paralysed. You quite likely wouldn't. So I just don't know what, what lies in all that future.

Here is a second reference to the same story, the events of '74, '75: but this time a terrible description of despair - a meaning that has lain behind the earlier 'impersonal' recounting of the facts; an action contemplated but not (yet) taken: the absolute destruction of the self contemplated because of the inescapable effect of the body on the self. It is not only the past that is being re-presented, but that re-presentation is re-representable. So what is 'the knowledge' that the knower, the speaker, is uniquely knowledgeable of, if that is not merely represented in (his) speech but infinitely representable?

What does the speaker say about his own knowledge of his self? "On the basis of past experience, all I can say is you don't know what you'll do if put in great, severe situations.
You can't predict" (36-9). Attached to the referent of "You" (which although a generalised 'you' by the same token includes any 'you' in particular) is a 'law' about selves which states that they cannot know themselves enough to predict the actions of their future self, cannot have complete self-knowledge. Yet paradoxically that is itself an axiom concerning the self, a negative form of self-knowledge, which has been achieved through experience ('knowing' the experience of the self). Not only that, but the law is itself a prediction, the very thing that selves have insufficient knowledge to make.

In tension with this paradoxical statement of the impossibility of complete self-knowledge, at once reinforcing the paradox and undercut by it, is the progressive accumulation of self-knowledge effectuated through the chronological sequence of his story: where it was always the earlier selves who (are now known to have been) less knowledgeable than the present-one-who-speaks. "I mentioned it much later to my wife" (22-3); "I felt I just shouldn't have told her" (27-8); and, again, perhaps most clearly in the passage previously cited: "If I had had the desire to know the worst" (51-2) in which the present speaking-I can face the alternatives the "I" then could not. If self-knowledge is, as the accounts of the past selves have demonstrated, always to be superseded (by each 'present' self), the self knowledge of the self is that it thinks itself always more knowledgeable than it ever really is.

So self-knowledge, the possibility of self-knowledge upon which the isomorphism of the speaker with the selves spoken of
is predicated, is itself denied by the speaker as he speaks.

To object that this undercuts only the claim to facticity of the dtext and not the speaker-as-the-unmediated-referent and origin of his own speech is to assume the continuity and unity of the selves through the past to the present of speech; that the referent of 'Daniel's speech' is Daniel himself: that, "the instance of discourse is self-referential" (Ricoeur 1981: 198) (my emphasis); that to know his speech is to have access to him, and the task therefore to understand or explain the difference(s) between what his speech means and what he himself intended it to mean (which may include the intention to disguise, mislead, omit and so on). That to interpret the dtext in order to understand MS from Daniel's point of view, his story of his life, is a question therefore of interpreting Daniel's interpretation of his life. To interpret the dtexts by de-inter pretation. All that, to repeat, premised on the identity of the speaker with the spoken of.

But: let me push this further: What, then is to be understood by the referents in this passage from another of the dtexts?:

1  George  If I wanted to I could be one of the biggest scivers out, I really could. I could get away with a heck of a lot.
2
3
4  AW    So why do you think you don't?
5  George 'Cos I get bored at home.
But you have some sort of moral feeling that that's wrong do you?

Oh yes I have because, I mean I know I've had a day off when I haven't felt terribly well, and I've sat here and thought about it, well you don't really feel that bad, you could have gone. And I say no. You know I'm really cross with myself, I haven't bothered to make the effort.

You're very critical in this dialogue, aren't you, with yourself?

Oh yeah, but I am critical of myself. People annoy me who aren't critical, well not annoy me, but people annoy me when they don't, they think they're perfect. Praps they don't or p'raps I'm wrong but they think they are great or something.

Here the referents for the pronouns "I", "me", "you" are not simply the co-present questioner and questioned, "AW" and "George". The questioned, George, also becomes the narrator of 'himself' and of yet another 'himself' who carried on a dialogue with yet another - who appears to have some autonomy in that it resists: "you could have gone. And I say no" (11). The "you" that AW then seems to be responding to with her comment (14-15) is George, the present narrator of the story of those other selves who are yet himself.

What of the "I"s in the first line? Who are they? The present George? The George always present? And the "you"s in the third? Are their referents the same 'you' or different? The first the you present in the interview, the teller of the tale, and the second the you(s) of the past? What "I" is cross? and with which "I"? (12-13). The referents of the "I"-who-is-speaking, who is doing the speaking would appear to be multiple selves - not, as in the previous passage in the sense of past selves, selves in each
point in the past - but some at least simultaneously existant (9-13).

Yet this appears an absurd observation: despite the extraordinarily complex use of pronouns, the exchange appears mutually comprehensible (as the absence of any requests for clarification may bear witness). If the argument outlined just previously were to be followed, the very knowledge of the George-who-speaks of that internal dialogue would be supportive of the view that George is his story - because of the demonstrated insider's knowledge of the protagonist that, because no-one else knows what people say to themselves (outside the world of fiction), the demonstration - by example - that he is (ie. constantly, always) a person with particular standards about "sciving" works in turn to support that supposition. But: it is the protagonist(s) who act, "I've sat here and thought about it (9-10); "I'm really cross with myself" (12); and George who describes the protagonist's actions which are as if fixed (the history which underlies the historical record): although they may be re-interpreted by the narrator, they may not be altered. In order to be able to speak of the self, there is a split made/assumed between the narrator and the protagonist of the narrative, one present (speaking) and one/many not present. There is a difference between them. But this difference has been opened up by speaking of the self. The 'multiple selves' are therefore the product of the mechanism of self-reference, the language of 'speaking of the self'. The di-unity of the self therefore appears to be not a quality of the
self, 'Daniel's' or 'George's' view of a problematic of the concept of the self which has given rise to and therefore can be worked back from the dtexts, but an effect of language: an artefact. And therefore one may go on pre-suming the unity of the self, of the self who speaks with all those other selves in the past.

But: this relief has in turn to be put into question. For there are passages in the dtexts where knowledge which, according to the speaker, if learnt of by the self - who is also in the present - would utterly destroy its tranquility and yet which is nevertheless spoken of.

1  AW  Do you um how do you feel about the no treatment bit?
2  Does that I mean I don't know what your view of medicine is, whether
3
4  Meg  Yes - I can't say that I've ever thought about it very much, I mean I've always known there's no treatment and no cure, I mean
5
6  AW  I mean that makes it different than having measles, doesn't it?
7
8  Meg  er yes
9
10 AW  In what way?
11  Meg  I've never actually thought to myself you know how awful that there isn't a treatment or or because I've never crossed my mind to worry about whether there was or not. I do respond to cortisone very well so that I s'pose that my own sort of particular psychological defense is that every time I get ill, I'm going to have cortisone, I'm going to get better. And er, that'll go on I suppose until they find a cure is probably the way my mind thinks about it, I mean I've never thought it through. But I suspect if I didn't respond to the cortisone I might feel quite differently. I mean I've never thought it through,
Here Meg refers to "my mind" (14, 20) as if independent of the self who is offering the account, objectifying it as a self or a part of the self that is not present at the moment of revelation (the interview); about whom/which, as a privileged yet not co-extensive raconteur, it is possible to speculate "...is probably the way my mind thinks about it..." (20). To which "I" is AW addressing her questions: "how do you feel about..." (1); to the present person, the narrator? Or to the one who seems to be the subject for both speakers, an absent "I", the protagonist whose delusions about treatment are the subject of discussion by, it seems, a pair of disinterested, distant conversationalists? This protagonist is not a self distanced by the past as in the example of George above; but is in the same present (and presence) as the speakers. And yet it cannot, it seems, hear what is being said.

The more closely the referents of the pronouns are considered, the more extraordinary the exchange becomes. It is as if by this distancing technique (which cannot be a mere technique) the terror and the pity evoked by the "I"s situation become an Aristotelean spectators' cathartic experience of tragedy, about which it is possible then to speak, a Wordsworthian "emotion recollected in tranquility"; and thus both parties, not just AW but also Meg, are protected from experience's immediacy. But if there is a current self who 'cannot hear', the self who speaks cannot be the same as the one whose story is being narrated. The speaker is not the protagonist.
And a final example:

1  AW  ..but you don't think, you don't wonder how other people see you?

3  Anne  Well I try not to think about how other people see me. Because I try and think of myself as normal as possible. If I start thinking about how other people see me um then you start thinking about how abnormal you are. So I try not to, I should think, I should think I consciously try not to, think about it.

In order for the narrator to be able to say, "If I start thinking about how other people see me then you start thinking about how abnormal you are" (5-7), she has to know already that she is abnormal. And yet to which "she" does that knowledge belong? That the speaker can say, "I should think I consciously try not to, think about it" (7-8) that is can speculate how that "I" that knows its abnormality defends itself from that knowledge but not know how "she" does it, reinforces the distance, the difference between the speaker and the protagonist. If it was 'the same self', the knowledge would not be hidden. And yet the narrator, the person who speaks, can speak both of the knowledge and the distancing technique as a technique: as if it was nothing to do with her. As if, therefore, she who speaks is not the origin of the knowledge, although a knower of it.

Which returns us once again to the question of the self and its knowledge. According to Mead:

It is the characteristic of the self as an object to itself that I want to bring out. This characteristic is represented in the word 'self' which is a reflexive, and indicates that which can be both subject and object.

Mead 1934: 145
For Mead, the splitting between the narrator and the protagonist in the dtexts, far from being a puzzlement, would be a confirmation of his delineation of what the self is. The dtexts would simply exemplify that delineation, for such splitting would be the distancing necessitated by reasoning itself, the pre-requisite of the process of being objective, rational:

The apparatus of reason would not be complete unless it swept itself into its own analysis of the field of experience (...). Reason cannot become impersonal unless it takes an objective, non-affective attitude towards itself; otherwise we have just consciousness, not self-consciousness. And it is necessary to rational conduct that the individual should thus take an objective, impersonal attitude towards himself, that he should become an object to himself. For the individual organism is obviously an essential and important fact or constituent of the empirical situation in which it acts; and without taking objective account of itself as such, it cannot act intelligently or rationally.

Mead 1934: 146

The argument takes the form that, since it is to be assumed that people do act rationally (at least some of the time), they must be able to regard themselves-as-objects and therefore the self is that which can take itself as object, "be both subject and object”.

But to say that is to take as axiomatic that which is being questioned here through the dtexts: what it is to be a self. It would be to take as unproblematic the question of referents and of representation: that the referents of the dtexts are, and that the dtexts have therefore arisen from, selves, conceived of as lying outside, behind, originating, the text.
Even more critically, Mead is taking a feature of the grammar of (our) language, that the word self, "is a reflexive" as the axiom - which allows what then follows in the argument to be said: it is only by assuming 'the self' to be the referent of the word that the argument can get off the ground.

*Why could the world which is of any concern to us - not be a fiction? And he who then objects: 'but to fiction there belongs an author?' - could he not be met with the round retort: why? Are we not permitted to be a little ironical now about the subject as we are about the predicate and object? Ought the philosopher not to rise above the belief in grammar? All due respect to governesses: but is it not time philosophy renounced the beliefs of governesses?*

Nietzsche 1984: Note 34

And of course according to Mead's dictum, the dtexts would be acceptable as evidence of an extra-textual reality, as a passive literal record of what happened.

***

In order to think about what kind of texts the dtexts are, the question of their relation to the speaker as referent is one of the ways marking them as, at least not-fiction. And yet there is an analogy with a certain kind of literary theory about fiction:

As they have been read, in order to speak about the self as in the dtexts, the self has to put itself in the place of the other - but the other who is also the self - in order to refer to and 'translate' it, thus the self who speaks has always to be and yet be outside itself.
Bernard Sharratt writes of this oscillation, this *extasie*, as being taken, insisted on, as the quality whose presence evoked by a reading marks certain texts out as literary: that, precisely, being its value, placing oneself as reader voluntarily within this process of "suture". But in the case of the 'speakers' in the *dtexts*, the vertigo induced by this constant oscillation between always having to read, play the part of the self and of the other self produces not the 'pleasure', the 'extasie' of reading (Barthes' *Pleasure of the Text* (1976)), as if the flickering was an heuristic freeing from the prison of the self, but a threatening, obligatory trembling on the brink of nothingness: no self at all.

But this is an effect of speaking of the self, of the language of self-reference, *insofar as the *dtexts* are not-fiction.*

'There is thinking: therefore there is something that thinks'; this is the upshot of all Descartes' argumentation. But that means positing as 'true a priori' our belief in the concept of substance - that when there is thought there has to be something 'that thinks' is simply a formulation of our grammatical custom that adds a doer to every deed. In short, this is not merely a substantiation of a fact but a logical-metaphysical postulate. Along the lines followed by Descartes one does not come upon something absolutely certain but only upon the fact of a very strong belief.

If one reduces the proposition to 'There is thinking therefore there are thoughts', one has produced a mere tautology; and precisely that which is in question, the 'reality of thought', is not touched upon - that is, in this form the 'apparent reality' of thought cannot be denied. But what Descartes desired was that thought should have, not an apparent reality, but a reality *in itself.*

Nietzsche 1968: 268
Because what happens is attached to "I" and the narrator speaks in the "I" form, it can (only) be assumed that the person who is entitled to use the pronoun "I" is her/himself that "I". Except: in fiction the author can use the pronoun "I" to denote any first-person narrator s/he chooses - Robinson Crusoe, Jane Eyre, Ishmael. (And where, whatever the voice of the fiction, critics read the story as autobiographical, the writer using another's voice as a device to disguise her own. It is assumed). So you have to assume, presume the dtexts to be non-fiction, to go back to the beginning, presume their referential relation to 'the self who speaks'.

6.4 THE EFFECT OF THE NARRATOR

But: there is also the 'whole form' of the dtexts (that cannot be represented here, but only referred to) against which the question of the question of the self plays; the form as a narrative, unified by the narrator as a unity, unifying the narrative of the self through each dtext.

To read the dtexts according to this way (suspending our own work of reading them) would be to understand 'the narrator' as the reader of what s/he has brought to discourse, in the process through time (the time speech takes) of referring to, reading his/her own life.

Thinking then about the form within the form, of the un-folding of the dtexts as a story in the process of being told, an event in time, referring back on itself as it is being un-folded, it is possible that, as the past is re-understood in
being spoken of, 'the self' (in all its forms) rather than lying there awaiting dis-cover-y, is being produced in front of that re-understanding, by the reading of the past by the self who then speaks.

In his analysis of the activity of reading, Paul Ricoeur refers to the hermeneutical relation between distanciation and appropriation as qualities held in tension. According to Ricoeur, "'(D)istanciation' is the condition of understanding" (1981: 144), and thus the condition of understanding the self (the reader, understanding 'herself' as a text is read) and produced in the narrative,

"By appropriation I understand this: that the interpretation of a text culminates in the self-interpretation of a subject who henceforth understands himself better, understands himself differently, or simply begins to understand himself. This culmination of the understanding of a text in self-understanding is characteristic of the kind of reflective philosophy which, on various occasions, I have called 'concrete reflection'. Here hermeneutics and reflective philosophy are correlative and reciprocal. On the one hand, self-understanding passes through the detour of understanding the cultural signs in which the self documents and forms itself. On the other hand, understanding the text is not an end in itself; it mediates the relation to himself of a subject who, in the short circuit of immediate reflection, does not find the meaning of his own life. Thus it must be said, with equal force, that reflection is nothing without the mediation of signs and works, and that explanation is nothing if it is not incorporated as an intermediary stage in the process of self-understanding. In short, in hermeneutical reflection - or in reflective hermeneutics - the constitution of the self is contemporaneous with the constitution of meaning."

(Ricoeur 1981: 158-9) (emphasis in the original)

Amongst the "cultural signs in which the self documents and forms itself" could be, in the dtexts, for example the signs of the body, which are imbued with cultural meanings (cf. Douglas 1973;
Turner 1984; the first section of this chapter) and cannot be
understood outside of meaning: not "the signs" per se, but the
signs' meanings brought to the account of the life as it is being
referred to; the understanding (in the present of the narration)
of what the past (is now understood by the narrator to have) meant.

And as the talk unfolds, may not what has already been said
become the text(s) in front of which, from moment to moment, the
self is constituting itself; the way in which the self and the
life have been referred to the basis of that constitution in the
immediate, the 'self understanding' of the narrator of his own
self.

1 AW You said that it took you a long time to come to terms
with the idea of having it.

3 Daniel Yes.

4 AW ..can you remember how you came to terms with it, I
mean beginning, you know, did you begin with anger, did
you begin with horror, or not thinking about it? Can you
remember it in sort of..

8 Daniel Anger. Anger certainly there was, yes. I mean thi-
cliches. 'Why me?' and 'What did I do wrong that caused
the, the MS if any?'

11 AW Morally or physically?

12 Daniel Not really either specified, 'Did I do something wrong?'
that 'wrong', that's again, too, that's not the right
word. 'Could it have been avoided?' Did I make
some foolish step and therefore brought it on myself?
The way I think about it now - I mean it's such a
mad line of reasoning I can hardly believe I used to
do it - but I did. 'What did I do to bring it on myself,
if anything?' And then I would run through a checklist
of possibilities but er they were such wild...
Is this 'splitting' - of the narrator 'Daniel' from that past self - "I mean it's such a mad line of reasoning I can hardly believe I used to do it - but I did" for example - Ricoeur's distanciation? The condition of understanding that cannot be done in the "short circuit of immediate reflection", for to understand a life is to have lived and passed on.

And as for appropriation:

Ultimately, what I appropriate is a proposed world, The latter is not behind the text, as a hidden intention would be, but in front of it, as that which the work unfolds, discovers, reveals. It is not a question of imposing upon the text our finite capacity of understanding, but of exposing ourselves to the text and receiving from it an enlarged self, which would be the proposed existence corresponding in the most suitable way to the world proposed. So understanding is quite different from a constitution of which the subject would possess the key. In this respect, it would be more correct to say that the self is constituted by the matter of the text.

Ricoeur 1981: 143-4 (emphasis in the original)

Is Ricoeur's "I" who appropriates equivalent to 'Daniel' as he reappropriates his own life and the world of which MS is a part, proposed and revealed by, through, the distanciation of narrator from protagonist, the narrator learning of the world the protagonist experienced (the "matter of the text")? the narrator engaged in a "hermeneutical reflection" on his own life?

Unlike with Mead's theory of the self, this would be to say that what there is to be understood is not being reproduced but produced, for the very first time. Every production of an account of the self, every re-reading of the protagonist by the narrator is another first time, constituted by what has been
produced before, another "meaning of his own life".

George  Well what's anyone else done to deserve it, what
        has anyone done to deserve any illness or anything
        you know that harms them in any way? So that was
        my answer anyway, you know, we're all being tried
        for different things, it just so happens, it's just
        run of the mill, it's just life you know.

AW  So you're saying that you believe we're all tested
    in various ways?

George  No, not really cos ah, who's testing us? I don't
        know..

AW  Did you mean that under

George  I don't know not really. I..you see..ah it's very
        difficult to say..I..I don't know you see it's like
        what is the meaning of life? There's no real answer
        is there?

But still that is to propose that to read the dttexts is to dis-
cover the self (the narrator) in the act of producing new under-
standings of himself as he 'reads' his own words, recalls his own
selves in the past: to retain, albeit in a dynamic and continuous
mode, the narrator as the referent of the dttexts and therefore
as the unmediated origin of the knowledge of the self, ("the matter"),
independent of any effects of the form of the dttexts, effects
produced by the technical structuration inherent in the language of
self-reference.

***

So: what difference do the dttexts make to the question of the self?

For in this chapter, both the absence and the presence of the
question of the self has been discerned 'in' the dttexts: first
made absent through looking at the substance, the matter of the
dtexts in respect of the concept of the body-self relation as
conceived through the irruption of MS; second, made present through looking at the constraints engendered by the form of the language of self-reference. So: the referential relation between the dtexts and their speakers, the first 'fold' of the question is not indeterminable, but dependent upon what is questioned about that relation, what is held, 'philosophically' or otherwise, to be the problematic of their interpretation, dependent upon the question against which they are read.

But this dis-integration of the authority of the speaker as source of the dtexts qua knowledge, produced by these questionings, have emerged insofar as the dtexts are read as the representation of speech. That is, insofar as they are read as a particular form of text: voice-in-writing.

But "their form" is not inherent in the dtexts. For they are also a form of autobiography, the representation in writing of a life, in which the uniqueness of the life and the authority of the author seem to re-cohere the disintegration of the authority of the speaker. Read against, not a philosophical question but against a literary genre, not as speech-in-writing but as writing, another mode of interpretability comes into effect.
CHAPTER SEVEN

READING AGAINST FORM: OF AUTOBIOGRAPHY

The story of Ivan Illych's life was of the simplest, most ordinary and therefore the most terrible.

Tolstoy 1960: 109

The second 'fold' of the question of the three-fold referential relation of the dtexts, as it was posed at the beginning of this Section, is that of the text to the life.

As works of writing, in that each is rooted in a single subject who is the author of the text which is about that subject's life, the dtexts seem to share the essential characteristics of the form of writing that is autobiography. To re-read them against the idea of autobiography is not only to think about what effects that has on their interpretability, but at the same time to recognise that what has been done so far has been done simply in terms of particular passages, forgetting that it is their whole form that provides the context within which extracts are interpretable; a context which lends a coherence to particular passages as parts of a whole form. What, then, to reiterate, is the relation of the dtexts to the life?
But what is particular about autobiography as a form of writing? For literary theorists it is, according to de Man, problematic in two major ways, for it does not seem precisely to fit into that kind of writing classifiable as literary: first because of its quasi-factual status and second because of the difficulty it presents in terms of aesthetics, generally regarded as the mark of literature (de Man 1979: 918). It is the nature of these difficulties concerning what autobiography is as a genre of text which resonate with the problematic of the dtexts qua texts and which suggest that this re-reading may be a fruitful way of thinking further about them.

In the first place, according to de Man, it is uncertain apparently whether autobiography may indeed by "properly" elevated to the status of a genre, for then, as literature, it would be at once placed in the domain of the aesthetic. And yet:

Compared to tragedy, or epic, or literary poetry, autobiography always looks slightly disreputable and self-indulgent in a way that may be symptomatic of its incompatibility with the monumental dignity of aesthetic values.

de Man 1979: 919

To read the dtexts from the concerns of aesthetics, that is with regard to the effects aspired to or achieved by their authors on their readers, would be to consider such matters as the tone and the style of their writing, to consider the language in which they are written in its difference from language used literally. To read the dtexts in this mode would be to put aside questions of their epistemological referential status as irrelevant, such
matters traditionally being regarded as incompatible with, or
rather as incommensurable with, language used in its poetic
or literary mode.

The Doctor

You are not looking at all well, my dear
In fact you are looking most awfully queer
Do you think the pain is more than you can bear?

Yes, I find that it is more than I can bear, so give me some
bromide

And I will go away for a long time and hide
Somewhere on the seashore where the tide

Coming upon me when I am asleep shall cover
Me, go over entirely,
Carry beyond recovery.

Stevie Smith 1938

Given the present context, the central question of the dtexts'
referential status, the concentration here has to be on the
former, the epistemological problem, rather than on the latter.
Yet it would be absurd to deny that the dtexts provoke in
their readers pity and terror which were, as has already been
said, for Aristotle, the very marks of Tragedy: their power to
do so magnified insofar as they are writings about lives that
are not-fictional.¹

I began to wonder what would be the limits of non-
recovery. If the legs didn't recover and I had
already had several attacks, eyes and so on...clearly
it would end with paralysis and how would I face that?
And I wondered whether life was worth living really
and whether I shouldn't just get a big bottle of
sleeping pills and take them. I thought about that.

For literary theorists, the problematic suitability of autobiography
for aesthetics is entailed and compounded by its second character-
istic, lying as it is held to do, peculiarly athwart the modes of
fact and fiction.
Autobiography seems to depend on actual and potentially verifiable events in a less ambivalent way than fiction does. It seems to belong to a simpler mode of referentiality, of representation and diegesis. It may contain lots of phantasms and dreams, but these deviations from reality remain rooted in a single subject whose identity is defined by the uncontested readability of his proper name.

As a form of the written, the dtexths too "seem to depend on actual and potentially verifiable events." In that our question is concerned with what that "seems" involves, to re-read them against the form of autobiography to which they are, prima facie, similar, is a way to think about what the dtexths are as texts and the kind of interpretive work that is involved in their reading. For sociological readers, the crucial issue lies in the dtexths' possible similarly "quasi-factual" status, a problematic which would be conferred by their similarity to autobiographical writing.

To think first about their similarity will be consequent-ially to think about their difference.

7.1 READING FOR SIMILARITY

As one form of written text, autobiographies require, as each kind of text does, a particular framework for reading, an interpretation that recognises them in their particularity; their difference from other kinds of writing.

What is involved may be opened up through playing on their characteristic as essentially partial: a play against which the question of the dtexths will at first be present only by implication.
Each of the many respects in which autobiography is recognisably partial invites a cautious interpretative response yet at the same time seems to provide a strategy for their interpretation, an interpretation, it will be emphasised here, that is relevant to the question of the texts that is chiefly concerned with their factual status, the author's text as a representation of her/his life. (This strategy is thus already a partial reading).

Autobiographies are partial-as-opposed-to-whole in many respects. Being written from a single viewpoint, the scale is necessarily small, individual, personal. Events, what happened, the substance of the life can only be represented from the author's view, from the author's knowledge. Being rooted in a single life, the story cannot go beyond the arc of time and the geographical sites on which the author has lived the life. Others' viewpoints may be included in them, but these are represented through the author's (limited) understandings and interests. Any references to matters beyond the first-hand experience are to be understood therefore as less authentic, less factual.

Because of this inherent limitation to a single view, a personal perspective, it follows that there must be gaps in the story of the life as given. Not only is the viewpoint necessarily partial, but also the context. Crucial events perhaps happened off-stage that the author was unaware of or only partially knew of; other events, persons, forces that were hidden to the author operated on the life that, if known, might have made the
story of the life altogether different; a hidden hand, deus ex machina, that made the life as it seemed to be rather than what it was.

In that autobiography is written at a particular point in a life, it may be assumed as the time at which the author had something to say, deemed it for some reason to be an appropriate time from which to reflect back on the life. But by the same token, autobiography is partial in that the point of writing is not the end of the life and unknowably distant from it. The author is still in the midst of the plot, and what is to come might change everything, make the time of writing of greater or in any case of different significance. The meaning of the life might utterly change.

Autobiography is also partial in the sense of being subject to the technical limitations inherent in the form: by the author's capacity for insight or for self-expression; by her writing skill. Partial because subject to the technicalities and fallability of memory; because of its retrospection (differing from a diary or a letter), partial in that what is recalled is necessarily mediated by the present of writing: can only be an interpretation of the past, never 'the past' itself. 4

They are partial in that most of the life is not included in the writing. No-one can refer to everything that happens. 5 It follows that what is written has been selected, consists of episodes in the life chosen as representative of it, of the chosen theme or period, or which present the life most desirably: which is the other face of their partiality.
In the sense that they are written by the author about her own life, the author has an interest in what is written, a concern with the way she and the life is displayed. For the text will be a representation remaining beyond the death of the author.

At the grave of Henry James, Auden asked intercession for the vanity of the writer's calling, for the treason of all clerks. Vanity and treason there were... but there was also the hope of creating against time, of making language outlast death. That is the essence of classic literacy. Not very many, today, admit to the arrogance, to the obsessive aloneness needed for that hope.

Steiner 1975: 178-9

That which is written down as autobiography may also be deliberately partial, full of omissions, suppressions or lies. In any case, cannot be taken at face value.

This double implication of partial-ness as a framework for the interpretation of writing which is autobiography is precisely apposite here, for it involves the complementary ideas of a whole and of truth: that there is a complete or at least a more complete story which could be (have been) related, and that there could be (have been) an objective, less partial, more dis-interested, more truthful version. The differences between the partialities of autobiography qua form and the wholeness of its referents, which it cannot but fail to represent, constitutes a space - for the essentiality of interpretation. As writing, the autobiography is essentially an interpretation of 'the life'. As a text to be read, it is an interpretation.
of a life which it is the work of reading to (de)interpret. For how could the overwhelming partialities of autobiography as a form of the written be compensated for, except by what the reader can bring to the text? Which principally is the very idea that what is written is (only) partial and that the partialities have to be compensated for. To be able to fill these spaces is the task and triumph of expert readers, to add to, to complete or at the very least to supplement the text, particularly incomplete by the nature of the genre to which it perhaps belongs.

In that the dtexts are autobiographical and, ergo, partial, but in particular ways, particular kinds of interpretive work are made possible, required, demanded by their form.

One kind of reading that may be opened up by their need for supplementation might be a concentration on the veracity of the accounts. Did what the author said happened, happen in fact? Thus checking with their GP or hospital records for confirmation of the status of their condition for example or their date of diagnosis; with their spouses for congruent details of family life; or with their employers for their current and past employment history, for example; to know that about some points of the life referred to would be, according to such a reading, to have a criterion against which to interpret the general referential status of the whole story via an assessment of the credibility of the author on at least some of the elements of the life.
Or supplemented by psycho-analytic theory, for example, a reading could work to compensate for the dtexts' obvious incompleteness vis à vis the life in terms of the psyche of the author; of the power of the unconscious to block from consciousness and thus from what can be referred to by an individual, the deep trauma or psychic conflicts which, in the absence of a skilled Analyst, has to remain as the hidden-but-readable-truth of the life:

If one considers the problem from a psychoanalytic viewpoint, the first and most obvious explanation would be this: 'Instead of talking about their psychic feelings directly, they talked about these feelings by talking about physical matters; consciously unacceptable thoughts and feelings are repressed into an unconscious mode of experiencing, and it is possible that your subjects could not tolerate a conscious experience of their feelings about their disability, so they repressed them and when asked about them, displaced them onto physical matters.' This process is called 'somatization', 'Soma' = the body, so 'somatizing = body-izing feelings, as a substitute for consciously experiencing them, and representing them through the explicit medium of words. (emphasis in original)

James 1981: 25

Read through constructivist sociology (and see Chapter 1.3) that which was absent would be absent by virtue of the premise of such a reading, that no one individual can ever refer to, because they cannot by definition comprehend, the full social context within which all are necessarily partially ignorant or deluded. Such a reading might look in the dtexts for the social forces constituting and constraining 'the life' as represented: class, economic circumstance, institutional practices and so on (cf Wright & Treacher 1982; Chapter 1 and this thesis, passim).
For example:

It is argued [in our paper] that readiness to accept responsibility for one's health depends partly on views held about the aetiology of illness and this position is explored using material on causation and the circumstances where blame is attributed, derived from semi-structured interviews with a sample of 41 working class mothers...Roughly half the sample held fatalistic views on the aetiology of illness and thought they were only morally accountable in very restricted circumstances. These women tended to be less well-educated than the rest of the group and they were less likely to be buying their own homes.

Pill & Stott 1982: 43

The incompleteness of the authors' accounts of MS in the dtexs may also be read as technical, owing to the interviewer's inadequacy, her failure to elicit a fuller account by failing to ask the most fruitful or pertinent questions (see Chapter 4: 138-9) or by failing to pursue what more there was that could have been said. Who had let pass the surface account which should have been recognised as partial at its face value: a face value which was a mask for the true or at least more complex, more complete story of the life.

These forms of reading however would all be explanations of absences in the dtexs, absences which in their particulars become visible as such to such readers by virtue of the explanations themselves and read(able) into the spaces of absence by an expert reader who knows what is lacking: brings to their reading and understanding of what was written what ought to
have been there, but that which also, by virtue of the same work of reading, cannot but be absent from them. The explanation of absence(s) as well as the substance of what is absent has to be extra-textual, brought into the partial text by the reader yet verifiable from the text itself (whatever that could be) by a particular supplementary reading.

And: as the hypothetical or actual readings above showed (as does my use of them here), although those readings concur that there would be absences in the dtexts, they do not concur in what those absences are. The relation of what is present in the dtext to what is readable as absent is not the same in each case.

* 

So: whatever else, the relation of autobiography as a form of writing to 'the life' it represents is one which consists of distance. Given this generous and necessary space for interpretation, predicated on the dtexts' similarity to autobiography as form, that space may at the same time also be supplemented by their difference(s) from it.

We will look at them in two respects. To re-cite de Man:

Autobiography seems to depend on actual and potentially verifiable events in a less ambivalent way than fiction does. It seems to belong to a simpler mode of referentiality, of representation and diegesis. It may contain lots of phantasms and dreams, but these deviations from reality remain rooted in a single subject whose identity is defined by the uncontested readability of his proper name. (my emphasis)

de Man 1979: 920
7.2 READING FOR DIFFERENCE

7.2.1 (A Simpler) Mode of Referentiality

As autobiographies, the dtexts require to be brought into being, have no existence except that they are read; a reading which is essential in order to bring the life (back) to life.

I was scandalised when I read that thing, 'Living with MS' book by a doctor, whatever, I can't think of the lady's name, I have it upstairs. I'll look for you. Ransome? Elizabeth Ransome? [8] But, she's a medical doctor and she went through all this business of being considered hysterical and so on. And her husband left her. And I, admittedly her story may not give as full an account of the other side's motives as one might have, but all the same, I was scandalised that anybody could do that. It's all very well to say in general what people might or might not do, but when you read one person's account of what actually did! And I know of other cases where husbands also, in most cases, walked out on their wives like that.

Daniel

As autobiographies, the dtexts are addressed to someone, to those both contemporaneous and in the future who will be their readers (although circuitously, via AW and the tape-recorder; cf Chapter 5). They are inherently addressed communications, produced for some reason, intended to communicate - something. What that intention is held to be makes a difference to their interpretation, to the supplementation of that aspect of their inherent partiality.

Unlike autobiographies, however, the dtexts cannot be construed to have originated in the desire of their authors to place their own voices permanently in the world to transcend the silencing of death; rather perhaps that they gave their
consent to relate their lives in order that the life may be laid out for the scrutiny of experts, laid out in order to contribute to knowledge about MS; to provide, in being so picked over, unwitting evidence for an explanation of the disease.\textsuperscript{9} In any case, told for the virtual\textsuperscript{10} purpose of knowledge about the disease and thus for the sake not of the authors but of others; not the readers of the dttexts perhaps but for those on whom the readers may then have an effect (the analyst as first reader, her analysis then, perhaps, influencing others who read her text who will then, perhaps, understand MS differently, more clearly, and may use that knowledge in their relations with people, other 'people with MS').

As a consequence of the assumption of this greater yet narrower communicative purpose than autobiography in general, it would follow that, far from attempting to conceal, to disguise or to delude their audience (to present themselves in the 'best' way), the language in which the life is described is an effort to be literal rather than literary, for it would be only by relating that life literally that they could be so fruitful for those greater purposes. Thus for readers, the task would become that of disentangling the literal referents from the literary effects inherent in the form in which the life is given.

Read, then, for that which is literal in them, the dttexts as writing contain more than is required, that will have to be (readily can be) discarded. For it is not in that they consist of a whole story of a life that this usefulness may come, but from small pieces (which happen to be tied together within a
a whole story); events, material for the readers to pick out and piece together - the circumstances prior to the onset of the disease for example or the process leading towards its diagnosis - the rest to be cast away, leaving only the essential components exposed. But which elements are essential? Those which would have this "(simpler) mode of referentiality"; those directly related to MS.

Would it be preferable to say, then, that the dttexts are (more) like history, the record of a network of stable and actual facts which, although they may have to be carefully de-interpreted by readers (insofar as the record is in autobiographical form), remain as it were the basic text to be read? their author-narrator properly able to refer to such a stable set (even though partially)?

But: there is a peculiarity in the dttexts in that the dominance of the theme of MS in the life appears as the explanandum - that which is to be explained - by virtue of the biographical details. Yet those very details, the details of the lives which are referred to are those very details which explain, further, the theme of MS; provide the context against which the difference MS makes, what MS is, takes on its poignancy and concreteness:

(I) tend to push myself a bit too much sometimes, I know, and then I regret it the next day. Well my legs feel very wobbly, well one thing, I do come back very quickly. I can't read. That's the one thing that really annoys me. Well I can read for a reasonable period but whereas before I'd get into a book and start enjoying it, I, there's no point in me - well there is a point in me picking up a book and
doing it, but I can't do it continuously. Have to have a long rest in between you know because the eyes start playing up (after) say half an hour, the eyes start to merge.

George

Each detail, read as having been told for its significance towards the theme (of MS) has been chosen, presumably (by virtue of the dttexts' communicative purpose) for its force of signification, selected from all possible things that might have been referred to. And as such, what that significance is is constituted by the episode itself, tied to it (as if the 'descriptive rhetoric' could be stripped away, leaving the - literal - essence, the "actual and verifiable events" that autobiographies at least partially consist in). Insofar as the details of a life are used to illuminate, to explain about a disease, the details that are chosen by the author - that are in the text - are those that produce that understanding: constitute that which there is to be understood.

Furthermore: insofar as the dttexts were elicited in response to a request to be told, not about the life but about the disease, the possibility that they may be peculiarly occasioned accounts arises, to fill this aspect of their partiality. For it may be the case that "MS" features throughout each one (one of the "essential elements") and in many ways structures and dominates them simply because of that request. Left to themselves, it would follow that each person may not have accorded such a potent presence to MS in their own unsolicited or differently
solicited accounts of their lives: their genuine autobiographies. And as such, the dtexts may seriously mislead if the purpose of reading them is, as it may be, not only for information about MS but in order to understand the place of MS, the difference having MS makes in a life, or the way knowledge of MS is accounted for. In that sense, far from the surplus in the dtexts being that which is not directly of relevance to MS (which may be "cast away"), it may be the case that there is too much 'about MS' in the story of the life. But by the same token, it follows that since there is the possibility that the prominence of MS in the life was an artefact of the occasion of their telling, neither can it be concluded that the prominence is not what would have been given to MS in any case.

They are not, in any case, by virtue of their autobiographical partialness, a "whole life" but selections, on request and in regard to their communicative purpose. But that which has been 'selected' to refer to is unverifiable in terms of its degree of representativeness of the life, because no other details are available to readers about the life apart from those given in the dtexts (and those selections, selected from again in this text). 'The life' as referent of the words and the silences has to be extrapolated from the assumption of their typicality, of their power to reveal the character of the individual referring (partially) to his/her own life, and thus the dtexts have a readability predicated on the possibility of making at least intelligent and defensible guesses, as reader,
about other aspects of that life not present(ed) in the dttexts.

And: to do this, it has to be assumed that what is sought does indeed exist prior to its being sought 'in' the life that is being referred to (and that it is representable in speech/writing). Thus in order to read the dttexts as being about the life, about MS, what that aboutness could consist of and how it may be readably discerned beneath the surface, as the referent of the dttext, also has to be already known, understandable. (And this would account for why the dttexts are not surprising: to me as reader, they describe what it is like to have MS in the very way I would expect. It would also account for why, for me, that absence of surprise is itself surprising and that it is not).

So it seems that even if the language in which the life is described is understood as tending towards the literal and away from the literary to an extent significantly different from that in 'genuine' autobiographies (cf. 7.1), nevertheless, in that even to begin to assess the referential relation between the dttexts and the life they are about demands supplementation by readers, a supplementation that has itself no verifiability outside what they already know or are concerned with, the dttexts' 'literalness' vis à vis the life is both assumed and imposed. In that sense, it is the reading of the dttexts that tends towards the literal.
Furthermore: This reading work is working on a dtex that as much forms as is formed by 'the life'. Rather than the life (lying as a reservoir of all that could have been drawn on, certain aspects of which are drawn to the surface so to speak) giving rise to the autobiography which partially represents it, there are ways in which autobiography as a form of writing gives rise to 'the life' that it ostensibly (if problematically) describes (literally).

For instance: the idea of discrete facts. To seek for at least some literally described events as that which the author's interpretation interprets presupposes a neat, linear, sequential and therefore divisible thread of a life: into 'events'. Yet it would seem that the representation of a life is made according to the exigencies of narrative form - of "temporality and emplotment" (Ricoeur 1984); the requirements of narrative inexorably conferring form. It follows that that which is (re)told must in another way form rather than be formed by the life 'itself'.

And so, how does the pattern go? I mean does it have a pattern of remission and

Fortunately, well I hope not. I had a major relapse in seventy-four, and I lost most of the use in both my legs and that was when I was in hospital for about two and a half months, something like that. But then I had physiotherapy and ACTH. You know that do you? [14] - and got back a considerable amount. Then I had, two years later another relapse and I lost the use of my right hand, and then I had another relapse about two years later and I started losing the sight of one eye but since then I've not had a relapse, so I'm hoping the pattern is broken.
To say this then is to say that the chronology of a life is undercut by the fact that everything, every 'event' related (can) mean something else later, to a later self, to someone else (to another reader); is a general figure of this reading. And those second interpretations themselves may be replaced with yet another: "it" was incontinence...then venereal disease...multiple sclerosis...no longer MS (Bob). But it is not that "it" comes to mean different things, be differently defined, but that the referent of the "it" is not itself stable. There is no stable 'chronology' or even successive re-interpretations of 'the same' facts: no "it".

So whilst in their language, the dtexts may represent the (impossible) attempt on behalf of their authors to be clear (literal) about the life: yet, qua autobiography, there are ways in which it is only this that can be achieved. Only things that are now clear, at the time of producing the autobiography, are referrable to: things about which it is 'now' possible to know what 'clear' would be. So it is impossible to represent chaos - and thus chaos is not (re)presentable, not here. Although the (original) authors do refer to what they do not know, they can specify what it is they do not know and why. The reason given for not knowing is for example because they are not themselves knowledgeable about that thing ("I'm not a doctor" for instance: cf 130 above), not that anything about the life referred to is non-sense, meaning-less to the person whose life it is. So in that sense, the clarity with
which the life is described cannot be taken as arising from
the life, as the knowledge about the life held by the person whose
life it is, but is imposed on it by the limits of language.
It seems, then, that there is a residue which cannot be brought
to language; that cannot be represented, cannot be presented
except as an absence, and therefore can only be read into the
dtexts by those who could understand what else could have been
there. And even that has to be speculation.

[MS seems to be] the ultimate exemplar not only of
ontological but of epistemological nihilism.

And yet there is in the dtexts no chaos, nothing
that resembles the abyss. There is no pain. Indeed
everything is "just normal, just life, you know" (George).
The worst is always over, or somewhere else, never in the
present, the present of the dtexts' time. Yet that re-
markable absence of chaos is also 'in' the dtexts. This
extraordinary absence of 'madness', of nihilism...

Chapter 6: 172

And yet in a sense that is pre-figured in/by the dtexts themselves.
The authors state that "others" cannot understand what it is like
for them, even those who share the same situation ("other people
with MS" who are, according to George, for example, "so self-
centred"), an existential isolation that cannot itself be repre-
sented as such, only referred to as referent of an absence that
can be spoken of: the absence of "understanding". As authors,
referring to their own lives, it appears that they know that their
lives cannot be truly, literally, represented (to others). As
communicative texts, the dtexts cannot but fail to communicate.
And yet of course they do. But what it is that is (appears to
have been) communicated is unknowably related to the author's
intentions, to their view of their own lives. Others cannot know whether they really understand (or fail to understand) about that life: only feel as if they might by reading, re-enacting the words, "bringing the life (back) to life" (above:p223). What they understand is what they can, not what there is.

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But all that just written is also interpretation.

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How have we got here? By reading the dtexs in terms of their author's intentions - of a particular communicative purpose, to contribute to knowledge about MS - from which arose the interpretation of their texts' tendency towards "simpler referentiality": a beginning from which all else has followed. All that has just been written depends on the concept of 'the author's intentions', which is to say, has been predicated on the assumption of the dtexs' representing, or rather the question of the dtexs being that of the relation between the text and their authors' intentions. Which brings us, or rather returns us, to the other question of difference between the dtexs and the form of autobiography.

7.2.2 The (Uncontested) Readability of the Proper Name

It seems that the word [person] originally and exclusively meant a 'mask'. Naturally, the explanation of the Latin etymologists that persona came from per-sonare, the mask through (per) which the voice (of the actor) sounds, was invented retrospectively. (Despite the fact that there was a distinction made between persona and persona muta, the mute role in drama and pantomime).

Mauss 1973: 78
According to traditional literary theory, the verifiability of autobiography as a text of the author's own life (its epistemological status qua quasi-factual text), rests on "the subject whose identity is defined by the uncontested readability of his proper name" (de Man 1979: 920) which uniquely identifies them; places an individual on the point of intersection of time and space, amongst those others, past and present, to whom s/he is related and distinguishes her/him from all those others to whom s/he does not relate, is not; the proper name of a person which authorises and authenticates a text as autobiography.

The authority of a work qua autobiography rests on an identity between the voice of the author of the text and the nominated author who has affixed her name to the work. This naming of/by the author marks the claim of the text that the life written about is not fictional; that, even though only partially representable, even though the interpretation and significance of those elements which make up the life might have been glossed over, (cannot be taken at face value), in its bare bones of events and actions, what happened according to the text did in fact occur, and to that person.

De Man, however, contests, "the uncontested readability of the proper name", contending that, insofar as autobiography, as writing, cannot represent the person themselves, autobiography as a particular form of writing, is "a figure of reading", that produces effects, the effect of representation which, in being
representation and not the thing itself, masks the author-as-origin of the story: removes them beyond ascertainability by readers. Autobiography, as figure, both giving and taking away a face.

To the extent that language is figure (or metaphor or prosopopelia), it is indeed not the thing itself but the representation, the picture of the thing, and, as such, it is silent, mute as pictures are mute. Language, as trope, is always privative.

To the extent that, in writing, we are dependent on this language we are all...deaf and mute - not silent. That is to say eternally deprived of voice and condemned to muteness.

de Man 1979: 930

Thus "the author", whose proper name seemingly authorises the facticity of the life that an autobiography is about, is an effect of the figurality of the text, the form of writing that is autobiography. But for de Man, this is insofar as it is a literary rather than a referential form of text, "made up of tropological substitutions".16

Socrates: You know, Phaedrus, that's the strange thing about writing, which makes it truly analogous to painting. The painter's products stand before us as though they were alive, but if you question them, they maintain a majestic silence. It is the same with written words.

Plato The Phaedrus: 275e

What of the dtexts then? For their authorisation is effected differently: there is a deliberate disconnection between the dtexts and the proper name of the individual. The practice of anonymysing sociological data, regarded as part of the ethic
of the discipline towards its subjects, of altering "the proper name" (eg Chapter 4, Fn 4) violates, then, one of the ground rules of autobiography. The point is that the story(teller) cannot be identified.

The pseudonyms used to identify the dtexts do however refer to that proper name, or rather to the space where the proper name could have been. It is the space that identifies the words as the words of a (once) extant person; that refers to the face to which the words belong (out of which they once issued). But that face is masked by the name of anonymity, the mask (to follow de Man) re-masked by any-name: Daniel, George, Anne. The (only proper) name that is affixed to the dtexts is that of the researcher, my name, Anna Wynne ("AW"); a signature which works at once to protect the privacy of the individual, (for it is that proper name that identifies the names used in the dtexts as pseudonymous) and at the same time works to authenticate the relationship between the dtexts' actual authors and the life the dtexts refer to. The "contract" concerning the veracity of the name that has been hidden is made, in the dtexts, by surrogate, by the researcher stepping into the space left by the anonymising of the author(ship) of the original: authenticating the relationship between the empirical individual and the tale told; through the rhetorical figure of ethos persuading the readers of her own authority. Thus the author/ity of the dtexts as not-fictional is the name of the researcher, not only of her own part in the dtexts - as "AW" - but of the whole dtexts.
But it is not the researcher's proper name which authorises in itself, but insofar as it links her to the patronymy of tradition, refers back to the discipline from which 'she' takes her authority, a linkage/lineage by which the analyst/author is uniquely distinguished from other authors, authors of other kinds of writing: fiction, autobiography. And thus establishes the dtexts as not those kinds of text. Authorises them as what, at least, they are not. Insofar as, that is, the 'life' to which they refer is not hers, its meaning not hers. It is that reference back to the authority which is also the referent of her proper name which empowers the analyst-as-author (an author no less de-faced by the "readability of (her)proper name") as the impenetrable interface between readers and the originary authors of the dtexts.

But this is only the first turn of the "de-facement" of the original author(ship) of the dtexts. For the mask of anonymity, whilst masking each individual, works to unify each of the dtexts as one, their original authors as a collectivity.

*It was by conferring this unity on the authors that the reading of the dtexts as addressed communications in the previous section was made possible.*

For what matters for analysis, for analytic interpretation, for reading the dtexts (here), is that all their authors are members of a collectivity, collected for their sameness ('people with MS' rather than the individuals (re)named George, Daniel, Meg). Thus while each individual's autobiography represents that
individual and that unique life, at the same time it represents (to follow this reading) an experience presumed to be common to all or at least these particular people-with-MS (the claim may be more or less modest). To begin with, in order to begin, it was assumed by the researcher (a presumption of research) that MS imposes a certain symmetry amidst whatever individual differences there may be in the people's lives. And that in the individually produced, elicited, autobiographies, that symmetry will be unwittingly produced by each author and available to be discerned by a reader who knows what to look for, who expects that certain symmetry. The anonymising of the dtexts works to make the differences between each person as epiphenomenal, as it were, rather than fundamental. Each individual is a (any) subject.

Thus although the dtexts may be autobiographical, it is as a collectivity rather than as a collection that they are presented for sociological interpretation. But that subjecthood, that collectivity, is an effect - made possible by its presumption and inserted in the space opened up by the dis-placement of the original Proper Name.

Thus reading the dtexts as being about 'people with MS' is an essential de-facing the face that autobiographical writing confers: gives the authors, but now as a collectivity, an autonomous, recognisable existence. But at the same time taking it away, to be read beneath to the common phenomenon of MS. Assuming that all lives in which MS occurs have a commonality which makes them into one life - one form of life - makes any
individual life stand for MS and stand for any individual-life-with-MS. So the individual is created and de-faced. Through the work of language in the mode of reading to create and destroy.

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So: what has happened to the author/authority of the dtexts as texts whose putative referent is 'a real life', this second fold of the three-fold question of their reference?

First, the original author, whom the figure of autobiography already masks, is de-faced by a pseudonym; dis-placed by the proper name of the analyst, whose own name also has as its authenticating referent the discipline in whose name the work is produced.

But the pseudonym does not only open up the possibility for that different source of authority: it also makes all the original authors as one, the collective referent of 'any name', which in turn allows for the finding of a cohere-ing referent as 'the life' of that any-name - the life-with-MS.

And it is that tradition, the discipline which author-ises the epistemological status of the dtexts which provides the substance of that referent - the life-with-MS, the knowledge of which that discipline consists. In that sense, then, one could say that indeed, like all autobiographies, the author and the life the author's text is about (insofar as
they are readable; produced by reading) are identical. That in this respect there is no difference between the dtexts and 'genuine' autobiographies.

But then, what has happened to the question of the literalness/literariness of the language in which the life is represented? If we retain this latter reading that the author of the dtext is the discipline, then the tendency towards literalness is still predicateable on the intentions of that de-facing, dis-placing author: that it is the (sociological) intention of the dtexts which tends towards the literal. All that was said about the de-stabilising of that tendency/intention by the form of autobiography, qua genre, however, still holds.

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Since we have come to this point having begun with de Man, let me return to him in order to move away once more.

For de Man, the defacement of the subject and thus of the life as the life of that subject which is the effect of the figure that is autobiography is a consequence of the tropes exclusive to and definitive of literary language: its literary form. What I am doing is saying that in reading the dtexts against the form of autobiography (and it is my contention that this is not optional), the same stabilising/destabilising occurs.
But, *pace* de Man, this is not insofar as the dtexts are literary productions but as a consequence of the very nature of the reading to which they have been here submitted. It is in their being written down which makes it possible to read them (impossible not to read them) against the form of autobiography in the first place. In their being in writing, independent of its possible genre. Which seems to bring us again to Plato: but to oppose him. For far from "maintaining a majestic silence", the dtexts, as writing, never cease responding to (my) questions.

BUT: This has been to read the dtexts paying attention only to one voice: the person-with-MS; to autobiography as a figure of reading that is essentially mono-logic, a reading that has produced an argument about the interpretability of the dtexts that has, perhaps consequentially, ignored the fact that the dtexts are also dialogue. Which brings us to the third fold of the three-fold question of the dtexts' referential status with which this Section began: of the dtexts to the interview as the event which is their most immediate referent.

The question of the dtexts as dialogue also raises at last the question of the difference between writing and speech: a difference which has, unintentionally perhaps had its own
effects on the last two chapters. For the Question of the Self was concerned with self-reference in speech, and the re-reading against autobiography concerned the dtexts as writing. This third fold of the question of the dtexts' referential status concerns for the first time both speech and writing: writing as the representation of an event, writing whose putative referent is an event in speech.
CHAPTER EIGHT
READING AGAINST FORM: OF DIALOGUE

This essential drifting, due to writing as an iterative structure cut off from all absolute responsibility, from consciousness as the authority of the last analysis, writing orphaned and separated at birth from the assistance of its father, is indeed what Plato condemned in the Phaedrus. If Plato's gesture is, as I believe, the philosophical movement par excellence, one realises what is at stake here.

Derrida 1986: 316

So: to the third fold of the three-fold question of the dtexts' referential status, to that which gives the dtexts their uniqueness vis à vis other forms of the written: the relation of the dtexts to the event - the interview, the conversation - that is their "most immediate" ostensible referent.

* 

By certain typographical conventions, what each dtext represents as it lies on the page is a dialogue. But how does that bear on the question of their referential status? For the written form of dialogue does not in itself necessarily entail that it has as its referent an actual event.

The description of writing as 'majestically silent', cited
in the previous Chapter (p234) is from the Phaedrus, one of the Dialogues of Plato (1952), which are perhaps the classic exemplars of the form. But Plato's Dialogues are generally regarded as having a highly complex and mediated relation to the events to which they refer:

Socratic Dialogues

This term may be conveniently given to the dialogues which on stylistic grounds may be assigned to the earliest period of Plato's writing: probably before his last visit to Sicily and Italy in 387 BC. It is possible that he began writing even before Socrates' death...Towards the end of the period the dialogues become more and more elaborate and Plato is beginning, it seems, to wrestle with the problems for himself.

Ferguson 1970: 35

As Plato, however, began "to wrestle with the problems for himself" (and the Phaedrus is generally assigned to the later Middle Period of Plato's writing), as the Dialogues become ever more distant from their originating source, Plato retained the dialogue form, not for the sake of its representational faithfulness to those long past events, but, it is argued, for its unique appropriateness as the vehicle for his philosophy.

The reason for its appropriateness is, according to Rowe (see Figure 8.1 overleaf) eminently plain (cf. also Hamilton 1988; Nussbaum 1986: 87-8 and passim; Rowe 1984: Chapter 4):

Plato regards the dialogue form as capable - at least to a degree - of transcending the limits of ordinary written discourse...One of the dangers of such compositions (as Socrates tells us) is that they inevitably give us a false impression of certainty.

Rowe 1987 (Fig 8.1)
Living dialogue

Self-Knowledge in Plato's Phaedrus
by Charles L. Griswold Jr
Yale University Press, 328pp, £28.50
ISBN 0300035942

The Phaedrus is generally recognized as a rich but deeply problematic work. Two problems in particular confront the interpreter. One is that while Socrates is allowed in the course of the dialogue to lay down clear rules for the arrangement of written (and spoken) compositions, the dialogue itself seems to disobey those rules, and indeed to lack a unifying structure or theme of any kind. The first part consists largely of three set speeches on love, the third of which contains the famous image of the soul as a charioteer and his horses, driven upwards by erotic impulse towards knowledge and truth; then, in the second part, Socrates and Phaedrus embark on a long discussion of rhetoric, which appears completely to lose sight of the substantive topics of the first part.

The second problem concerns the final section of the conversation, where Socrates devalues the written word by comparison with the spoken—or with one form of it, the living conversation between philosopher and pupil. This alone, he suggests, can lead to genuine intellectual progress. Does Plato then mean to devalue his own written products (including the Phaedrus itself)? But why then did he write so much? Or is the criticism of writing undermined by the very fact that it is itself made in a written work?

In this second problem, Griswold firmly sides with what is probably the majority view, that Plato regards the dialogue form as capable—at least to a degree—of transcending the limits of ordinary written discourse. But he argues the case in a thoroughly original way, which is closely linked to his answer to the first problem, about the structure of the Phaedrus. Through its systematic arrangement, Griswold thinks the dialogue form deliberately sets out to mirror that of real dialogue, and to distance itself from "artfully written compositions." One of the dangers of such compositions (as Socrates tells us) is that they, inevitably give a false impression of certainty. Live conversation avoids this danger because of the constant possibility of the questioning of any idea as it is put forward; while its written counterpart in the Phaedrus achieves the same effect by means of what Griswold identifies as a dialectical or "palinodic" relationship between its various sections, whereby each successive section partially undercuts and qualifies the one before.

So in the third speech of the first half Socrates recants the results of the earlier two, but goes beyond anything he can justify in rational terms; the section on rhetoric then develops an account of a technical dialectical method, which while it allows some advance towards clarity is seen as incapable of illuminating many of the essential dimensions of the objects it studies (hence, according to Griswold, the stripping away of the "metaphysical, mystic, and religious" aspects of Socrates's main speech in the account given of it in this section); and finally, in a kind of Hegelian synthesis, the last section proposes a third form of discourse—actual, living dialogue—which will combine both the "madness" of Socrates's speech and the sober techniques of true rhetoric. In its own way, the Phaedrus itself invites us, as readers, to enter into conversation with it.

The purpose of the exercise is that self-discovery (the "self-knowledge" of Griswold's title) which Socrates identifies as his project at the beginning of the dialogue. This Griswold sees as the unifying theme of the work. What Socrates seeks, and each of us is to be persuaded to seek, is to know himself both as an individual and as a member of the human species; which will include knowing those things that are truly desirable for him. But the continuing reflections on method themselves already teach the importance of one dimension of the Socratic concept of self-knowledge: knowledge of one's ignorance, and of the limits of human understanding.

The immediate question, according to Griswold's own strict rules of interpretation, is how much of this is in any clear way verifiable from the text. The answer is perhaps about half. Down to the end of Socrates's central speech there is much that is genuinely illuminating in Griswold's treatment; from then on, however, his interpretation looks increasingly forced. Why, for example, does he discuss of the rhetorical so consistently pretend to be about the right way of speaking and writing, if it is really about an inferior sort of dialectic? Irony, Griswold will reply, and there is certainly plenty of that in the Phaedrus; but if it is quite as extended as he suggests in this case, it begins to look pointless and perverse. Nevertheless, this is a useful book, which amounts amply to a full commentary on the dialogue. But its special merit is that it makes an imaginative attempt, of a kind that has rarely been made in any detail, to come to terms with the Phaedrus as an artistic (and philosophical) whole.

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If to that suggestive depiction of the virtues unique to the form of dialogue is added not only the question of the relation between the form(s) and the interpretability of the dtexts already under discussion here, but also of the consequences of the difference held to exist between writing and speech (opened up in the previous two chapters as fundamentally involved in the problematic of the dtexts), (writing about) a re-reading of the dtexts against the Phaedrus becomes rich with possibility.

Having come to a point where the question of the difference between writing and speech — and all that is held to entail — began to be pressing, and alerted to the interpretive relevance of the form of the dtexts as dialogue by Rowe's review, I was reminded7 that Walter Ong (much cited in current sociological interest in the question of writing)8 substantiated his summary of the classical opposition to writing in this same Dialogue:9

Writing, Plato has Socrates say in the Phaedrus, is inhuman, pretending to establish outside the mind what in reality can only be in the mind. It is a thing, a manufactured product... Secondly, Plato's Socrates urges, writing destroys memory. Those who use writing will become forgetful, relying on an external resource for what they lack in internal resources. Writing weakens the mind... Thirdly, a written text is basically unresponsive. If you ask a person to explain his or her statement you can get an explanation; if you ask a text, you get back nothing except the same, often stupid, words which called for your question in the first place... Fourthly, in keeping with the agonistic mentality of oral cultures, Plato's Socrates also holds it against writing that the written word cannot defend itself as the natural spoken word can: real speech and thought always exist essentially in the context of give-and-take between real persons. Writing is passive, out of it, in an unreal, unnatural world.

Ong 1982: 7910
Having never read Plato, I myself turned to the primary text: and read something quite different. It should go without saying that this is as much reading the Phaedrus against the dtexts and the general concerns of this thesis as the other way about. It cannot perhaps go without saying that the interpretation of the Phaedrus which is to be advanced in this chapter is in fundamental opposition, so far as I am aware, to that of traditional and even not so traditional philosophers' interpretations (Derrida 1981; Nussbaum 1986).

Although certainly throughout the Phaedrus continual reference is made both to the spoken and the written word, what is at issue are the differences between bad and good, proper and improper writing-and-speech - as media for the pursuit of knowledge. To read the Phaedrus as consisting of Plato's injunction against writing as such is to ignore the form in which the dialogue is written. On the contrary, the substance of the Dialogue occurs within the folds of such a convoluted form that no "statements" can be taken at their face value. For in reading any particular passage of the text, the whole form of the Dialogue as dialogue and the movement of the argument within it are set in play against each other. In a similar way, no few lines may be taken to stand by (for) themselves in the dtexts without remembering (an idea of) the whole form of the text. Although in both cases, when I say "no few lines...may be taken", I mean that it is only when they are so extracted that it is possible to interpret them with confidence: to extract the things that Plato held against writing, what people say about their MS. It is only close reading that
makes either text difficult, sets each part into oscillation. At first reading, both are excessively simple to understand. One of the things that makes them so simple and, if thought about, so difficult, is their common form — as dialogue.

So: what effects does the form of dialogue have on the interpretability of a text’s substantive matter? What may be learned about the dialogues as knowledge, the knowledge of people with MS, from the Phaedrus? Or, to put it more complexly, what is engendered14 in the reading of the two dialogues against each other?

8.1 EFFECTIVE FEATURES OF DIALOGUE

As a form of the written, dialogue:

I Refers to a unique event:

i in space and time;
ii involving actors;
iii in relation to each other;
iv occurring in a particular way.

II Refers to an event of speech:

i in which speech represents speakers;
ii which is about something.

III Refers to the time dialogue takes:

i giving a context for the interpretation of particular passages in their sequence,
ii and their place in the whole,
iii for action and re-action. For something to happen. For the movement of inquiry.
I DIALOGUE REFERS TO A UNIQUE EVENT

The first essential feature of dialogue is that it is an event, occupying a unique position in time and space. Insofar as a text in dialogic form claims through its particulars an identifiably unique position qua event, this marks its basis in and therefore the text's relation to reality. Such particulars do not only identify an event as the event it is (as referent of the text) but in their precise nature, become one of the means whereby the event is interpretable.

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277 Socrates: Where do you come from, Phaedrus my friend, and where are you going?

Phaedrus: I've been with Lysias, Socrates, the son of Cephalus, and I'm off for a walk outside the wall, after a long morning's sitting there. On the instructions of our common friend Acumenus I take my walks on the open roads; he tells me that is more invigorating than walking in the colonnades.

Socrates: Yes, he's right in saying so. But Lysias, I take it, was in town.

Phaedrus: Yes, staying with Epicrates, in that house where Morychus used to live, close to the temple of Olympian Zeus.

Socrates: Well, how were you occupied? No doubt Lysias was giving the company a feast of eloquence.

Phaedrus: I'll tell you, if you can spare time to come along with me and listen.

Socrates: What? Don't you realize that I should account it, in Pindar's words, 'above all business' to hear how you and Lysias passed your time?

Phaedrus: Lead on then.

Socrates: Please tell me.

Phaedrus: As a matter of fact the topic is appropriate for your ears, Socrates, for the discussion that engaged us may be said to have concerned love. Lysias, you must know, has described how a handsome boy was tempted, but not by a lover—that's the clever part of it. He maintains that surrender should be to one who is not in love rather than to one who is.

Socrates: Splendid! I wish he would add that it should be to a poor man rather than a rich one, an elderly man rather than a young one, and, in general, to ordinary folk like myself. What an attractive democratic theory that would be! However, I'm so eager to hear about it that I vow I won't leave you even if you extend your walk as far as Megara, up to the walls and back again as recommended by Herodotus.

Figure 8.2

There is a difference in the means by which the event referred to is represented in the dtexts and in the Phaedrus. For example, whereas in the Phaedrus the place of the event in time and space is vividly established within the first few exchanges (Fig 8.1; 227a), in the dtexts such details as occur, occur as if incidentally, dispersed throughout the texts and never all those necessary. Thus although both texts powerfully assert a present in which the event occurred, the very richness and sufficiency of the detail given so early in the Phaedrus, when compared to the way real dialogue (the referent of the dtexts) only incidentally and tangentially refer to their own setting marks the former, by its very artistry, as the invocation of (rather than the record of) an actual dialogue; the text a product of skillful writing.

As such, the details of the Phaedrus' setting may not only be understood as a fictional 'literal' description but, because of the reading invited by language used literarily, the movement of the text's interpretation is urged towards figure, the descriptors in/of the Dialogue as figuratively evocative as they can be: occurring outside the city walls, through the heat of the afternoon and so on, adding to or even providing the key for an interpretation of the whole text as a philosophy (cf. Nussbaum 1986; Ferrari 1987).

The details in the dtexts, insofar as each dtext is by comparison understood as the record of an actual event, on the contrary it seems require only to be read literally. The identification of the dtexts qua event occur in the main outside the event 'itself' - given for example in Appendix 3.2, this text. They too however work as 'more' than literal in that being cited
outside the main text, in an Appendix, they symbolise, stand as evidence for the status of the dttexts as authentic, the dttext as the record of a dialogue which actually occurred (and see 236 above).

I. ii ... a unique event involving actors...

What happens in a named time and place is... activity. To name a setting presumes activity; awaits, forecasts, demands the presence - of actors. In the Phaedrus the speakers present themselves and their setting together, at the very beginning. Thus there seems to be no author producing effects by writing but rather the unmediated immediate presence of Socrates and Phaedrus come upon by us as they come upon each other, revealing who and where they are in their speech. The dttexts do not set themselves up in this way: the scene is explicitly set by the presenter, the author/analyst who places the speakers on the appropriate co-ordinates and then as it were steps back into silence (cf Chapter 3.3). Because of the 'actual' or 'artefactual' absence of the author in/of the dialogues, in both texts the voices become readable as speakers speaking for themselves. But the quality of that becoming is tied to the quality of the absence of the authorship. The fact that the dttexts appear unauthored in this sense makes their reading as voice, unmediated presence, much stronger: no 'Plato' using dialogue to express his philosophy, playing on the possibilities of form. Consequently, the interpretation of the dttexts' substance appears to be free of the problem of author's
intentions; what an author, a third party, presumed living
dialogue to be like or the form to be useful for, but simply
arising out of the event itself; an event reciprocally authenti-
cated as the unmediated and actual referent of the dtexts.

Insofar as there 'is' no intervening author, this in turn has an
effect on the relationship of the reader with the text. For
rather than the reader's position being the one addressed by the
text (as is still the case with prose for certain contemporary
literary theorists, even given 'the death of the author', cf.
Tompkins 1980), with dialogue the reader is placed in the
position of (as if in the position of) being, rather, a witness
to the communication between others: the communication occurring
discretely within the (referent of the) text, the referred to
event. This position of the reader as over-hearer renders the
object of interpretation the interlocutors themselves, via their
own words; of seeing how the speakers understand or fail to under-
stand each other on the basis of how one responds to the other.

But insofar as the absence of the author in the Phaedrus
is a stylistic choice (cf. 243 above), 'Plato' may be re-invoked
and the involvement of the reader as the author's addressee
restored: the interpretative relationship the problematic one
of reader-text-interlocutors-author. Insofar as the absence of
an author in the dtexts is deemed actual, the reader's inter-
pretative position apparently remains simpler: reader-text-
interlocutors.
The kind of event a dialogue refers to offers a further context for its interpretation. How the speakers speak together, the relationship between them, opens the interpretation of the substantive matter of their talk in a particular way.

In the Phaedrus, the relationship is named at the very first as friendship: "Phaedrus, my friend" (Fig 8.2; 227a). Insofar as we know what friendship is (then, still now), their talk is then yet another instance of what it is to talk between friends, to be interpreted in that light. Intractable additional interpretative problems are thereby opened for the reader of such a written dialogue, who then cannot ignore the possibility that the tone of the original speech might have been informal, teasing, sarcastic, familiar; possibilities unmarkable in speech in its written form but which, insofar as the dialogue is deemed to be authored (the very specification of the relationship as friendship of significance) may be part of the author's intended meaning. If one were to retain 'Plato's intentions' as essential to the interpretation of the Phaedrus, this very ambiguity of the written as opposed to the spoken word could arguably be part of his 'case against writing'.

The dtexts appear to represent a different kind of relationship between the interlocutors. In several of them AW asserts at the beginning "I know nothing about you", and in any case, as the event unfolds, her questions and her interlocutors' replies demonstrate their lack of mutual personal knowledge. As strangers
then, following this interpretation, their talk will differ from talk between friends; as writing, the wild disruptive effects of tone almost certainly absent from the dtexts not because un-markable in writing but because they were unlikely to have been there in the original event.

I.iv ...coming about in a particular way.

In the Phaedrus, the setting of the talk describes what is to come as that which has occurred by chance, Socrates and Phaedrus coming upon another out walking (Fig 8.2; 227e). Introducing themselves as friends, meeting each other by chance, it follows that they can talk about anything that occurs to them. The fact that philosophy is what they talk about echoes/exemplifies what Socrates will later say and with which Phaedrus will agree, that to have a concern for philosophy is to have a concern that consumes all that life, is that life (257b). But that interpretation is dependent upon the 'chance nature' of their encounter, the nature of the unique event represented in/by the text of the Phaedrus.

By contrast, as a pre-arranged meeting between strangers, for a specified purpose ('tell me about your MS' - cf. 190 above), the dtext dialogue is more closed in its potential scope than the Phaedrus, or rather readable as closed: the substance to be about MS, the interest in talking about it not necessarily mutual; the occasion of the unique event thus reinforcing the 'fact' that it is between strangers and to be interpreted as such. The dtexts thus do not conform to Rowe's demarcation between "living dialogue"
and the written as lying in the former's comparative open-endedness. For the very idea of open-endedness at every moment which Rowe describes (Fig 8.1) is an effect that lies within the power of a writer (Plato-as-author) to represent. (Rather undercutting his endorsement of Griswold's argument, Rowe, in deploring the lack of a unifying theme in the Phaedrus, ie the Dialogue's very open-endedness, is pointing to the very success of Plato's conformity to the idea (Rowe's idea) of the unique characteristic of living dialogue).

After the first time of reading, the opening of the Phaedrus can also be read as opening up from the very beginning the possibility of reading the dtext, its form and its substance, in play against each other. For the question, Socrates' very first inquiry - from whom and from whence Phaedrus comes - are to be precisely those questions which Socrates will later reproach him for asking: he will admonish Phaedrus that the proper question concerns only the truth or falsehood of what is said, not who speaks or what country he comes from (275c). But by then, it will have been that very question of who the speaker is which has been set up as the enquiry of philosophy, the question of the self inextricably bound with the pursuit of knowledge of the truth (230a). Except: yet again to anticipate, the method which is apparently being favoured in the Dialogue for that pursuit is precisely dialogue; the process of statement and refutation in which Socrates needs to have Phaedrus say that which appears to be important but will be shown not to be so; to state that which is obvious in order for its grounds to be
put into question. In which case it is critical who says which words; that it is Phaedrus who replies to this question. And so on (and see III.1 below).

The infinite contestability of any proposed interpretation of the Dialogue's meaning that is advance in/by the text that is to unfold is present from the beginning; made present through the possibility of re-reading passages of the texts against the context of others. In that that possibility is given by writing which evokes the event of dialogue, the content of the text is rendered doubly ironic; attention oscillating between the form in which it is written and the event to which the writing purports to refer: to the difference (pointed to) between its appearance and the reality which it could have been representing as referent.

Ironic play in the event of the dtexts is readable in the voice of the narrator as was discussed in Chapters 7 and 8; but this is a different mode and attribution of irony. And besides, such potential for irony of the dtexts comes after, in the writing about them (rather than in the re-reading of their substance). This difference between the two dialogues makes in turn a difference; unlike with the Phaedrus, it turns attention away from the form of the dtexts as writing towards their referent as that which is interpretable (and see the early chapters of the thesis and the break at 4/5).

But: the readable presence or absence of irony in the dtexts is effected by their referential relation to an actual event, a relation that cannot be determined from the texts themselves.
II DIALOGUE REFERS TO AN EVENT OF SPEECH

...in which speech represents speakers...

The event which written dialogue represents is an event in and of speech. What follows (although differently in each text because of how the absence of the author is construed) is that the words on the page represent speech which represents speakers, present in their own words. What is involved in the reading of dialogue insofar as it refers to an event in speech is an apparently unproblematic instance of the classical (and currently contested) prioritisation of voice over writing. For as the representation of an event in speech, the written form (would be that which) clearly comes after the speech. This is vital to the argument (by Rowe, for example, Fig 8.1) that being (written) in dialogue, the Phaedrus represents Plato's preference for speech as the medium for philosophic enquiry. It is also, of course, vital to the interpretation of the speakers through the texts (of their 'understanding of science', their 'experience of MS').

That (written) speech represents the speaker is apparently a premise of the Phaedrus:

At first, Phaedrus evinces reluctance to report (paraphrase) Lysias' speech, but having been teased into the admission that he has been practicing its oration for just such an occasion, consents to begin. But before he can, Socrates interrupts:

Socrates: Very well, my dear fellow, but you must first show me what it is that you have in your left hand under your cloak, for I surmise that it is the actual discourse. If that is so, let me assure you of this, that much as I love you I am not altogether inclined to let you practice your oratory on me when Lysias himself is here present. Come now, show it me.

Figure 8.3
That is, Lysias himself is brought to the present of the dialogue-as-event in his "actual discourse."

At the level of the event, read after the first time, after, (to concur with Ong, p245 above) what will be said by Socrates against writing, in that Socrates then proceeds in fact to 'misuse' and 'abuse' Lysias' argument about love without Lysias having any opportunity to defend his own words, this passage becomes interpretable as an exemplification of one of the very things about the written that he is later (held) to denigrate: the fact that writers cannot protect their own work from abuse (275e). But speakers can. Actually being "here present" in his speech, Phaedrus can defend his own opinions of Lysias' speech to Socrates (234c/e). At the level of the text, in that Phaedrus' speech is itself in written form, that he cannot now defend his words from my 'abuse' (p263 below) merely endorses the contrast with and this ground of preference for speech. However, in that the Phaedrus is written, the 'abuse' that Lysias' speech is subjected to can be put into question, re-interpreted by reading this passage in the context of what is to come after: a possibility unique to writing.

What at the level of the event appears as an irony against writing reappears, read at the level of the text, as one of the virtues of the written, the play offered by one reading against the other itself a virtue unique to the written form (and/or that very play, that very indecipherability of the intended meaning, a further mark against writing, demonstrable only in a written text about the difference between writing and speech).
However, the opposition being discussed in this passage of the Dialogue is in any case not as simple as speech as against writing. Lysias' 'speech' is a written text, delivered orally. Phaedrus' offer was to recite what he remembered of it, a recitation that would represent only its essential gist (228d/e). Socrates, then, is asking to hear the written speech because it is the primary source, not because it is written by and 'is' Lysias himself here present; unmediated by another's interpretation. But re-read in the light of what is to come, perhaps the crucial target of Socrates' attack is the fact that Lysias' speech is an argument designed to persuade an audience to his view of a question, that is not written in dialogic form, giving thereby a "false impression of certainty" (Rowe 1987) about the question; the fact that it is monologic, making it an example of the improper use of the art of rhetoric: for the sheer amoral joy of manipulating an audience (see 291 below).

At this point in the event, the presence that is directly being put into question is the presence of Lysias-in-his-writing, the presence of author-in-text. At the level of the interpretation of the Dialogue as a whole, form against substance, the double questioning of presence expands to that of 'Socrates' in 'his own speech'; and of 'Plato' in 'his writing' - a play made possible by (reading) writing in the form of dialogue which invokes a referent though it does not have one ("Play", that is, in the engineering sense, of the slack that allows, is essential for the movement of one surface against another: for movement).
As in the *Phaedrus*, in the dtexes the presence(s) invoked by speech at the level of the event is not just that of the interlocutors. But the others who are brought into the dialogue as if they were "themselves here present" are brought to the present of the dialogue's time as event through reports of their speech (see for example pl91 above) (and pace the ostensive reference held, for example by Ricoeur, to be the differentiating and exclusive mode of reference of spoken-as-opposed-to-written discourse (Ricoeur 1981: 148-9)).

In the *Phaedrus* it is Lysias' absence from the time of the dialogue which is present in the text, his possible presence invoked through his writing. In the dtexes, those others are also absent, but brought to the present of the dtexes' dialogue by the invocation of their speech: that is more immediately. Indeed, the dialogue of the dtexes to a great extent consists of the reports of dialogues with or between others: "I stopped and I said, 'wait a minute. Did I hear you right? Was that what you said?' And he said, 'you haven't been told?'" (Daniel); "I was talking to my neurologist about it and he said, 'well fine..!'" (Mea) and so on. To tell of a life is to tell of dialogues with others, through speaking of which the life is represented.

But the referred-to-speech of others is not given verbatim. This opens towards an interpretation by the reader of the dtexes of the referer, the speaker present in the dtex-event who is referring to them. For in reporting the speech of the absent others, their meanings and intentions, the speakers in the dtex dialogues in exposing (only) their own interpretations of the original speech, offer insight to the character and understanding of them-
selves - as persons who would interpret others' speech in the ways they do, revealing how they make sense of 'the world'.

Thus the reader's work of interpreting the interpreter, how the 'author', the speaker in the dialogue, is interpreting those to whom/whose speech s/he is referring, is intermittantly operative. This produces as an effect a constant shifting of the reader's relation to the dtext and to its referents beyond the immediate, in a way different from the reader of the Phaedrus (in that the latter has no actual referent, but a consistently mediated appearance of a relation to one).

But this effect is a concomitant of the premise that speech recorded verbatim (the speech of the dialogue-as-event) is the unmediated representation of speakers' speech, a premise of difference that holds only insofar as the form of dialogue-in-writing is held to be neutral in effect.

There is yet another interpretive effect of the reference to past speech with others in the dtexts: for in that those referred to dialogues, qua dialogues, share the effective features of the form - as event, having a relation to reality and so on - the time of the past in which the life was lived through is brought not only to the present of the dialogue's time but straight through to the present of the reading of the dtexts. A continuous time-dimension is thus effectuated as the environment, as it were, in which (all) speech occurs, the present to which the dtexts refer a continuity with the time of reading which is always now: connecting the reality of reading, this event, to the
referent of the dtexts, thereby producing the reality of that to which the dtexts refer — beyond the textual record itself. It is this continuity of the past present of speech with the present of reading that engenders the obviousness of interpreting the dtexts through what the reader (now) knows. (at every time of reading). But it is an interpretative opportunity effectuated by virtue of the constitution of the continuity of time by the interlocutors' reference to dialogues which are/were in speech and not, as in the Phaedrus, in writing.

At the same time, in that the constitution of a seamless web of time as the dimension in which speech occurs is one of the effects inherent in dialogue qua form, once rendered visible, rests in turn on an adherence to a difference between presence in writing and in speech.

III DIALOGUE REFERS TO THE TIME IT TAKES

As written form, dialogue not only refers to an event at a point in time, it has as its referent the passage of time: the time talk takes. As the dialogue as an event presages actors, so the dialogue as an event which itself takes time presages action; something that happens through the dialogue, the speech of one with another. Action and re-action.

This time dimension opened up by the form once again operates differently as an interpretative context in the case of the Dialogue and of the dtexts (a difference, to reiterate, engendered not least by holding a difference between them as referential texts).
In that the Phaedrus is a representation rather than the record of an actual event, unlike with the dtexts, its order could have been written otherwise; it thus invites a reading which pays attention to the precise topography of the speeches, their relation in the internal order of the dialogue-event; to the argument of the text as a whole.

Thus: that which occurs (is said) early in the Dialogue is that which is open to change, to development, through the time dialogue takes. Phaedrus, having read the text of Lysias' speech aloud to Socrates, asks for his opinion:

Phaedrus: In fact it struck me as an extravagant performance, to demonstrate his ability to say the same thing twice, in different words but with equal success.

Phaedrus: Not a bit of it, Socrates. The outstanding feature of the discourse is just this, that it has not overlooked any important aspect of the subject, so making it impossible for anyone else to outdo what he has said with a fuller or more satisfactory oration.

Socrates: If you go as far as that I shall find it impossible to agree with you; if I were to assent out of politeness, I should be confuted by the wise men and women who in past ages have spoken and written on this theme.

Phaedrus: To whom do you refer? Where have you heard anything better than this?

Socrates: I can't tell you offhand, but I'm sure I have heard something better, from the fair Sappho maybe, or the wise Anacreon, or perhaps some prose writer. What ground, you may ask, have I for saying so? Good sir, there is something welling up within my breast, which makes me feel that I could find something different, and something better, to say. I am of course well aware it can't be anything originating in my own mind, for I know my own ignorance; so I suppose it can only be that it has been poured into me, through my ears, as into a vessel, from some external source, though in my stupid fashion I have actually forgotten how, and from whom, I heard it.

Given time, he can then be shown to have been drawn by the movement in/of the dialogue from confidence in his own opinion towards
a recognition of the fallibility of his pretentions to knowledge: his own ignorance. Or rather, readers, overhearing the event as it proceeds, can understand Phaedrus to have understood nothing about his own ignorance, as he repeatedly agrees with whatever Socrates says: "Yes indeed"; "Of course" (Fig 8.9: 258c/d).
Indeed his final utterance in the Dialogue will be to say that, "friends have all things in common" (279c), apparently surrendering to Socrates the entire onus of the work of thinking. The potential for change, for an increase in his knowledge opened up by the time dialogue takes marks Phaedrus' failure. And our own success at noticing his failure, a mark of our own enlighten-
ment effected, unlike him, by Socrates, reading the Dialogue through time (a time that may be repeated, re-lived through, which is the exclusive virtue of the written): the time of the dialogue 'itself' however crucially being uni-directional, mono-
linear. In fact it is the irreversible linearity of speech that allows for the self-questioning movement that is the Dialogue's quintessential virtue as a form of enquiry. Inquiry which needs time.

By contrast, and in that the order of speeches arises in the dttexts from the event and not as a consequence of decision, not arranged as constituting an argument, it is not so much the sequence of the speeches that is the relevant context for interpretation but rather the 'whole'; the period of time it takes to transfer the experience/knowledge of MS from one to another, for all that ideally could have been transferred, the question then becoming
one rather of how sufficient, how complete, that transfer was.

But when it comes to their reading, the temporal dimension
of the dialogue qua referential form suggests a way of interpret-
ing the substance against the sequence in which the speech occurred
during that period. Given the effective features of the form, in-
sofar as the dtexts' referent is a dialogue which began between
strangers, the time to which the dtext refers provides the time
for a deepening level of intimacy, of trust, of frankness. Read
in this way, a horizontal dimension of movement is discernible in
the dtexts that offers a criterion for the interpretation of
passages according to where they occurred along this time dimension.

As strangers, not friends, not everything can be spoken of
at first: what is spoken of rather beginning with the neutral and
most distant events; reaching in those interviews which were the
most successful (the experience of MS the most faithfully trans-
ferred) intensely private talk of life and death: winding up again
to the surface, to end the interview at the level of the everyday,
the reversion to strangerhood. Marks of such a cadence such as,
"I've never told anyone this before..." (James) and, as already
referred to in Chapter 6, Daniel's return to an incident the
second time revealing it to have been in fact utterly negative,
utterly bleak and despairing, themselves rely on time: both
revelations occurring well on into the time the dialogue took.

To identify a cadence of intimacy developing through the
time of the dtexts' referent however relies on taking the usually
unspeakable as the most significant; to interpret through the
fore-knowledge of what is usually sayable in such circumstances: above all, that that which is the most terrible is the most true (what is said most faithful to the experience itself). This sits in stark contrast against the Phaedrus' portrayal of the true as, "the right, the beautiful and the good" (278c). But that is a contrast arising from the other effective features of the reference of dialogue: the sequence of speech through a period of time between strangers.

As a context for interpreting the matter of the text, then, the temporal dimension of talk opened up by dialogue works in the texts to offer the idea of a whole experience (the time for every aspect of MS to be referred to) against the idea of the depth of the experience, the closeness of the words to the experience, the matter to be transmitted.

But in that sense, against appearances, the time referent of the dialogue increases the interpretative problematic of the texts, for the breadth/depth of what is conveyed does not cumulatively form a richer dimensionality to the experience as that which may be being conveyed. Rather they are in conflict. For whilst the cumulation towards the completion of a full account requires each reference to the experience to be equivalently faithful to it, the 'cadence of intimacy' developing through the time the dialogue takes suggests that each reference cannot be equally faithful.

However, the relation of particular passages of talk to the experience spoken of, neither their breadth nor depth (their
completeness nor their faithfulness) can be ascertained either from the words themselves or from their place in the context of the time the event took without reference to the dimension of time opened up as an effect of the form, effectuated in/by the dtexts as dialogue-in-writing.

The passage cited in Figure 8.4 from the Phaedrus also raises another aspect of the effect of temporality on interpretation (the consideration of which will prepare the way for the second part of this chapter, concerning the kind of inquiry dialogue represents), which has to do with the kind of movement of argument that the form of the dialogues allow.

Compared with Phaedrus' early claim to knowledge (Fig 8.4: p262 above), Socrates' early claim to an ignorance of himself in the same passage marks (as) the beginning of an unfolding through the time of the Dialogue of what it would be to pursue his own questioning. At this point in the Dialogue, that Socrates may be ignorant and still speak authoritatively, qua philosopher, is a puzzle that may be accounted for by reading the text rather than through to the event. For what at this point he feels "welling up" in him to say does not, he says, originate with him. He knows himself (refers to himself as) merely a "vessel" for the "something better to say" on love than Lysias, the precise source of which he has "actually forgotten" but the general source of which is the works (writings amongst them: 235a) of others more knowledgeable than him. His speech, his knowledge, originates elsewhere.
But this reference to the authority of his speech residing in the works of others, his endorsement of the authority of others, will be undercut in its turn (in time). For the speech on love he is then "impelled" to make (237-241d) is the very speech that he will regret (see Fig 8.8). This endless refutation of statements (deemed to be the essence of Socratic dialogue) is dependent on the monolinear temporality of dialogue. (To pursue this point here: this passage, ironicised by what comes after - by a particular passage, that is, that comes after - is then re-interpretable as meaning that the fault lay not so much in his reliance on sources from elsewhere which should have been rejected tout court, but his failure to engage them in dialectic, the movement towards truth that requires the (works of) others in order to begin but requires that the other be questioned. (That 'Plato' is not 'against writing' is arguable upon this basis: that the dialectic essential to enquiry is the mode made possible by the representation of speech in writing rather than in speech itself: in the Phaedrus's referential form).

Initial claims to ignorance are also made early in the dtexts' dialogic event - by AW: thus it is the people with MS who are deemed 'the others', those who are knowledgeable about the matter under inquiry, of themselves and their knowledge of MS. They tell AW through the time the dialogue takes more than she tells them: it is they who respond to her questions; she questions only them. During the course of the dialogue it follows that the latters' knowledge is similarly being "poured into" AW.
If the claim is at this point in the *Phaedrus* that knowledge
does not originate in an individual but that each individual
is a medium through which the knowledge of others more knowledge-
able speaks, the same may be said of AW as an individual, through
whom her tradition speaks. But this would be to say that both
interlocutors in the dtexts' dialogue were passive: that what
the people with MS 'pour into' AW as vessel is what they want to,
and is all that they have to say. But on the contrary, being
knowledge produced in dialogue, action and reaction through time,
responses are evoked from/by the other. She, speaking from her
tradition, asks the questions which set the grounds, the domain
for response. Thus although what the others know about themselves
may be being transferred through the time of the dialogue, what is
transferred is only 'what they know' that she knows how to ask,
and, what they can hear her to be asking.

Nevertheless, given this limitation, it appears that at the
level of the event to which they refer, there is offering and
acceptance of knowledge: the transferral of the matter, knowledge
of MS, from one to the other, its substance terminating with the
end of the time of the event. In this sense the time dialogue
takes is not, as in the *Phaedrus* a time, at least potentially,
for mutual change.

But as a text to be read, the time of the event of the dtexts
*in effect* opens up to rather than marks the end of dialogue; opens
to the use of the event of speech for the development of a thesis
through the other no longer present; the action opened indefinitely
to a silent dialogue between dtexts and reader, reading against
the event, questioning the grounds of speech, a dialogue which continues the movement of what passes in the dtext qua event.

In the Phaedrus, this movement of/by dialogue, the shifting of the grounds, occurs as the exchanges proceed (as if) through the time of the event. In Figure 8.4 above (p262):

Phaedrus: says Lysias' speech is as the final word on this aspect of the subject of love;

Socrates: says that he cannot agree because he knows that better arguments have already been made on the topic;

Phaedrus: demands, who said better? Identify your sources.

Socrates: does not only not cite his authorities but refutes Phaedrus' assumption that Lysias, or anyone, could be the source and origin of their own speech - thereby putting into question the grounds upon which Phaedrus' opinion was premised.

Socrates does not only point out that Phaedrus is mistaken, but to a deeper ground that his question concealed, that he had failed to be aware of. The movement in the Dialogue is not question-and-answer; "When were you first diagnosed?" "Well it was about..." and so on. Not the transferrence of information but a shifting, a movement beneath what is said; a movement that will continue, can be interpreted as having continued by virtue of the time dialogue was to take. The temporal monolinearity of speech as referent of the Dialogue is critical for this under-cutting; the identification of this movement of theorising in/by the text; engendered in the activity of reading against form.
It is not, pace Rowe (Fig 8.1), the similarity of the Phaedrus to "living dialogue" but its difference from it (at least in the case of the dtexts as the basis of comparison) that the open-endedness of the Dialogue lies: its being written, not its being dialogue that allows for the representation of a proper uncertainty.

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To re-capitulate in order to move away: questioning the referential status of the dtexts, their relation to actuality, ends in an aporia: their immediate characteristics, their immediacy - as a unique event, of speech, between interlocutors - conferred on 'the referent' by their form as dialogue. One can see how they may be read as having/read through to those origins (as was done in Chapter 4), but not how one may securely ground a claim that they do.

The question of the interpretation of the dtexts has been tied since the beginning of Chapter 6 to a question of reference. Implicit in this pursuit, in the sequence of the thesis, has been that their sociological interpretation cannot begin until that question has been addressed. Yet it has been in the work of interpretation that their status is engendered, interpretation which is effected - in specific ways - by the form in
which as texts they are structured; the form(s) against which the dtexts have been here read.

What happens if we suspend the problem of referential status at this point but nevertheless continue with re-reading the dtexts against the Phaedrus, moving towards what becomes its substantive matter, the nature and limits of enquiry? For each of the dtexts is, like the Phaedrus, itself an inquiry, the production-of-knowledge-through-dialogue.
Unique to inquiry in the form of dialogue, its most obvious feature, is that it is personified; is the engagement of one person with another. For the movement of dialogic enquiry, the other is essential to each interlocutor.

As a way of beginning: what difference does it make that what the inquiry consists of happens as a response of one to the other, that movement of responsiveness the impetus for the happening of 'what happens'? What is this requirement for the other?

As far as the dttexts are concerned, to enquire into the experience of MS, into lay conceptions of science, to do this research, it is that knowledge is to be passed in/by dialogue from one to the other, that the researcher is face-to-face with a spokesperson of the world of experience that is being enquired about, that it is the latter who is questioned by the former, that exemplifies what such inquiry is. In each other's presence, in the immediacy of speech, inquiry occurs closest to the original source of experience. Furthermore, it is only in co-presence that the other can be questioned according to the interests of the inquirer, pursuing the matter. It follows that the other is absolutely and obviously necessary to the inquiry (at this first stage: although as has been remarked, their subsequent absence is no inhibition when it comes to the work of interpretation of the dttexts).
According to Rowe (Fig 8.1), in the Phaedrus, true philosophic inquiry is that which occurs through speech with the other, "the living conversation between philosopher and pupil. This alone (Socrates suggests) can lead to genuine intellectual progress," through the use of the other's responses.

Most obviously, the "intellectual progress" is that of "the pupil", for whom the "philosopher" is the essential other through whom enlightenment may be effected, the source of education. But "the other" is also essential for "the philosopher."

Much of the substance of the Phaedrus qua inquiry concerns an exposition of the relation between the erotic form of love - \( \varepsilon \omega s \) (eros) - with the love of Wisdom - \( \phi i\lambda\sigma\sigma\phi\alpha \) - philo-sophia. It is love of the other, the beautiful (male) other which is the essential impetus which begins a man on the quest for wisdom, is essential to the beginning of his aspiration. For through and only through the love aroused by the beauty of his beloved will he be reminded of the vision of the ideal world of truth that all souls once glimpsed, in the time when, before birth:

- pure was the light that shone around us, and pure were we, without taint of that prison house which now we are encompassed withal, and call a body, fast bound therein as an oyster in its shell (250c).

Only through love of the other will he be prompted to recapture that vision - of wisdom itself (246-257). Which is why there is no question so fundamental for this philosophy, the question of love not only its project but its origin. Mere introspection is sterile: the desire for wisdom only engendered through desire for the other.
SOCRATES: Lead on then, and look out for a place to sit down.

PHAEDRUS: You see that tall plane tree over there?

SOCRATES: To be sure.

PHAEDRUS: There's some shade, and a little breeze, and grass to sit down on, or lie down if we like.

SOCRATES: Then make for it.

PHAEDRUS: Tell me, Socrates, isn't it somewhere about here that they say Boreas seized Orithya from the river?

SOCRATES: Yes, that is the story.

PHAEDRUS: Was this the actual spot? Certainly the water looks charmingly pure and clear; it's just the place for girls to be playing beside the stream.

SOCRATES: No, it was about a quarter of a mile lower down, where you cross to the sanctuary of Agra; there is, I believe, an altar dedicated to Boreas close by.

PHAEDRUS: I have never really noticed it, but pray tell me, Socrates, do you believe that story to be true?

SOCRATES: I should be quite in the fashion if I disbelieved it, as the men of science do. I might proceed to give a scientific account of how the maiden, while at play with Pharmacia, was blown by a gust of Boreas down from the rocks hard by, and having thus met her death was said to have been seized by Boreas, though it may have happened on the Areopagus, according to another version of the occurrence. For my part, Phaedrus, I regard such theories as no doubt attractive, but as the invention of clever, industrious people who are not exactly to be envied, for the simple reason that they must then go on and tell us the real truth about the appearance of centaurs and the Chimera, not to mention a whole host of such creatures, Gorgons and Pegasuses and countless other remarkable monsters of legend flocking in on them. If our skeptic, with his somewhat crude science, means to reduce every one of them to the standard of probability, he'll need a deal of time for it. I myself have certainly no time for the business, and I'll tell you why, my friend. I can't as yet 'know myself,' as the inscription at Delphi enjoins, and so long as that ignorance remains it seems to me ridiculous to inquire into extraneous matters. Consequently I don't bother about such things, but accept the current beliefs about them, and direct my inquiries, as I have just said, rather to myself, to discover whether I really am a more complex creature and more puffed up with pride than Typhon, or a simpler, gentler being whom heaven has blessed with a quiet, un-Typhonic nature. By the way, isn't this the tree we were making for?
In both cases, for the inquiry represented by the dtexts and by the *Phaedrus*, the other is essential for their very existence.

But what is the purpose of inquiry, its matter, that is made possible in/by dialogue? And here it seems the texts substantively diverge.

Taking a passage this time out of the chronological sequence in the event, (as if) before the inquiry of the *Phaedrus* properly gets underway, before they have found the plane tree under which they will sit and talk, Socrates makes the famous statement that his concern is with seeking only to "know myself" (230a) (Fig. 8.5). This is regarded not only as an explicit statement of the matter of the *Phaedrus* in particular:

> The purpose of the exercise is that self-discovery (the 'self-knowledge' of Griswold's title) which Socrates identifies as his project at the beginning of the dialogue...the unifying theme of the work.

Rowe 1987 (Fig 8.1)

but is agreed upon by scholars to be the fundamental key to Plato/Socrates' entire philosophy:

> The inscription 'know thyself' upon the temple of Apollo at Delphi expresses the essence of the philosophy of Socrates, who turned philosophy away from the study of external nature to that of man as a moral being.

Hamilton 1986: 25

the matter of all Socratic inquiry.

> What Socrates seeks...is to know himself both as an individual and as a member of the human species.

Rowe 1987: (Fig 8.1)
The enquiry which the Dialogue represents is personified, then, to follow this line, because it is knowledge of persons, as individuals and as members of a species that is the matter for inquiry, that grounds philosophy. To enquire about persons requires persons to enquire from/with.

As characterised by Rowe, this telos of Socratic philosophy appears in one sense the same as the telos of sociological inquiry (at least that represented by the dtexts): knowledge of the individual and of society, of persons. Knowledge of a genus of persons, 'people with MS' in their particularity, derived from knowledge of the individuals through whom the inquiry is (has to be) effectuated: Daniel, Meg, George.

But whilst in both cases it seems the knowledge of the other is the object of inquiry, what of knowledge of the self? How is this related to the form of inquiry represented in/by dialogue in the Phaedrus and in the dtexts? For in the former we appear to have knowledge of the self as the first priority; in the latter, a concern only for the knowledge of the other to the inquirer.

Indeed, the inquiry directed towards and based upon the dtexts appears to fall under the mockery directed by Socrates against the "men of science" (Fig 8.5: 229c/d) in that it too is about "extraneous matters," about others not in their 'nature' but in their experience, a matter for inquiry which, like with that of the men of science, seems to operate with a presumption that it is possible to distinguish the truth of
certain statements as distinct from unlikely myths. In fact one might say that the matter for the dttexts' inquiry is to find good grounds for disbelieving myths - what everybody (thinks they) know about what chronic illness is like, how non-scientists regard science. The question of what the self is - the nature of the inquirer or the inquired of - does not seem to be the question at the heart of the dttexts.

And yet: dialogic inquiry, in being conducted between/by human beings, being personified, is *predicated* on a knowledge of 'human nature' - of the self-and-other, a knowledge of human nature that, however, both inquiries deny to be fully possible (denials which in turn are grounded upon what they deny).

According to Ong, the limits to 'self-knowledge' are those which could not have been known to 'Plato/Socrates himself':

In fact, as Havelock has beautifully shown, Plato's entire epistemology was unwittingly a programmed rejection of the old, oral, mobile, warm, personally interactive lifeworld of oral culture... Plato of course was not at all fully aware of the unconscious forces at work in his psyche to produce this reaction, or overreaction, of the literate person to lingering, retardant orality.

Ong 1982: 80-1

This is however to begin with a universal theory of the self: to take the existence of "the psyche" as axiomatic, beyond the possibility of question. With an ironic twist, to accept the "word of the men of science" (229d); not to take seriously the pursuit of knowledge of the nature of the self as that with
which inquiry has to begin, but rather to proceed with the discovery of the nature of others - "Plato" for example - as the first and achievable necessity.

To Ong, Socrates' project of knowledge of himself is limited by the irrecoverability of the unconscious; to Socrates himself, it is limited by the difference between gods and men who, being members of different species, do not have equal access to knowledge. Whilst gods exist in the presence of wisdom, souls are torn by birth from its contemplation, and life is the struggle to return to a position that is possible only, and only for some, beyond life. Both limits to self-knowledge are premised on knowledge of what human nature is: the bottom line of their inquiry (pre)formulating what (else) can be dis-covered about the nature of human beings, the self and/or others.

But: this is to identify a flaw at the heart of the Phaedrus qua inquiry, based on a confidence in the power of reason to lead to the truth. However, the Phaedrus does not allow the matter to be so simply settled.

Whilst knowledge of 'humans' in their nature is not the object of the dtexts, the 'matter' of their inquiry is similarly predicated on a concept of the nature of persons as the ground for the pursuit of inquiry with/of the other. For basic to the representation of inquiry as two speakers speaking with one another is the very normality of, the inherently human possibility of, communication - one mind, consciousness with another, the
words the media of this communication, the transmission of understanding, of meaning. That it is simply in the nature of persons that they can and do communicate: the very predicate of intersubjectivity.

At the same time, one of the limits of human understanding of oneself by others is, it is said many times in the dtexts, limited by the impossibility of knowledge without the same experience. That is, although the whole inquiry of the dtexts as a dialogic event is premised on the possibility of understanding what the other experiences through communication with them, one of those communications is that without experience of MS, no explanation or description can convey the whole authentic lived experience to another: that only one-self can know what it is to be that self. To say that though is to presume one can know that another does not know what it is like; is predicated on what it denies: the possibility of comprehension, of knowing the other. (And to understand that that is 'what the dtexts say' is to accept as knowledge that each self is beyond speech, in utter isolation. And that that knowledge, by the same token, has no grounds).

Furthermore, for the dtexts as a form of inquiry, it is also necessary that the others, the people with MS, know themselves, over and above whether they are-speaking of that knowledge, whether the 'experience of MS' can be represented in/by their speech, their responses to questions. That they can and do have 'self-knowledge'.
Both dialogues then stand witness to the absolute necessity of the self and the other for the engendering of their practice of inquiry. In being personified, inquiry, pursued one with another, is inexorably represented - as a feature of the form - as having its fundamental ground the nature of the self as the predicate of all knowing, all inquiry. And yet that ground is the matter of inquiry: that which is not known. In being personified, dialogic inquiry is formulated as apo-cretic. Deeply contradictory.

Far from this being a "flaw" discovered in the argument, there is in the Phaedrus, in the very passage quoted in Fig 8.5, a perfect self-descriptive image within/of the text:

> and I'll tell you why, my friend. I can't as yet 'know myself,' as the inscription at Delphi enjoins, and so long as that ignorance remains it seems to me ridiculous to inquire into extraneous matters. Consequently I don't bother about such things, but accept the current beliefs about them, and direct my inquiries, as I have just said, rather to myself, to discover whether I really am a more complex creature and more puffed up with pride than Typhon, or a simpler, gentler being whom heaven has blessed with a quiet, un-Typhonic nature. By the way, isn't this the tree we were making for?

Figure 8.6

The latent significance of the couching of the question of the nature of the self in a comparison to the nature of the Typhon was released by a coincident reading of Melville's Moby Dick (1988) during the second drafting of this chapter.
According to Tanner's Introduction, the Typhon occurs in a myth from Plutarch's *Isis and Osiris*:

Osiris is a priest/god/king who sails the seas hunting Typhon, an aquatic monster. Once a year Typhon dismembers Osiris who then disappears from the earth and the land is infertile. In a vernal phallic ritual, Osiris is healed and the fertility of the land is restored.

Tanner 1988: xxvi

Tanner is suggesting that Melville's whale stands for that which cannot be captured in words, in writing; for "the hopelessness of our categorizing ambitions" (viv) for knowledge of what things are; for inquiry; the whale which bears on its body scars that are "hieroglyphical", a self-describing writing that is both mythical and indecipherable.

Socrates' comparison between the question of the nature of the self with that of the Typhon, as that which creates and destroys Osiris, whose name represents dominant power that is both secular and sacred yet dependent on the Typhon for its existence, explodes.

Here in the *Phaedrus*, 'in' the very project of knowledge of the self, a perfect analogy for enquiry: having no ground to begin and yet having to. For philosophy which cannot begin with the empirical question as the "men of science" do, but has to be concerned with the grounds upon which their questions depend, a project that engenders its own death in that there is no stable ground for that or any other kind of inquiry to begin. No true knowledge of the nature of the self because no true knowledge of the other. No true knowledge of the other because
always contaminated by ignorance of the self. No ground for inquiry, only movement: the movement involuntarily engendered by response one to another. Yet the engendering, preservation and death of the sacred - truth, the human - only in/by inquiry. In its self-description, the Dialogue trying to grapple with the double face of inquiry: its true monstrosity, no less monstrous because disfigurement, disintegration and death are the source of fertility, the endless and essential cycle in which death and generation are not opposed but the same thing.

Having come so far and in such a way, this passage becomes key to the decipherment and the indecipherability of the Phaedrus: the central destabilising principle at the core of the text perfectly represented by the perpetual instability between its form and content. The fatal personification of inquiry in/by all dialogic forms.

And yet: there appears in both dialogues to be some kind of progress, some kind of movement, development, about the matter in hand, the impetus arising as a response to the other's statements, what is said by the other to each: a movement towards clarity that appears not to be dependent upon knowledge of the (self or) other themselves but upon their works. To revert to the chronology of the text:
Phaedrus urges Socrates to respond with his own speech on the question:

**SOCRATES:** But, my dear good Phaedrus, it will be courting ridicule for an amateur like me to improvise on the same theme as an accomplished writer.

**PHAEDRUS:** Look here. I'll have no more of this affectation, nor I'm pretty sure I have something to say which will compel you to speak.

**SOCRATES:** Then please don't say it.

**PHAEDRUS:** Oh, but I shall, here and now, and what I say will be on oath. I swear to you by—but by whom, by what god? Or shall it be by this plane tree? I swear that unless you deliver your speech here in its very presence, I will assuredly never again declaim nor report any other speech by any author whatsoever.

**SOCRATES:** Ahâ, you rogue! How clever of you to discover the means of compelling a lover of discourse to do your bidding!

**PHAEDRUS:** Then why all this twisting?

**SOCRATES:** I give it up, in view of what you've sworn. For how could I possibly do without such entertainers?

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Figure 8.7

In declaring himself to be a lover of discourse, it is not only Phaedrus himself "here present" but the works of others which Socrates is saying he could not do (philosophy) without.

Is it then that it is not the presence-in-speech of others that is essential to dialogic enquiry but their works? That the personification-in-dialogue is vital not in that it represents co-presence but that it represents the duality of inquiry? An impetus for inquiry that can occur even given the essential absence of self-knowledge?

But this possibility is immediately driven down in its turn. Socrates proceeds to argue against the case about Love put by
Lysias, but he breaks off his speech before Phaedrus expects:

**SOCRATES**: That was a terrible theory, Phaedrus, a terrible theory that you introduced and compelled me to expound.

**PHAEDRUS**: How so?

**SOCRATES**: It was foolish, and somewhat blasphemous, and what could be more terrible than that?

**PHAEDRUS**: I agree, if it merits your description.

**SOCRATES**: Well, do you not hold Love to be a god, the child of Aphrodite?

**PHAEDRUS**: He is certainly said to be.

**SOCRATES**: But not according to Lysias, and not according to that discourse of yours which you caused my lips to utter by putting a spell on them.

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Figure 8.8

He has, he explains, been carried away by arguing point-by-point against the case put forward in Lysias' speech, by the requirement to make a response symmetrical to Lysias' own. Has been lead to say what he did not mean to say at all. It was the symmetry that invited completion that was determining the argument of his own speech in response.

To answer a case (the work of others) by refutation, point-by-point, is to respond to its underlying form, to complete the circle of symmetry but, as Socrates has demonstrated, in thus responding, to collude in the case's grounds. Given the need for all discourse for the works of others, this acceptance that form is symmetrical (or a-symmetrical, which is within the same concept), it is not surprising that inquiry appears to be an accretion which suggests cumulative movement "towards clarity", through the negation or reversal, in any case oriented 'against' and engendered by what went before.
Through recognising that he said what he did not mean because of responding to the structure of Lysias' argument, and thus to its false premise, Socrates appears to be rejecting the imposition on substance by form, by positing an independent criterion.

However: the pivot of Socrates' refutation of Lysias' and then his own speech half completed in response rests on the rule of non-contradiction. Halting his own con-forming speech Socrates steps outside, or rather underneath, and is thus able to see that the fault of both speeches was their unexpressed assumption: that Love can be evil. If Love is a good (which it is), it cannot at the same time be bad. The identification of a contradiction reveals there to have been a mistake. On the basis that we know at least something (what a contradiction is: that something is a contradiction)\(^3\)\(^\text{6}\) we may be sure that the argument is false even if we do not know what the true statement of the argument would be.

But that axiom of non-contradiction is precisely a concomitant of the rule of form - something cannot be both a triangle and a circle because...forms are like that. The concept of form is itself part of the old \textit{mea geometrica} (Johnson 1983); profoundly epistemological, essentially linear; and is operating not only beneath Lysias' mistaken speech but Socrates' refutation of his conformity with it. (The spatial, geometrical, epistemological metaphors - "beneath", "outside" and so on themselves inescapably involved in the demonstration of the inescapability from the epistemology of form).
This responsiveness, the need for the other even at its most abstract - for Lysias' monologic argument - its most a-personal, pre-formulates inquiry.

And yet: to move with the text, even if (by demonstration) form is inescapable, Socrates appears to offer a way of escape for inquiry. For his response to Lysias was not only foolishly based on a contradiction but, he says, blasphemous. It was against the gods. Since the gods are/know the truth of things, inquiry that leads towards blasphemy must not be true (or should not be pursued?). At first sight, this transcendental moral criterion seems utterly alien to the practice and evaluation of sociological inquiry.

And yet: what still obtains as that which is to be held sacred - beyond question - is the idea of 'the human', the very absence of which in the works of deconstruction (and of ethnomethodology in earlier times: see Chapter 3.1) is the criterion invoked by critics to vilify such works as being beyond the pale. Over and above where theorising may lead is the anathema pronounced on all works which are read as de(con)structive of the idea of the human. One may attempt to unravel the question of how man came to be what he is but not question what is involved in saying "that he is". One may inquire, but is bound (for shame) to hold back from anything that leads to infinite regress: to the abyss. For such paths lead, blasphemously, to nihilism:
a(n axiomatic ally derived) contradiction of 'what is'. But by the same token, the sanctity of reason/method as that which drives inquiry is diminished, for it may not, for fear of the anathema, be freely followed to such a bitter end.

Yet: the question of responsibility, of the author for the text as it is written, the interviewer towards the interviewed remains: deeply troublesome.

And yet: even Socrates' certainty about the moral criterion of blasphemy is dissolved as the dialogue proceeds.
Socrates: Tell me then, when an orator, or a king, succeeds in acquiring the power of a Lycurgus, a Solon, or a Darius, and so winning immortality among his people as a speech writer, doesn't he deem himself a peer of the gods while still living, and do not people of later ages hold the same opinion of him when they contemplate his writings?

Phaedrus: Yes, indeed.

Socrates: Then do you suppose that anyone of that type, whoever he might be, and whatever his animosity toward Lysias, could reproach him simply on the ground that he writes?

Phaedrus: What you say certainly makes that improbable, for apparently he would be reproaching what he wanted to do himself.

Socrates: Then the conclusion is obvious, that there is nothing shameful in the mere writing of speeches.

Phaedrus: Of course.

Socrates: But in speaking and writing shamefully and badly, instead of as one should, that is where the shame comes in, I take it.

Phaedrus: Clearly.

Socrates: Then what is the nature of good writing and bad? Is it incumbent on us, Phaedrus, to examine Lysias on this point, and all such as have written or mean to write anything at all, whether in the field of public affairs or private, whether in the verse of the poet or the plain speech of prose?

Figure 8.9

For how may praise or blame be properly attributed?

In/by the Phaedrus it is addressed by Socrates as a question of the relation between rhetoric, dialectic and truth. Both rhetoric and dialectic are technē - which concern the ordering of discourse, formulations which wrest the virtual potency of discourse into powerfully effective methods. They are both methods that presume an other to discourse in that they are essentially addressed communications. In that they are technē, at the same time they presume a 'technician', with the capacity
to release discourse's latent power; a technician who may thus be praised or blamed according to the uses to which s/he puts it.

The uses are in turn adjudged according to their consequences, the telos which every inquiry moves towards.

According to traditional interpretation, the thrust of the Phaedrus is to elevate dialectic and to denegrate rhetoric as the proper mode of writing-and-speech, for inquiry. However:

According to Rowe (Fig 8.1), having dealt with the topic of love:

Socrates and Phaedrus now embark on a long discussion of rhetoric which appears completely to lose sight of the substantive topics of the first part.

Rowe 1987 (Fig 8.1)

But to read the text:

SOCRATES: Must not the art of rhetoric, taken as a whole, be a kind of influencing of the mind by means of words, not only in courts of law and other public gatherings, but in private places also? And must it not be the same art that is concerned with great issues and small, its right employment commanding no more respect when dealing with important matters than with unimportant? Is that what you have been told about it?

PHAEDRUS: No indeed, not exactly that. It is principally, I should say, to lawsuits that an art of speaking and writing is applied—and of course to public harangues also. I know of no wider application.

Figure 8.10

Phaedrus surrenders to Socrates' contention: that rhetoric occurs in all inquiry, it is an effect inherent in inquiry per se (and operative, then, pace Rowe, in "the substantive topics of the first part") in that the objective and the effect of inquiry is to influence others, whatever its substantive topic.
Phaedrus also surrenders another belief about rhetoric, its relation to that which is true:

**PHAEDRUS:**

what I have heard is that
the intending orator is under no necessity of understanding what is truly just, but only what is likely to be thought just by the body of men who are to give judgment; nor need he know what is truly good or noble, but what will be thought so, since it is on the latter, not the former, that persuasion depends.

Figure 8.11

For on the contrary, Socrates argues, the rhetorician has to have true knowledge - of the soul in its nature and its variations in order to, "influence men's souls", the object "on which our discourse is brought to bear" (270a), the souls of his audience.

**SOCRATES:** Since the function of oratory is in fact to influence men's souls, the intending orator must know what types of soul there are. Now these are of a determinate number, and their variety results in a variety of individuals. To the types of soul thus discriminated there corresponds a determinate number of types of discourse. Hence a certain type of hearer will be easy to persuade by a certain type of speech to take such and such action for such and such reason, while another type will be hard to persuade. All this the orator must fully understand, and next he must watch it actually occurring, exemplified in men's conduct, and must cultivate a keenness of perception in following it, if he is going to get any advantage out of the previous instruction that he was given in the school. And when he is competent to say what type of man is susceptible to what kind of discourse; when, further, he can, on catching sight of so-and-so, tell himself, 'That is the man, that character now actually before me is the one I heard about in school, and in order to persuade him of so-and-so I have to apply these arguments in this fashion'; and when, on top of all this, he has further grasped the right occasions for speaking and for keeping quiet, and has come to recognize the right and the wrong time for the brachylogy, the pathetic passage, the exacerbation, and all the rest of his accomplishments—then and not till then has he well and truly achieved the art. But if in his speaking or teaching or writing he fails in any of these requirements, he may tell you that he has the art of speech, but one mustn't believe all one is told.

Figure 8.12
But whilst the rhetorician (the inquirer) has to have a precise and ordered knowledge of souls in order to address each particular audience in the way that will be most effective, this knowledge may be put to any kind of use. Though Socrates deplores those who employ it merely, "for the sake of speaking to and dealing with his fellow men" (273e) (and see 258 above), its proper employment, "to speak what is pleasing to the gods" (273e) appears to be a matter of choice rather than necessity. Rhetoric is a techne that is a matter of structuring discourse in certain ways (expounded in the text in some detail: see Fn 45), a formal organisation that produces effects irrespective of the contents of what is communicated. It is then the form, the rhetorical structuring, that makes (any) content persuasive.

Nevertheless this cannot be done without true knowledge also of the topic in hand, or at least the effectiveness of rhetoric lies in the degree of the rhetorician's knowledge of the truth of it. Even were a rhetorician him/herself unconcerned with the truth but believed what was most effective was reference to the probable (as Phaedrus suggested: see Fig 8.10) (see also Fig 8.5: 229e, where the reduction of the question of the existence of centaurs, \(^{39}\) the Chimera and so on by "the men of science" to the "standard of probability" is precisely the business which Socrates himself has "no time for"):

The multitude get their notion of probability as a result of a likeness to truth, and we explained just now that these likenesses can always be best discovered by one who knows the truth (273d).

Knowledge of the truth, then, is essential to successful discourse,
but successful discourse need not in itself be true or have the telos of truth. It is then in knowing the truth and deliberately turning away from it that the reprehensibility, the proper attributability of "shame" to the discousser lies. The more effective the discourse, the greater the knowledge of the truth the rhetorician must have possessed and the greater his blasphemy. (Hence the deep disgrace of Socrates' response to the rhetorical form of Lysias' speech, the speech which, in responding with his own rhetoric, persuaded Phaedrus that he was hearing the truth from Socrates). Rhetoric, that is, persuades those to whom it is addressed into taking that which appears to be the truth as true.

Given the a-moral, virtual, ubiquitous effectiveness of the techne of rhetoric but the necessity of knowledge of the truth for its most effective employment, it is the method of dialectic which, Socrates urges, is the means by which truth may be approached - the apparently a-personal method of argument which, if followed rigorously, will lead inquiry in the right direction.

Within the text, dialectic is described as the method of division (Nussbaum 1986: 228); of things into their genus and species, the one as the one of many - the method by which Socrates has already discovered the immortality of the soul (245-246). But it is a form of inquiry which is concerned not simply with questions and answers about what things are, but with the grounds of questions and the relation of those grounds
to possible answers (see p269 above), dependent therefore on responses from and to the other. In that sense, the personification of inquiry in/by the Phaedrus represents in its own form the method of division, which by dividing inquiry between interlocutors is able to show what inquiry is. It seems then that the co-presence of the interlocutors is essential to activate the process, between whom dialectic may move.

Following this line of interpretation, Phaedrus' failure to understand his own ignorance by the end of the Dialogue, the ease with which he surrenders himself to whatever Socrates says, is, then, in itself an exemplification of the inadequacy of rhetorical techne, that it can evoke agreement that is not knowledge; that pacifies rather than incites further questions; fails to incite the desire for wisdom in those to whom it is addressed. Socrates' entire discourse has, then, been rhetorical.

The dialectic in the Phaedrus, then, consists of Socrates' speech against himself, his pursuit of the knowledge of what is through the questioning of his own arguments, a dialectic that does not need an external other, 'present' or not. For dialectic the personification is not essential. If this is the case, then the remedy of the attributability of shame and responsibility upon the criterion of blasphemy for the aporia consequent on holding knowledge-of-the-self-and-other as the essential ground of inquiry, of the nature of souls (and see 281-282 above) is in turn dis-solved.
For if dialectic is self-driven, interminable, what comes before modifying what comes after, to whom could shame or praise be attributed? Indeed the more rigorously the technē of dialectic was followed, the greater the impossibility of attributing shame.

But there are three elements to inquiry, not only rhetoric and dialectic but also its telōs: at its most ideal, "that which is pleasing to the gods". The proper writing-and-speech is that which moves towards truth. But if no-one is responsible for the telōs of dialectic-driven inquiry, does it follow nevertheless that if followed, it moves towards the truth of things? Strikingly, whilst the identification of the immortality of the soul is arrived at through dialectic (245-246), whilst dialectic is shown capable of producing the axiom: that the soul is movement and therefore immortal, the nature of the soul is approached only by analogy, couched in a rhetorical figure, in the image of the winged charioteer and his winged, disparate horses, thrashing between heaven and earth (246-257). On the one hand this is firmly stated to be merely a likeness of the nature of the soul and not what the soul is (the Truth of the soul) (246) and yet dialectic has come no closer. Truth itself is beyond both dialectic and rhetoric, behind the appearance of language. Inquiry cannot reveal it. How then could inquiry be blasphemous since what is true (what would please or displease the gods) is beyond mortal knowledge?

But in that case, the virtuousness of proper inquiry might lie in the acknowledgement of the ignorance of all humans; in
that inquiry which has the telos of uncertainty: the putting into question not only what we thought we knew but its grounds upon which we thought we could be confident, the inexorable movement of dialectic/rhetoric towards the truth.

Then, in that Socrates claims to have come to know the truth about Love, the soul, and the nature of proper philosophic inquiry (227a-279), the Phaedrus is an inquiry which is not proper, which has blasphemy as its telos. The dialogue as event has failed to induce "genuine intellectual progress". Dialogue is not, then, necessarily dialectical: does not need and need not incite the love of wisdom, true philo-sophia, in its interlocutors. May fail in effect.

But as a text, the Phaedrus effectively incites absent readers - absent that is from the event but present-to-the-text in the movement of reading; its very open-endedness soliciting their participation in the inquiry which has clearly failed to reach a satisfactory conclusion (in that Socrates has reached one), an open-endedness engendered in reading its written form. In which case, in reading, reading the play of the form against content has produced an effect on the reader, engaging her in dialectic with the text about which it is possible only to be uncertain. In which case the Phaedrus is not blasphemous, but a proper inquiry.

But this is not pace Rowe because Plato deliberately wrote in
palinodic form, designed the text intentionally to engage the reader in dialectic. The effect is not inherent in the dialogue form, in the text. For it may, as with Rowe himself, invoke a response that regards the uncertainty of the meaning of the text as, "deeply problematic" in that the irony (uncertainty) which Rowe perceives, "if it is quite as extended as he (Griswold) suggests is the case...begins to look pointless and perverse" (Rowe 1987 (Fig 8.1).

It does not incite 'the same reading' from all readers, but depends on what it is read against: the essential other that formulates inquiry, not least an inquiry into the meaning of a text.

But let us go back a little: it is not enough to aspire to, to have a desire for knowledge of the truth, nor even to follow the dialectical method in order to move towards it, for it cannot be dis-connected from techne. Rhetoric is the art of effective communication, of engendering agreement by the other to the appearance of truth: yet it is involved in any use of words that carry influence. Dialectic 'is' questioning, particularly the grounds of statements. But that questioning, in that it is discourse, in that it is not ineffective, involves a rhetorically induced response with/from its audience. Therefore the knowledge that may be so reached cannot be relied upon insofar as it is knowledge produced in discourse, achieved by discourse.
The traditional interpretation of this central theme of the *Phaedrus* is that it represents Plato's preference for the method of dialectic as the proper means of philosophy and his rejection of rhetoric. However, as I have striven to show, the relation between rhetoric and dialectic is represented in/by the *Phaedrus* as chiasmic: rhetoric is dialectical in its dependency for its effects on the other and dialectic is rhetorical in that it is influential discourse: the truth is that which, because of the chiasmic potency of technē in inquiry is produced and denied by that relation.

This despair, this 'nihilism' - engendered in inquiry - is what inquiry is. It is not that inquiry needs the other but rather that the other already and always is - whether absent or present. The personification of inquiry that produces the chiasm of dialectic/rhetoric - the personification inherent in dialogue as a form of writing - points to the fallibility of all forms: the epistemology inherent in all discourse. Inquiry, like the image of the soul as the charioteer, doomed to thrash through the sky, perpetually ungrounded and yet bound to move by the irreconcilability of its essential elements which can never become the whole of which they are parts.

But is this Typhonic, mythical, hieroglyphic, indecipherable, decipherable self-inscribed image of inquiry peculiar to a dialogue that resembles inquiry in its very form, embodying the
inquiry of what inquiry 'is'? Because the Phaedrus is the invocation of dialogue, the appearance of dialogue, written with consummate art?

So: to turn explicitly to the dtexts, to their form as/for inquiry in respect of rhetoric, dialectic, telos:

What of rhetoric? Within the event read through the text, as dialogue the discourse can be identified as movement-through-rhetoric on both sides. The interlocutor with MS appears to draw the other, AW, along towards a comprehension of their own experience, that is, persuaded in the sense that she may then know something of what it is like to be the other - known to have been persuaded by her consent to the version given - "I see", "and then what?", the movement from one aspect of the experience to another turning where comprehension appears to have been sufficiently achieved. And rhetorical in the other direction by the same token in that she is interested in what the other has to say and is prepared, committed to an uncritical acceptance of the other's knowledge. In that sense the rhetoric is not a persuasion from one view to the other's but from 'no view'.

That is, within the dialogue, the movement of inquiry as communication but above all as active and reactive (speech)
has a rhetorical impetus: effected in that the matter of inquiry unfolds. This is not to suggest that the rhetorical effectiveness of the dtexts dialogue is achieved by the knowing application of the technē of rhetoric - the ability to name and use its constituent elements; rather from concurrence with the Socratic definition of the presence of rhetoric in all uses of words which "influence men's minds" - and what words would have no influence? Were one an expert rhetorician, the formal structures could be nonetheless identified in the discourse, as Edmondson identifies the ethos, pathos and logos in sociological research texts (1984). It would then appear to follow that for the dtexts there is a way out of one of the paradoxes: in that the most effective rhetoric is based on the fullest knowledge of the nature of the other, that the hearer (the researcher/inquirer) is persuaded of the plausibility but is not convinced that she (I) know(s) the truth of the other's words, is a concomitant of their amateur and un-witting employment of the technē. Thus it is not essential to "know" the other in order for her to be persuaded, to be persuasive: An escape from the aporia of self-other-knowledge as the ground of inquiry inherent in inquiry personified.

At the same time, the failure in the event to question the grounds of the other's statements, the very absence of critical refutation, is a mark of the absence of dialectical method; the dtexts at the level of the event representing an inferior kind of inquiry, marked by the engendering through co-presence only of agreement
(this difference pointing up, by its difference, the effectiveness and expertise of the Phaedrus qua dialogic inquiry).

What, then, of their telēs? What are they, as inferior inquiries, towards? at the level of the event?

In the Phaedrus, the point of proper rhetorical/dialectical inquiry is to both reach towards and persuade the other of the truth, and thereby please the gods: in the dtexts, dialogue has as its telēs rather the gathering of the experience/knowledge held by the other: the telēs the faithfulness of, the correspondence of the representation of that experience to the experience itself (for which there is no criterion).

Yet this would appear, then, also to be a virtue unique to the dtexts: insofar as the interlocutors ineffectually employ rhetoric/dialectic, then so far are they free of the predominance of form-on-content, escaping by a twist of irony the fate of inquiries rigorously conducted according to the formal rules of the technēs. By the very lack of skill of the interlocutors. That is insofar as they are inquiry personnified.

At the same time, as with the Phaedrus, there is a dis-embodied form of responsive argument that drives their inquiry, apparently irrespective of their personnified dialogic form. There is a form formulating the inquiry, a presumption of what elements are required in order to elicit all those things necessary for the understanding of the experience of the other; for the responding, its full representation - of autobiography, the
form of a life, a narrative structure (Chapters 6 & 7). Structures which order that which is said and in so doing, produce an unbridgeable space between what the discourse consists of and what it could have, an unknowable distance between the referent(s) of the dtexts and their ordered representation.

There is also moment-by-moment the rationality, the epistemology inherent in language as a system, a structure of grammar - of subject-object-verb - ordering and categorising and inexorable as far as the discourse - qua text - is intelligible, which it indisputably is. Formulating the movement of inquiry, irrespective of its personified form.

But at the level of the text - and there is no other level - the form of dialogue is itself rhetorical - persuasive in all the respects brought out by reading the dtexts against the Phaedrus in 8.1; of the reality of the event as a unique, identifiable occasion, of a particular kind, between speakers, about something: the effectiveness of the form of dialogue (and: persuasive that these are the effects of the form).

The art, the techné of rhetoric is, as the Phaedrus persuades us, to make something appear in a certain way; it is the manipulation of appearance predicated, according to Socrates, on knowledge of what is. If the form of dialogue in writing is rhetorical in that it has effects on interpretation (for readers who are thinking about what dialogue entails) that reading is
predicated on knowledge of what living dialogue is, even if it is at one remove, knowledge of the knowledge of others of what living dialogue is (reading the text against Rowe).

But this truth is not ascertainable, for to inquire into the question, the dtexts have to be read, and reading is inexorably work upon the text in which it is not the case that the text is the passive object of the work of reading. The activity of reading belongs neither to the reader nor to the text nor to its readable referents. Although the truth of the dtexts may not be ascertained, nevertheless it is the telos of their reading. Reading is a form of inquiry.

Reading them as the record of an actual event, a dialogue between two speakers in communication about something in which that something is the object of the reading-inquiry, is predicated on the effects engendered by their form as dialogue rather than necessarily arising from the event represented. In that sense it is their form that persuades readers that there is a referent to which they refer. The form of dialogue produces an effect: is therefore itself rhetorical rather than referential. As readers we are persuaded by the form of the text (and here dis-suaded) that they are referential.

The dtexts as a written form are rhetorical in effect - and the question of their referential status, whilst it is a question that may be addressed as I have been doing since the beginning of Chapter 6, is dialectical as an event of reading. Reading
that puts not only questions to the questions and answers of which it apparently consists but that puts the grounds of reading into question, as the attribution of the form's rhetoricity.

The relation between reading and text is chiasmic: as with the Phaedrus, the necessity for the personification of inquiry in dialogue is at the same time its undoing.

But: the metaphor of chiasm - the likening of inquiry to the greek letter chi -χ- represents the ontology of binary-ism. A parallel and a cross between only two. In only two movements. Essentially misleading. Essentially leading.
NON-MEDICAL LITERATURE ON MULTIPLE SCLEROSIS

As has been indicated (Figure 2.2; p40), the epidemiological/neurological/clinical/laboratory-based research literature on MS is extensive.

The contribution of the social sciences to the study of MS has been on a far more modest scale. Chiefly it concerns psychological or socio-psychological aspects of the condition, descriptions of pathological or normative 'adjustments' to the disease or detailed accounts of certain psychological symptoms seen to be either manifestations or concomitants of it (for example Miles 1979; Brooks & Matson 1982; Maybury & Brewin 1984; Simons 1984). Medical sociologists do not appear by and large to have been interested in it (exceptions are Davis 1970; Cunningham 1977; Pollock 1984; Stewart & Sullivan 1982 - the first three unpublished MPhil or Doctoral dissertations).

Apart from these two categories of research literature are what might be called 'lay texts': books on its management and advice for other sufferers written by people who themselves have the condition (Dowie, Povey & Whitley 1981; Graham 1981), or by members of the medical profession (Matthews 1978; Burnfield 1978). There are also articles published in booklet form by the two major MS charities (the Multiple Sclerosis Society and Action for Research into Multiple Sclerosis), educational
series written by 'experts' specifically for an MS audience (as in the ARMS Education Service series (eg: Batchelor & Fielder (1981); Bauer (1978); Budd (1977); and Greer (1980), and autobiographical accounts, published either in book form (Forsythe 1979 for example) or in popular and/or paramedical journals or magazines (for example the article by Maggs (1981) in the Nursing Times).
"Almost every authority since Valentiner (1856) who has written about Multiple Sclerosis has mentioned the occurrence of remissions."

Brown & Putnam 1939: 913

"Multiple Sclerosis has been recognised as a disease for a long time. According to Sällstrom, it was probably mentioned first by Cruveillher in 1848."

Limburg 1950: 15

"The first pathological accounts of the disease were given by Cruveillher in 1835 and Carswell in 1838."

Brain 1951: 512

"(T)he question of the role of heredity in Multiple Sclerosis was posed by Charcot himself after hearing of a familial case from Duchenne (see Charcot's Oeuvre Completes 1894). Yet almost one hundred years after Charcot's definitive description of the symptomatology and pathology of the disease, to which little has been added since, the relative role of genetic and environmental influences in the etiology of Multiple Sclerosis has not been assessed with certainty."

Myrianthopoulos 1970: 88

"Credit for the first description of the clinical manifestations of Multiple Sclerosis is generally accorded to Charcot in the mid-nineteenth century, who became familiar with the disease through watching its gradual development in one of his servants. He was unable to identify the cause but suggested a relationship to an antecedent illness and cited cases that had developed following typhoid fever, cholera and smallpox. By the turn of the century, the condition had become well established in medical literature and knowledge was widening with regard to both its clinical manifestations and prevalence."

Office of Health Economics 1975: 4

"Although there had been earlier partial descriptions, Multiple Sclerosis was first identified as a distinctive disease in 1868 by the great French neurologist working at the hospital of the
Salpêtrière in Paris. It may seem strange that a disease that now seems so well defined should have remained so long unrecognised, but the methods of examining the patient with organic disease of the nervous system were only then being developed and Charcot's great contribution to medicine was in linking the careful observation of symptoms and signs of disease in life with the pathological findings in the nervous system after death. He called this new disease that he had separated from the many causes of paralysis to be found in the wards of the Salpêtrière, 'sclérose en plaques', a phrase that in his original lecture he feared would sound barbarous to his audience. The 'sclérose' or sclerosis of his title means hardening and refers to the scarring that is the end result of damage caused to the nervous system by Multiple Sclerosis."

Matthews 1978: 1

"In his classical work on MS in 1868, Charcot regarded nystagmus, intentional tremor and scanning of the speech as the clinical triad. In 1894 Devic considered cases of optic neuritis accompanied by myelitis as optic neuromyelitis. According to their original descriptions of the clinical manifestations MS and neuromyelitis could be easily differentiated. In recent years however Charcot's triad is observed only in 10% of chronic cases."

Baoxun et al 1980: 260

"Although the first pathological descriptions of Multiple Sclerosis were published by Cruveillhier in 1835 and Carswell in 1838 it was not until 1872 that Charcot provided the first extensive clinical description of the disease. The first clinical case of Multiple Sclerosis in England was reported by Moxon in 1873."

Poskanzer et al 1980a: 232

"It was first classified as a disease entity distinct from what was then largely described as 'choeriform paralysis' - 'chorea' being the name given to disorders involving loss of muscular control - by Charcot whose life's passion it was to sort out the neurological diseases nosologically, thereby improving their status. He identified Multiple Sclerosis largely thanks to his charwoman. 'In spite of her costing him a small fortune in broken plates,' Freud was to recall, Charcot kept her for years in his service; when at last she died, he could prove in the autopsy 'that 'choeriform paralysis' was the clinical expression of multiple cerebro-spinal sclerosis.' [...]

One obvious possibility [of cause] is the presence of psycho-social risk factors. Multiple Sclerosis, Charcot observed, was commonly related to long-continued grief and vexation."

Inglis 1981: 207
"Multiple Sclerosis was first described by a French pathologist, Jean Cruveilhier in 1830, and then by the Scottish physician and pathologist, Sir Robert Carswell in 1838. The French pathologist Jean-Martin Charcot studied several cases between 1860 and 1869 and observed three distinctive clinical signs: nystagmus, intention tremor and scanning speech."

Maggs 1981: 414
'OUTCOMES' OF MULTIPLE SCLEROSIS

The degree of depression found in spinal MS patients is related significantly to selected illness, demographic, and social variables. The most significant correlations were found between depression severity and both Perceived Social Support Friendship-based and Family-based scores. The loss of emotionally significant others, whether they be family or friends, apparently adds a further threat to the already severe burden of loss of functional ability and knowledge of the presence of the disease. According to Caplan (1976) emotionally significant others act as 'buffers' for a person pressured by short-term crisis or long-term difficulty. These patients may see themselves as abandoned, mis-understood, and rejected, and may feel responsible for the fact that the important people in their lives are turning away from them. It is conceivable that a part of the depression seen in these Ss is due to 'grief' over the loss of those significant others.

Reizen 1984:

The fact that hysteria and multiple sclerosis may occasionally both be present adds to the difficulty of diagnosis. An examination of the cerebrospinal fluid should always be made in any case of hysteria when doubt exists as to the possibility of a concomitant organic state.

A hysterical overlay in multiple sclerosis is by no means uncommon. It should be suspected if a degree of unsteadiness or apparent weakness of the limbs, accompanied by an unwillingness to walk without support, cannot fully be accounted for by the physical signs. This situation may arise in the early stage of the disease either because the patient, knowing the nature of the complaint, anticipates its downward course, or from the anxious over-protection frequently shown by parents, relatives and friends. The detection of this overlay is important, since a simple rational explanation followed by persuasion and walking exercises often results in a marked improvement.

McAlpine 1972: 253
The Interviewees

Mike ++
Ian ++
Des +
George +
Daniel ++
Hilary ++
James +
Bob +
Meg ++
Jane +
John +
Anne ++

MS
Status

+ diagnosed
+ diagnosed
+ diagnosed
+ diagnosed
+ diagnosed
+ Sister of diagnosed, deceased.
+ Husband of diagnosed, deceased.
+ diagnosed
+ under diagnosis
+ diagnosed
+ under diagnosis
+ diagnosed

+ obtained from the ARMS Research Unit's Research Register
++ obtained from personal contacts

All interviews were conducted in the interviewee's own homes except for James', interviewed in mine.

All names are pseudonyms.

All tapes, notes and transcripts are held in the Brunel-ARMS Research Unit, Brunel University, Uxbridge, Middlesex and are available for perusal on request from the Director.
ITEMS NOT DIRECTLY CITED IN THE CHAPTER, BUT CRITICAL TO ITS WRITING.

Barth 1967
Barth 1972
Borges 1964
Bradbury 1985

Cage 1970
Calvino 1977
Calvino 1981
Culler 1982
de Man 1983

Eagleton 1982
Eco 1983

Foster (ed) 1985

Gray 1981

Lyotard 1979; 1982

Nietzsche 1968, 1974
Norris 1982

Parker 1985

Ray 1984

Sharratt 1984
1. The Contents page of Wadsworth and Robinson (1976) nicely encapsulates the range of topics which constitute the domain of this kind of medical sociology.

2. "One of the central problems of the sociology of knowledge is the status of logic and mathematics. These branches of knowledge are so impersonal and objective that a sociological analysis scarcely seems applicable. Time and again, in his Ideology and Utopia (1936) Karl Mannheim's determined advocacy of the sociology of knowledge stops short at this point. He could not see how to think sociologically about how twice two equals four."

Bloor 1973: 173

3. See for example Spector & Kitsuse (1977) and the debates on their work. Also and especially the debates in the social study of science, for example: Latour & Woolgar 1979; Barnes & Edge 1982; Knorr-Cetina & Mulkay (eds) 1983.


5. The same applies to Woolgar's paper on Irony (1983).

6. This discussion is based on extensive notes taken at Bury's presentation of an early draft of this paper (subsequently published, Bury 1986) to the London Group of the BSA Medical Sociology Society, 11 July 1984, much of which was taken down verbatim. All the phrases within quotation marks are such.

7. Bury explicitly referred to Wright & Treacher's edited collection (1982) and to Berger & Berger (1975) as researchers working within the perspective of social constructivism. See Bury (1986) for a full list of those he was referring to.


CHAPTER TWO: FOOTNOTES

1. According to Firth, the account of MS given by d'Este is so "vivid" that: "the veriest tyro in medicine, reading it, could not fail to reach a correct diagnosis." (1948: 18)

2. According to Kurtzke:

For over a century, MS has intrigued workers in all the neural sciences, with perhaps more publications resulting than for any other neurological disease. However, we face today a situation different from that of Charcot: it is still really a disease of unknown cause, inadequate treatment, and unpredictable outcome."

Kurtzke 1980a: 170

3. According to Bauer:

It may appear curious that after more than a hundred years of clinico-pathological experience and research, the diagnosis of MS is still a problem: on the other hand, the influence of virologic, immunologic, biochemical, morphological and neurophysiologic data obtained with modern research technology confronts us with baffling questions: for example, do we really know what multiple sclerosis is and is it a specific disease entity? To remain on solid ground in the face of such questions we need a well-defined clinical basis of description and classification, reconsidered from time to time to meet the challenge of changing concepts and new knowledge."

Bauer 1978: 659

4. The benefits to be derived from 'triangulation' also applies according to Kurland and Westland (1954) to the relation between clinical, laboratory and epidemiological modes of research into MS:

The epidemiological approach may be considered as one of the three general methods by which we develop knowledge of disease, the others being the clinical study of the individual patient and the experimental approach in the laboratory. 'No single one of these suffices to the exclusion of others; each has its individual usefulness and the fullest measure of progress is to be expected when all three are brought into action.'(Gordon 1948; 1951). Indeed, we can expect that even slight progress in one field will potentialize progress in the others.

Kurland & Westland 1954: 682
5. See Footnote 1.

6. This constitutes another problem with the use of 'history' as a means of tracing the social construction of medical knowledge (See Chapter 1.3).

7. One of the few epidemiological studies which are based on prospective data is the series carried out in the late 1960's, early 1970's, by a research team including Kurtzke, Beebe, Nagler etc on the medical records of US Veterans of World War II and Korea. See for example Kurtzke et al 1979.

8. This is of course only a rough indicator, since each term in the title of articles is listed separately in the Index. However, it seems reasonable to suppose that the proportion of terms to numbers of articles would not differ significantly over a single decade.

9. Kurtzke, in two overviews of epidemiological literature, categorises extant studies into four levels of reliability (liberally citing his own work), and discusses the technical criteria by which such a categorisation should be made. He reserves for his Grade A those studies which "(A)ppeared reasonably complete as to case ascertainment...had followed appropriate (Kurtzke 1977) survey methodology and had used well-defined diagnosis criteria." In his conclusion he remarks:

"(T)he problems and pitfalls of population-based surveys are numerous (Kurtzke 1977). It is not possible for one person to know each site of survey so intimately that all the potential biases can be neatly discounted. My specific ratings of A, B, C for these surveys can certainly be contested and even in the 'A' works there are definable differences among many of the surveys. Still, the alternative is that, if only one's own study is 'correct', the situation as to the world-wide distribution of multiple sclerosis would remain that which would have been produced at the Tower of Babel.

Kurtzke 1980a: 78

Cf. the discussion of Bury's views on social constructivism in the previous chapter.

10. See Bauer, 1978, for a version of this view:

"Five years ago I went through all the research that was being done, the money that was being appropriated for MS research worldwide and I found that it was equivalent to the price of two bombers or, to put it in a less sombre vein, it was the amount of money that is spent in three days in West Germany for the consumption of beer.

Bauer 1978: 3"
315

11. In their 'Nationwide survey' of MS in Japan, Kuroiwa et al (1975) found:

**Prodromal symptoms:** There were no prodromal symptoms in 43% of the series of 1,084 case notes of MS. The most common prodromal symptoms were headache (17%), fever (11%), upper respiratory infection (7%), nausea and vomiting (6%) and skin rash (1%).

Kuroiwa et al 1975: 847

12. There are crucial issues here about how MS is researched, the kinds of arguments made as warranting one method rather than another and, most importantly, the interdependence of the relation of method to findings. See Wynne & Robinson (1983) for a detailed exposition of this argument in respect of the 'sex ratio' in MS (commonly held to be predominantly female) and the development of research towards what is seen to be peculiar(ly) female physiology/psychology.


14. Cf. the effects of the atomistic technique of Conversational Analysis in respect of talk, discussed in Chapter Three.

15. This still, even though, with the introduction of NMR scanning techniques, there has been found a puzzling lack of correlation between lesions in the Central Nervous System (the identifiers of MS) thus made visible and manifested symptoms in a patient; the fact that autopsies sometimes reveal lesions of MS unsuspected during the person's lifetime; and the possibility of asymptomatic or benign forms of the disease (See: McAlpine 1961; Castaigne et al 1981; Gilbert & Sadler 1983; Herndon & Rudick 1983).

16. There are also references in the literature to the role of the medical profession's expectations: for example Myrianthopolus (1970), who refers to the effects of expectations on the proffering of the diagnosis of MS; and Dean et al (1981) who suggest that because doctors in Sicily (where their research was located) expect MS to be rare, it is underdiagnosed.

This further complicates the matter of 'counting cases' of MS. It also of course reinforces the contention that 'the facts' do not simply arise from the documentary evidence, being made here. If the very possibility of MS is not amongst the 'already knowns' being brought to bear on a case, then the case for all practical purposes is not one of MS.
1. The concept of "theoretical auspices" is taken from McHugh et al (1974:2-3):

For the analyst any speech, including his own, is of interest not in terms of what is says but in terms of how what it says is possible, sensible, rational in the first place. Our interest is in what we call the grounds or auspices of phenomena rather than in the phenomena themselves. (Authors' emphasis)

The theoretical auspices which have produced the problematic of reflexivity as the issue with which the thesis is struggling have already constituted what has been written so far.

2. It should be reiterated that the question of theorising is not confined to this chapter but pervades the thesis in its entirety.

3. This use of the term technical is intended as the antonym of 'theoretical' or 'epistemological'. Rendering the latter as the former is one of the means by which, according to Woolgar (1983), the 'methodological horrors' are managed in practice.

This has the uncomfortable concomitant that the horrors are recognised prior to their 'management'. As will be argued later in this chapter, the fact that the 'horrors' are horrors already for the people with MS appears to preempt this particular difficulty.

4. The concept of the parallax effect was explained by James Burke in his television programme on the historical relativism of scientific knowledge 'The Day the Universe Changed' (1985).

5. By which I mean the epistemology, the means by which absent knowledge is identified and then produced.

6. That is, the thinking that machines can be imagined to be doing in the freedom of fiction is essentially recognisable as such. The very concept of 'thinking' precludes the absolute alienness of 'aliens'.

7. Cf Horace Miner's paper, 'Body ritual among the Nacirema' (1965) for a witty and serious attempt to apply the making strange device to the reader's own (American) culture.
8. See Fn 3.

9. Quite which of the many studies described in Chapter 2 of *Studies in Ethnomethodology* (1967) properly come under this rubric of breaching is not altogether clear: but there are at least the following seven, in which Garfinkel instructed his students to:

i. report on what was said in a conversation, and what was understood by each party to have been talked about;
ii. ask subjects to clarify their commonplace remarks;
iii. to view the activity in their own homes as if they were boarders;
iv. ditto, but to act as boarders;
v. engage in a conversation, but respond on the assumption that the other is lying;
vii. to breach normal body space in the course of a conversation with friend or family member.

10. In the book's dedication to Cicourel, "For Aaron, who first served us the sweet poison."

11. For example:

Conversational Analysis is currently the most productive and prolific form of analysis which has been developed with ethnomethodological concerns in mind.

Benson & Hughes 1983: 154


Speakers included: G Jefferson; E A Schegloff; W Cosaro; J Heritage; J M Atkinson; C Heath; S Levinson; A Pomerantz and, in theory but not in the event, H Garfinkel.

13. I have for the most part not named the originators of the various comments I report, although their names were included in my fieldnotes.

14. It cannot of course be presumed that silence indicated agreement. I have concentrated in this informal ethnography on the contributions of self-avowed practitioners of CA, a strategy designed for my immediate purposes and which should not be taken to represent the views of all the participants.

15. Figure 3.3 shows an audiogram, developed by IBM as part of the drive to reproduce human talk on (in?) (by?) machines, which makes the same kind of presumption.

It was notable that at the Symposium held at the University of Surrey 'Language and Knowledge: AI and
Sociological Approaches' (1984) it was the work of CA's that was of most interest in terms of its practical relevance to the participants who came from the field of AI research.

16. See Birnbaum et al 1984; Szuchet & Sh 1984. See also my comment on p54 and Chapter 2, Footnote 14, regarding the effect of the detail in Broman's diagnostic criteria for MS vis à vis Schumacher et al's.
1. To be more precise (and see Appendix 3.2) two of the twelve people interviewed were relatives of people who had had MS and had died, and two had not at the time of the interview been confirmed as having MS. The interview material from the former has been excluded here: the latter has been used (and its status indicated in the text).

2. There is a debt to be acknowledged here to Barthes S/Z (1975) and Hofstadter's Gödel, Escher, Bach (1980), not so much in terms of content but for their confirmation of the substantive necessity of resisting conforming to form.

3. In the theoretical sense of Alan Dawe's proposal of:

Another metaphor for sociological analysis than that of science: the metaphor of the conversation.

Dawe 1978: 409

4. The names are pseudonyms.

5. An extrapolation from advice tendered to postgraduate research students in the Methodology Seminars of the Department of Sociology, Brunel University 1982-3, and at the ESRC Post Graduate Summer School, University of Surrey, July 1983.

6. Although George had recalled that it was the faith healer who had told him he had MS because, as will be seen below, he referred to his doctor's confirmation of the diagnosis as the end of his quest for knowing what was wrong, I am taking the status of the faith healer's knowledge as being by George's account, 'less than' that of the doctor even though 'greater than' his own.

7. cf. 'The Discovery of the Diagnosis' (Brunel-ARMS Unit 1983) for accounts of the distress sometimes caused by this not infrequent initial doctors' response to patients' presenting symptoms (and see Appendix 3.1), interpreted by the latter as meaning that in the doctors' view; their troubles were 'all in the mind'. There are three cases on record in the Unit where the early diagnosis of hysteria lead to admission to psychiatric hospitals and in one case to Electro-Convulsive Therapy. Whether these individuals would have regarded these as instances of 'incorrect' or incorrect diagnoses, and whether they would have accounted for them in a similar manner, is a moot point. There were no such instances in my resesarch group.
8. This respondent - Bob - had reported that he had understood his earliest symptoms - incontinence and blurred vision - to be connected with his practice of "self-abuse" and somehow then with syphilis when an uncle, returning from the trenches of the First World War had told him of the lectures on VD given to the troops. This had prevented Bob from seeking treatment for twenty years - and severe physical and social suffering - because he 'already knew' what was wrong.

9. See Chapter Two which could be interpreted along these lines.

10. See Chapter Two, 49-55 and passim.


12. The Clinical Epidemiology Branch, National Cancer Institute, Bethesda Naval Hospital was one of the research bases for the unique series of prospective epidemiological studies of MS which drew on the records of 5,305 Veterans of the Second World War and the Korean Conflict. See for example Kurtzke et al 1979.

13. Theoretical life [to the Greeks] meant more than science because it was not conceived as arising from mere curiosity or from practical necessity, but out of wonder, as an attempt to escape from ignorance.

Blum 1971: 303-4

See also Blum's later book (1974) in which he draws a fundamental distinction between the Aristotelean model of theorizing as a progression (through questions and answers) and the Platonic, for which the form is rather a continuous questioning of theorizing's own grounds.

A version of this chapter was subsequently published in Woolgar (ed) 1987.
CHAPTER FIVE : FOOTNOTES

1. See Appendix 5.1 which lists those works whose reading informed the writing of this chapter but which are not cited within the text.

2. For example:

The speaker is an enthusiastic supporter of Spencer. But on this issue he accepts that Spencer may be wrong, although personally he doubts it, and he constructs two alternative accounts of what is happening in current research into stoichiometries.

5. We are seeing many experiments done now on stoichiometry. I don't think the question is solved yet, so let's keep an open mind and let's pursue both possibilities...

Mulkay & Gilbert 1982: 171

and

Although 'medical science' appears to feature strongly in the data, in the talk both of myself and of the people with whom I was speaking, how can instances of it be picked out as being certain talk's topic without presupposing what is to count as 'talk about medical science?' Our talk seems to provide for a hearing that our topic is substantively about knowledge: what is known and by whom and from whom knowledge may be sought. One could say that all along we are displaying the project of research into MS.

Chapter 4: 138, above

3. Putting aside, that is, the possibility of fraud.

4. Cf. Marcel Proust for the classic fictional representation of the creativity of remembrance (1981). See also Gerrard Genette's Narrative Discourse (1980) for a modest and infinitely rich analysis of the narrative structure readable as producing the text's effects. He concludes, however, "Perhaps the best thing would be, as with Proustian narrative itself, never to 'finish', which is, in one sense, never to start." (1980: 268). That is, there can never be any end or beginning to the phenomenon of recall.


8. The idea of music as having a relation to questions of philosophy - to which this is only the most superficial reference - reaches its apogee in Thomas Mann's (1949) *Dr Faustus: the life of the German composer Adrian Leverkühn, as told by a friend*, which is 'about' Nietzsche.

9. This is not to become involved in the question of how composers actually compose but rather a means of thinking.

10. I am indebted to Gerhard Baumann (pers. comm) for a caveat: that there are two forms of notation, prescriptive and descriptive, the former instructing players what to do the latter describing the sound which the player then produces in any way that s/he can. As Baumann commented, scores which use the second form can be read and enjoyed; the first requires that the reader has a knowledge of how the instrument could be played. My reference seems to be to the prescriptive system.

11. Ong makes a point similar to this in an essay published after this chapter was first drafted (Ong 1986).

12. The use of the term 'technical' in this way is itself questionable. I use it here in the sense other analysts have, as an assumed antonym for 'theoretical'. As is increasingly recognised, and as this paper itself witnesses, things 'technical' have an epistemological import. For the most profound exegesis, see Heidegger's 'The Question Concerning Technology' (1978). Cf. also debates on Artificial Intelligence; Torrance (1984) for example.

13. Again I am indebted for a comment from Baumann, who informed me that in Kantian terms, this means a perception not based on previous sensory experience but on logical deduction, a philosophical definition of which I was not aware, used as a technical antonym to a posteriori, induction, from effect to cause. In the sense I am using it here, it is intended to make the point that a knowledge of the structure of grammar has to come before the hearing of sound as words, a knowledge that is based on experience which in turn is based axiomatically on logic; ie in a reflexive rather than either a deductive or inductive relation.

14. I am reminded of Searle's much cited example of The Chinese Room - which he uses to sustain the claim that it is possible to manipulate symbols effectively without understanding or intentionality. This claim he then employs to attack the
proponents of 'strong' AI - that machines can be intelligent (Searle 1984).

15. And see above re. remembrance of the meaning of talk rather than of the words themselves. See also Garfinkel's breaching experiment (Garfinkel 1967: 38-42).

16. I am assuming this is not possible.

17. NB: an art based on science:

Isaac Pitman devised his system after a profound and epoch making study of the phonetic structure of the English language. The system is a result of his scientific analysis.

Pitmans New Era Shorthand: iii

See also Nisbet (1977).

18. The "generally" is added in order to exclude those signs known as 'short forms' which are non-phonetic abbreviations of common words and phrases such as Dear Sir, The Chairman said and so on.

19. See Fn 11 above. In the same article, Ong points to the utter impossibility for members of a literate society not to think of words "vaguely perhaps, but unavoidably - in handwriting or typescript or print." (Ong 1986: 24).

20. Sign language, I understand, allows the deaf a literally three-dimensional space for speech, the space in front of the body in which the signs are placed. The question of the relation between the form and the possibilities of language this suggests appear extraordinarily interesting.

21. The idea of the Textual Commentator is taken from a paper by Anna Wynne (1983). Wynne distinguishes between comment and original text by changing the typography, as I have done above in Introductions.

Mulkay 1985: 77, fn 2


1974:4
For their text, this collaboration requires, it appears, different speakers-and-responders. But they add:

in so far as ego and alter and collaboration are serious ideas there can be nothing that 'corresponds' to them in the real world.

1974: 6


24. Jane Austen made precisely this point towards the end of Northanger Abbey (1979):

The anxiety which in this state of their attachment must be the portion of Henry and Catherine, and of all who loved either, as to its final event, can hardly extend, I fear, to the bosom of my readers, who will see in the tell tale compression of the pages before them that we are all hastening together to perfect felicity.

Austen 1979: 237 (of 239 pages in total)

25. For more on the question of the assumption of things that lie outside the margins of 'the text proper' see Derrida (1979) on Nietzsche's marginal (sic) note regarding his umbrella.

For an exposition of what should be included, see Ellmann's Preface to the recently published Corrected Text of Joyce's Ulysses (1986). Which raises another difference than that between the oral/aural and the written - between the handwritten, the drafted, and the 'final printed version' of texts (a question dealt with technically by the traditional hermeneutic exegesis, according to Ricoeur (1981).

26. (T)here is, after all, a limit to what a young physicist can learn from even a Newton. Having once grasped the fundamental points of the Principia, he is not likely to draw very much as a physicist from re-readings (.).

How different is the relation of the sociologist to a Simmel or a Durkheim. Always there will be something to be gained from a direct reading: something that is informative, enlarging, and creative.

Nisbet 1977: 480
Nisbet's argument is applied, however, only to sociological works, to the writings of great sociologists. Such an author was:

(W)ith deep intuitions, with profound imaginative grasp, reacting to the world around him, even as does the artist, and, also like the artist, objectifying internal and only partly conscious, states of mind.

Nisbet 1977: 477

However, their works:

(F)or all the deep artistic sensitivity and intuition, no more belong in the history of art than the works of Balzac or Dickens do in the history of social science.

Nisbet 1977: 481

Although Nisbet's theory of reading, upon which his argument about the benefits of re-reading texts rests, belongs to the now displaced Romantic tradition, it is not without irony that I acknowledge that it was re-reading this article that partly provided the impetus for the current chapter's re-approach to the question of analysis.

27. There have been some 'beginnings' of a "radical rethinking" of the implications of the fact that analytic writings are, as texts, "involved in all the technical problems of words, including rhetorical problems" (Frye, cited in Marcus 1980: 507), particularly as applied to the genre of ethnography in anthropology (cf. Clifford 1981, 1982; Marcus & Clifford 1985). The problem, for Marcus (1980), admits however of a means of solution: that is that once readers are aware of the rhetorical content of such texts, s/he may then as it were subtract those superfluous effects and thus recover the unrhetorical essence. For them the lesson is that:

(F)ieldwork and cultural description emerge as newly complex practices rather than observational empirical or inductive research activities.

Marcus & Clifford 1985: 267

A similar consciousness of the effects of analytic writings is manifest in the works of discourse analysts within the sociology of science (see Ashmore 1985: Mulkay 1984a, 1984b for example), effects which they have employed in their attempts to draw attention to reflexivity in/by analysis.

However, and see Fn 2 above, for them 'data' retains a curious - once thought about - immunity from their re-thinking.
1. The intersection is at the writing of Husserl (e.g. 1964) although the relation of each, of continental philosophy and of ethnomethodology to Husserl and of phenomenology, either philosophical or sociological, to Husserl's philosophy are not simple to trace, and perhaps await writing.


2. Hilary Lawson, whose book 'Reflexivity: The Post-modern Predicament' (1985) discusses the concept against Nietzsche, Heidegger and Derrida, and who has been an occasional member of the Discourse Analysis Workshops which have been concerned with the implications of reflexivity for their own practice in the sociology of science, was a student of Montefiore's at Oxford, a connection I only learnt of after the first drafting of this chapter.

   Lawson does not discuss sociological work with reflexivity in his book. There is as yet as far as I am aware no text which relates the former explicitly and in depth to philosophy.

3. Particularly arising from his strategic concentration upon texts of those writers who, like Rousseau, were themselves critics as well as practitioners of literature. Because, according to de Man, "(c)ritics are a particularly self-conscious and specialised kind of reader", in the writings of such authors, "the complexities of reading are displayed with particular clarity" (de Man 1971: viii). In that essay, however, de Man reserves the "complexities" as being the consequence of "the distinctive characteristics of literary language" (ibid: ix). It is only later that he comes to argue that the figurality of language brings a complexity to the reading of other kinds of writing - notably to philosophy (see his 'The Epistemology of Metaphor' 1978).

4. For example: "(L)iterary fictions do refer by virtue of their referring components, which (exactly as in ordinary discourse) we take literally, approximately, or metaphorically so as to adjust their relation to reality." (Martin 1982: 235); and, "In spoken discourse, this reference by discourse to the speaking subject presents a character of immediacy that we can explain in the following way. The subjective intention of the speaking subject and the meaning of the discourse overlap each other in such a way that it is the same thing to understand what the speaker means and what his discourse means," (Ricoeur 1981: 200).
5. The word 'deconstruction' has become as polysemic in terms of the concept to which it refers as the word reflexivity (or the word ethnomethodology). A use perhaps unique to sociology is where it appears as the opposite process to constructivism, ie to designate the process by which the social construction of, for example, knowledge, is exposed/brought to analytic visibility (See for example Woolgar 1988a:8). For the most explicit account of deconstruction as not only a philosophical but as a political strategy, see Derrida's 'Time of a Thesis: Punctuations' (1983) and also the last section of his 'Signature Event Context' (1982).

6. Although, according to Bryan Turner, "(T)he social sciences have often neglected the most obvious 'fact' about human beings, namely that they have bodies and are embodied. When they have taken this factual substratum into account, the results have often been trivial." (1984: 227)

   What I mean here is that that "neglect" is not an absence but on the contrary, it is from its being taken as the axiomatic grounding of "empirical selves" that certain consequences have arisen. Turner, after (some might say a long way after) Foucault (1977) takes up the body's significance for the order of the political. One of the arenas where knowledge of the body ("this factual substratum") is taken as the grounding of analysis - medical sociology/medical anthropology - and some of the consequences of that axiomatic basis has already been discussed in Chapter I, although in a way that would now at this stage of the thesis be seen to be perhaps enviably naive about the reading and interpretation of texts.

7. Although, not for Socrates for whom, according at least to two thousand years of subsequent commentators, knowledge of the self was philosophy; and for whom philosophy was "a preparation for death" (See Book X of the Republic) (But also Chapter 8 of this thesis). And perhaps I have no right to presume this to be the case for other philosophers/literary critics/sociologists.

8. Cf: 'Beyond Good and Evil'. And, for Nietzsche perhaps himself: "Philosophy, as I have hitherto understood and lived it, is a voluntary living in ice and high mountains" (Ecce Homo, Foreword). The propensity to read the man in the work, despite the declaration, "I am one thing, my writings are another" ('Why I Write Such Excellent Books' in Ecce Homo: 69) is not confined to biographical accounts (Hayman 1980 for example) and 'fiction' (Mann's 'Dr Faustus'), but is used by philosophers as the basis of the interpretation of his writings, the Introduction to 'Ecce Homo' being an excellent example of the latter.
9. As is stated in the Introduction to 'Being and Time', (A) dogma has been developed which not only declares the question about the meaning of Being to be superfluous, but sanctions its complete neglect. It is said that 'Being' is the most universal and emptiest of concepts...Nor does this most universal and hence indefinable concept require any definition, for everyone uses it constantly and already understands what he means by it. In this way, that which the ancient philosophers found continually disturbing as something obscure and hidden has taken on a clarity and self-evidence such that if anyone continues to ask about it he is charged with an error of method.

Heidegger 1962: 21

For a much later re-addressal of the Question of Being, see 'What is Metaphysics' (1978) where it is approached through the Question of the Nothing.

As I have come recently to comprehend, the direction of Derrida's work is to uncover the metaphysical predicate of presence - presence exemplified in the assumption of the immediacy of speech to self, of self-presence in speech - an uncovering 'against' what was the founding insight of Husserl's phenomenology, itself formulated by Husserl against Descartes as the foundation of modern philosophy. Perhaps the most interesting thing about this chapter is that it came to a concern with the effects of the presumption of the self through another route than the discipline of philosophy, through the close reading of the dtexts.

10. It has been suggested that there is a need for a footnote addressing "the effects of AW's utterances" and the extent to which the dtexts might only exhibit "occasioned pronouncements on self, body etc rather than what the victims felt" (Woolgar 1988b).

Certainly if one reads the dtexts with that kind of concern, the role of "AW's utterances" in eliciting the topics is far from passive.

But certainly here in this chapter the intention has nothing to do with representing (and subsequently analysing) "what the victims felt"; the case made out in Chapter V for the unknowable difference between the dtexts and 'the original interview' precludes any such intention. That it is so difficult not to read them in that way, to resist that frame for their interpretability is precisely the theme: the assumption of 'speech as immediate self-presence'.

In this section of the chapter the invitation, the temptation, to read the dtexts as referring directly to 'empirical selves' has been deliberately courted, both by
the extent of the passages quoted and their matter (cf. the generally trivial substance of the texts which certainly early CA worked on) in order put up the hardest case for re-consideration.

11. "For the able-bodied, normal world, we are representations of many of the things they most fear - tragedy, loss, dark and the unknown. Involuntarily we walk - or more often sit - in the valley of the shadow of death. Contact with us throws up in people's faces the fact of sickness and death in the world. No-one likes to think of such things which in themselves are an affront to all our aspirations and hopes. A deformed and paralysed body attacks everyone's sense of well-being and invincibility. People do not want to acknowledge what disability affirms - that life is tragic, and we shall all soon be dead."

(Hunt 1966)

12. Included in the recently published 'The Anthropology of Experience' (Turner & Brunner (eds) 1986) is a paper by Phyllis Gorfain, 'Play and the Problem of Knowing in Hamlet: an Excursion into Interpretive Anthropology', in which she interprets Hamlet as, "not only an expressive text through which our culture tells itself about itself, but also a reflexive text" (1986: 200).

13. The lengthy passages quoted here which allow for the long sweep of narrative to get under way are being used to seduce the reader into another form of resistance to the question of the referent. That the content is so terrible is also part of this strategy (see also Footnote 10).

14. I could, on reflection, have used another passage from 'Daniel's' dtext here. Perhaps the use of a different dtext weakens the argument by opening it to the accusation that 'different people' speak differently of themselves. But I would contend that any of the dtexts contain similar passages. The ones I have used here are those which first struck me as intensely puzzling.

15. "Tragedy, then, is a representation of an action that is worth serious attention, complete in itself, and of some amplitude; in language enriched by a variety of artistic devices appropriate to the several parts of the play; presented in the form of action, not narration; by means of pity and fear bringing about the purgation of such emotions."

(Aristotle 1981: 39)

16. Wordsworth thus describes the position of the poet to the experience he writes about in 'The Prelude'. 
17. This comes from his reading of Lacan.

18. For Aristotle (see the passage quoted in footnote 15), the presence of narration disqualifies a text from the genre of tragedy. By this criterion, the presence of the narrator in the dtexts would by the same token qualify them as - whatever is the opposite to the poetic mode of writing.

19. This will be returned to as the question of the difference between writing and speech as discussed in dialogue in Plato's Phaedrus.
CHAPTER SEVEN: FOOTNOTES

1. As will become apparent, this view is not that of de Man.

2. See Chapter 6's Footnote 15.

3. The use of this collective pronoun in preference for the singular which is the approved practice in sociological argument (and which differs from much of philosophy, which uses the "I" form as a matter of course) involves some not inconsequential rhetorical (epistemological) effects. To anticipate a point that will be made later (235-6), it is this collective pronoun which invokes the authority of the discipline as part of the referent of the proper name of the researcher. It also works to bind readers to writer, anticipating their agreement by including them in(to) the collectivity (see Blum: 1974).

4. There are throughout this chapter many returns to questions raised earlier in the thesis, only some of which are made explicit. Here, for example, to the earlier reading of the Diary of the Comte d'Estè (Chapter Two) and the issues concerning the theory of history in Chapter 1.3.

5. And another: a re-iteration, but which is not the same, of the point made in Chapter 5 passim and in its Footnote 4, returned to in a new context. Such cross-referrals, such cross-referability, which supplement the text read in the context of the thesis as a whole make a substantive point: the interminable and yet very specific supplementations brought to a text in reading.

6. This is an extract from a letter written to me by James in respect of an earlier research project. As he put it:

   I understand from XX that you have been interviewing some MS sufferers, and that they have been responding to questions which were intended to elicit responses about psychological feelings by talking about material phenomena, including their bodies and arrangements surrounding the maintenance thereof. I understand you are pondering why this might be.

7. An extract from the Abstract of their paper. Pill & Stott's research was not about MS (cf Chapter 2, Appendix 2.1 for the relative paucity of sociological research into MS). I have used this example because their data was not dissimilar to mine ("semi-structured interviews") and because it is not implausible (see Chapter 1) that such an approach would be taken to the 'patients' perspective' on MS.
8. I add, with some diffidence, that the correct reference is to Elizabeth Forsythe (1979). Also see 7.

9. .. as the concomitant of participation in a research project.

10. I use this term for its polysemic resonance: the sense of non-persuasive; of virtuous; and 'in essence or effect though not nominally' (OED).

11. See Chapter 6, Fn 10.

12. See also Chapter 6.4.

13. Elements Ricoeur takes from Aristotle's Poetics (see Chapter 6, Fn 15). The presence of narrative, incidentally, is, for Aristotle, incompatible with the form of Tragedy, which must be the representation, rather, "of an action".

14. ACTH is a steroid often given as an immunosuppressive therapy during acute episodes of MS to control symptoms.

15. See Chapter 4: 119 and its Fn 8.

16. According to de Man:

   The interest of autobiography, then, is not that it reveals reliable self-knowledge - it does not - but that it demonstrates in a striking way the impossibility of closure and of totalization (that is the impossibility of coming into being) of all textual systems made up of tropological substitutions.

   de Man 1979: 922

   From which he excludes non-literary textual systems.

17. According to Edmondson, Aristotelean rhetoric consisted of three elements which, in combination, produced the persuasion of speech: *ethos* (self presentation of qualities of the speaker which suggest they are worth listening to, such as reliability, maturity and moral authority); *pathos* (sensitisation of the audience, putting them in the desired frame of mind); and *logos* (the form of argument which constitutes reason and logic). (I am paraphrasing her version of Aristotle).

   For Aristotle, apparently, rhetoric is essentially manipulative, a view that is also by tradition attributed to Plato (but see Chapter 8). Edmondson opposes this, arguing that "sociological communication cannot be achieved without rhetorical uses and rhetoric is concerned in the first place with conveying meaning suasively" (1984: 28). For Edmondson, though, these are figures which facilitate the conveying of
the meaning of the text from author to reader but which have no effect on its 'meaning' as such. Nor does she appear to have any problem with the question of reference. It is worth noting, in view of what is to come in Chapter 8, that the techniques and effects of rhetoric identified by Aristotle were those in speech. Edmondson transfers them unproblematically as applying to writing.

18. See Fn 3 above.

19. Although of course the appropriateness of the inclusion or exclusion of particular individuals or groups of individuals may be and is contested, as is the relationship of one set of subjects to the 'whole-set' of society.
CHAPTER EIGHT: FOOTNOTES

1. It is, I am coming to realise (see Fn 12) impossible to over-estimate the influence of the Dialogues - by which I mean the influence of the interpretive tradition of Plato - not only on the subsequent course of philosophy but on academic enquiry. Even Derrida - whose constant struggle is against the metaphysics of logo-centrism - in tracing its origins back to Plato as responsible for the "philosophical movement par excellence" substantially concedes to that tradition. For his concept of 'general writing' is dependent on a de-construction and dis-placement of the prioritisation of voice over writing which he attributes to Plato (cf. Derrida 1982, but also 1977; 1981).

2. A major element of philosophical debate concerning the Dialogues is, precisely, which ideas are attributable to which of the two philosophers. As Ferguson explains in his Preface: "(I) have assumed on the evidence of Aristotle ... that the Theory of Forms belongs to Plato not Socrates" (1970: xi). "(T)hrough contact with the Pythagoreans in Sicily and South Italy in 388-87BC he (Plato) came to the Theory of Forms as a solution...and propounded it in Phaedo; he elaborated it in Phaedrus; the Banquet (Symposium) and the Republic" (1970:2). It is on this ground amongst others that Ferguson in fact excludes all except two short paragraphs of the Phaedrus from his book which is, as its title makes clear (Socrates: A Source Book) concerned only with the "main source material about Socrates" (1970:xi).

A further complication, and a further ground for asserting that the Dialogues are not literal records is that as Ferguson explains, there are two major extant sources for Socrates who did not himself write; not only Plato but Xenophon. But whilst, "both knew him personally...and both depict him in dramatic situations...in the pages of Xenophon, Socrates appears as an eminently worthy but dull, prosy and sententious moralist...in the pages of Plato he is witty, humorous and a great metaphysical thinker as well" (1970:1).

3. According however to a more recent edition of the Dialogues (Saunders 1987), this certainty about the dates and the order in which they were written is currently replaced by uncertainty, except "within rather broad limits".

4. For example: "(W)e are not to suppose that the dialogues written by Plato actually took place in the form in which he has written them. Obviously they must be based on something historical: but their literary elegance suggests an extensive degree of Platonic manipulation. In the end, however, what matters is not their historical accuracy but their philosophy." (Saunders 1987: 18).
5. For example: "The dialogue form permitted Plato...the playfulness and the bitterness, the irony and the fairness for which the dialogues are also famous. It allowed him almost the freedom of the contemporary novelist. As a form it imposed no limitations on his poetic imagery, and it allowed him the utmost philosophical seriousness. But notwithstanding his unrivalled mastery of the dialogue, he never subordinated meaning to form. Contentless art, he held, is not art" (Hamilton: 1962: xiv).

6. To be noted is the ease with which the absence of the text referred to - Griswold's book - fails in practice to inhibit readers from forming their own view of it which may differ from the view of the text which refers to it, here Rowe's critique. How is it that it seems so plain that Rowe has missed the point of Griswold's book? It was what I took to be Griswold's point about the Phaedrus that persuaded me to read the Dialogue for myself.

7. "Socrates: If men learn (writing) it will implant forgetfulness in their souls: they will cease to exercise memory because they rely on that which is written, calling things to remembrance no longer from within themselves, but by means of external marks...(it is) a receipe not for memory but for reminder. And it is no true wisdom...but only its semblance, for by telling them of many things without teaching them you will make them seem to know much, whilst for the most part they know nothing, and as men filled, not with wisdom, but with the conceit of wisdom, they will be a burden to their fellows" (275a/b).

8. See Baumann (ed) 1986 for example.

9. The capital is used throughout to denote the Phaedrus.

10. This passage is from the chapter entitled 'Writing Restructures Consciousness', in which Ong argues that "the use of a technology (such as writing) can enrich the human psyche, enlarge the human spirit, intensify its inner life" (1982: 83), an argument clearly refuting (his reading of) Plato's view, similarly based on a fundamental difference held to exist between writing and speech. This also presumes, it should be noted in anticipation, that speech historically came before writing, originating not in consciousness but, "wells up into consciousness out of unconscious depths" (1982: 82). Whilst writing "heightens" consciousness, it does not "inevitably well up out of unconsciousness". The consequences and grounds for this view will be discussed later: here I am concerned to discuss the interpretability of that which is only in writing. But in that the dtexts (and the Phaedrus) readably represent speech, what the source of speech is held to be utterly affects that interpretability. The immediate and explicit
relevance of citing Ong here is to substantiate the relevance of reading the dtexts against the Phaedrus.

11. That is, the text in translation. As reader of an already re-interpreted text I am involuntarily subject to the problematic both in practice - particular translator's reading of the greek - and in principle (see de Man 1986 and the attached references). This is particularly frustrating when the argument is dependent upon close and rigorous attention to reading the text. (See also Fn 28).

12. Although I have since studied Plato under the aegis of academic philosophy at the University of Sussex, the interpretation of the Phaedrus I am advancing in this chapter - produced originally as a response to the text in ignorance of how it is traditionally interpreted - although one I can now say with some authority differs from that philosophic tradition, is not one I would wish to substantially amend.

13. There is the same kind of contestable traditional reading of Plato's being 'against poetry'. For example: "Plato's polemic against the poets is at its most vigorous and sweeping in Republic X. All the poets...speak to the lowest elements in us...The poets present us with false models for behaviour, false images of virtue. They make no distinction between good and bad and have no concern for the truth" (Rowe 1984: 148). See also Murdoch 1977; Sesonske 1966.

14. I use this verb because it is particularly apposite in that it depicts an activity that produces a third by contact between two: the production of something which is new and yet which is at the same time rooted in both those things which have been conjoined. It also implies a birth which itself may become the source of the birth of others - an interminable continuity, a dynamic of becoming. Like all metaphors it is ontological and therefore epistemological: but faithful to the text:

Socrates: ...the dialectician selects a soul of the right type, and in it he plants and sows his words founded on knowledge, words which can defend both themselves and him who planted them, words which instead of remaining barren contain a seed whence new seeds grow up in new characters, whereby the seed is vouchsafed immortality, and it possesses the fullest measure of blessedness that man can attain unto. (276e/277a).

15. With a few exceptions, the dtexts will not be quoted from in this chapter.
16. This is to follow the philosophic convention of reference to the text by paragraph numbers.

17. And also throughout: see for example 230b; 242a.

18. Cf. Nussbaum on the relation between the Dialogues and dramatic art, 'Interlude 1: Plato's anti-tragic theater (sic)' (1986: 122-135). She also writes: "Plato is said to have given up a promising career as a tragic poet to write them: scholars have observed that the dialogues show many traces of his former metier" (1986: 126).

For another example of the technique, see the opening scenes of any of Shakespeare's plays in which the speeches describe the speakers, the setting in which the drama is to take place and the dramatic situation which will be unfolded.

19. "Phaedrus... is going from the urban house where he has been conversing with Lysias to take a walk, for the sake of his health, outside the city walls, in what we shall see to be a place of burgeoning sensuous beauty. It is also a dangerous place: a place where a pure young girl was carried off by the impassioned wind god... where the traveller risks possession by the power of eros at the hottest hour of the day. In this same way, some important features of Plato's thought and writing seem to have left the Republic's city house and to be moving in the direction of greater wildness, sensuousness and vulnerability" (Nussbaum 1986: 200). See also Fn 28 below.

20. As the title of the collection of essays by contemporary literary theorists edited by Tompkins indicates - Reader Response Criticism: from formalism to poststructuralism - (1980), whilst the various essayists differ in their theoretical positions, "New Criticism, structuralism, phenomenology, psycho-analysis and deconstruction (and hence in) their definition of the reader, of interpretation and of the text" (1980: ix), their common ground is that the central issue, "the status of the literary text" is one which can only be addressed as the relation between reader-text.

21. "The crucial point for the reader of the dialogue... is that when Plato explores through Socrates the difficulties and implications of Socrates' own assumptions and arguments, he is not exploring matters of purely theoretical interest. For to Plato and Socrates, philosophy is morals, philosophy is politics, philosophy is life" (Saunders 1987: 34).

22. By which I mean that the theoretical grounds of statements made in the early sections of the dtexts are not put into question by those that come after.
23. Principally by Derrida (see Fn 1). It is upon this prioritisation of voice that Husserl's phenomenology is based (Husserl 1964; see also Hindess 1977); and also Searle's and Austin's work on speech as performative (Searle 1969; Austin 1962 but see also Searle 1977 in which he gives a critique of Derrida's 1982 critique of phenomenology). It is also drawn upon by Ricoeur for his contention that the human sciences may be addressed hermeneutically (Ricoeur 1981) (and see Chapter 5, this text).


25. "Phaedrus: I didn't learn the speech by heart, Socrates, I assure you, but I will summarise point by point from the beginning of the argument almost all that Lysias said." (228e).

26. See Chapter 4, 110.

27. Nussbaum's translation is (and see Fn 11): "People who love each other share everything" (1986: 233) - one of her interpretive themes being the Dialogue's representation of the speech between Phaedrus and Socrates as itself the birth of eros one for the other, in which Phaedrus, being the beloved, would be the one who properly submits to he who he loves. See also Fn 19.

28. Or: the failure of dialectic at the level of speech and its success at the level of the written. See 8.3 below.

29. According to Hamilton, the greek word kalon means both the fine and the beautiful - as the term for the shameful aichron, also means the ugly. See Fn 11.


31. According the the philosopher Agazzi, that philosophy allows introspection to be a valid and legitimate means of philosophic inquiry is that which distinguishes it from the human sciences (Agazzi 1988).

32. As implied by the discussion immediately following, and in 8.3, this interpretation that the two are alternatives is a misreading of the text.

33. The claim does not, perhaps, have to be so carefully circumscribed.
34. See pp 278-278, below.

35. Hamilton's translation uses the word "Typho" which, according to his footnote, is the name of the hundred-headed father of the winds, who may or may not be Typhon under another name (Hamilton 1988: 25).

36. But cf. Nietzsche on the assumption of non-contradiction, which may only be a 'human' "inability", not an absolute guarantee of non-sense (Nietzsche 19

37. See Chapter 3.1.1.

38. See Fn 9, Chapter 6, on Heidegger and the Question of Being.

39. The case of the centaur is a favourite example used by those philosophers interested in the question of language and reference in that the word has as its referent something known not to exist (an example often raised in discussions on the MA Philosophy course at Sussex).

40. Socrates: our proof assuredly will prevail with the wise, though not with the learned. Now our first step toward attaining the truth of the matter is to discern the nature of soul, divine and human, its experiences, and its activities. Here then our proof begins.

All soul is immortal, for that which is ever in motion is immortal. But that which while imparting motion is itself moved by something else can cease to be in motion, and therefore can cease to live; it is only that which moves itself that never intermits its motion, inasmuch as it cannot abandon its own nature; moreover this self-mover is the source and first principle of motion for all other things that are moved. Now a first principle cannot come into being, for while anything that comes to be must come to be from a first principle, the latter itself cannot come to be from anything whatsoever; if it did, it would cease any longer to be a first principle. Furthermore, since it does not come into being, it must be imperishable, for assuredly if a first principle were to be destroyed, nothing could come to be out of it, nor could anything bring the principle itself back into existence, seeing that a first principle is needed for anything to come into being.

The self-mover, then, is the first principle of motion, and it is as impossible that it should be destroyed as that it should come into being: were it otherwise, the whole universe, the whole of that which comes to be, would collapse into immobility, and never find another source of motion to bring it back into being.

And now that we have seen that that which is moved by itself is immortal, we shall feel no scruple in affirming that precisely that is the essence and definition of soul, to wit, self-motion. Any body that has an external source of motion is soulless, but a body deriving its motion from a source within itself is animate or besouled, which implies that the nature of soul is what has been said.

And if this last assertion is correct, namely that 'that which moves itself' is precisely identifiable with soul, it must follow that soul is not born and does not die.
41. Why could the world which is of any concern to us -
not be a fiction? And he who then objects: 'but to the
fiction there belongs an author?' could he not be met
with the round retort: why? Are we not permitted to be
a little ironical now about the subject as we are about
the predicate and object? Ought the philosopher not to
rise above the belief in grammar? All due respects to
governesses: but is it not time philosophy renounced the
beliefs of governesses?

Nietzsche 1968: Note 34.
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