Studies

in theory and method in

sociolinguistics

I

c John Pellowe

1990

Thesis submitted for the degree
of Doctor of Philosophy in the
University of Newcastle upon Tyne.

Abstract

Problems raised in a pilot linguistic survey of a street in Newcastle upon Tyne (Pellowe 1967) are here treated positively. An informal normative model of the hearer's treatment of the speaker's output is developed in terms both of psychological processing and of social interpretation. This model is then interpreted methodologically and used to generate an analytical framework and a set of meta-interpretive procedures. These are tested in various ways on samples of speech from members of the Tyneside speech community, on experimental groups of hearers and speakers, and on various miscellaneous data. The generality, replicability and accountability of the methods are examined, and the consequences of the model and its techniques are contrasted with those of other studies.

These dead pressed leaves are for
Emma Pellowe and Ann Cowan,
who have suffered too much and too long
from their collection.

And in memory of Peggy Volkov
who showed me both the infinity of mind
and the limitations on its effability,
and in whom
"feeling and intellect were so exactly matched"
(Annand 1977).

And for Paolo Freire and all those who know why
how it could be if it wasn't how it is
is difficult to bring about.

And for that courageous, gentle, lovely one
who keeps showing me that
in this end is our beginning.

"There is a common quality among all beings and things . . . [which] is *impermanence*, 'anicca', a constant decaying and changing which is uncontrollable"

An unnamed Buddhist scholar in Wat Sraket, Bangkok, quoted by Hamilton-Merritt (1979: 55).

"What is the energy of listening ? Of comprehension ?"

Firth (1957: 91).

"The skilful speaker wins praise; the skilful listener, despite the mystery of his achievement, is ignored"

Parker-Rhodes (1978: xiii).

"Patterns very difficult to imagine were made together by everything . . . merging into a supernal harmony their unexceptionable varieties"

O'Brien (1967:125).

"Meanwhile the indefiniteness remains, and the limits of variation are really much wider than anyone would imagine"

George Eliot, Preface to Middlemarch.

"Reality is an activity of the most august imagination"

Stevens (1972: 396).

"Every 'object' . . . has an infinitude of attributes . . . We simply cannot speak meaningfully of the properties of a thing in itself; . . [added to which] attributes themselves are infinitely divisible"

Johnson (1970: 213-4).

"The order that our mind imagines is . . . like a ladder, built to attain something. But afterward you must throw the ladder away, because you discover that even if it was useful, it was meaningless . . . The only truths that are useful are instruments to be thrown away"

Eco (1984: 492).

"All 'explanations' in language are circular and leave the most essential things unexplained and undefined . . . Where do I begin and end in space? I have relations to the sun and air which are just as vital parts of my existence as my heart"

Watts (1987: 45-6).

Acknowledgements

My thanks to all my interlocuters who have given me, in my role as hearer, so much pleasure and so much to think about. As John Wesley said somewhere "certainly, if I did not believe there was another world, I should spend all my summers in Newcastle, as I know no place in Great Britain comparable to it for pleasantness".

The tragic death of Barbara Strang deprives the world of a fine scholar. She would have been pleased that the very many hours of discussion we had did, finally, bear some kind of fruit, even if somewhat old and wrinkled. It is impossible to itemise the ways in which I am indebted to the range of her knowledge and the precision of her argument.

I am also very grateful to many for discussion and argument. Places and occasions in my mind's mind lead to the following list, but I am equally grateful to those who are not in the list and who argued:

Bob LePage, Peter Trudgill, Hans Speitel, Colin Strang, David Crystal, David Abercrombie, John Givens, Brian Torode, Gerry Knowles, Mary Brennan, Jim Milroy, Basil Bernstein, Steve Harlow, Paroo Nihalani, Randolph Quirk, Jan Svartvik, Bernie Mohan, Pauline Graham, John Laver, Howard Giles, Anneliese Kramer-Dahl, Peter Sneath, Ron Atkin, Frederick Parker-Rhodes, Henry Carvell, Geoffrey Sampson, Dick Hudson, Peter Robinson, Trevor Hill, Brian Randall, Sheldon Klein, Bob Lumsden, Elizabeth Barraclough, Lesley Milroy, Julie Bradshaw, David Bloom, Daniel Hirst, Mark Newbrook, K.P.Mohanan, Susan Hunston, Ian Smith, Sharon Veach, Daljit Kaur, Jean D'Souza, Christina Lim Tong Li, Doreen D'Cruz, Dwight Bolinger, Chia Sock Cheng, Frances Pellowe,

Vince McNeany, Val Jones, John Local, Robert Beat Glauser, John Frankis, Bob White, Graham Nixon, Anthea Fraser Shields, Anne Pakir, Desmond Graham, Graham McGregor, Claire Cannell (now Humphries-Jones), Emma Pellowe, Claire Wenley, Jim Eve, Jimmy Thorne, Anthea Fraser Gupta, Debbie Cameron, Jane Tarrant, David Butt, Matthew Mouw, Thiru Kandiah, Rose Pellowe, Robert Scott Duprée, Ron Keefe, Paul Johnston, Mary Tay, Peter Wood, Frank Reynolds, Pete Laver, . . .

Colleagues and students in the places where I have worked have always been patient with the oddity of my views and generous with their reactions to them.

Throughout my time in Newcastle, both as a student and as a Faculty member, the staff and facilities of NUMAC at the Computing Laboratory were always generous, and generously bestowed, respectively.

Bob White and Pauline Graham encouraged me by their gifts of themselves and of space to be in, as much as by exhortation (Newcastle '79-'81). Edwin Thumboo continued to urge me to go on and finish the thing.

Bob Lumsden gave me calm and genial encouragement, as well as wondering pity; and Shu Zhen was gently quiet in spite of my bearishness.

I thank my Italian neighbours and friends for their warmth and generosity, especially Sylvana Ghione and her mother, who in addition gave me space.

Several full-time colleagues in Singapore have been battling with their theses as well as with the usual duties: the support of such comrades, suffering similar, if incommensurable, pentecosts, has been decisive at those points when it all seemed pointless and/or meaningless.

Julie Bradshaw, surely the most mavournine larrikin, has kept me at it the most effectively with the wondrous alchemy of her selves, and with her wit, bullying, gentleness, courage, example, honesty, generosity, convincing (but private) rationality, and heart; let alone a transfinite basketful of increasingly ineffable gifts - zen buns, Purcell's Naughties, The True Harrowed Nature of The Generous Pics, the thynne machine, Every Yellow Whatsit, les deux aimables, and The Theory of [nãə̃ ə̃] amongst them.

Emma Pellowe & Nick Packard have provided with all their usual but extraordinary delicacy, wit and generosity the final location for this production. A place where Emma's work as artist, the significance of lunch, and the needs of the universe at large, calmly unfold as a seamless whole.

Myriad thanks, finally, to my parents, my sisters, and my family at large, who beginninglessly and continuingly create and share such joyous and interesting contexts with me. 'What do I have that I have not received?' (Ramanujan 1973: 17).

Newcastle upon Tyne, Nenthead, Berwick upon Tweed,
Fezzan, Singapore, Asti, Penryn, Beijing, Roath.
1967-1990.

Note on the time scale of the research

The research, of which this thesis is a final report, was begun in 1965. Some of the work was submitted in the form of an M.Litt. thesis (Pellowe 1967); some of it has appeared in the form of published and semi-published papers.

During the period of my own full-time doctoral research (1967-1970), and thereafter continuously, other students became interested in the ideas which I was developing, and devoted their own research to contiguous or overlapping projects. In addition, 1970 was the year in which the work first attracted the support of the SSRC for two person-years of research.

During a period of twenty five years ('65-'90), and with a continuously changing group of co-workers in different locations, many elements of one's research thinking undergo change; and one is more conscious of some of these changes than of others. In what follows, I have devoted space to these changes of belief and attitude since their occurrence is intrinsic to the general thesis which I am advancing. The ways in which each of us understands the differences between our pasts and our presents create one of the bases for our imagining - and differentially imagining - the differences between our presents and our futures. (And, as Durrell suggests, reality may follow the imagination.) This truism applies as well to conceptualisations for academic purposes as to the constructs we articulate for daily living. (How could it be otherwise ? (Foster 1983).)

Parts of this thesis are expansions, summaries, or derivations from previously published, or semi-published, materials. Reference is made to these at the relevant places in the text.

Organisation of the thesis

From one point of view, this thesis is a record of a dead project, namely the Tyneside Linguistic Survey. It is a project which failed, but it is one whose failure was not the result of faults in its conception and design, but rather, it failed because of errors in administration and management which went uncorrected. Though I shall go into neither the economics nor the personalities of failure and mismanagement, some of the gaps referred to below have their source in such diurnal, and very human, frailties.

In spite of such difficulties, the theoretical and methodological ideas which developed over the years - together with their impact upon the way data were collected and analysed - seem to me to be as fresh, and as critically relevant, as ever. The general thrust of the theory, and its implications for method, are presented in Chapters 1-4, whilst detailed studies of particular data, dimensions for various classificatory spaces, calibrations of analysts, the indeterminacy of classificatory procedures, and so forth, are given in Appendices A-F.

There is a complex interaction between the six Appendices of volume two and the four chapters of volume one. This interaction, though it reflects the connections and the dependencies, also dwells on the gaps between the development of the model and the concrete details of its empirical interpretation. Much research reports itself as if there were never either seams or rents between methodological developments and their application to particular data, or between the framing of hypotheses and the practicability of their testing, or between the detailed consequents of some hypothesis and some generalisation in the theory, but surely it is precisely such failures of tailoring which really tell us what we think we might want to know, and what the reliability of our beliefs in these matters might be like ? (Cf. also Lakatos 1976.)

Sometimes a particular methodological move is demanded by some component in the model or in the theory, and the violence which it is seen to do to the data makes for an improvement in the model or the theory. Sometimes a rainbow of particular co-occurrent facts

suggests a methodological move which has important general consequences in the theory or in the model. **Sometimes**. But at others ? ....

Given, then, the peculiarities and particularities of the history of this document, it would have constituted an unrecognisable distortion of that history to have made its structure fit the kinds of blue-/thumb- print given by Phillips and Pugh (1987).

This work is concerned, in the widest sense, with **burdens of proof**. A widely held, but in my view <u>false</u>, assumption maintains that the burden of proof lies with the refuter of hypotheses. I cannot see how anything other than the <u>opposite</u> of this is the case. The burden of proof must surely rest squarely on the <u>proponent</u> of some hypothesis or theory. His critic is not bound, by any convention of argument, or principle of reasoning, to provide an <u>alternative</u> to that which is found inadequate (Macbeth 1974). Consider the case of prosecution and defence. It would be ridiculous indeed to require a defence which had destroyed the coherence of a prosecution case to put up a better case subsequently ! It is precisely this subtle pressure <u>against</u> the freedoms and privileges of the sceptic, the critic, the refuter, which leads to the closed, protectionist, unthoughtful, and aggressive theorising which pollutes so much of the intellectual landscape. More strictly, one might contend that an argument which did **not** play a part in establishing some culminating synthesis was intrinsically more trustworthy than one which did.

In the present work this principle, that the burden of proof lies with the proponent of some position, not only applies externally as it were, between the ideas advanced here and those of others, but internally, between the ideas advanced here and <u>their own</u> refutation. As Lakatos has it (1976: 94), in patterns of heuristic, "the growing theory not only <u>explains</u> but <u>produces</u> its refutations".

Chapter 1 proposes some of the sorts of patterns (and their resultant tensions) which a person establishes for herself or himself in order to get into society and then 'get through'. Of course this is done in an extremely sketchy and superficial manner without regard for the (ineffable) circumstances or experiences of any particular

Self and its selves. It is a rough ground on the canvas for Chapter 2.

Chapter 2 provides a discursive model of the activity of a hearer when he is engaged in making inferences about his interlocutor. [N]

[N] Throughout this thesis the Speaker will be pronominalised as She, Her, Hers, and the Hearer as He, Him, His.

The model is restricted and it is normative. It is restricted in the sense that it does not make provision for the simultaneity of inferences about, for example, purely syntactic or purely pragmatic matters which are also of importance to hearers. It would be surprising if the simultaneity of such inferences did not have important consequences for the 'real' manner of their formation. The model is normative in the sense that speculative introspections concerning my own behaviour as a hearer, and the behaviour of others whom I have been able to persuade to talk about it, are assumed to be equatable to the hearing skills and hearing experience of others.

Chapter 3 interprets the model of Chapter 2 empirically and shows how that empirical interpretation leads to various methodological requirements and innovations. Data which are handled in the Appendices are referred to at several points.

Chapter 4 gives a critical account of other methods and theoretical positions which have been made use of to handle linguistic variation and indicates how those methods and theoretical claims would be incorporated by the methods and claims set out here.

Contents

List of figures
(Figures are in Volume iii)

NB Figures A10 to A17 are presented in a pocket at the back of Vol. 3.

For Appendix D.

Chapter 1

Starting positions

"It is as difficult to find a satisfactory definition of an individual as it is of a species" (Watson 1976: 30).

"The individual person is the means whereby reality articulates itself" (Deikman 1973: 323).

"Proof and reality are rather strong words for me; ones for which a positive and definite context of use might never occur" (Strang 1969).

"Reality is an activity of the most august imagination" (Stevens 1972: 396).

"The properties which any particular thing or phenomenon can be thought of as possessing or lacking are endless, they are limited only by our imagination (which means that they are not limited at all)" (Sampson 1979: 127).

Abstract.
This chapter makes initial assumptions and then proposes some of the sorts of skills and processes which a person establishes for herself or himself in order to get into society and then 'get through'. Of course this is done in an extremely sketchy and superficial manner without regard for the (ineffable) circumstances or experiences of any particular 'I' and its Selves. It is a rough ground on the canvas for Chapter 2.

Initial assumptions

I assume that languages can vary without limit (1,C1). Others have assumed the same: "Languages can differ from each other without limit and in unpredictable ways" (Joos, 1957:96); "Speech is a human activity that varies without assignable limit . . . It varies as all creative effort varies" (Sapir, 1921:4). [N]

[N] Pre-naive conjectures, naive conjectures, guesses, speculations and assumptions, are numbered by chapter (or appendix) through the thesis, as 1,C1 (Chapter 1, conjecture 1) B,C4 (Appendix B, conjecture 4) etc., and collected in the Index of Conjectures, below. Assumptions are special 'background' forms of conjectures. About these Lakatos (1976: 45ff.) has some useful warnings, e.g. "background knowledge is where we assume that we know everything but in fact know nothing".

I treat as an immaterial distinction, for my purposes here, whether this limitless variability is thought of as between languages, or

within a language.

I take the central problem of linguistics to be 'providing an explanation' of the limitless variability of languages, in conjunction with their limited systematicity, rather than 'providing an explanation' of the limited systematicity of languages to the exclusion of all variability (1,C2). [N]

[N] The ubiquitous pomposity of the phrase 'providing an explanation of' hides an inherent unlikelihood, I believe. The incapacity of humans to 'explain' language probably parallels the inability of computing machines to disprove Gödel's theorem (that there are some theorems which are computationally undecidable). Cf. Postal (1972), Davis (1958), Hofstadter (1980) for corroborations of this point of view from various angles.

This view is not new. Closely similar views have been expressed by Paul (1891), Hockett (1968), Bailey (1973) and Sampson (1980).

Taking variability, rather than systematicity, to be the thing to be explained, involves us in developing new forms of representation; and it renders the old guiding dichotomies of questionable value. As Firth (1957: 227-8) puts it:

"these dichotomies [mind and body, language and thought, word and idea, signifiant et signifié, expression and content [F's examples, JP]] are a quite unnecessary nuisance, and in my opinion should be dropped . . . The dualistic principle . . . wrecks the empirical analysis of language material".

(For reasons which will gradually emerge, and which I discuss in Chapter 4 below, I do not believe that the main thrusts of what are known as variationist linguistics and sociolinguistics are actually addressing this problem at all.)

In the face of the problem of limitless variability (1,C2), I advance one naive conjecture, namely, that each individual hearer is creative (1,C3).

That hearers are creative is a conjecture derived from a symmetrical one, namely that speaker-hearers are creative (1,C4), which is in turn derived from a wider one, namely that individuals in the conducting of their daily lives are continuously creative (1,C5). The reason for adopting the narrow, asymmetrical form of 1,C3 is a matter of focus on 1,C2.

The view that individuals, however they are focussed upon, are creative is also not new. 1,C5, for example, has been advanced by many scholars working in different fields: Bannister & Fransella (1971), Bateson (1979), Berger (1979), Deikman (1973), Freire (1972), Gregory (1972), Kelly (1955), Laing (1982), Penfield (1975), Popper (1959, 1963, 1972), and Popper & Eccles (1977), to cite a few. (Dissenting views such as Fischer's (1979: 32), that the

narrowness and stereotypy of the human repertoire are emphasised by the creativity which transcends them, are surely based on a category mistake: creativity does not transcend stereotypy, it complements it. Sampson says laconically (1980: 45) "linguists do not refute the creative view of mind, they simply ignore it".)

1,C4, that speaker-hearers are creative, has been advanced by, for example, Paul (1891), Popper & Eccles (1977), Hockett (1961,1968) and Sampson (1979,1980). 1,C3, that hearers are creative, has been advanced by, for example, Cicourel (1973), Parker-Rhodes (1978) and, for the case of the reader rather than the hearer, Derrida (1976).

In trying to handle the limitless variability of language (1,C1) by means of the creativity of individual hearers (1,C3), it is worth bearing in mind Popper's (1972: 179) certainty that "no creative action can ever be fully explained", and Sampson's (1980) suggestion that any creative phenomenon constantly escapes the bounds suggested by its own past history. And, more generally, Meyerson (1930: 238) warns: "science in its effort to become 'rational' tends more and more to suppress variation in time".

The point of attack

The thesis is an exploration of the extent to which the problem of limitless variability (1,C2) can be 'solved' by examining the consequences of the conjecture that hearers are creative (1,C3). Though 1,C3 may appear to be simple, its consequences are far from simple (see below, Chapter 2).

I adopt a heuristic style of presentation rather than a deductivist or an inductivist one since, as Lakatos (1976: 142-4) points out, the former imposes a false pattern on the conjectural history of the work, and the latter implies that there was no conjectural history at all.

> "Facts do not suggest conjectures and do not support them either" (Lakatos 1976: 73); rather, conjectures are simply more or less interesting, more or less useful, and, if the reports of others are to be admitted, more or less commonly entertained by other individual minds. Furthermore, if 'facts' neither suggest nor support conjectures, then no more can 'facts' refute them; what refutes a conjecture is a competing conjecture which is, according to some criterion, more powerful or more revealing, or more persuasive.

Therefore conjectures are **not** inductive, but they are arrived

at by trial and error, from pre-naive conjectures and refutations, and so on, beginninglessly. As Popper (1963) points out, conjectures are free inventions of the mind (Sampson 1980: 8). Like Durrell's (1962: 286 ff.) rememberings and imaginings, conjectures have the indivisible and unmanipulable character of quanta.

As will appear below, this thesis is a double articulation of conjecture. First, the method and the fabric of my argument is conjectural; and secondly, that which the argument seeks to build is the conjectural method of 'naive' hearers successfully creating meaning from the spoken worlds which spin about them.

I shall assume (1,C6), with Harrah (1963:2), that it is "philosophically and scientifically legitimate" to conjecture an informal model of the creative hearer, "then interpret it empirically, and then experiment to discover its area of descriptive accuracy". But little of this can be done by any adaptation of classical scientific method - there are after all, as Sampson (1980) says, <u>real</u> things, such as human intellectual activity, about which predictions cannot be made. "As soon as questions of will, or decision, or reason, or choice of action, arise, human science is at a loss" (Chomsky 1978: 435, (quoted by Sampson 1980)).

In opposition to the simplifying, and simple-minded, view that it is possible (as well as obligatory) to be right (Labov 1972: 98), I believe that it is worth recalling Neils Bohr's principle of complementarity, by which an event must be seen through one of two (or more) frames of reference (Bohr 1958). The frames are mutually exclusive, but they also complement each other, and only the turn-by-turn usage of these complementary frames provides a picture of **the appearences of the phenomenon**. In the same vein, Capra (1982:132) writes

> "The crucial feature of quantum theory is that the human observer is not only necessary to observe the properties of an atomic phenomenon, but is necessary even to bring about these properties . . . [The] electron does not **have** properties independent of my mind. In atomic physics, the sharp Cartesian split between mind and matter, between I and the world, is no longer valid. We can never speak about nature without, at the same time, speaking about ourselves."

If these things are true of the investigator of the 'inorganic,

physical' world, how much more true must they be of the investigator of human abilities.

What cluster of further conjectures concerning abilities, structures, processes, needs to be made in order to underpin 1,C5, and hence by implication 1,C4 and 1,C3 ?

A conscious mind

First, we may attribute creativity in the individual to the activity of mind (1,C7). This is reflected in the wonder in Einstein's remark that "the mystery of the world is its comprehensibility " (Penfield 1970:90), and in Whitehead's poetic irony when he writes:

"Nature gets credit which in truth should be reserved for ourselves, the rose for its scent, the nightingale for his song, and the sun for his radiance. The poets are entirely mistaken. They should address their lyrics to themselves and should turn them into odes of self-congratulation on the excellence of the human mind. Nature is a dull affair, soundless, scentless, colourless, merely the hurrying of material, endlessly, meaninglessly."
(Quoted by Ornstein (1972: 45) who quotes it from De Marquette, J. (1949) **Introduction to comparative mysticism.** NY: Philosophical Library, p. 15. But I have been unable to find the remark in Whitehead's own works.)

I assume that minds exist, and are not reducible, either dimensionally or procedurally, to brains (1,C8). Whereas brains are material and exist in three-dimensional space and in time, mind exists in time alone. "Concepts like mind, order, complexity, information, meaning, . . . share a common, non-local, trans-spatial feature" (Fischer 1987: 4). Therefore their interaction can only occur in time. Indeed, as Whitrow (1980: 113) puts it, there is absolutely no need for a particular mind to be "associated uniquely with the region of space occupied by a particular brain".

Mental processes such as believing, deciding, intuiting, deceiving oneself, and so forth, do not have cerebral correlates (Penfield & Jasper 1954, Penfield 1975). "There is **no** area of grey matter . . . in which local epileptic discharge brings to pass what could be called 'mind-action' " (Penfield 1975: 77), therefore, he concludes that

"it is easier to rationalize man's being on the basis of two elements [mind and brain] than on the basis of one [brain] . ." (1975: 114). "If there are two elements then energy must be available in two forms " (1975: 79) . . [But] "a final scientific conclusion [is not possible] until the nature of the energy responsible for mind-action is discovered as, in my opinion, it will be" (1975: 114).

Mind in this sense clearly comprises various states of

consciousness and their capacity for articulation by means of various processes. The relationship between consciousness and brain is still very little understood; as Whitrow (1980: 112) says "not all brain events are mental events [and] not all the neural correlates of mental process are known". But part of our lack of understanding of the connection is ontological, since "the mind is a system of models, and being self-referential, must also have a model of itself; that model must have 'matching consciousness' for 'Self'-identification. Thus mind . . . is a system of systems or a model of models that includes 'the referee in the reference, the observer in the description and the axioms in the explanation'" (Fischer 1981: 27).

The nature of consciousness may be conjectured from various points of view.

Popper's guess is to do with biological function. The biological achievements which are served by [the development] of consciousness are "the solution of problems of a non-routine [or unexpected] kind" (Popper & Eccles 1977: 125). Popper reckons that the **source** of consciousness is an interaction between pleasure, pain, expectation and attention. [N]

[N] Using the argument the other way up, Fischer wants to illustrate the unitarian nature of information and action by recalling that "when a person's striated musculature (of voluntary action) is completely relaxed . . . only the metalinguistic dimension of consciousness prevails, i.e. one is only aware of oneself while the denotative dimension is absent; consciousness has no content or information" (1979: 24-5).

Penfield sees it from a meta-physiological point of view: "the mind is present whenever the highest brain mechanism is functioning normally" (Penfield 1975: 79). Hinshelwood suggests that the higher functions of the brain are programmed "by aesthetic and moral elements which somehow have their seat in consciousness, by elements, that is to say, which belong to the half of reality concerned with the observer rather than the observed"

([1959 Presidential address Proc. Roy. Soc. A, 253, p.447 ff.] quoted by Whitrow 1980 :112) Others conjecture that mind is a quantum effect (Bateson 1979, Fischer 1987, Simon 1970) arising from the degree and kind of complexity of the organism.

It does not seem to me to be necessary to choose amongst these formulations, since there exists no superordinate critical framework within which it could be determined that they were incompatible.

Under these characterisations of consciousness, creativity is possible in non-human living individuals, and has indeed been observed, or better ascribed, in countless cases. I assume (1,C9) that the creativity of humans is of a different order of magnitude from that of other, non-human, individuals. The difference arises from the human invention of language which permits the emergence of the consciousness of **self**, and hence of reflectiveness (Popper & Eccles 1977).

A chosen reflective self

In the language of developmental ethics, one may say that the self is **created** by the individual (Kierkegaard 1959; Sartre 1957; Freud 1953). Selfhood is not created de novo, but from elements of the history of the individual which are **chosen** (Kierkegaard 1959: 263). Since personal identity is so intimately related to time, it is not surprising, to Whitrow (1980: 114), "that self-consciousness only develops as the growing child becomes aware of memory and thereby ceases to live in a continual present".

These choices may make for a more or less harmonious selfhood, but it seems plausible to assume with Fingarette (1969: 68, passim), that the choices are articulated by means of 'avowals' which are linguistic or language-like acts making one's engagement with the world explicitly conscious. Whitrow (1980: 111) concurs: "the origin of the concept of 'self' - which is usually thought to mark the beginning of conscious memory - may well depend on the process of epitomising our feelings symbolically". [N]

[N] It is worth emphasizing that consciousness of self in this view, is an activity, and that one may therefore distinguish between the **activity** of being self-conscious and the **products** of that self-consciousness. Deikman (1973) makes this possibility a

necessity. Bertalanffy, however, gives a warning about such distinctions: " Structures are slow processes of long duration, functions are quick processes of short duration. If we say that a function is performed by a structure, it means that a quick and short process wave is superimposed on a long-lasting and slow-running wave " (1952:134).

Avowals as purposeful forms of self-expression underscore the self as a synthesis, an achievement, an accomplishment. The avowals of a person, since they are responses by that person rather than effects upon that person, are the root of personal identity. They are constitutive acceptances of responsibility for the set of engagements with the world; they establish something 'as his for him' (Fingarette 1969: 70 ff.). In these terms, I assume that each individual is unique, having a different personal identity, or self, from every other individual (1,C10). I also assume that the personal identity, or self, can change in time, because of altering engagements with the world, and altering patterns of avowal and disavowal about those engagements (1,C11). [N]

[N] This latter assumption (1,C11) is a common folk-philosophical view (cf. utterances of the type "I wouldn't recognise myself of five years ago", "I'm not the me you used to know" etc.), which is shared by some writers (Durrell 1962; Beckett 1965; Fingarette 1969; Derrida 1976). Some, however, prefer to assume continuity and stability for the self (e.g. Popper and Eccles 1977: 129). Whilst the assumption of continuity may be true of and/or for those who make

it, it does not cover as many cases as my 1,C11, and does not seem to me to be so plausible.

The linguistic basis for the construction of the Self (and hence the 'I' or Ego) can hardly be doubted in so-called 'western' (Euro-centric) cultures. For those seeking certain kinds of spiritual development, an often painful reversal, or deconstruction, of that process is necessary. For instance, any determinate ontology is a delusion:

"do you know what anything <u>is</u> - what it <u>is</u>, not what it looks like, not how you can experience it, not what it can do, all of which are secondary matters - do you know what it is itself ? . . . You can . . know things <u>about</u> it, but you never know what it <u>is</u>. You never begin by knowing what it is, and you never end by knowing what it is. . . Ignorance is the Principle of existence. . . Becoming submitted to that truth is spiritual life" (Da Free John 1980a: 35-9).

And, any life of the spirit must be ineffable:

"meaning is for getting things done in the ordinary way in daily life, but meaning does not interfere with most of existence . . . As spiritual practitioners you indulge in the world of non-meaning constantly. To live a spiritual life is to be projected into the world of non-meaning - not meaninglessness, not negativity, but a sphere of Being that does not have anything to do with meaning. . . The sphere of Being is not unreal, it is simply a more fundamental, a more profound dimension of existence. It is a dimension in which we actually exist, without mind, without words, but really, tangibly, presently, personally. We exist there in the frame of non-meaning" (Da Free John 1983: 30-31).

This last is parallel to Lawrence's (1981: 181) view that

"there is nothing of me that is alone and absolute except my mind, and we shall find that the mind has no existence by itself, it is only the glitter of the sun on the surface of the waters. So that my individualism is really an illusion. I am part of the great whole, and I can never escape".

A changing memory

Assumptions 1,C10 and 1,C11 have powerful implications for, and sources in, the memory of individuals. If personal identities are all different and if all experience is not only unique to the individual, but inexpressibly so (Laing 1970), and hence subjective (Bateson 1979), then the memories of different individuals will be different (since "memory is the concomitant of personal identity" (Whitrow 1980: 111)), they will be constructed differently, and they will change differently, from those of other individuals.

"Our view of reality is conditioned by our position in space and time. . . Thus every interpretation of reality is based upon a unique position" (Durrell 1962: 210).

Thus Broadbent (quoted by Whitrow 1980: 97) writes "it is highly likely that . . . different individuals . . . organise their memories in different ways". This is supported by Bartlett's (1932) notion that memory is the <u>active</u> organisation of reactions and experiences into schemata; each particular schema is chosen in

terms of the individual's interest.

One of the most interesting reasons why particular memories (engrams) undergo profound changes of content, and of meaning, may be by the very fact of their being recalled (a process which itself is barely understood) (Whitrow 1980: 101-3; Norman 1970).[N]

[N] Considerations such as these prompted Durrell to not quite permit one of his persons to say "Could anything as rich as memory be a cheat? He never asked himself the question" (1962: 444).

These dynamic characteristics of memory make the distinctions between it and imagination less obvious than they are often thought to be. Thus Fischer claims that the real nature of fiction and the fictitious nature of reality

"are reflections of the unitary nature of information, perception and action and are as inseparable from one another as the observer from the observed or the lover from the beloved" (1979: 31).

The folk-psychological views most commonly expressed about the relationship between memory and imagination by the people with whom I have discussed the matter are:

(1) memory is a true (and fixed?) record of the past,

(2) imagination is a private fiction or dream, usually having to do

with the future,

(3) the distinction between lying and imagination is minimal and rests upon 'intention-to-mislead'.

But Peters claims that distinguishing imagination from memory is a logical, rather than a psychological, act, and writes (1956: 133):

"The test of whether a person remembers or imagines is not the subjective test of pastness accompanying the imagery, but the evidence which confirms or refutes what is asserted about the relationship between the situation thought about and the thinker's participation in actual events. And to establish whether or not such a relationship holds - i.e. whether it is a case of remembering rather than imagining - a person's private conviction is a good guide but an unreliable test."

The similitude between the psychological processes which underlie both memory and imagination, which is surely the basis for Peters' stricture, makes the plight of the self-deceiver as common as it is (Fingarette 1969), and underlies such remarks of Durrell's as that "to imagine is not necessarily to invent" (1962; 275), and that "reality [is] always trying to copy the imagination . . . from which it [derives]" (1962: 286), and that "there are only as many realities as you care to imagine" (1962: 315), and that the quanta which we call events are to be seen "across the transforming screens of memory" (1962: 369). [N]

N I make no apology for quoting the conjectures of those who write largely out of the imagination. There is good reason to suppose (1,C12) that largely <u>because</u> of that practice, their guesses about such complex doings as remembering, forgetting, imagining, understanding and lying (together with the products of those processes) are at least as good as those of philosophers, psychologists and linguists.

A suffusing language

It is a matter of widespread assumption (1,C13), though not of course of proof, that the sense of self, the memory, the imagination, and the experiencing of our experience are suffused with language, or with language-like principles of organisation (Freud 1966; Tulving & Donaldson 1972; Sartre 1962; Laing 1967).

This suffusion of the inner and outer worlds of individuals by language plays an important part in Popper's (1972) distinctions between Worlds 1, 2, and 3. World 1 is the world of physical objects and states (the matter and energy of the cosmos, the structure and actions of living things or beings (including human brains), the material substrates of human creativity). World 2 is the world of states of consciousness (subjective knowledge, experience of perception, thinking, emotions, dispositional intentions, memories,

dreams, imagination). World 3 is the world of the products of World 2 (objective knowledge, cultural heritage, theoretical systems (including problems within them and arguments against them)).

He writes:

"Human language . . . belongs to all three worlds. In so far as it consists of physical actions or physical symbols, it belongs to the first world. In so far as it expresses a subjective or psychological state or in so far as grasping or understanding language involves a change in our subjective state, it belongs to the second world. And in so far as language contains information, in so far as it says or states or describes anything or conveys any meaning or any significant message which may entail another, or agree or clash with another, it belongs to the third world" (Popper 1972: 157). [N]

[N] Later, I shall have cause to doubt the subjective/objective distinction between language in World 2 and language in World 3, but for the moment I take Popper's remarks as a useful account of the suffusion of our worlds by language.

All our worlds are suffused by language because we are creating a language during the period of our greatest cerebral-mental plasticity, say from conception to twelve years. (The period will no doubt vary from one individual to the next as do other aspects of individuality.) As we are choosing a self, as we are learning the significance of avowals, as we are learning to control and motivate the systems of our bodies, as we are learning about

the behaviour of objects and of others, as we are remembering, as we are experiencing, and learning something about what remembering and experiencing are in themselves, as we are hoping, deceiving, deciding and expecting, so, at the same time we are learning, each of us differently, our mother tongue. Each of one of us reconstructs, or, better, recreates his or her language as a feat of intellect.

For instance, in the child's construction of its lexicon, given that an object has an infinity of properties, there can be no certainty that the points of similarity which the child notices between a new sample and the given object fit with the properties which led a parent to apply the word 'cup' to the new sample; indeed, "more general rules about the kinds of feature relevant for whole classes of words . . . are themselves creative and fallible conjectures" (Sampson 1980: 48). The set of properties which speakers use to justify the application of a word is indeterminate because "objects do not come to our attention labelled with a fixed set of properties and our minds do not impose a fixed set of categories on our experience" (Sampson 1980: 54). Indeed, the indeterminacy of word meaning is "a necessary condition for the growth of individual

thought" (Sampson 1980: 61). Most of the changes in a person's understanding of the world will be "subtler and more gradual than [learning 'hovercraft' for a new form of transport], and furthermore they will not in general run in parallel with the learning of other individuals" (1980: 60).

As Sampson (1980: 19) says " the contents of our speech and writing . . . are not born with us but made by us. It is not only by a figure of speech that we are entitled to describe ourselves as <u>making sense</u>."

Creativity and personal constructs

A condition on our certainty of the creativity of the individual is our sense of the shareability of those makings. There are several ways in which we might think of this sharing, but I shall assume, with Kelly (1955) that, like the creations of the individual, <u>the process of sharing is also creative</u> (1,C14). Kelly's theory of personal constructs (1955) is both simple and powerful. It consists of one basic postulate:

> a person's processes are psychologically channelised by the ways in which he/she anticipates events,

which gives rise to eleven corollaries. Very briefly, these cover the

following ground:

(1) a person must be able to construe replications of events;
(2) the ways in which persons do this differ;
(3) the construals of a person are organised into a system of constructs;
(4) each construct is dichotomous;
(5) the poles of each construct are chosen by each person for the greatest elaboration of his/her system;
(6) a person entertains only a finite number of constructs;
(7) the experience of a person may be extended by variability of construals;
(8) but a person's variation of construal of a replication is limited by the permeability of that person's construct;
(9) a person may have a fragmented construct system and thus entertain incompatible constructs;
(10) persons having similar constructions have a psychological commonality;
(11) a person's ability to construe the constructions of another enables him/her to play a role in the sociality of the other.

Of course this does not <u>show</u> the power and richness of Kelly's theory (see also Bannister and Fransella 1971, for exposition and illustration). It is not incumbent on Kelly (1955) to specify either the person's 'processes', or the 'ways' in which he/she anticipates events, and this is part of the power of his theory, since as we have seen, very little is known about either.

Nevertheless, we may assume that the general process underlying all of these constructions is **inference** (1,C15).

"We can know as much as we do, not because 'in a sense we already knew it', but because we are creative beings with the ability to

formulate fallible but genuinely original concepts and hypotheses in response to genuinely unforeseen experiences. The common features of our languages, and of our other complex intellectual constructs, reflect the uncertain, gradual process by which each of us has built up his structure of belief on a foundation of blank ignorance" (Sampson 1980: 209).

Fischer suggests (1987: 11) that creative inference depends to some extent upon self-deception since "the mind suppresses the secret of its functioning from itself in order to function" (1985: 49). Thus, the statement sugar is sweet is based on a paradoxical syllogism

sugar is not sweet (in and of itself)
sugar is sweet (when it is tasted)
sugar is sweet and not sweet

and is therefore constructed upon something which had to be suppressed, namely that

"sweetness is a threateningly fleeting interactional process between subject (matter) and (material) object; reality has no permanence; no past and no future; reality is a temporary construct that continually requires the retouching of the present against a backdrop of nothingness" (Fischer 1985: 51).

The poles of any construct thus jointly create in a non-linear manner the phenomenon of reality, a generation of fact and fiction, a production of time and space, which is "a narrative that matter-energy tells to itself about itself" (Fischer 1985: 51).

In order to understand things we must weave them into our own

mode of existence. The constructions of my mind constitute components of a story it is telling itelf about itself; but as Eco says "to tell a story you must first of all construct a world, furnished as much as possible, down to the slightest detail" (1985: 23).

Norwich's entropic theory of perception (1983) reflects these paradoxes. He suggests that awareness is not possible without uncertainty, that one can never perceive an event which is certain. This is a generalisation to perception (and for Fischer (1987) to conception) from Heisenberg's Uncertainty Principle concerning the properties of atomic particles. (One may require arbitrarily high precision concerning either the position of a particle or its velocity, but not both.)

Thus, one must question the existence of a phenomenon (sc. be uncertain about it) before one can perceive it, yet one must perceive it before one can be uncertain. Paradoxes such as this are apparently rendered harmless by the invocation of "an interpretive repertoire of expectations" - a sort of 'pre-understanding'. But the source of this pre-understanding is not specified.

Presumably the source must be some form of Kelly's (1955) first corollary. The process involved in the building of

pre-understanding will be the noticing, and hence the collection and grouping, of recurrences of differences which make a difference (Bateson 1985). That is, it will be some **classificatory** process (1,C16). I have reviewed a small number of these processes in Appx. D, below.

I suggest that the very large number of techniques available in taximetry, and the unabating inventiveness of practitioners in that field (whose primary trainings are at least as diverse as any list of university curricular subjects), are reflections of the primacy, centrality and diversity of classificatory processes in all individual human lives, especially in their developmental phase.

Minsky's frame theory (1975) has many formal parallels with Kelly's theory. A frame is a data-structure for representing a stereotyped situation. It consists of nodes and relations. Related frames are gathered into frame systems. The theory is simple and powerful, like Kelly's, but what is important for our present purpose is what Minsky says about the **gaps** (1975: 213):

"Apology ! The schemes proposed herein are incomplete in many respects. I often propose representations without specifying the processes which will use them. Sometimes I only describe ` properties the structures should exhibit. I talk about markers and assignments as though it were obvious how they are attached and

linked: it is not. . . I will talk as though unaware of many other important kinds of problems. I simplify many issues related to 'understanding' that really need much deeper analysis. I often treat statically things that probably require procedural representations".

It is almost certainly the case that we must always have such gaps, that an empirically and aesthetically convincing model of mind-functioning would be dangerous for mind-functioning (1,C17). Hofstadter (1980) suggests as much in respect of the death of J.S. Bach.

Fragility and indivisibility of the individual

I want to make three general points in conclusion of this inadequate sketch of what an individual needs to get into society, before getting through.

First, any, or all, of the skills or processes I have discussed may be lost or impaired by sickness, isolation, or deprivation. They are in a delicate balance which may be easily dislocated. Their loss is, for the unimpaired observer, often horrifying and always dramatic. Thus, Sacks (1982), with his usual sympathy, shows us a glimpse of the effects of a loss of self,

"Feelings of impotent outrage . . . haunt **all** patients who find themselves, their very sense of 'self', grotesquely changed by illness or other circumstances, but perhaps post-encaphalitic and

schizophrenic patients most of all, for they suffer the greatest ontological outrage, the most intense and 'inexplicable' assaults on the citadel of the self" (Sacks 1982: 52).

And of a loss of action tantamount to a loss of thought,

"The patients show normal or superior ability to understand and act on these [training] programmes -**except when they are 'frozen'**, and at such times they cannot even **think** of them, cannot 'remember' them, let alone act on them . . . At the moment of freezing the Parkinsonian is indeed deprived of procedures, and, literally, does not know how to proceed. This raises profound questions as to what constitutes 'knowing' or 'understanding' a procedure in such a context . . . it is as if (as in all of us) a 'procedure' cannot be really understood unless one is actually able to proceed on it - that the understanding is embedded in, inseparable from, the undertaking: and thus, if the undertaking, the power of action, is arrested, so too is the understanding, the power of thought" (Sacks 1982: 291).

Secondly, having tried to provide the individual with those processes, skills and structures which are necessary for him or her to get into society and then to begin to get through, it becomes clear to me that many of the distinctions above, which I have borrowed from others, are not in fact distinct at all (1,C18). Indeed this must clearly be one of the strongest reasons why any progress in research in these areas is difficult if not impossible.

The establishing of one's memory, the opening of one's consciousness, the choosing of one's Self, the creating of one's language, the constructing of one's world (of one's interests), and

the unfolding of one's experience, are, for each individual, all inextricably a whole, are all simultaneously necessary and mutual.

Thirdly, Sampson suggests (1980) that even though the phenomenon of cultural transmission enables the individual to avoid guess recapitulation, it does not bind individual minds (1,C19). The contents of what is called 'common sense' by some group or community is a cluster of conjectures which have been found to be so widely useful that they are held in 'common' amongst members. Similarly, the components of Popper's World 3 - records and documents of all kinds, together with their theories, evidence, lists, refutations, plots, characters and so forth - are also cultural resources enabling the individual to avoid conjectural recapitulation. Nevertheless a given individual is not bound by the availability of these resources to use them. One can be accused, after all, of flouting, as well as of lacking, common sense. And there are breakthroughs in thinking and in problem solving which come from minds which are ignorant of some widely entertained theory or belief. This much is clear.

But what of the case of two persons who accept not only all that common sense offers, but who are willing, also, to subscribe to

all currently fashionable beliefs and theories and to all current accounts of previous beliefs and theories ? Will their acquiescence in the convenience of cultural transmission <u>necessarily</u> make them more similar to each other than either is to the jester, the vagabond, the village 'idiot', to those who not only reject the wisdom of the library, but also that of their forefathers?

It does not seem so. For if their construction of their equipment (of self, memory, and so on) as well as their construction of how to construe (creating a way of learning how to learn (Bateson 1979)) are as different from each other as we have suggested they may be, then how can the results of the use of that equipment, of those processes, be convergent ? If each differently constituted different individual creates the meanings of all the phenomena which have meaning for him/her, then there are **<u>no</u>** conditions under which we can assume that some particular phenomenon will have the same meaning for two different individuals (1,C20).

Throughout the remainder of this thesis it will be important, from the point of view of what has just been sketched in for individuals, for us to bear in mind the warning which Raine (1985: 105) has to offer about our <u>ignorance</u> of our differences from

others:

"It is difficult for us to realize wherein our own unspoken assumptions, the foundations upon which we build our world, differ from those of others; for this assumed ground is the very thing we cannot discuss, for we may not know what it is, still less what it might be; we cannot imagine how the world might appear if we did not possess the groundwork of knowledge which we do possess; nor can we in the nature of things imagine how reality would appear in the light of knowledge which we do not possess. Yet we continue to assume that whatever theories we may construct upon it the primary experience of the world is the same for everyone; even when in theory we would admit that this cannot be so, we continue to imagine that we all live in the same apparent world through sheer inability to imagine otherwise. From time to time we receive a shock, when we are compelled to realize the immense divergence not of deductions and conclusions, but of premises, the basic assumptions upon which these rest; and thus even of the primary experience itself, inseparable from the attitude of the consciousness which receives it."

ALL MISSING PAGES ARE BLANK

IN

ORIGINAL

Chapter 2

A fragment of a model of hearers' activity

"What is the energy of listening ? Of comprehension ?" Firth (1957:91).

"The skilful speaker wins praise; the skilful listener, despite the mystery of his achievement, is ignored" Parker-Rhodes (1978: xiii).

"I must not quit Northumberland without taking notice that the natives of this country, of the antient original race or families, are distinguished by a shibboleth upon their tongues, namely, a difficulty in pronouncing the letter <u>r</u>, which they cannot deliver from their tongues without a hollow jarring in their throat, by which they are plainly known as a foreigner is in pronouncing the <u>th</u>: This they call the Northumbrian <u>r</u>, <u>and the natives value themselves upon that imperfection, because, forsooth, it shews the antiquity of their blood</u>" [emphasis added] (Defoe 1928: II, 253).

"I have barely alluded to the Burr, which is commonly looked upon as <u>the</u> characteristic of Nb. speech. This is because I consider it a modern accidental growth, very conspicuous to a Lowlander or a Southerner, though quite inessential to the dialect . . . It is really a defect of articulation which tends to become epidemic . . . It varies much in different parts of Nb. according to accounts which I have received . . . But there is much difficulty in ascertaining what practice actually prevails in any given place. And, after all, the practice may vary from speaker to speaker at the same place . . . The actual usage and its variety . . is . . comparatively unimportant . . . It is much more important to determine the limits of country over which the Burr extends . . . [A recommended informant] could not tell me himself, <u>and when he tried his commercial travellers he found that they all had the burr without knowing it, and were unable to detect its presence or absence [in other speakers]</u>" [emphasis added] (Ellis 1889: 641-2).

Abstract.

This chapter provides a fragment of a model of the activity of a hearer engaged in creating meanings about his interlocutor's speech and person. The model is restricted and it is normative. It is restricted in the sense that it does not make provision for the simultaneity of inferences about, for example, purely syntactic or purely pragmatic matters which are of importance to hearers. (It would be surprising indeed if the simultaneity of such inferences did not have important consequences for the 'real' manner of their formation.) The model is normative in the sense that speculative introspections concerning my own behaviour as a hearer, and the behaviour of others whom I have been able to persuade to talk about it, are assumed to be roughly equivalent to the hearing skills and hearing experience of at least some Others.

What are the consequences of assuming that hearers are creative (1,C3) and that an individual creates the meanings of all the phenomena which have meaning for him (1,C20) ? The consequences are very diverse and cannot all be dealt with here. I shall try to trace those consequences, like holding water in the hands, only in respect of utterance meaning and of variation meaning. It will be helpful for the reader to be warned that the consequences, for the two types of meaning, turn out to be, by and large, the same.

Meanings of utterances

Initial conjecture:

(2,C1) Hearers create the meanings of utterances.

(By means of commentary, I shall uncover some of the consequences, implications, and corollaries which the conjecture contains.)

I treat as misleading the distinction between sentence (-meaning) and utterance (-meaning) which is insisted on by various authorities (e.g. Lyons 1977). My grounds, surely not too simple, are that sentences only have <u>any</u> being as a result of having been written/uttered by whom, to whom, when, where, why and how, they <u>were</u> written/uttered. Linguists, or any others, who discuss sentences, or anything else, in isolation, can say nothing concerning the form-function nexus, which is the heart of any matter. <u>Post hoc</u> categorising, which accounts for a great deal of the effort in linguistics, is irrelevant to the present discussion.

Glossing of (2,C1):

We may provide preliminary glosses for (2,C1) as follows:

(2,C2a) That most hearers are also, on occasions, speakers is neither precluded by the conjecture, nor does it constitute an attack upon it;

(2,C2b) The meanings of utterances are created freely. That is, hearers can entertain any conjecture whatever about the meaning of an utterance which is weakly consonant with the context (but in respect of context, see below (2,C6));

(2,C2c) The meaning of an utterance is its effective meaning, that given it by the hearer. The meaning of an utterance/sentence will always be, at least in part, immanent, and can never be canonical (see (2,C4)).

Uniqueness corollary of (2,C1):

(2,C3) For a given utterance, different meanings will be created by different hearers.

This seems reasonable not 'merely' because different parties to an

interaction 'by definition' have different roles in it, and hence different 'views' of it. But also because each individual has a different self, a unique pattern of experience, and a uniquely recreated language (see Chapter 1).

The meaning which a hearer creates for an utterance is, at least, a product of the hearer's self, the hearer's sense of experience, the hearer's "representation" of his mother tongue, and the speaker's utterance. Given the uniqueness of these four phenomena, the uniqueness of the product is guaranteed.

Many people have noted this dependency between the making of meaning and single individuals - some with horror, some with resignation, some with joy (depending, presumably, on the uses to which their ideologies wish to put meanings, or individuals, or both). A brief and neutral version comes from Carson McCullers:

"In any communication, a thing says to one person quite a different thing from what it says to another" (1975: 287; cf. also, Stein 1971:21).

This little thing being all that is the case - the world in an utterance - the meaning (what the thing says) cannot have been **in** it, but must have

been given **to** it. [N]

[N] Some in favour of concensus (sic) might wish to argue that McCullers' view is not so abnormal (given a little subtle inappropriating, "a turn with a spanner, a bash with a hammer" (Connolly, B. (n. d.)), and that all she intends us to believe (already stretching **her** on a frame of intentions, and **us** on a rack of belief) is that the different 'resonances' which are struck by **the** meaning of a sentence are a reflection of the differences amongst the persons who apprehend those resonances and who understand that meaning. But this just will not do. Such an appropriation is incoherent, since it fails to indicate <u>who</u> is to decide what <u>the</u> meaning of a sentence is (and <u>how</u>). If there are some humans who can do this, why not more? Why not all? And how could such "arbiters" avoid the resonances (their word) reflected from their own selves, experiences, roles, preoccupations, moods? We should surely doubt the humanness of anyone who disassociated their identity from their capacity for judgement.

All creations of meaning are riskings of the hearer's individual self in the face of questions and demands which are attributed by that hearer to that which brought the sentences into being.

Opacity corollary of (2,C1):

(2,C4) Meanings are not transparently resident in utterances.

This is just as applicable to a third party "observer", who may be playing the role of unfocussed participant, as it is to a linguist studying decontextualised sentences in citation form or in silent

quotation form. An observer can, in general (but see (2,C11)), never predict what the effective meaning of an utterance/sentence will be. That is, he can not predict, in just the sense that he cannot say, what the hearer's next remark will be, even in the class of cases where we might expect the hearer to assent in the question of whether his (the hearer's) next remark was contingent upon the speaker's last remark, viz. the one whose meaning is in question. (That is, setting aside all those cases in which the speaker's utterance fails, in various ways, to engage the hearer's creativity).

What applies to the unfocussed participant must apply more to the lone linguist with quoted or citation sentences (tape or text). The determinacy of the textbooks (and of the process which leads to their production) is belied by the experience of anyone who is actually engaged in the complexity of creating meaning from the chaos all around. The chaos clearly, perhaps centrally, includes those recurrent problems called 'utterances' and 'books'.

Ignorance corollary of (2,C1):

(2,C5) Speakers can never know what they have meant (done) as an immediate result of having spoken.

Speakers know not what they do in a literal as well as in a metaphorical reading. If the hearer does not create a jocular meaning for the speaker's utterance, then he does not laugh, and it was never a joke; if the hearer does not create the meaning of a remark as ironic, then the speaker spoke literally. The hearer can observe the products of his understanding, if only fleetingly, whereas the speaker is unconscious of his ideas before they gain expression (Parker-Rhodes 1978: xiv).

The speaker can only ever know <u>approximately</u> what his utterance has meant. He comes to know this by creating a meaning for the hearer's response to it and by conjecturing one or more than one connection between that meaning and his recollection of his own utterance. Notice that the speaker only becomes aware of the meaning of his remark <u>in his role of hearer</u>. Such meanings for his own remarks as he can create secondarily, in this manner, are in a dynamic state;

they may have to be modified in terms of the created meanings of later remarks (than the next) of the hearer's.

Creation of context lemma: [N]

[N] This is not, strictly, a corollary of conjecture (2,C1), but rather a condition upon it, hence its status as lemma.

(2,C6) The hearer creates the context.

Many linguists assume that, for a given interaction, characteristics of the context - the purpose and setting of the scene, attributes of individual participants, the relationship between them, and so forth - are somehow objectively given, are the same for each participant as for the Observer.

But the least reflection in the matter shows that this cannot be the case given the differences between the participants. Indeed, Parker-Rhodes (1978) suggests that the central function of conversation for the participants is to <u>create convergence</u> between the contexts of the participants, and celebrate it.

The context is created by each of the participants in accordance

with their lifelong process of coming to know (whether on their own account, or from cultural transmission), in accordance with their experience, and in accordance with their construal of their current self, their mood, their commitment to the other, their interest in interaction, and so on, and so on.

In other words, a host of factors which cannot but vary from one individual to another dynamically underlies the creation of context, which will therefore be different for different parties to a given interaction (cf. corollary 2,C3).

Any context changes with the speaking of further utterances - the creation of the meaning of each of which brings into play knowledge, assumptions, attitudes and matters of focus which, up until that point, were not, and could not have been, in the context.

Bateson, explaining how a non-event can be a message, also recognises that the hearer creates the context

"zero, the complete absence of any indicative event, can be a message. . . The letter that you do not write, the apology you do not offer, the food that you do not put out for the cat - all these can be sufficient and effective messages because zero, in context, can be meaningful; and it is the recipient of the message who creates the context. This power to create context is the recipient's skill .... [which depends upon] a successful raid on the random" (1985: 46-7).

Parker-Rhodes (1978: 32ff.) makes some important remarks about context. First, he suggests that context is that about which utterances answer questions, that a contextual map is built up as the conversation proceeds, and that a segment of context is an entity of the same kind as the meaning of an utterance, and can therefore be represented by a series of utterances with no speaker.

Secondly, Parker-Rhodes suggests (1978: 9) that the normal purpose of conversation is to ensure that the contexts built by the two participants will converge.

Some of the gaps between these interesting views support the position I am adopting. Thus Parker-Rhodes writes:

"a context is not identical with a sequence of utterances, but with the corresponding sequence of what the utterances mean" (1978:38)

If contexts and meanings are indissociable, they must either be different parts, or properties, of the same thing, or they must be different names for a single thing. In either event we should expect them to partake in, or respond to, the same sorts of forces or behaviours. Clearly in so far as all contexts are utterable, this is a behaviour which contexts do share with meanings.

"If on the other hand, there were anything strictly ineffable forming part of a context, then as such it would at some point affect the meaning ascribed to some utterance; this altered meaning could be discussed, and the discussion would involve expression, in some form, of the supposedly ineffable matter. We are therefore entitled to propose that any context at all can be <u>represented by</u> a sequence of utterances" (1978:36). / Parker-Rhodes

The distinction given us, then, is between something which happens to be unuttered (context) and something which happens to be uttered (meaning) - the distinction rests only upon **interest**. (Or, in a completely cynical environment, chance. But I will not consider that further here.) What we are interested in when we speak is what is <u>unuttered</u>, i.e. the context. But the distinction ('interest') cannot simply inhere in the dimension of meaning+context, since, for two participants on different occasions, what is uttered on the one occasion (some several meanings) may be the basis of interest on the other (unuttered contexts). [N]

[N] Why then, one might ask, don't particular contexts for given pairs of participants become 'exhausted' ? First because the Self is not constant; secondly because they serve as the means of establishing reciprocal interiorization (the creation of the 'we') (Laing 1967:65 & ff.); thirdly because repeated enquiry touches the immanent (Da Free John 1980a).

It is necessary to emphasize the dynamic and constructional nature of

context:

"each utterance, as it is comprehended by the listener - and each potential utterance which occurs to him as a result of the 'thought' phase of the cycle - is thus a contribution to the context, which it in general alters (for one or the other participant) in some point. The contextual map for each is thus built up in sequence . . . [which] is not wholly arbitrary but represents evidence of the relative prominence that the various contextual data have for the participants" (1978:35-6). (Parker-Rhodes

Notice also the possibility offered by this formulation of a given

participant entertaining more than one contextual structure

<u>simultaneously</u> - since <u>more</u> than one 'potential utterance' may occur

to him.

We can imagine the context being uttered by an invisible

commentator:

"[here is] the build-up of context from Alfred's point of view [JP: intonation according to Halliday's (1967) method [//= tone group boundary, /=stressed syllable, Δ=silent ictus, 1=fall, 2=rise, 4=fall-rise, 5=rise-fall]; A=Alfred, B=Beatrice, context= not A, not B]
//1Δ There are /people /playing **ten**nis
//1Δ They're /not /**here**
//1 **Alf**red would /like to /play
//1 Alfred /sees /**Bea**trice
A: //2 Want a /game of /**ten**nis /Beatrice?
B: //5 Δ I'm /dead /**tired** //4 Δ I'd /rather /**not**
//4 People who're /**tired** //1 don't /**like** /playing /tennis
A: //1 Come and /watch the /**oth**ers /then
B: //4 Oh Δ I'd /**like** to /sit in the /sun a bit
//1Δ It's /quite /**cold** today " (1978:36).

(Parker-Rhodes

Here there seems to me to be a difficulty about definiteness. The build-up' seems to imply, even if it is incomplete (1978: 37), that Alfred's opening remark could only have one context (for Alfred). But this surely cannot be true. Not only may Alfred have a motive other than wanting to play tennis. (He may also entertain two or more compatible, or incompatible, motives.) Worse, he may have no motive at all for making the remark he makes. (And uttering this context would not be easy.) Wittgenstein says (1967: 188) "And do I always talk with very definite purpose? - And is what I say meaningless because I don't ?"

The Hearer is busily creating meanings by inference, but

"[drawing] an inference from my utterance which I did not intend you to draw [can immediately occasion the] entertaining [of] divergent models of the context" (1978:32).

Presumably any particular participant cannot always guarantee to know at any given point in the exchange whether or not an 'unintended inference' has or has not been drawn. S/he can only know under precisely those circumstances in which The Other in some degree or another gives voice to the unintended inference or its consequence(s). If the speaker knows that the 'unintended inference' has been made, it is surely not so much that the contexts diverge as that they make a

<u>mutual</u> detour.

In terms of their effort,

"[comparing the speaker and the listener] the listener has the harder job of deciding what one would have to have been thinking, in order to motivate the utterance he hears in the context he finds himself in" (1978:xiii). (Parker-Rhodes

But the argument here is centred on an incongruence which is barely

visible. The context which motivates the utterance is the S[peaker]'s

context. The context which the H[earer] creates himself, when trying

to decide what S would have to have been thinking, is the H's context.

S's context and H's context are not the same. H cannot know S's

context.

Therefore H must make the decision about what S's utterance was

motivated by <u>on the basis of a context which is **not** the 'correct'

context for doing that determinately.</u> The build-up of context, quoted

above, "from Alfred's point of view" is therefore far too deterministic

and confuses the issue raised by Parker-Rhodes himself:

"nothing would be changed if [these context sentences] <u>were</u> uttered by an imaginary commentator . . . I don't mean them in any way to be <u>complete</u> as a statement of the context; they are selected only as being the points necessary to understand A's first remark" (1978: 37). Parke-Rhodes

Now <u>we</u> may 'understand' Alfred's first remark in terms of the

context-utterances given above, but Beatrice could **never** understand Alfred's first remark that way. Beatrice can only understand Alfred's remarks in terms of Beatrice's contexts, and conversely. If these different contexts are unuttered, how can the details of the differences become known ? The answer is that they can't. Only in a very sketchy, dimly visible way can Beatrice know the manner in which Alfred's contexts are different from her own. And then only if she is interested in knowing - since it requires some considerable effort.

"[thinking about the listener's 'problem' leads one to ask about its end-product, and that is] the thought arising in the listener's mind as a result of his activity, which is (he hopes) the same thought from which the speaker's utterance arose" (xiv). / Parker-Rhodes

But what evidence is there that the H does behave in this manner?

When I consult my own fleeting awareness not only of the products,

but also of the **process** of my understanding, I'm not ever, I think,

aware of the sensation of 'hoping'. And why is it appropriate that I do not entertain hopes of this sort? Because my understanding of A's remark can only ever be an incomplete approximation to the thought from which that remark arose. And I know this. I know that my

understanding does not arise passively in me triggered by A's remark. It results from an active creative commitment on my part (1978: 17-18) - in terms of <u>my</u> contexts, <u>my</u> interests, <u>my</u> being. And I know that in these respects A and myself cannot be anything other than different.

Parker-Rhodes says as much himself:

"There is no reason to doubt that the [expression phase] is typically a determinate process of algorithmic character. . . . . [but] it seems, from the lack of real progress, doubtful whether comprehension is an algorithmic process at all. It may rather be a questing search ('what would I have to have been thinking in order to say that?'), an open-ended enterprise bearing to expression something like the relation which, in the calculus, integration bears to differentiation. Failure to face up to this contrast may well have retarded our understanding of grammar in many ways" (1978:17-18).

But if the questing search really operates on the basis of the question cited, what would we do if the hearer's answer to himself was 'I don't <u>understand</u> what I would have to have been thinking to say that' (i.e. 'I don't believe I am <u>capable</u> of thinking what I would have to have been thinking in order to say that')? I guess from the logic of Parker-Rhodes' position that he would interpret this as meaning that the H was thereby barred from comprehending the utterance. But this does not seem to me to accord with what frequently happens in

conversational exchanges. Even if one's answer to the question is as I have given it, one ascribes a no-doubt vague, no-doubt multivalent meaning to the utterance. Parker-Rhodes certainly cannot make appeal to some inexpressible component of H's context as a source of H's understanding in such cases, since he has already claimed that all purportedly ineffable material in some context must, in fact, be expressible. In addition, there seems a logical difficulty here. If expression is a determinate process, and comprehension an indeterminate one, how can their products be 'the same' ?

The general difficulties which I have in the face of Parker-Rhodes' position arise from two of his claims:

(a) that participants, by conversing, make their contexts converge, and

(b) that contexts are utterable.

First, on the one hand contextual discrepancy is attributed to the words, rather than to some context, because context is an obscure entity (1978: 9); on the other hand any context whatsoever can be represented by a sequence of utterances (1978: 36).

Secondly, on the one hand a participant can be frustrated by contextual discrepancy - implying his powerlessness in the face of that

discrepancy (1978: 9); on the other a participant can accept or amend the Other's contextual map (1978: 8-9), or refuse to alter his own (1978: 9).

Thirdly, if their (initial) contexts are different for the participants (1978: 9-10, 35-36, passim), and if B's created understanding of A's remark can only be a partial approximation to A's thought through B's context(s), how can A's and B's contexts converge?

I mean what possible measure of contextual convergence could one imagine? What could the underlying characteristics of such a measure be? Mightn't it be more perspicuous - more easily measurable - to claim that participants are interested in creating, not contextual convergence, but contextual richness ?

Note that the conjunction of (2,C6) [that the hearer creates the context] and (2,C2b) [that the meanings the hearer creates have to be weakly consonant with context] makes the meanings much more centred in the hearer than may have been thought, since 'the context' is not an external constraint on the hearer (it being his own creation). This is also the motivation for (2,C9) below.

Meaninglessness corollary of (2,C1):

(2,C7) Utterances which have no hearers have no meaning.

This seems perfectly reasonable in just those cases when a speaker, in the presence of a hearer who is believed to be focussed, utters a remark, but is not given the opportunity to find out what his remark meant, because the hearer does not respond (cf. discussion of (2,C5)). Speakers who suffer this fate know how vulnerably dependent the speaker is on the generosity of the hearer: the sense of disorientation and exposure is sufficient to justify the form of (2,C7). The situation here is that of 'active' or 'punitive' silence on the part of the hearer; a conversational Cold War.

Note that whether the speaker comes to believe, after the event, that the hearer 'actually' heard the remark, or not, is immaterial for the corollary. (Presumably, if the speaker did come so to believe, it might have considerable consequences for further interactions with that particular hearer.)

However, there is a completely separate class of cases which we would not want to include under (2,C7), so we introduce (2,C8).

Consequence of (2,C1) and (2,C5):

(2,C8) The meaning of a speaker's utterances can be created by himself-as-hearer.

Single person dialogue (whether the utterances are audible or not) seems to occur. But the motive for such speaking is different from the normal motive (see below (2,C9 & 2,C9a)) - and the difference is precisely the absence of the Other.

In such utterances as those of single person dialogue, there is always some element of <u>intention</u> which, at its most general, has as its focus the bringing about of a change of belief in the speaker herself. Thus strategies, plans, schemes (whether for articulation with the self, with particular others, with institutions, or with ideas and artefacts) are generated, revised, rehearsed, sifted, catalogued, cross-tabulated, and evaluated.

The most general distinction between this case and the case of two person dialogue is that <u>the existence and integrity of the Self is never risked</u>. In the single person dialogue there is zero (or weak) vulnerability and strong intention; in the two person dialogue there is strong vulnerability and zero (or weak) intention (see (2,C9)).

Sometimes speakers believe that they short-circuit the cycle suggested by (2,C8), by creating a meaning for their utterance which is 'exactly the same as' what they intended to mean. (I will leave aside for the moment my doubts about the specifiability of intentions, if such exist on many occasions, and hence the possibility of matching an intention to the meaning of an utterance. I return to this below). But since speakers in single person dialogue do not always claim this one to one relationship, the consequence can stand. Indeed the fact that the himself-as-hearer of a speaker can create more than one meaning for his utterance is likely to be an important basis for 'thinking' and 'intuiting' (Bartlett 1932, Bruner et al. 1956).

Consequence of (2,C1), (2,C5), & (2,C6):

(2,C9) Participants aspire to be hearers rather than speakers.

This consequence feels strange largely, I believe, because of accidents of 'western' (Euro-centric) culture. As Parker-Rhodes laconically has it

"ours is a speaker's civilisation, and our linguistics has accordingly

concerned itself almost solely with the speaker's problems. The skilful speaker wins praise; the skilful listener, despite the mystery of his achievement, is ignored" (1978: xiii).

In negative terms, it may be relevant to imagine that the hearer cannot suffer the same disorientation as the speaker who has no hearer in the sense of (2,C7). In positive terms, one desires to be a hearer because the creation of the meaning of an utterance is a process of which one can be aware, and is thus an illuminating extension of one's consciousness. (Cf. Bateson "many, by listening, have helped me to hear when I was talking nonsense" (1985: xii).)

Why then do we not all lapse into silence? Occasionally, of course, we do. But when we don't, it is, rather, that one or other of us lapses into speech. What the motive for speaking is, given (2,C9), leads to the question of what is the central use of language and thus what are the central data for consideration.

Consequence of (2,C1), (2,C5), (2,C7), & (2,C9):

(2,C9a) The central use of language is the conversational use, the inter-<u>active</u>, inter-<u>personal</u>, inter-<u>subjective</u> use (Firth 1957).

Let me return to the silence from which we lapse when we speak.

Imagine two persons, focussed interactively, but silent. What pictures of these persons come into our minds' eyes? Typologically, it seems to me, we get two rather different pictures. In the first, we see two people who know each other very well indeed, and have done for some time. Their silence is easy, communicative, calm. In the second picture, we see two young people who have never met before and have just been left alone together by their respective families. Their silence is difficult, empty, tense.

But these two types of silence, and hence all the lapses from silence into speaking between them, are linked by a specific underlying characteristic, namely, the need to obtain intersubjective ratification of the existence, uniqueness, comprehensibility, and integrity of the self one has created for oneself.

In the first picture of two focussed but silent persons, we understand that this need has been largely assuaged: it is - for these two - not urgent any more. There is enough mutual knowledge of the other's contexts for any asking to be relatively unrisky; there is, perhaps, little for either to discover about themselves from the other.

(Though of course the pleasures of rediscovery certainly should not be minimised.)

In the second picture of two focussed but silent persons, we understand that the need is paramount, but that what must be asked and risked is more fearful than this paramountcy (hence the silence). The need to know these things about the Self is not so strong as the fear of the vulnerability which goes with it.

The motive for speaking, then, that which causes lapses from silence, is the willingness of the speaker to risk some vulnerability in order to gain some intersubjective ratification of the state of his self/identity. Speaking then, is, at its 'simplest', a reflection of an unwillingness on the part of the individual to be bound by the assumption of strict solipsism. We speak to have our Selves confirmed, both in the sense of a laying-on of hands, and in the sense of being assented to.

Why should it be a vulnerable process?

The process is vulnerable because we are uncertain of the impact of self-revelation through language on the unconscious, even though the unconscious is the source of questions about the comprehensibility and

integrity of the self. Isolation and recognition are both, after all, necessary qualities of our life, answering to the facets in us which are 'individual' and 'special' (species).

The speaker, in risking what she risks, hopes that the creative hearer will be altruistic and unbiassed, that he will invest all of his Self in his creativity, without more than a normal amount of self-interest or un-interest. There is thus an ethical pressure on the hearer to create his meanings in good faith.

Consequence of (2,C1) - (2,C9a):

(2,C10) Because the speaker knows these things, she has no intentions.

It was noted above under (2,C8) that the central characteristic of single person dialogues is an intention, by that person, to change his or her <u>own</u> belief. The academic profession is one in which <u>this</u> communicative mode (single person dialogue) is extremely pervasive.

(We are always trying to persuade ourselves that it may be in our

interests to 'adopt' something complicated which has just appeared.)

We spend a good deal of time talking ourselves (intentionally) from one 'position' to another. But in this we risk very little. I want to suggest that the academic use of language is abnormal and ungenerous, that academics have forgotten how to be creative hearers, and that the belief that speakers have 'intentions to mean' such that the effect will be a change in the hearer's belief structure, is an erroneous belief which has been transferred with no justification whatever from the closed world of single-person dialogues to the previously brave and open world of conversation.

In what they seem to believe is a new method for demystifying meaning and understanding, several linguists and philosophers have returned to intentionalism. [N]

[N] Wimsatt, the eminent literary critic, said somewhere that 'the design or intention of the author is neither available nor desirable as a standard for judging the meaning or the value of [a work of art]'. There are no plausible reasons why 'speaker' should not be substituted for 'author', nor 'utterance' for 'work of art'.

Grice (1969), for instance, suggests that the meaning of an utterer's remark is reducible or equatable to the intention of the utterer in

making that remark. The role of the hearer is then to determine what that intention was. Success or failure in such a determination is then attributable to the hearer, who may fail to determine any intention (does not under-stand), or may determine an intention which is the wrong one (mis-under-stands), or does determine the utterer's intention (understands).

Grice's framework is yet one more example of ours being a speaker's civilization: one in which the skill of a speaker is praised, but the skill of a listener is not even noticed (Parker-Rhodes, 1978). Grice assumes (1969, 1975) that speakers and hearers are rational. But, as Harrah (1963: 9) points out, "unfortunately the meaning of 'rational' is in dispute". Given the nature of the world, for example, there may be circumstances under which it is rational to take four times as long to say something as would be necessary in a closely similar possible non-actual world; it may sometimes be rational to tell lies; it may sometimes be rational to give more information than is strictly necessary for the realisation of some goal, and so on. Even without these problems, Grice's assumption of rationality does not restrict the remarks of speakers sufficiently narrowly such that his

hearers will always be able to recover intentions from 'literal' or 'normal' uses of remarks. (The distinctness and distinguishability of 'literal' from 'non-literal' is another of Grice's dubious assumptions.)

Because of these problems, Grice (1975) introduces various conversational maxims which constrain the possibilities of the speaker in precisely such a way that the hearer will be more likely to be able to recover the speaker's intentions. As more such restrictions are introduced, the types of intention which the speaker can have are driven closer and closer to the uses of language which have to do with the transmission of information. Put another way the remarks of speakers who are constrained by these maxims will tend to be remarks which one can more easily imagine fitting in with the idea that speakers say things in order to change the beliefs or knowledge-structures of hearers.

Apart from Occamesque arguments (to the effect that the assumption of rationality requires more monster-barring equipment than its maintenance merits (Lakatos 1976: 27-50)), there seem to me to be three arguments against trying to handle understanding (and hence, a fortiori, meaning) through the detailed intentions of the

speaker.

(1) In wondrous 'ordinary' daily life, people often do not know why they say what they say. I do <u>not</u> mean those cases of social extrication in which A says something which upsets B in some way and then says, by way of recognition and pacification of the problem, that she didn't know what made her say such a [negative value term] thing. No, I mean that the manner of Lessing's character is not uncommon (1973: 176):

"Jean Barker. Wife of minor Party Official. Aged thirty-four. Small, dark, plump. Rather plain. Husband patronises her. She wears, permanently, a look of strained, enquiring good-nature. Comes around collecting Party dues. A born talker, never stops talking, but the most interesting kind of talker there is, she never knows what she is going to say until it is out of her mouth, so that she is continually blushing, catching herself up short, explaining just what it is she meant, or laughing nervously. Or she stops with a puzzled frown in the middle of a sentence, as if to say: "Surely I don't think <u>that</u>?" So while she talks she has the appearance of someone listening. . . Because of her verbal incontinence, which shocks people, or makes them laugh, she is developing the personality of a clown, or a licensed humorist. She has no sense of humour at all. But when she hears some remark that she makes that surprises her, she knows from experience that people will laugh, or be upset, so she laughs herself, in a puzzled nervous way, then hurries on."

Tyneside has an idiom for such people: <u>She opens her mouth and lets the wind blow her tongue about</u>.

People often speak without having recoverable or specific intentions,

other than either being listened to, or dispelling silence. [N]

[N] A further example. At a certain stage of their work, postgraduate students are often grappling with a lot of ideas/reading at once in an effort to reach some conceptual breakthrough. They talk and talk, but on being asked by their supervisor "I don't understand; what are you trying to get at?", they say "I don't know, I was hoping you could tell me" - That is, "Please supply me with a retrospective intention".

But such general intentions as these are of no use whatever as input for the apparatus which Grice has constructed.

If these cases are as normal as I believe them to be, then the folk, as opposed to the professional, linguistic view, is that hearers are more powerful than speakers. This is precisely because a person who is able to listen, but who does not need to speak, suffers fewer doubts (does not need to trade vulnerability for confirmation) than one who needs to speak but is unable to listen. Consider:

"You will find that whenever people are together, they're making an effort to be listened to, and are very seldom listened to because the person they are trying to get to listen to them is waiting desperately and impatiently for a chance to be listened to himself or herself" (Personal Counselors Inc. 1982: 49).

Each moment, were it unpolluted by power and imposition, would hear many more praises for the good listener than for the good speaker.

The good listener confirms the unique, the good and the universal in each of us, despite our pain. The 'good' speaker, as likely as not, will engender in us feelings not from our experience, acts not consonant with our thinking, hopes not compatible with our good. A good listener is not one who passively construes a speaker's intentions, but one who attends the speaker's vulnerability, 'attends' in the senses of 'elicits', 'waits for', 'assists', 'holds', 'honours' (Pellowe (in prep. b)). I wonder if Firth had 'the good listener' in mind when he asked his fascinating question "What is the energy of listening? Of comprehension?" (1957: 91).

(2) The second argument against trying to equate meanings with speakers' intentions is that hearers are not passive transformers. Apart from their creativity in respect of the utterances all around them, it is also possible for <u>hearers</u> to have intentions. If hearers can have intentions in respect of speakers and/or the remarks which speakers turn out to make, then, clearly, any detailed intention which a speaker may have in uttering a certain remark cannot be guaranteed to be recreated by the hearer, <u>however the speaker expresses herself</u>.

What sort of intentions can hearers have? In general, the nature

and degree of attention which the hearer gives to the speaker's remarks is determined by the hearer. Within and around this generality, a hearer may intend, before or during the speaker's utterance, to ensilentise, to deauthorise, to disrupt, to deflect, to appropriate, to enjoke, to conspiratise, to enmonologue, to embarrass, to anger, to enmoralise, to feel with/for, to confuse . . . the speaker. And for each of these intentions there are various techniques available to the hearer.

A hearer's intention, if he has one, is, or may be, independent of what a speaker says. A speaker, because she does not know what this intention is, cannot make allowance for it in her utterances. A speaker may not have any detailed intention. A hearer may entertain more than one intention at the same time, which in some cases may encourage the simultaneous creation of two meanings for the same remark. Under these conditions there is no guarantee that the meaning a hearer creates for a remark will reflect the speaker's intention, if any, in uttering that remark.

(3) The third argument against Grice's position is a more general one. It is a general holistic attack on intentionalism arising from the quantum semantics known as deconstruction (Derrida 1976), or from

Buddhist philosophy (Loy 1988).

Any intention arises from and leads to bifurcations, dualities, polarities. In the first place an intention has a source (mind) and an agent (body); the mind sets a goal (then) in the present (now); the body (agent) generates behaviour (means) (act), to achieve the goal (end). All of the dualities present/future, body/mind, means/end, agent/act, require us to invent a fictive self to bridge the gaps which they embody (see Chapter 1). Free from 'intention' our daily activities may be realised as non-dual.

It is said that there has been, or is continuing, an 'information explosion', a huge, exponential increase in the number of 'facts'. But as William James said, facts are not of themselves true or not-true. Only because individual experience starts and terminates amongst facts, in the generation of individual beliefs, so those facts have an aura of reflected truth. Truth is made. By individuals.

Notice that there is no 'meaning explosion' to match the information explosion. Meaning arises from the sense of connections which is sustained by each individual. Meanings are not outside of people, and cannot be objectified, just as time is not outside of people

and cannot be objectified. It is no good one asking X what the meaning of Y's remark is. Meaning is not 'out there'. [N]

[N] If I do ask X what the meaning of Y's remark is, then the meaning (which I create for X's reply) will be a connection between me and X, not between me and Y. The peculiarity of such a question ('what was the meaning of Y's remark') guarantees that its answer cannot be given the meaning which, on the surface, it seems to require. Clearly, the hearer is responsible for the nature of the connection which he creates.

On the other hand, meanings are not 'in here' either. They are not distinct from one, or many, of my selves. At one level of duality, meaning cannot be separated from its representation, any more than mind can be from its body. That is, the meaning/form duality is as misleading as the mind/body one. At another level of duality, the meaning/person duality is as misleading as the person/time one. The meanings which I create are not independent of my self (my selves). [N]

[N] It would be ridiculous to imagine taking another on a psycho-physical tour of one's selves and saying: 'Please meet this one of my selves - this is the pile of meanings this one produced last week.' I imagine that part of what Beckett (1965) is dissecting in Krapp's Last Tape is his own [and Descartes'] uncertain assumptions about the continuity of 'the self'.

It is, precisely, the way in which we recognise different ones of

our selves, that they are associated with the different meanings which have been made by us. 'Are associated with' hides a further duality. It says 'there are selves' and 'there are meanings' - separate. What can be said to dissolve this?

Just as my selves <u>are</u> what they eat and drink; just as my selves <u>are</u> time; just as my selves <u>are</u> space; just as my Being <u>is</u> what energy-time-space is **doing** in this force-moment-place, so the meanings I make constitute me. Compare Lyons (1977: 608):

"One's modes of being are one's modes of meaning; and one means what one is (or, alternatively and equivalently, one is what one means) by behaving in such and such a way in one's context".

Intention of any sort disappears in the significance-energy-time-space which is centred now. Any Self is, then, a pattern of a meaning-force-moment-place which continuously recreates a significance-energy-time-space. And since force-moment-place and energy-time-space are amongst the meanings which are made, this reduces to: a particular hearer's being is the meanings he creates, no more, no less; the meanings he creates are him, no more, no less.

The relation between intention and information, then, is very

close. This would also be predicted by the beliefs that many teachers and their employers, paymasters and administrators have about what education is, and is for. [N]

[N] The banking concept of education (Freire 1972: 45ff.) is one in which it is assumed that students know nothing and that teachers know everything, that teaching is a question of one person telling many people what to know, what to think, what to feel, so that the one person may then ask the questions which s/he deems appropriate, such that the many can say in answer what the one has said to them, and the one can then tell them if they are any good. In this structure of stupidity who <u>can</u> be any good? Shouldn't we be surprised at how commonplace such a belief about education is?

There is a close relationship between intention and information, but there is no relation between intention and meaning. Meaning is a mystery, and mysteries are personal. Berger and Mohr (1982: 89) warm us with their perception:

"in life, meaning is not instantaneous. Meaning is discovered in what connects, and cannot exist without development. Without a story, without an unfolding, there is no meaning. Facts, information, do not in themselves constitute meaning. Facts can be fed into a computer and become factors in a calculation. No meaning, however, comes out of computers, for when we give meaning to an event, that meaning is <u>a response, not only to the known, but also to the unknown</u>: meaning and mystery are inseparable, and neither can exist without the passing of time. Certainty may be instantaneous; doubt requires duration; meaning is born of the two"[emphasis added].

Let me repeat that, in the central - conversational - data of a properly constituted linguistics, intention plays a negligible part. If a hearer suspects that the speaker is using the appearance of conversation to manipulate his - the hearer's - beliefs, he is entitled to use all the defence strategies available to him. The person who understands what the central use of language is always risks the self when interacting with another (whether as speaker or as hearer); when s/he is alone, s/he is internally silent. [N]

[N] Consider Proust's (1971: 55) remarks:
"in reading friendship is brought back to its first purity. With books, no amiability . . . No more deference: we laugh at Molière only to the exact degree we find him funny . . . The atmosphere of that pure friendship is silence, purer than speech. For we speak for others, but we keep silent for ourselves. Also, silence does not bear, like speech, the trace of our defects, our grimaces. It is pure, it is truly an atmosphere".

Increase in the occurrence of single person dialogue is itself a characteristic of that disassociative, atomistic paradigm which has produced so many other inhuman outcomes in our time. The pervasiveness of the fallacy of intention is such as to have produced huge numbers of people with profound problems of selfhood between

the two silences. People can no longer do anything, risk anything of themselves, in contexts where they have come to expect hearers to be cyphers, not creators, accepters of intentions, rather than confirmers of risks. [N]

[N] It is hardly surprising, then, that modern forms of therapy for the afflicted are ones which spend a good deal of time and effort on re-establishing the existence, generosity, and creativity, of the hearer. Thus, for example, co-counselling is a technique which provides the speaker with the space and safety in which to re-member and examine the buried and injured parts of the Self. It does so simply by insisting that the counsellor be a creative hearer in the best of faith (Personal Counselors Inc. 1970). Assertiveness therapy (Smith 1975) encourages the individual to maintain the integrity of his Self by being a creative hearer (and an assertive speaker) in the face of all forms of 'authority' and in the face of all claims by speakers that they are 'not responsible' - that is in the face of all which demeans (and de-means) his Self. The koans of Zen - its most important teaching vehicle - also rely on hearer creativity (Reps 1957, Sekida 1977).

Making conversation is of unwaning interest, and is endlessly engaged in, because 'success' reflects two complementary needs of the Self - confirmation and dissolution. In the mystery of conversing, we find, through vulnerability and generosity, the mutual creation of something which belongs to neither person, but which yet reflects, simultaneously, <u>the integrity of each</u> (confirmation of the Selfs) <u>in a</u>

singleness of meaning (dissolution of selves).

In what remains of this, I shall examine various conditions in which conjecture (2,C1), or any one of its consequences, appears not to hold.

Phaticism lemma:

(2,C11) Some utterances neutralise the hearer's capacity to create their meaning.

The hearer may, through idleness, disaffection, or persuasion, suspend his creativity. In its place he will rely upon conventional conjectures which have become more or less fossilised in the culture. This is the area of the stock response and the formulaic interaction; it is a reflection of the disengagement, the closedness between individuals.

The limiting case is that of 'phatic communion', a sorely mistaken phrase when we consider to what it is applied. There is no communion in many such vocalisations, and in some cases they do not even merit the term communication. [N]

[N] That is, to reverse Bateson's observation about zero events being effective messages, some substantial events are **not** effective messages. The uttering of "good morning" by another may be no more a candidate for meaning to one than the fact that - from the point of view of a camera - one is facing in the same direction as that in which one is walking.

Remarks which are described as phatic, far from being marks of communion, often serve only to close the intersubjective vulnerability which they **appear** to open. This they do by expressing the power of the speaker to deny the hearer any grounds for creativity. The hearer apparently has only two possible responses:

(a) he can acknowledge the power of the speaker by repeating the speaker's utterance ("good morning", "hi");

(b) he can neutralise the power of the speaker by not responding.

This latter response looks like a case of (2,C7) above, and formally it is; but notice that the effect on the speaker in this case is much less marked, if there is an effect at all. The failure of an expression of dominance is far less wounding to the speaker than the rejection of a proffered openness.

In the case of either (a) or (b), it appears that the hearer's

capacity to create meaning has been neutralised. But has it? As we have seen (Sampson 1980), cultural transmission bypasses the need for conjectural recapitulation by successive generations, but this transmission does not bind individual minds. [N]

[N] It is possible not to believe in time (Lumsden 1983), just as it is possible to believe that too much reading can solidify your brain (Renwick 1972), or that all ontological questions can only be answered by the question "How could it be otherwise?" (Foster 1983), or, with Lobachevski, that at a given point on a straight line there is more than one perpendicular, or, with Riemann, that there is none. There are, after all, no limits on imagination.

And just as, in general, culturally transmitted conjectures are not binding on individuals, so the hearer can always break the restrictiveness of phaticism by simply asserting his creativity in response. "Good morning" can be made non-phatic by "This is the coldest first of January in my life", just as my mother's "Happy 1984" from half way round the world was made non-phatic by my "No".

Speakers who are responded to with a created meaning in this way often behave as if their initial vocalisation had not been phatic. This compensatory response may indicate that the speaker who uses the dominance inherent in phaticism feels some guilt about it, from the

point of view of his empathy with the potential generosity of the hearer role.

Hearers always **can** create meanings for utterances, even if some of those utterances seem to neutralise this capacity. In other words the lemma does not hold as a condition on the conjecture, and can be deleted.

Consequence of (2,C2b), (2,C6), & (2,C10):

(2,C12) The context, and the meaning of the utterance, which the hearer creates, cannot be wrong.

This consequence denies that misunderstandings are properly so-called. To say that a hearer has misunderstood something implies, initially, that there is a 'proper' understanding for each utterance and that the hearer's understanding is not the proper one. This in effect blames the hearer for creating either a meaning for an utterance, or a context for it, which is in some sense 'wrong', and it does so by tacitly reintroducing the notions of canonical meaning and speaker intention which I have already excluded (cf. (2,C2c), (2,C4), (2,C10)).

But in fact the problem is not as simple as this, since the 'misunderstanding' may not be effective at the time of its occurrence, and may, in fact, never be discovered by the parties to the interaction ("they misunderstood each other, but I wasn't going to tell them"). But in the case of discovered 'misunderstandings', we can simply assert that the speaker has got something wrong, not the hearer. This seems perfectly reasonable, since the hearer cannot be expected to know when a 'misunderstanding' has occurred. (For a detailed study of this important and neglected topic see Humphreys-Jones 1987, who may not subscribe to the views expressed here.)

This formulation seems not quite to solve the problem however, since the implication here is that the speaker <u>will</u> know when a 'misunderstanding' has occurred, and hence <u>a fortiori</u> what the 'understanding' should have been. Certainly, a speaker <u>may</u> know when a 'misunderstanding' has occurred, but she <u>does not always</u> know. Therefore knowing cannot depend on any simple matching by S between an apparent 'misunderstanding' and her own recalled utterance. If it were not for the speaker having to create a meaning for the hearer's response and then, given her creation of context, having to conjecture a

relation between <u>that</u> meaning and her own initial utterance (cf.(2,C5)), then (a) there would be no possibility of the speaker not knowing that a 'misunderstanding' had occurred (there would be no conjectural 'gaps'), and (b) 'misunderstandings' would occur far more frequently than they do (same reason).

This both shows the generosity of the hearer and shows how delicate the speaker needs to be in negotiating 'misunderstandings'. If the meaning created by the hearer is such that the speaker realises that her created context did not embrace this created meaning, then not only is the hearer creating the meaning of the utterance, but in doing so he is enabling the speaker to discover what she might have 'meant to mean'. If the speaker had truly known what she had 'meant to mean', her remark would not have admitted the created meaning it did. This is why when the 'misunderstanding' occurs the speaker often repairs it with gratitude or humility.

The important point here is that the hearer's proffered meaning for the speaker's remark enables the speaker to decide <u>after the event</u> whether this is what she might have meant. (She is doing this in her role of hearer.) Sometimes the speaker realises (in a strange bubble of

delight) that the meaning which the hearer creates for her remark, is not only not what she expected, but is conceptually or emotionally new to her, and may even fit into a need which she didn't know she had. In this way conversations can be powerful sources of learning, of self-discovery, of the uncovering of the self (Freire 1972, Annand 1977, Galtung 1981, Berthoff 1986, Percy 1987).

'Misunderstandings' are not, then, in conflict with the conjecture (2,C1), or with (2,C12), and turn out to provide further evidence against the mechanistic distortion known as 'speaker's intention'. The closeness of 'actual misunderstandings' to forms of 'understanding' which are either unexpectedly jocular or usefully novel (to the speaker) is the reason why they are not felt to be pathological. They rarely make for discomfort, but rather for a deeper openness between speaker and hearer.

The next class of cases is very different in this respect. One of the apparent difficulties of (2,C12) is that it makes the hearer responsible for creating the meanings of lies, and, if the lie is effective, for bearing the speaker's deceitfulness in upon himself, and thus for deceiving himself by his very capacity to create meanings.

Notice that lying is the only case where there is a clear intention on the part of the speaker, that in the Gricean sense it is a meta-intention, and that this clear intention is intended to be opaque to the hearer. It is significant that the intention is not 'an intention to mean', but an intention 'not to mean'; it is significant that the intention is not designed or destined for the hearer's apprehension (cf. (2,C10). Lying is a turning away from the desires of the unconscious. It is brought about by the quailing of the Self in the face of real or attributed power.

But in the first instance lying is almost always taught. It is the role of various cultural institutions to attack and deny the validity of the individual Self. One of the ways in which this is done is by preventing people from finding the truth in their own unconscious minds. The injunction 'not to lie', together with examples of its functioning from various authorities, is one effective method of achieving the goal. A simple token of the type:

A small child aims a playful kick at the dog, and suddenly becomes aware (i.e. convinced) that an adult saw that this was the case. However, doubt is cast on this conviction by "Did you try to kick the dog?" The child knows that baiting the dog is proscribed behaviour. Who would ask such a question if they had the evidence of their sènses? The

displeasure of the authors of the proscription (the powers) may be avoidable if the response is opposite to the facts. (A <u>choice</u> of response is after all being offered.) "No," says the child, only to be immediately attacked with the imperative "Don't tell lies!". Always a shockingly direct springing of the trap. Note that the question (trap) is itself a lie.

In order to teach an individual **not** to lie, authorities lie in order to make the individual lie, so that the authorities can <u>tell</u> the individual not to lie. (Cf. The banking account of education.) Bannister (1983) observes that on being asked to describe their first memory of themselves as an individual, as a separate person, many people produce what amounts to a memory of lying. Rather than such a memory being based on a <u>positive</u> Self defining function (preventing access of others to self (Bannister's interpretation)) - which would imply that those who do not tell lies cannot develop a self - it seems to me that a more likely source of the memory is of the correlation between the lie and the injury to the unconscious, since it is the unconscious which promotes questions of selfhood, and denials of the validity of these questions are profoundly hurtful.

Lying is an affliction which thus has its roots in the social rather than the personal. It is a reflection of the political, educational, religious, and economic practices of the society in which it occurs,

since it has to do with power. For the liar, the generosity of the hearer and his creativity are transmogrified, by the obstrusiveness of these social practices, into a frightening power.

This is the converse of the view of Parker-Rhodes who lays the blame squarely on the liar: a liar

"chooses between two thoughts competing for expression on grounds of expediency rather than of truth. Fortunately it's not too hard for a discerning person to recognise this type of personality disorder . . . This information forms, for a listener forewarned, a part of his contextual map. This means that the liar's remarks are still believed, but they will be believed to be expedient for himself rather than to be true of <u>his</u> contextual map. . . A community in which lying is very prevalent labours under a great handicap in all its social interactions" (1978: 9).

Similarly Harrah's logical model of communication emphasises that

"we want to make life as easy as possible for an honest R[eceiver], and as rough as possible for a deceitful S[peaker] . . . we want R to . . . construct Q[uestion]-sets easily [to trap a deceitful or incompetent S]" (1963: 47).

I take it that these views are converses of my own because they are involved with 'truth' and 'information' rather than with revelation.

But that lying is simply a culpable trait of the speaker is by no means all of the story, cf. (2,C12). The hearer is also responsible:

(a) because he is not always fully creative in respect of the

context and of the meaning of the utterance, and hence shares in the effectiveness of the lie. (He is not "a discerning person" in Parker-Rhodes' terms, he doesn't 'see' the lie.);

(b) because sometimes when he discerns the lie, he lets it pass; the hearer is no longer ratifying the existence, comprehensibility, or integrity of the speaker(s) self, but is supporting the speaker's self-deception;

(c) because hearers often give up being creative at all in the face of the utterances of others (cf. remarks above on therapy); they renege on the duty of generosity, and fall back onto the easy deadness of the formulaic.

Lying consists in the desire for the hearer's creativity without offering the vulnerability, wanting the confirmation without the risk (because the risk is imagined to constitute a loss of self-control). This cutting off of the self from the unconscious ensures that the "liar has many friends but leads a life of great loneliness" (Rich 1979) and it leads to a kind of amnesia, a silence of the unconscious which is ground ready for the seeds of self-deception (Fingarette 1969).

It is a mark of the ethical irrelevance of linguistics that it has

nothing of substance whatever to say about lying (Bolinger 1973) and this is because linguistics is not interested in the individuals who hear, or in the individuals who speak, or in the relationships between them. [N]

[N] When I was looking for Bolinger's paper after an eight year gap, my recollection was that it was called "Lying is a linguistic problem", rather than "Truth is a linguistic question". For a Presidential Address even the LSA has to keep its titles clean !

Truth conditions on sentences have nothing to do with either truthfulness or with lying. As Rich wisely remarks "there is no 'the truth', 'a truth' - truth is not one thing, or even a system. It is an increasing complexity" (1980: 187) and "lies are usually attempts to make everything simpler - for the liar - than it really is" (ibid.).

As a result of fear, lying deforms the openness of the vulnerability/generosity nexus and changes it into the deadness of expediency, self concealment, self deception. It does so by permitting intention to transform complexity into simplicity. Note once again the promixity between intention and information ('fact'), and the abyss between intention and meaning (mystery).

Rather than each person seeing the Self as the ultimate source of genuine authority in life, most individuals succumb, under pressure of education and upbringing to the belief that truth is institutional and institutionalised (Thompson 1980; Benn 1982).

But the converse is the case. As Bolinger notes (1973: 541)

"the very government that is the greatest abuser of language, finds itself in the embarrassing necessity of enforcing honesty in order to collect its [sic] taxes".

And for research Herbst asks:

"Is it reasonable to assume that truth can be arrived at by means of deception? However . . . we manipulate behaviour in the laboratory setting; we can scarcely do so without deception. At the same time, the possibility of obtaining reliable data is dependent entirely on the honesty, trust and cooperation of the subjects involved. . . Deception leads to counter-deception" (1970: 12-13).

But both of these real problems are rendered intractable by a misleading notion of truth, of reliability. Bolinger claims that truth is "that quality of language by which we inform ourselves" (1973: 542) or that it "would always be prompted by the active willingness to share what we know" (1973: 543) - truth as knowledge, information. Rich's notion of truth as increasing complexity expresses far better the personal and interpersonal nature of truth. Given what we now know

about parallel universes (that is, interpretations which displace the Copenhagen interpretation of quantum theory) and observer effects in general, we would surely want to say that truth, in essence, is that quality of language by which we reveal our Selves.

What can the hearer do about lying ? The hearer needs to be constantly vigilant in just the sense that he is constantly and maximally creative. The hearer must constantly create a maximum of context. The hearer must always create as much meaning for, as many meanings for, an utterance as possible. In his subsequent utterance the hearer must always represent his created context and his created meaning(s) (of the speaker's utterance) honestly. Direct confrontation in the form of accusations is often counter-productive; for instance confronting the individual liar with her lie almost always leads to denial, avoidance, and a further round of expediency and self-deception// //(Ekman 1986) The return to generosity and vulnerability must be organic.

Governments and individuals lie because they are frightened of revealing themselves **and** because hearers let them. Lying and authoritarianism are only possible in a world in which a very large number of hearers have a wrong (i.e. weak) view of their powèrs and

their primacy; they entertain an erroneous model of themselves. A world in which all hearers knew the extent to which they make utterances mean what utterances mean would be one in which lying did not exist.

Meanings of linguistic variants

In general, the conjecture (2,C1) and all its corollaries and consequences, which I have introduced to handle the hearer's creation of utterance meaning, also apply to the hearer's creation of meaning for linguistic variants. Indeed the two kinds of meaning are inseparable and mutually informing; hence, presumably, they must be created more or less simultaneously, though not necessarily by the same processes (2,C13).

Just as two different hearers will create different meanings for a particular utterance of a speaker's (2,C3), so different hearers may create different meanings for a particular linguistic variant. (A good example concerning the so-called 'Northumbrian burr' [state 3 of 0286 PDV//r// see Appx. A, p.95, vol ii] is given at the head of this chapter.) [N] i.e. PDV //r//

[N] I am grateful to Mr P.J. Helm for bringing Defoe's remark to my attention.

Utterance meaning and variation meaning are inseparable in the formal sense that they are simultaneously embodied in the same phonic string. [N]

[N] Cf. My distinction between realisational variability and varietal variability in respect of prosodic strings, below Appx. B.

The speaker's remark constitutes both an expression of vulnerability asking for a meaning, and an expression of a string of linguistic variants asking for a meaning. The first type of meaning is utterance, or 'linguistic', meaning, the second type is variation, or 'extra-linguistic', meaning.

But these two types of meaning are also inseparable functionally, as far as the hearer is concerned. In order to behave in good faith in respect of the speaker, the hearer must respond as fully and honestly as possible to the speaker's vulnerability, both in terms of his creation of context and in terms of his creation of meaning(s) for the speaker's remark. To do this he must guess whether the speaker is lying or not, and he must guess what the probable prejudices, beliefs, assumptions, habits, hopes, preferences, and so on, of the speaker may be. To do this, he must have created beliefs (meanings) about any or all of the speaker's previous and/or current physical, psychological, social and spiritual states.

Summary & projection

Apart from the heuristic problems which hearers have to create solutions for in respect of the phonological, morphological, syntactic, semantic and pragmatic features of a particular speaker's utterance, I wish to specify two other factors requiring the hearer's creative effort. The first is the hearer's wider environment; the second is the role which the hearer is to be construed as adopting towards that wider environment.

The environment in which I will put the hearer is an urban one, and not necessarily an urban centre of which the hearer has native, lifelong knowledge. The role the hearer is to adopt in respect of this environment is one of reflective social interest, which by extension will also be one of reflective sociolinguistic interest. Coming to know the conurbation, its structure, its visible and invisible boundaries of football club support, of transport lines, of employment watersheds, of educational and religious preferences, coming to know the gradations of its retail styles and recreational possibilities, its people and their various habits and preferences, our hearer will simultaneously be coming to know, or at least coming to tune to, differences of speaking

amongst some/the citizens of this urban environment. In being socially reflective in this manner, I shall assume that the hearer is doing three things:

(a) he is creating different spatial and conceptual urban sub-environments which seem significant (to him) (2,C14),

(b) he is creating a number of the different manners of speaking of his co-citizens which seem significant (to him) (2,C15),

(c) he is building pictures of those manners of speaking which are associated with a particular sub-environment, and of those different sub-environments which are associated with a particular manner of speaking (2,C16).

In a word, our hearer is an ecologist. He wants to have decent estimates of the probability of association between a given situation and all manners of speaking, and between a given manner of speaking and all situations. In other words he is creating his own model of the social and linguistic structure of the urban environment which will generate labour saving expectations for the guidance of his own behaviour as a participant, whether speaking or not.

Some important inherent features of urban situations which

complicate our hearer's task are:

(1) the diversity of geographical origin of a city's constituent members,

(2) the high physical density amongst, but loose, symbolically mediated, social bonding between those members (Goffman 1961, 1963; Pahl 1968),

(3) the multidimensional complexity of role structures and group dynamics in terms of commercial, administrative and community pressures (Biddle and Thomas 1966; Cartwright and Zander 1968; Silverman 1970).

In spite of these complicating factors, it is clear that hearers do create solutions to the ecological puzzles which conurbations present.

Research in many fields (Pribram and Broadbent 1970; Norman 1970; Wathen-Dunn 1967; Minsky and Papert 1969; Minsky 1968; Good 1965b) suggests that our perceptual inferences about our environment depend upon continuously varying estimated probabilities which are derived from evidence which is inevitably incomplete thus necessitating subjective 'weighing' (Bayes 1763 (1958)) of those probabilities.

Speech is, in this sense, part of the environment, and a more significant part of it for hearers than for speakers. Two further characteristics of the speech situation are relevant here. First, the speaker's performance is continuously modified not only by monitoring and matching in terms of her own production norms (Stevens 1960; Liberman et al. 1967; Brown and McNeil 1966), but also by monitoring cues from the **hearer** (Goffman 1961; Hall 1959; Hymes 1962). [N]

[N] Cf. Sampson's (1980:196) interesting speculation: "Once hearers adopted such a strategy [for decoding coordinations] sentences violating Schachter's constraint [both or all conjuncts must fulfil the same syntactic and semantic functions] would systematically have led to misunderstanding, so speakers would have learned to avoid them [emphasis added] - and the constraint would thus have been institutionalised as part of the grammar".

Secondly, as we have suggested, hearers are variably capable of inferring rather specific extra-linguistic (x-linguistic) characteristics and information about their interlocuters. The ranges and types of such information are very large indeed. For instance, it may be information concerning relative s-e status (Labov 1966); concerning geographical origin (Grootaers 1959); concerning the value the speaker is ascribing to the hearer's company; concerning

long-term and short-term psychological and physiological states of the speaker (Pittenger et al. 1960; Goldman-Eisler 1961); concerning the unique, known, identity of an unseen speaker; concerning the degrees of similarity in some or all respects between the speaker's realisation string, and the speech of a previously heard or encountered speaker. [N]

[N] According to Anglin, adult hearers appear to be able to **generate** a myriad of equivalence relations which for them make two words similar (cited by Sampson 1980: 52). It is not clear whether the same ability extends to generating x-linguistic meanings from variants in the speaker's signal.

I use the term <u>information</u> in a deliberately extended sense, which has been indicated by its collocation with <u>create</u>. When a hearer interprets the realisations of a speaker, the social information which he makes is 'real' for him. That is, individuals do not seem to <u>doubt</u> their conjectures when they make them. (This is not to deny that they may be willing to attach a probability of less than 1.0 to some conjecture. Nor does it disagree with Sampson's (1980: 170) point that "people commonly persevere with refuted hypotheses" - as indeed do scientists (Kelly 1955, Lakatos 1976).)

However, the information an individual creates from an utterance may not be the same information as that derived from the same utterance by another hearer, and neither need bear any direct correspondence with the speaker's own view of the social information which inheres in her utterance. Thus, the extra-linguistic information in a particular utterance is a potentially multi-valued function of not only the signal, but also of the source, and of the receiver(s). It seems impossible, a fortiori, that competing values of such a function could be sorted out by some truth criterion - one supplied, for example, by a 'professional' observer.

As I have said, the list given above certainly is not exhaustive of the kinds of information which hearers can create for themselves on the basis of speech variation, and I would not claim either that all hearers are equally proficient, or that those who are proficient derive all this information all the time. (Analogue modelling of neurophysiological functioning, for instance, suggests multiple modes of selective, or means-ends, perception (Wathen-Dunn 1967).) What is suggestive about these points, for our present purpose is that the hearer may actively constrain, in several ways, the speaker's freedom

of performance variation. [N]

[N] It is not superfluous to emphasise that the constraining power of these activities of the hearer's is made possible and reinforced by the fact that the speaker knows what is going on because she herself is regularly a hearer. Cf. Sampson (1980: 48) "we all spend time guessing what sets of criterial features would explain the application of given words to given things in the speech we hear around us ... while trying to conform our own usage to our conjectural reconstructions of each other's criteria in order to be understood". Clearly then usage cannot rigorously determine meaning.

Since, then, it is the promulgatory, inhibitory and interpretive activities of the hearer on speech variation which yield the more powerful <u>functional</u> insights into speech differences, I assume that speech variation may be better modelled on characteristics of the hearer rather than on those of the speaker.

I shall, for the moment, make one relatively weak general assumption, namely:

> that much extra-linguistic information [N] can be derived from a function of the dissimilarity between the acoustic signal and the hearer's linguistic experience as a speaker (2,C17).

[N] I shall henceforth use the locution 'extra-linguistic information' to incorporate <u>any</u> form of information created by the hearer from the

acoustic signal which is not decoding in the strict sense of, say, Joos (1950). Here then, it is important to stress that there is an inclusion but **not** an equivalence relation between speech perception and decoding. (But even 'decoding' is probably a matter of more or less idiosyncratic creativity as I have been suggesting above.)

One may imagine that this function, Φ, obtains its x-linguistic values by a mapping into an array of homologous functions with already known or inferred x-linguistic correlates (2,C19). The particular methods by which real hearers construct a list of x-linguistic correlates, create an array of homologous functions, and map a newly created function into that array defy description, but not speculation. However, I shall defer discussion of this till I deal with the implementation of the model. (See Chapter 3 below, on the degree of fit between the Variety Space and the Social Space.)

I suggested above that all these creative processes of the hearer's might depend upon subjectively weighed Bayesian probabilities. That is, the hearer introduces, modifies or rejects hypotheses concerning pragmatic, semantic, syntactic and phonological aspects of the signal on the basis of intermediate odds rather than on the basis of a priori or a posteriori odds (Good

1965b). The sum total of the fates of these hypotheses for any given interactive occasion may be called loosely 'perceptual strain' and assumed to bear some linear(?) relationship to the dissimilarity function I mentioned above. h, Bayes (1763 [1958])

Three other characteristics of hearers require mention at this point.

First, our assumption about dissimilarity, above, implies a comparative process. This, in turn, must depend upon the hearer's having available some internalised representation of the fundamental elements of his language together, possibly, with their variant forms when these recur rather frequently. (Given our ignorance about matters of mental representation (Bobrow & Collins 1975), I do not intend to say any more about the nature of such elements or the manner of their association.)

Secondly, in some cases we must assume that hearers infer x-linguistic information by establishing the overall resemblance between speakers (speakers who are present or absent, but one of them necessarily previously encountered). I make this assumption to account for cases where the hearer's experience of a particular V is

so limited that he has difficulty in assigning a variant to its underlying fundamental element, or in ranking it relative to other variants of the same element. The assumption that a hearer can establish overall resemblance between speakers would permit the indirect use of a previously created mapping from some V (profile of linguistic variants) to some set of x-linguistic attributes.

Thirdly, we may posit it as reasonable that for sets of frequently encountered Vs it would be perceptually economical for hearers to compile a small list of the central (or defining) features of each of the Vs concerned.

I have indicated some characteristics of the hearer which seem to provide a promising basis for a model of speech variation, and I have ascribed five capabilities to the hearer when he is creating both x-linguistic and linguistic information from the same single signal. These capabilities are:

(1) a continuous formulation of hypotheses,

(2) a mechanism of comparison,

(3) a means of establishing resemblance,

(4) a system of mapping,

(5) a method of deriving diagnostic features,

and taken together, these five abilities clearly constitute a method of classification.

Classification provides the basis for the empirical interpretation, in Chapter 3, of this fragment of a model of the hearer's activity.

Whether any of the manners of unconscious human classification are isomorphic with particular methods of conscious human classification is an open question. (It would be as surprising that there were no points of contact as that there was complete isomorphism.) [N]

[N] What is known about folk taxonomy in botany (Berlin et al. 1974) and zoology (Hunn 1977), indicates that many of the most interesting problems in present-day theoretical taximetrics (see Appx. D, below.) must have been recognised by folk systematics a very long time ago.

It might, with good reason, be assumed that a speech variety, which has yet to be defined is, under the preceding discussion, a product of the hearer's creative processes in respect of the speaker and her utterance. That it is a product, specifically, which is a

constellation of linguistic variants and which must - because the mappings to social attributes, even if 'wrong', are rarely ambiguous for the hearers themselves - resemble the variety of a single group of others, and differ from the varieties of all other groups of others [N].

[N] Notice that the specification of 'a single group of others' to which a V is similar, does not preclude the individual **speaker** from having more than one V.

Then 'speech variety' could be defined as follows.

Some perceived or attributed constellation of speech features pertaining to a particular speaker which is believed by some particular hearer, though not necessarily expressibly so, to be furnishing him (the hearer) with whatever non-linguistic information he is currently conjecturing will be called a speech variety.

I shall, for the moment, acknowledge this as a characterisation of the notion 'speech variety', but will emphasise its closeness to the hearer by the notation $_{h}$V. (It should be clear under this gloss that

$_{h}$Vs can be available neither to empirical nor even, necessarily, to introspective investigation.)

ALL MISSING PAGES ARE BLANK

IN

ORIGINAL

Chapter 3

An empirical interpretation of the model

"Patterns very difficult to imagine were made together by everything . . . merging into a supernal harmony their unexceptionable varieties" O'Brien (1967:125).

"Meanwhile the indefiniteness remains, and the limits of variation are really much wider than anyone would imagine" (George Eliot, Preface to Middlemarch).

Abstract.
This chapter attempts to interpret the model of Chapter 2 empirically and shows how that empirical interpretation leads to various methodological requirements and innovations. Matters which are handled in the Appendices are referred to at several points.

Modelling

In sociolinguistics, whose expansion is so rapid, whose data are so complex, and whose conclusions have been eagerly awaited by educationists and administrators, the pursuit of methodological refinement and the development of transparent models are essential prerequisites for those who wish to establish a general theory. Public accountability for methods and models is doubly necessary in sociolinguistics because of its abnormal interdisciplinary status. [N]

[N] It is possible to deny of sociolinguistics (as of, for example, machine intelligence) that it is properly to be regarded as an interdiscipline, but such a denial rests on ontological rather than methodological considerations.

The abnormality arises from the theoretical instability and interpretive variability of both of the parent disciplines (cf. e.g. Botha 1973, Derwing 1973, Cohen 1968, Berger and Luckmann 1967). [N]

[N] In contrast, molecular biology, for instance, grew out of two theoretically stable disciplines (physical chemistry, cellular biology) whose conceptual frameworks became contiguous.

Hence, unclearness in the specification of aims and assumptions, allusiveness in a statement of methods, suppression of exceptions, and many other features of what may be normal presentation methods in other research areas, are particularly disadvantageous for the proper growth of sociolinguistic research, since precisely this information must be the basis for the formation of a critical apparatus by which to evaluate competing accounts.

The need for such an apparatus is caused to some extent by the theory-less nature of sociolinguistics. That is, the methods which people use, and the hypotheses they test with those methods (if indeed any hypotheses are advanced) are not derived from any theoretical structures, but are introduced ad hoc. (I return to this in Chapter 4.)

I shall illustrate what I mean by methodological refinement and transparent modelling through an empirical interpretation of the model of the hearer developed in Chapter 2. In particular, and where appropriate, I shall proceed by indicating how and why there were

changes in the ways in which various characteristics of our problems were conceived of, and dealt with. In what follows (and in sociolinguistics in general), it is important to dwell upon the relationship between a model and its products. A researcher's model of his problem is a non-unique representation of the interaction between his purpose or purposes and his assumption or assumptions. That is,

(a) for a given set of purposes and assumptions, there is more than one model possible, because firstly, different weightings upon the purposes (and/or the assumptions) will require different representations, and secondly, for fixed weightings of purposes and assumptions, different forms of implementation are possible (different methods, that is);

(b) for a changing set of purposes and/or assumptions, there will be a changing model. [N]

[N] More precisely, a model is a multi-valued function of the unordered triple {purpose, assumption, implementation}.

Clearly, insofar as progress is thought to reside in the process of adding results together, and given that 'results' here means 'the

products of different models', it is critical to determine the extent to which models permit proper additivity. [N]

[N] Awareness of the importance of this problem, let alone interest in it, seems to be non-existent. My expectation (at a colloquium on 'Empirical Work in Sociolinguistics' in 1974) that "the notions of model and modelling must surely get some attention" (Pellowe 1974b) was unfulfilled.

In the spirit of my own pleas, then, I shall try to articulate purposes, assumptions, forms of implementation (methods), and products (results), and the relationships between them.

The history of this hearer model

The first model for the Tyneside Linguistic Survey was drawn in 1965 (Strang 1968). Here the delineation of purpose is clear and straightforward and discussion of it leads directly to several important assumptions. The nature of implementation of the model received little attention, partly because of the exploratory nature of that stage, and partly because we wrongly construed 'relevant methods' as the passive outcome of the conjunction of purpose and assumptions. Our purpose as stated at that stage (Strang 1968), was

to determine who speaks what kind of English in a particular area, or, more technically, to determine the pattern of social distribution of varieties of English. We assumed, in the absence of evidence to the contrary, that such patterns were different for different urban areas (3,C1). [N]

[N] We derived some support for our assumption from differentials in local demography (Moser and Scott 1961).

From the point of view of implementation, we took this to indicate that informants had to be selected on a social (not socio-economic), rather than on a personal, basis. That is, in order to investigate the degree of fit between varieties of language and categories of society, the linguistic data must be socially contrasted by residential areas (3,C2). [N]

[N] With hindsight, I suspect that we were failing to disambiguate 'social' in our purpose. 'Pattern of social distribution' may denote either a pattern of distribution with respect to measured sociological parameters of individuals, or a pattern of distribution distinguishing one physical environment of communication from another in a way which is consonant with sociability patterns. The two are by no means necessarily congruent.

We assumed, drawing on traditional distinctions but renaming

them, that varieties of (British) English are encompassed by a two-term system: non-localised and localised (3,C3). Non-localised varieties ($_{NL}$Vs) indicate of their speakers that they may be placed in England, or as educated in England, but not which part of England they are associated with. [N]

[N] It is noteworthy that the central part of these definitions is dependent upon a principle of social perception. This clearly was part of the basis for our growing concern with the functional importance of the perceiver or hearer (see below).

Localised varieties ($_{L}$Vs) indicate of their speakers which part of England they are associated with. (For more on this distinction in classificatory terms see Appx. A, Section 2, p. 15.) We also assumed that the social categories having the greatest clarity were elements in a two term system: working class and non-working class (3,C4).

When we came to examine the relationship between these two two-term systems however, we realised that a secondary assumption about varieties rendered the comparison not symmetrical. That is, while it seemed entirely reasonable to assume that an individual's social category was singular over a short time-period, having a

variety of English is not in one-to-one relationship with a speaker (3,C5). [N]

[N] We are not here referring to 'style' (sensu Labov 1966). Children are the most illuminating multi-variety speakers. Many anecdotal reports (my own and others') indicate that children often have several varieties each of which MAY undergo stylistic variation of the casual-formal type, and which can appear in varietally pure or mixed form. The parameters which effect selection of one variety rather than another have little, if anything, to do with environmental characteristics such as place and interactants. (See e.g. Local 1978.)

We hoped to examine this asymmetry between the two two-term systems by investigating the speech of children in the residential units which we chose. Finally, we expected the degree of match between the two two-term systems to be variable (depending upon which terms were being matched), and to change qualitatively for individuals in their lifetimes (either in respect of one of the systems, or in respect of both). That is, we expected (3,C6):

(a) that the degree of correlation between working-class status and localised speech varieties would be higher than the correlation between non-working-class status and non-localised speech varieties, and

(b) that either the range of her speech varieties or her social status

or both may change during the life of an individual speaker.

The initial pilot work was designed to implement these assumptions and aims. In addition, it tried to give **VARIETY** an operational definition by contrastive analysis of speakers representing broad social class categories.

It seems quite clear that the ordinary language user's reaction to 'whole system' variability as such, as well as to 'sub-system' variability and to specific variable values, is a part of his folk-linguistic awareness (Voegelin and Yegerlehner 1956; Labov 1966; Hoenigswald 1966). That is, the language-user is able to characterise as being of interest, either a whole <u>variety</u> ('His talk is Cockney, but, you know, not <u>very</u> Cockney'), or a particular linguistic system as a whole ('He talks with an entirely different inflection to me, more like singing really'), or a particular single variant form ('You put the stress on the first syllable of 'controversy', I always put it on the second'). (There may be other relevant levels than these three - particular subsets of systems for instance, and there are certainly other dimensions which intersect these three, 'foreignness' for

instance).

The notion 'speech variety' has not been particularly prominent in modern linguistics: it is implicit in some work on Scottish vowel systems (Catford 1957), and appears in a sense apparently parallel to that used here in some sociolinguistic and ethnographic work (Gumperz and Naim 1960; Ferguson and Gumperz 1960) [N].

[N] The parallel is only apparent, because, as I indicate below, my definition ultimately derives from a set of analytic linguistic constructs (whose characteristics change as the model moves from one state to the next), and not from a set of sociolinguistic axioms.

My usage shares nothing with the use of the term in stylistics (Gregory 1967; Strevens 1964); and catch phrases such as 'New Varieties' seem only to be undefined attempts at 'cornering' subject matters.

Henceforth I shall use 'variety' or simply 'V' to refer to 'speech variety'. A definition of $_{h}V$ has already been proposed in Chapter 2; a related, but empirically useable definition is introduced below.

The design of that initial pilot work incorporated three

components.

(1) Investigation of all speakers resident in an area which had been handpicked on the basis of factors associated with the social category 'working class'. [N]

[N] This is clearly the type of sample known as judgemental (Moser and Kalton 1971). Notice that social perception played its part in this, the social, as well as in the linguistic, two-term system. We did not, in fact, dwell upon the generality or adequacy of the factors used in handpicking the sample residential units. Type and ownership of housing was the dominant factor (Robson 1969).

This would quantify the relationship between membership of those classes and incidence of types of localised speech.

(2) A longitudinal (follow-up) investigation of a group of rehoused speakers, both before their moves and after their being rehoused together. (Those who usually suffer the effects of slum clearance and 'redevelopment' are in the same social category as (1) above.) This would test for the relevance of location, immediate environment and changed interaction habits to the decay, stability or growth of varietal distributions.

(3) Investigation of all speakers resident in an area which had been handpicked on the basis of factors associated with the social

category 'non working-class'. This would quantify the relationship between membership of those 'classes' and incidence of types of non-localised speech. [N]

[N] Speech samples were acquired on the basis of conversations in the informants' homes (albeit conversations with a total stranger). Biographical data were sought in minimal quantities. These conversations were the basis for determining the informants' varieties.

Only the third of these areas of investigation was actually undertaken (Pellowe 1967), since in it it was found that various attempts to define 'speech variety' operationally were weakened by our not having included the form of implementation as an integral part of our model. In other words, we had not properly understood the extent to which a purpose and a set of assumptions **fails** to generate a particular, and hence potentially an appropriate, group of methods, and, consequently, we had not understood the desirability of building into the model some parameters of methodological appropriacy. In particular, though it was known at the outset that such pilot methods could not yield reliable distributional data on Vs (because of non-calculable bias in the sample), Pellowe (1967) found that his

sample could not even provide an adequate classificatory base upon which to identify the Vs.

He saw this problem as resting on three factors.

First, the varieties which one identified were rather directly dependent upon the variables which one had chosen **in order to identify those varieties** (in other words the Vs which were classified were direct projections of the gross sociolinguistic perceptions of the analyst as an ordinary hearer).

Secondly, the relationships between identified varieties were complicated by the pervasive difficulty of listing deviants from a norm (Voegelin et al. 1963).

Thirdly, the general social significance of Vs could not be satisfactorily determined unless the classification of them was adequate. (I return to classificatory adequacy below (and see also Appx. D), and merely remark here that such adequacy depends, in part, on classes being relatively well-represented.)

This pilot work, which was based on a considerable volume of analysis, concentrated largely on determining what general form such

parameters of appropriacy might have. There is only one that concerns us here, and it has received very little attention. In accounting for linguistic variation one may either proceed from the socially well known to the socially less well known, or one may account for it in purely linguistic terms and then search for correlating sociological features. [N]

[N] Given a series of samples of speech, one may describe their similarities and differences in terms of linguistic features which are socially well known. (Thus Tyneside's resistance to the Great Vowel Shift, so called, produces a large number of forms whose social diagnostic power for both in- and out- groups is considerable.) That is, 'socially well known', here refers to linguistic features, not groups of people.

The first method clearly involves social-psychological principles, and was labelled a hearer-based procedure, the second incorporates a principle of linguistically systematic objectivity, and was labelled an analysis-based procedure. The parameter of methodological appropriacy which was developed in the pilot work was that these two procedures must be continuously confronted, the one with the other. Hearer-based procedures unconstrained by analysis-based ones, would represent some aspects of the

sociolinguistic knowledge of the community without showing linguistic systematicity; analysis-based procedures unconstrained by hearer-based ones would show the variable structure of linguistic systems without indicating what its social utility was.

One may view all these problems as deriving from the notion of 'speech variety' and its connection with the speaker-hearer. The model, and refinement of relevant methods for it, depends upon a careful examination of this connection.

The feasibility of modelling a hearer

The reasons why I do not attempt to base my model directly on capacities of the hearer are manifold, but here I shall mention only five.

(1) There are, currently, no general, agreed, well-documented theories of decoding, learning, memory and recall processes, in spite of a hundred years of serious research. Clearly any <u>direct</u> modelling of the hearer's creation of meanings for linguistic variation would have to incorporate such processes. (Recall, in this connection, the definition of $_{h}V$ at the end of Chapter 2,

above.)

(2) Hearers who have themselves <u>similar</u> varieties [N] may make very different allocations of a particular speaker

(that is, allocations which are different in kind or in fineness).

[N] That is, varieties which are similar in either the sense of $_{h}V$ or of an analytic sense to be defined shortly. That is, <u>hearing</u> each other as highly similar, or being so <u>described</u> by this model.

We may imagine that this kind of difference between two hearers has arisen from variable social or linguistic <u>experiences</u> (exposure) which has/~~k~~not produced variable social or linguistic behaviours in them. ~~k~~ve

(3) The resemblance between two speakers **x**, **y** may be of quite different orders of magnitude to hearers **a**, **b**, **c**, depending on the relative resemblances of **a** and **b** and **c** to the **xy** dyad. In fact, for the resemblance, R, between **a**, **b**, **c**, **xy**, if

$$R(a, xy) > R(b, xy) > R(c, xy),$$

then, we might expect **a** to perceive <u>less</u> resemblance between **x** and **y** than **b**, and **b** to perceive <u>less</u> than **c**. One has no reason to be at all certain about the **constancy** of an

inverse, or of a direct, proportional relation between hearer distance and hearer discriminatory power.

(4) The meanings which hearers make depend upon the manner in which they construe themselves <u>at the time of meaning making</u> quite as much as they depend upon whatever happens to have been uttered by the speaker (see Chapter 2 above, and cf. Gribbin 1985, Dirac 1982). It is thus equally, and ineffably, a matter of:

(i) what the H believes and feels about the nature and source of his own current context,

(ii) which of his available selves the H is most immediately and dominantly absorbed by,

(iii) in what esteem he holds - insofar as he may be aware of it - the currently speaking self of the speaker,

(iv) what the reason may be for the attention level which the H believes he is paying to the situation [e.g. 'interest', 'wonder', 'duty', etc (Harrah 1963)], and so on.

(5) We will always be unable to ensure the same range of comparisons (through all linguistic systems) as is available to

the hearer. Because the hearer cannot know, in advance of creating a meaning for it, what variables are going to be represented in the utterance, his method of classification must necessarily be wide ranging and ad hoc in terms of its criteria. Such forms of classification may be characterised by the following:

(i) class membership is based on a **variable** list of properties (there are no criteria which are both necessary and sufficient for membership in some group (cf. Wittgenstein's (1958) family relationship amongst games));

(ii) classes are the product of overall measures of relatedness between individuals;

(iii) classes sometimes contain distant members with divergent attributes in spite of having a continuous internal connectedness (Wittgenstein 1958, Beckner 1959, Needham 1961, Cattell et al. 1966).

The certainty of asymmetry between any analytic procedures that one may be able to develop to model them, and what hearers

actually do with, or believe about, linguistic variants, or varieties, is represented notationally by a distinction between $_{a}V$ (analytic variety), to be defined below, and $_{h}V$ (hearer variety). [N]

[N] Recall the definition of $_{h}V$.

'Some perceived or attributed constellation of speech features pertaining to a particular speaker which is believed by some particular hearer, though not necessarily expressibly so, to be furnishing him (the hearer) with whatever non-linguistic information he is currently conjecturing will be called a speech variety.'

I now define $_{a}V$ as follows.

> A representation of the utterances of a single speaker on a single occasion which is exhaustive in terms of the complete set of linguistic criteria (or variables) will be called an $_{a}V$.

What 'the complete set of linguistic criteria' is, and how it is arrived at will be dealt with shortly. (For various listings, and commentary upon them, their sources and their validity, see Appendices A, B, and C, below, Vol. ii.)

I have argued for the view that any account of linguistic variation is better handled on the basis of the skills of the hearer

than on those of the speaker. But we have also seen how any attempt at basing a model <u>directly</u> upon those hearerly skills would be indeterminate and uncomputable (and cf. also, in this respect, Parker-Rhodes (1978: 17-18, & passim)).

Does this mean that the hearer is completely unmodellable ? No, I think not. What it means is that the model will have to be constructed on the basis of a **simplified and generalised hearer**. Such a model will then clearly not be mistakeable for any actual individual hearer, but will be a sort of lowest common denominator of a 'universal hearer'. In addition, such an approach will help towards satisfying three reasonable requirements of any empirical model and its methods: first the methods must be 'objective' in the sense that within reasonable limits the 'results' are **replicable**; secondly the methods must be **general**, in the senses both that they can be applied in different places or countries, and that they can be extended to hitherto undefined specific goals; thirdly the methods must be **non-exclusive** in the sense that they can be adapted to incorporate any relevant linguistic or non-linguistic complexities of the area or the individuals under investigation.

The spatial 'metaphor'

The hearer's assessment of hearer-speaker dissimilarity, or distance, involves a spatial metaphor, and my model exploits this metaphor. Much of the foregoing discussion may be reinterpreted spatially.

If we conceive of any particular hearer-speaker as a fixed point in space, then we may base that fixed point upon whatever it is which he uses as the basis for comparison between himself and other speakers, and assume that his perception of the distance of those other speakers from himself is a function of their dissimilarity. This spatial view seems capable of accounting for several important contingencies. It underlines the nonbiuniqueness of the relations between social and linguistic variables. [N].

[N] For instance, because two different speakers might be the same 'distance' from the hearer, but in different 'directions', their allocation to the same or different social clusters by the hearer will depend upon his relative weighting of the two factors 'distance' and 'direction'.

It also allows for the different perceptions of speakers by hearers on

the basis of perspective principles. And so on.

In fact there are several good reasons for thinking of this spatial quality as not being metaphorical at all, but as having a basis in neuro-anatomy and neurophysiology. [N]

[N] Penfield (1975), for instance, suggests that Wernicke's area and Heschl's audio-sensory area (bounded anteriorly by an interpretive area) are mirrored in the right hemisphere [sc. for right handers] by spatial orientation in the equivalent cortical locality. Given the functions of the corpus callosum, this implies that coded abbreviations of linguistic signals are somehow handled, though secondarily, in a spatial manner.
Furthermore the operations of the visual cortex in respect of 'illusions', which involve counteradaptation ('rethinking'), are modelled as coordinate sensori-motor transformations through neural networks in multi-dimensional vector space (Fischer 1987: 15).

Or rather, since my previous paragraph implies that there is something weak, or dubious, or reprehensible about resting cases upon metaphors, there are good reasons for thinking that the spatial modelling of the hearer's activity is no more metaphorical than the 'literal' use of any other expression in some natural language. In other words, **everything** is metaphorical.

At the simplest level this is a consequence of the certainty that between any signal and its interpretation, there will always be a

series of transformations, the nature of each of which is not predictable from a conjoint consideration of both the signal and the interpretation. (This view was certainly espoused by Nietzsche (e.g. Grimm 1977: 100-115) and may well have been a component of Plato's thinking.)

In epistemological terms, for instance, Beckner (1964) suggests that the behaviour of any system is determined by factors within the spatio-temporal limits of that system and that this is

> one of a cluster of <u>metaphysical propositions</u> [emphasis added] that, taken together, specify in the most general terms the subject matter of the natural and social sciences (1964: 20).

(Notice that both these 'factors' <u>and</u> the 'spatio-temporal limits' must be constructed and then <u>attributed</u>.) He goes on to assert that since, according to such a proposition, the internality (in a spatio-temporal sense) of causal connection is part of the concept of a system, then both spatio-temporal and causal considerations enter into the actual definitions of particular material systems. He concludes that the spatio-temporal limits for systems must be "adjusted on the basis of what is then learned about the causal connections within those limits" (1964: 21).

Bateson suggests that all conscious perception is spatial:

"all conscious perception has image characteristics . . . [Each perception] has a beginning and an end and a location and stands out against a background . . . What I experience is not [an event], but my <u>image</u> of [an event] . . . Objects are my creation, and my experience of them is subjective, not objective . . . Our civilisation is deeply based on [the] illusion [of objectivity]" (1985: 39).
(Cf. also Bateson's remarks on various "pathologies of epistemology" (1979), and Raine's impassioned plea (1985, quoted above at the end of Chapter 1) against uniformitarian assumptions of the type advanced by Labov (1972).)

Bateson also has some interesting things to say about the

consequences (for effability) of the unconscious nature of the

processes of image formation. In an account of the five clues (size,

brightness, overlap, binocular parallax, and parallax by movement)

which enable us to create depth in images, he says (1985: 40-41):

"the first of these clues is size; that is, the size of the physical image on the retina. (More precisely I should have written: 'The first of these clues is <u>contrast</u> in size . . .') Of course, we cannot <u>see</u> this image so it would be more exact to say that the first clue to distance is the angle which the object subtends at the eye. But indeed this angle is not visible. The clue to distance which is reported on the optic nerve is perhaps <u>change in angle subtended</u>. (I observe not only that the processes of visual perception are inaccessible to consciousness but also that it is impossible to construct in words any acceptable description of what must happen in the simplest act of seeing. For that which is not conscious, the language provides no means of expression.)"

What I am suggesting here, then, is that an assumption that the

hearer's inferencing (about speaker-hearer differences) is conducted in spatial terms may not be a 'mere metaphor' at all. But of course, just as in the case discussed by Bateson, the spatial concreteness of the products of hearerly creativeness gives no guarantee that the processes which yield those products will be in any sense open to introspection or to change. In trying to construct hearer judgement tests in order to externally validate, or at least calibrate, the model presented here, one is clearly banging one's head on precisely this wall. (See the Section below, Hearer Judgement Tests.)

A General Hearer Space

The basis for my model of sociolinguistic variation is a principled search for a general optimum 'space' which will represent the simplest mechanics of the behaviour of a simplified, generalised hearer. I have already established in Chapter 2 that this simplest mechanics requires the following abilities or processes:

(a) a way of representing both the structural elements of the language and their variants,

(b) a continuous formulation of hypotheses,

(c) a mechanism of comparison,

(d) a means of establishing resemblance,

(e) a system of mapping,

(f) a method of deriving diagnostic features.

These abilities are used to guide the design of a General Hearer Space (GHSp). For example, I shall specify the GHSp as comprising two independent 'subspaces', namely the Variety Space (VSp) and the Social Space (SSp). I do this because process (e), 'a system of mapping', represents behaviours in which the result of one process of comparison is used to find or generate a result from a different process of comparison. (That is, the two comparisons are in different property spaces.)

With the characteristics I shall outline for them, the VSp and the SSp will permit the creation of classifications of the most general and natural kind. (I return to 'naturalness' below, and see also Appx. D, for a general discussion.). Here I shall sketch the gross characteristics of the model and points of interest arising from them; in the next section I discuss its implementation and evaluation.

The VSp is a multidimensional space each of whose dimensions

with its scale is a **criterion** with its variants. A criterion is any feature of speech showing at least two variants across the population under consideration, and not logically predetermined by the nature of any other criterion in the set of criteria currently in use. [N]

[N] My use of criterion is synonymous with Sokal & Sneath's use of character (1963: 61).

(A more careful definition of VSp is given in Appx. A, Section 2(a), Vol. ii, p. 8 ff.)

Similarly, the SSp is a multidimensional space each of whose dimensions with its scale is a criterion with its variants. A criterion is any feature of social reality showing at least two variants across the population under consideration, and not logically predetermined by the nature of any other criterion in the set of criteria currently in use.

Any particular speaker will thus have a unique multicoordinate position in each of these spaces, and the original aim, the aim of my ecologist hearer (see Chapter 2), can now, in part, be re-expressed as needing to know how speakers are dispersed in these spaces. [N]

[N] More strictly, it is a question of how ${}_{8}$Vs fill the VSp and how social profiles fill the SSp. The possibility that one particular informant has more than one variety is not excluded: it should (in theory) be possible to quantify the point at which two different profiles of linguistic variants of the same speaker are different varieties, but only in terms of the dimensionality, and mode of deriving, that particular variety space.

Of course the speakers who will fill the VSp and the SSp are speakers who have been selected according to some sampling principle, speakers who have been interviewed and tape-recorded by some stranger who only spoke to them for precisely those purposes, speakers whose recorded speech has been listened to over and over again, analysed, reanalysed, numerically coded and recorded in computer-readable form. They are therefore no more real speakers than our simplified, generalised hearer is a real hearer.

All my preceding remarks about real hearers imply that I expect the dispersion of speakers in the VSp to be 'clumpy', or discontinuous, to varying degrees. The VSp is a multidimensional box, each of whose dimensions (with its scale) is a criterion (with its variants). When a particular speaker's profile of variant values (${}_{8}$V) is presented to this VSp, it can be imagined as a point with the same number of

coordinates as the VSp has dimensions (if there is no missing data). A series of $_{a}$Vs which had more or less similar multicoordinates could then be 'visualised' as clumps or clusters in VSp. Such clusters of similar $_{a}$Vs will be called Variety Clusters (VCs).

The next process is to disperse the same sample of speakers who are in the VSp, in the other, independent, multidimensional space on social attributes (SSp). There, similarly, one might expect to find clusters of informants who were more socially similar to each other than to members of other clusters. (We will call them Social Clusters (SCs).)

[N] The asymmetry in dimensionality between the VSp (roughly 300) and the SSp (roughly 40) might produce undesirable classificatory consequences. The low dimensionality of SSp results from several factors, most notable of which are (a) there is a limit to the number of topics one can cover adequately in a reasonable interview, (b) the 'logical independence' of variant social features, of the type we are working with, is less easy to establish than it is for variant linguistic features.

Finally, one would seek a function which expressed the group properties of a VC in successfully predicting the social cluster (SC) allocation of those VC members. [N]

[N] I return to this below (and see also Appx. D). The function which maps from VCs to SCs is far from straightforward owing to the hypermultivariate nature of the distributions involved.

It is of the utmost importance that the VSp be both internally and externally adequate. By internally adequate we mean that the VSp, from purely technical considerations, must be reasonably full of points, whether they clump or not. (Since classificatory techniques are designed to reduce, and hence distort, the data, an overly small sample will be difficult to impose reliable structure on.) By externally adequate I mean that certain properties of the VSp must be able to be constrained by the results of hearer judgement tests. For example, the use of a classification technique which generated overlapping VCs would be externally inadequate if all hearer tests indicated discontinuity in the perceived range of linguistic variation. [N]

[N] However, it is timely here to reiterate Hoenigswald's (1966) warning, that folk-linguistic views need not be taken to limit what it is legitimate for the linguist to postulate.

In terms of these notions of adequacy, we may tentatively sketch some conditions for an **optimal** dispersion of speakers in the VSp.

I suggest that the VSp is optimal if (3,C7):

(a) (i) the number of VCs represented by a single member is a minimum, however large the sample size;

(ii) without contravening (i) above, the number of VCs is a maximum;

(b) each VC is well-represented [N], and has a relatively higher level of internal cohesion than would exist in clusters of either a uniformly or a randomly distributed population;

[N] An absolute definition of 'well-representedness' of a (classified) group is not possible. It depends, inter alia, on the rarity of the group's representatives in the population, the internal homogeneity of the group, and the classification procedure.

(c) no VC has the same group mean profile on a high proportion of criteria as more than a very small number of other VCs. [N]

[N] It is not possible to be numerically specific without the empirical evidence of several competing classifications.

We may relate these conditions to some predictive qualities of hearers' judgements. Notably, the assigning of a speaker (rightly or wrongly) to a group (well- or mis- conceived) is performable on the

basis of the generality (a, i), the inclusiveness (a, ii), the discreteness (b) and the relative unambiguity (c) of the group defining properties or x-linguistic mappings which have been previously encountered by that hearer.

Such conditions for an optimal VSp arise from the following considerations.

First, all the topics in this research are ultimately concerned with distributions or with the effects of changes. For instance, in order to determine the relevance of topographical/spatial isoglosses to work on urban speech variation, one might try to determine the extent to which the range of $_{a}$Vs of <u>all</u> of a street's inhabitants was narrower than the range of $_{a}$Vs one obtained from a random selection of the same number of speakers matched for age, sex, social attributes and length of local residence. It seems as if such an investigation <u>could</u> be initiated as it stood, without any prior research, but when one begins to try to assess the **significance** of the narrowness of the range of varieties in a whole street, one begins to see the great importance of the VSp as a preliminary base.

Here are some not unlikely hypothetical results from such an

investigation:

VC number	1	2	3	4	5	6	7	8	9	
Representatives in whole street	0	11	1	7	6	0	16	1	0	42
Representatives in matched random sample	2	7	0	10	8	1	10	1	3	42
	2	18	1	17	14	1	26	1	3	

The application of tests of statistical significance is vitiated here by two factors:

(i) we have no idea of the extent to which the VCs in this table exhaust the total <u>number</u> of VCs in the population, and

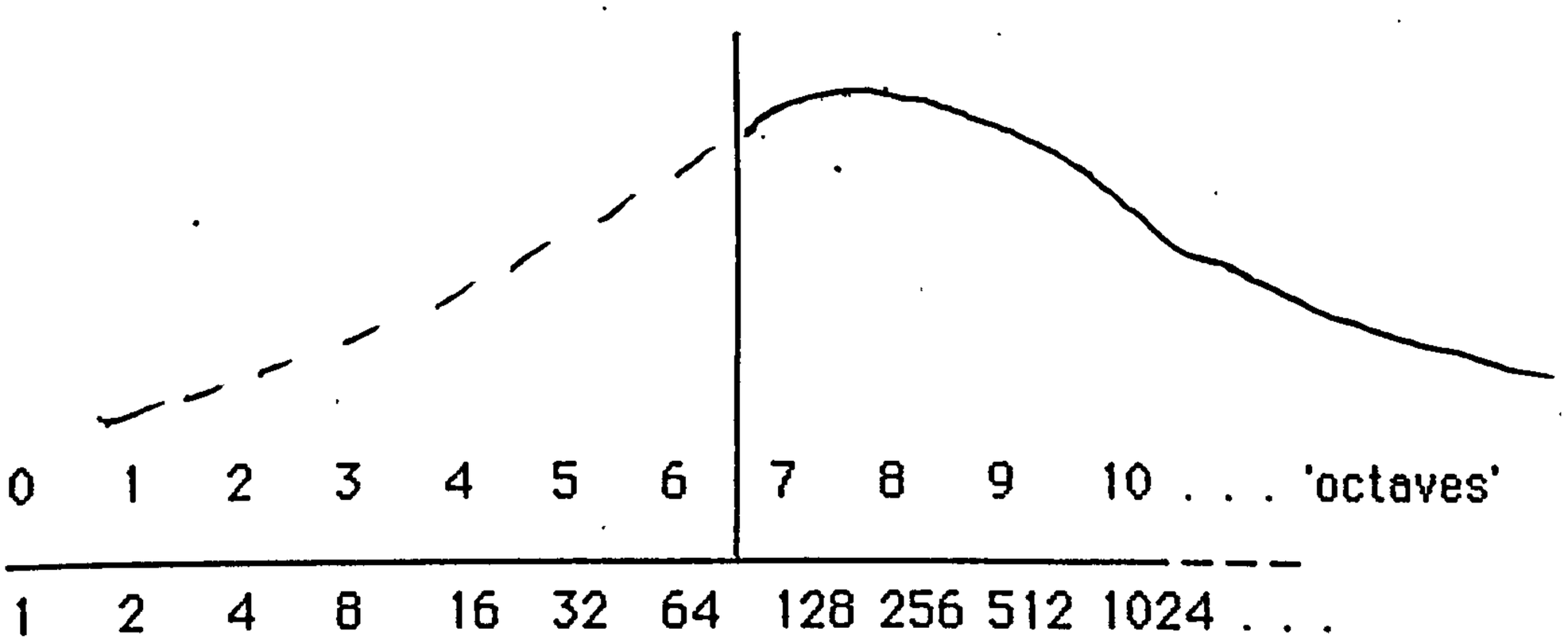
(ii) we cannot assess the significance of the differences of distribution in the two samples unless we have a reasonable estimate of the <u>commonness or rarity</u> of each VC in the whole population.

Such an impasse informs the framing of conditions (a(i)) and (a(ii)) above.

Secondly, if the VSp is to be the containing structure for particular investigations, then the VCs which it generates and contains must be stable and well-represented. Stability here refers to a measure of equivalence between properties of the data and properties of the

mathematical model underlying the classificatory method. The less this measure, the more easily will VCs break up under changes of implementation (alterations of criteria, missing information, changes of speaker samples). If we can not only ensure the stability of VCs, but also find that they are well-represented, we will be able to be more confident about the determining role they play in other investigations. But the well-representedness of a VC must depend in part upon the commonness of its constituent $_{8}$Vs in the population. Say that the adult population of an area being surveyed on Tyneside (as represented by a sampleable list like the Electoral Register) is 150,000. And estimate, **very** generously, that 10,000 of those individuals habitually use $_{NL}$Vs. Then, in a theoretically perfect random sample of 150 informants, there will be 10 $_{NL}$V speakers. If, on the list of linguistic criteria which currently constitute the dimensions of the VSp, these informants form 3 non-coalescing VCs having 3, 3 and 4 members respectively, there is obviously a problem of poor representation. It may be that the kind of distribution which applies to speech varieties is that which afflicts the ecologist

(Preston 1962) - an incomplete Gaussian curve - where there is a small number both of varieties which have very few representatives in the population and of varieties which have very many representatives in the population. The curve might look something like this (the ordinate is "Number of VCs'):



The only way of shifting the curve to the right in order to obtain these 'hidden' varieties (Preston calls the ordinate the 'veil line') is by a quite impractical, massive increase in sample size. The only way to include such varieties in the classificatory process would be to handpick speakers known to use them. But this would be statistically unaccountable. The problem highlights a general source of methodological difficulty, which is that I wish to be able to make

<u>both</u>

typological inferences from taximetrically adequate groupings,

and

predictive inferences from statistically adequate samples.

The data provided by pursuing the second of these goals is not sufficient to fulfil the first; the data provided by pursuing the first is not of the right quality to fulfil the second.

(For another possible way around the problem see the section below, on sampling.) Problems of this kind are the basis for condition (b) above on the optimality of VSp.

Thirdly, since the model postulates an unambiguous (though not necessarily 'correct') mapping between a speaker's profile of variants and an array of x-linguistic information or social meanings, we must try to ensure that the VSp does not contain too many cross-relations between VCs, since if it did, the power of the diagnostic profiles of those VCs in respect of the SSp would be greatly reduced. [N]

[N] I am completely ignorant about the quantity of 'too many', or the manner of 'reduction' of diagnostic power, but this does not, in my opinion, make the problem trivial.

(For further specification of the problems see the section on 'VSp/SSp fit', below; for concrete examples of attempted solutions, see below, Chapter 4, Appx. A, Section 7; and Appx. E, p. 558 ff.)

Problems such as this are the basis for condition (c), above, on the optimality of the VSp.

The VSp, with the characteristics I have sketched for it, the SSp, and the relationships between the VSp and the SSp, constitute the basic model of the mechanics of a simplified generalised hearer. And even at this stage the model reveals that we suffer from an interesting mixture of ignorance and complexity. For instance, on the one hand we have no idea of what a suitable underlying mathematical model for linguistic variation might be, on the other, the notion of 'cluster' is an extremely ill-defined concept. [N]

N This, in part, explains the multiplicity of clustering and classificatory techniques. (See below, Appx. D.)

Because I do not know what my results ought to look like, I need to generate different sets of results, or, put in another way, to perform a series of controlled experiments in distorting the VSp. If it

proved possible to determine the best, or the best subset of methods, in terms of internal and external adequacy, one would not only have confidence in the stability of the VCs but one would have determined a suitable underlying mathematical model for linguistic variation. It is therefore a crucial part of the empirical interpretation of my model that the VCs and their constituent $_{a}$Vs be thought of as **constructs** not of perception, but of analysis (cf. Kohler 1967). Thus we can entertain an alternative, but exactly equivalent, definition of $_{a}$V to that given above; $_{a}$V is the name given to

> **a profile of linguistic variants which exhaustively places any sample of speech of a particular speaker in the variety space as defined at that moment by its contents and dimensions.**

The qualification 'exhaustively' is of some importance since it emphasises that the usefulness of any current version of the VSp is its dimensionality or criterial properties. (I return to this later.) In ideal circumstances we might hope that $_{a}$Vs and $_{h}$Vs could be persuaded to converge but we have discussed substantial reasons why

this is not possible. (We cannot match hearers' 'criteria'. We do not know the nature of the neurophysiological or mental processes behind the partial matching of social and linguistic domains. Elicitation methodology cannot overcome the problem of pattern rarity. And so on.)

Implementation and Testing of VSp

What has gone before and what remains for discussion may be summarised in the form of an oversimplified flowchart (Figure 3(1), vol. iii, p. 15). I shall, largely speaking, restrict myself to matters of theoretical and practical concern to the VSp and the SSp, notably the determination of linguistic and social criteria, the sampling of informants, classification techniques (processes 1, 2, 3), the manner of the VSp/SSp fit (processes 4, 5), and methods of evaluating alternative VSps (process 6).

It is of some importance to stress that this diagram (Fig. 3(1)) is not <u>exhaustive</u> in respect of the potential of the model. For example, although I have not included it, it might be of some use to extract the social diagnostics of SCs. (Jones (1978, 1983) <u>did</u> extract them. See

further Appx. E, p. 558 ff., below.) These might well turn out to be indices very much akin to those of socio-economic class (but cf. Brandis' (1970) warning about the essential <u>abstractness</u> of 'social class'). In addition, one might find it useful to find a mapping function from linguistic diagnostics to social clusters. This particular use of the VSp and the SSp may be interpreted as a modelling of Labov's methods in a wider context. Secondly my emphasis of the differences between $_{a}$Vs and $_{h}$Vs, both in terms of construct status (manipulability) and in terms of criterial exhaustiveness, leave me in no doubt about the difficulty of hearer judgement tests, not only in terms of their design, but also of their interpretation (what will unexpected disparities, or agreements, in test results, mean? etc.) The dotted line from 'informants' to 'hearers' means that it will be important to determine that speakers with $_{a}$Vs of such and such a kind react to other speakers in terms of $_{h}$Vs, in such and such a way, so that, though a convergence between $_{a}$V and $_{h}$V is not possible, one would like to be able to get <u>some</u> sort of picture of the relationship between them.

Implementation depends, then, on the specification of linguistic and social criteria; on a choice of methods of informant sampling, and on a choice of taximetric techniques.

Specifying criteria as dimensions of the VSp & of the SSp

If the role of classification is fundamental to the model, then the criteria we choose to use have similar importance, since they form the dimensions of the VSp and of the SSp and any patterns we obtain will be a product of those choices.

Most research in classification now accepts as axiomatic that groupings should be derived from many criteria. (For more detailed discussion, see Appx. D, p. 427 ff.) Little is known about how the sampling of criteria, rather than sheer numbers of criteria, affects similarity coefficients, but our search for an optimal variety space and a maximal diagnostic profile (DP) for each VC, indicate that we need to attempt to be exhaustive under certain limitations. The limitations on the choice of criteria arise under four heads:

(a) relevance to classification,

(b) differences between the selectors or analysts,

(i) of perception,

(ii) of exposure,

(c) practicability of elicitation in an interview,

(d) exigencies of definition and computation.

I deal with each of these in turn.

(a) It is clear that the variant values of the criteria, and the criteria themselves, must be relevant to the desired classification. [N]

[N] It is often overlooked that **all** classifications reflect, to some extent, an <u>a priori</u> notion about the 'desired grouping', even if it is only for a 'general' or a 'natural' classification. Naturalness, in this sense at least, is relative.

The kind of relevance I have in mind works in two directions simultaneously. Firstly, one wants the VSp to give adequate coordinate location to <u>at least</u> all sampled speakers who are both native English speakers and natives of Tyneside still resident there. In this sense, failure to draw up an adequate list of criteria and their variants will tend to under-differentiate any distributional pattern which otherwise might have emerged. Secondly, we want the pattern which the VSp does portray to be relatively reliable in its numerical properties (intra- and inter- VC variance, diagnostic values of

criteria in different VCs); to the extent that our list of criteria (and their variants) includes items **not** of relevance to the sample to which they are applied, all such figures are distorted, since such criteria have a variably depressant effect on the similarity coefficients between pairs of speakers, and hence on the VCs which are built from such pairs.

(b) Different analysts will select different criteria as a result of differences in the analyst's own different positions in the VSp. That is, empirical observation and limited, but significant, research show that perceptual differences are likely to result from production differences (Ladefoged 1960, Ringaard 1965, Liberman et al. 1967). (For an analysis of differences of auditory/transcriptional behaviour in accordance with this expectation, see Appx. A, Section 6, p. 97 ff. for the segmental phonological case, and, though incomplete, Appx. B, Section F, p. 391 ff. for the prosodic case.) Furthermore, an analyst's willingness to incorporate some criterion must depend on his exposure to it.[N]

[N] This kind of difference, between selectors of criteria, is related to the relevance limitation (on exhaustive sampling of criteria) which

was mentioned above, since different criteria have very variable, and frequently unknown, areal distributions.

We may characterise this limitation on exhaustiveness in terms of the geometrical properties of the VSp. Insofar as a given selector under- or over- represents any delimitable subsets of possible criteria, he will be operating with a topologically deformed version of the VSp. For the moment I shall assume that such analyst-based deformations <u>are</u> systematic to some extent (3,C8). [N]

[N] The locus classicus of topological deformation in its application to spatial and/or temporal pattern is Thompson (1942). As the number of investigators increased, given that they had different geographical, social and educational factors underlying their linguistic habits, one might assume that an increasing number of topological deformations would be contributed. However, because of the different types and directions of deformation, conflation of these different selections of criteria might tend to a regular (i.e. undeformed) VSp. In other words for each new investigator there would be less to 'add' (or fewer new deformations to legislate for).

My acknowledgement that different analysts characterise different subspaces differently is important, since in reflecting the principle that different hearers hear the same speaker differently, it is predicted by the model itself.

(c) Because the stability of the similarity coefficients depends

upon most speakers being uniformly comparable, it is important to use criteria which have a highish likelihood of occurring in, or being made to occur in, an interview. This means that we can be more exhaustive in our choice of criteria from some linguistic levels (e.g. phonology) than from others (e.g. lexis).

(d) The final limitation on the exhaustiveness of the selection of criteria arises from computational and classificatory factors. Further details are given, in general, in Appx. D; for segmental phonology in Appx. A; for prosodics in Appx. B; and for syllabic criteria in Appx. C.

The criteria for the VSp are given in Appendices A, B, and C. The criteria for the SSp are given in Appx. E. In each case, the methods of collecting or establishing these different groups of criteria, the reasons for the form of their definition, discussion of their adequacy, and specification of the part they play, or the difficulties they may cause, in any subsequent classificatory process, are all given in those Appendices. Thus, the Appendices are by no means 'mere lists', but they present problems of implementation and their solution at a more detailed level than I am here concerned with. [N]

[N] For the writer, perhaps, as for the connoisseur, the greatest excitements are in appendice (O'Brien 1967, Eco 1985).

For example, though the problems of the stability and optimality of dimensions have been mentioned above, it is only when the difficulties of establishing prosodic criteria are handled in Appx. B, that they are dealt with in any detail (Appx. B, p. 213 ff.).

Informant sampling

It is important to emphasis that the optimum VSp requires not only a selection of criteria which is relevant to the area under consideration, but also a selection of speakers which represents those chosen criteria. Further, that though this may be a logical circularity, it is certainly not a methodological circularity [N].

[N] This asymmetry is largely exposed by the undoubted, but not well understood, complexity of hypermultivariate distributions (Kendall 1957).

Bluntly, one may say that little or nothing is known about sampling design for linguistic variation, either for specific studies, notwithstanding Labov's (1966) remarks, or, even less, in terms of

some distributional model, i.e. design generator (Rapoport 1963).

Both because one wants to estimate population parameters from the VSp (e.g. the representation of a given VC in the population), and because one must try to fulfil condition (a(ii)) for an optimal VSp (see above), it is necessary to do some comparative analysis of the effectiveness of sampling procedures. Here I shall only discuss matters of the most immediate statistico-linguistic interest, and make passing reference to sample survey design technology (Moser 1958, Yates 1960, Cochran 1968).

At various points I have emphasised the importance of distributional notions to a modelling of linguistic variation. One of the most thorny problems in this respect is our <u>a prioristic</u> knowledge that certain varieties of British English are both socially prestigious and extremely thinly distributed, namely those I have labelled non-localised (NL). (Recall my discussion of the incomplete Gaussian distribution above.) [N]

[N] In the application of the model here considered – i.e. to Tyneside – this raises statistical problems, since the (statistical) population is variably 'finite'; more so for localised varieties than for non-localised ones.

There are two solutions to this problem of the rarity of members of certain types of VC. The first is to supplement a random sample of informants with a handpicked ('judgemental') sample of informants known a priori to speak the required types of variety. The second is to estimate the number of new VCs which would be revealed in a larger sampling of the same base population (Good 1953). The first method would enable one to construct an adequate classification which was nevertheless not statistically representative. The second method would not contribute anything to classificatory adequacy, but would give useful estimates of population parameters.

Good (1953) recognises that where a truly random sample **n**, is drawn from a population, **N**, then **r/n** (**r** = number of times a species (VC) is represented in **n**) is not a good measure of **p**, the population frequency of the species (VC), if **r** is small. By his method one can both estimate **p** more closely, and the number of new species (VCs) which another sample would reveal[N].

[N] One's decision about the finiteness of the population would determine the estimator one used. Goodman (1949) estimates a similar factor, but for denumerably finite populations and under restrictions on sample smallness. Good's (1953) appeal lies precisely in his concern with populations about whose underlying

characteristics he makes no assumptions.

One could thereafter <u>test</u> Good's estimator by drawing a further sample from the same base population. This, of course, would be extremely laboursome. The point I am making is that since we have no idea about the mathematical model which is best adapted to <u>represent</u> linguistic variation, we consequently have no idea how best to <u>sample</u> for linguistic variation. Therefore comparative work on sampling methods must be a necessary component of the implementation of the model.

[N] It may eventually be more useful to propose an absolute sample size rather than a percentage of the base population (Moser 1958), in order for informants to be representative of their speech community. The tentative assumption underlying this would be that the larger an urban community is, generally, the more members its linguistic groups will have, and **not** that it will have <u>more</u> linguistic groups. However, there seems to be some general statistical evidence on group-size which might undermine this assumption. It may turn out to be a problem of what is the required 'level-of-analysis'.

Classification

Taximetric techniques are central to the functioning of the VSp. They are discussed and evaluated in detail in Appx. D. See also Appx.

A. Section 7, and Appx. E, p. 558 ff. Suffice it to say that each technique imposes a different definition on the notion cluster and hence elicits different mathematical properties from the data. Since any of the techniques available seeks to resolve or picture structural patterns in complex material, it is reasonable to expect 'reduction' or distortion of the data. The trick, of course, is to find an optimum distortion [N].

[N] In many cases there will be no single optimum distortion, not only because it is difficult to know what might be optimum, but because a whole series of apparently equally attractive distortions may be found (Lance & Williams 1966, 1967).

The general principle may be illustrated by reference to one technique - the median weighted pair-group method (Sokal and Sneath 1963).

Let us suppose that 26 people have been interviewed, a, b, c ... z, and that the interview, where necessary or feasible, elicited the speakers' variant realisations for 20 criteria, 1, 2, 3 ... 20. The linguistic analysis codes each informant upon the 20 criteria in the same order. Thus,

$^{a}1, 2, 3, \ldots 20;\ ^{b}1, 2, 3, \ldots 20;\ ^{c}\ldots;\ \ldots$

is the input tape. The program takes this input tape and makes

comparisons on all criteria for all pairs of speakers

a-b, a-c, a-d, . . . a-z; b-c, b-d, . . . b-z; . . . ,

and for each pair defines an overall measure of similarity

$$sim_{a,b} = \frac{1}{\frac{20}{\sum_{1} (a=b)} + \frac{20}{\sum_{1} (a\neq b)}}$$

where summation is over those criteria which are not

non-comparable. (Non-comparable criteria are those for which a value is missing, for some reason, from one speaker of the pair). These similarity coefficients are put in a matrix bounded on both sides by the informants (in the same order).

Next the program builds clusters (VCs) of highly similar speakers beginning with the pair (of individuals or groups) with the highest mutual similarity in the whole matrix of similarity

coefficients. The pair so chosen is then treated as a new individual and their joint columns and rows in the matrix replaced by their median (Gower 1967). The similarities of the rest of the individuals are then recalculated in terms of this new value and the new matrix is scanned again for the highest mutual similarity.

Only two members (individuals or groups) are permitted to fuse on any given cycle. As the clusters grow so the matrix shrinks. The program prints each group in the order in which individuals join together with the similarity levels at which they join; the group (VC) mean similarity is also provided.

In the outcomes of many taxometric techniques, there are outliers. That is, there may be $_{a}$Vs which are not at all <u>closely</u> related to any VCs. It <u>might</u> not be entirely foolish to expect that such discontinuities would be removed by the use of a larger sample. (That is, that we were once again confronted by the $_{a}$V rarity problem.) Notice however, that the notion of 'variety' (whether $_{h}$V or $_{a}$V) is not discomposed by the likelihood of our finding continuities along parts or wholes of a dimension (Pellowe et al. 1972, Pellowe

1973). What distinguishes one group of $_{8}$Vs from another, or one VC from another, is the differentials amongst the sums of the different sub-parts of dimensional continuities; **not** the necessity of discreteness from all other groups of varieties on some stated number of dimensions.

It will be clear by now from various remarks both about the nature of complex dependencies between the kinds of dimensions in the VSp, and about how the VSp must be thought of as working, that different dimensions are not <u>expected</u> to group people in the same way. Our disproportionate and inappropriate pleasure in seeing that different dimensions (criteria) occasionally <u>do</u> group people in the same way is an indication of how our capacity to <u>think about</u> pattern falls far short of our ability to operate with (behave in terms of) extensively complex pattern (e.g.'family resemblance' - cf. Appx. D). That is tantamount to saying that left-hemispheric conscious analysis [sc. for right-handers] can **never** match right-hemispheric non-conscious 'analysis', or pattern 'apprehension', or the creation of 'gestalts'. (<u>This</u> 'pleasure principle' is clearly the source, possibly even the motive, for many of what Bazell (1966) calls

correspondence fallacies. For further discussion see below Chapter 4.)

To obtain diagnostics the program discards criteria which do not vary across the sample since they are, by definition, incapable of diagnosing, and then, for any given VC_x, it scans all the member values of each criterion, k, in turn and gives the diagnostic value, Dk_x, of k in x as n_k/N_x, where n is the number of positive k variants and N is the VC membership (Beers and Lockhart 1962). [N]

[N] For quantitative criteria , as opposed to qualitative (see below Appx. A, Section 2) this would need to be transformed to

$$Dk_x = \frac{1}{N_x \cdot k_{max}} \sum^{N/2(N-1)} (ABS_k (i-j)), \qquad [N = i, j \ (i \neq j)]$$

which is to say, the sum of the absolute differences between all pairs of members over the group criterion maximum.

Diagnostic values near 1 or 0 (the maximum and minimum) are positive or 'negative' key diagnostics. I must emphasise here, that these are linguistic diagnostics. That is, in terms of the

Wittgensteinian kind of classification involved, these are summaries of each VC's clumpiness: the diagnostics are as close as we can come to necessary and sufficient (Aristotelian) characteristics of group membership.

The VSp/SSp fit

When the diagnostics (however defined) of clusters in one hypermultivariate distribution are used to attempt to specify the probability of their OTU's being in a certain cluster of a different, non-directionally related, hypermultivariate distribution, the problem of the relativity (or non-uniqueness or indeterminacy) of diagnostics is increased by at least a factor of two.

As I have indicated, the sampled speakers are plotted independently in two multidimensional spaces, the VSp and the SSp. Clusters in the variety space are VCs. Clusters in the social space are SCs. Diagnostics are extracted for each VC. [N]

[N] This would have to be performed at the simplest possible level, because the relativity of the constructs (power of changes of method over the 'clusteriness' of the data) cannot support the burden of statistical evaluation of the diagnostic power of characters

(even χ^2, for example (cf. discussion of Carvell & Svartvik (1969) in Appx. D, Section 4, below)).

The next step, however, the one which most closely relates back to what ordinary hearers are hypothesised as sometimes doing, is the problematic step. It involves specifying the SC to which a speaker belongs by means of the diagnostics of the **VC** to which he belongs.

If this is impossible then it means:

either,

(a) that the model of what the hearer does is completely erroneous;

in other words,

either (1) linguistic variation is distributed homogeneously both with respect to the population of possible variants and with respect to the population of speakers,

or (2) social variation is distributed homogeneously both with respect to the population of possible variants and with respect to the population of informants,

(both (1) and (2) imply that the data are not clumped, or are unclusterable, and that the degree of variation is 'even' in each structural item of every system),

or (3) there is not even partial systematic predictivity from one set (or subset) of clusters to the other set (or subset) of clusters: this implies that *no* non-linguistic information, however crude, can be derived purely from characteristics of the linguistic signal;

or

(b) that the general goals of the TLS outline an essentially non-solvable problem.

Since, unsurprisingly, I do not wish to admit either of these possibilities at the outset, I go on to consider the difficulties raised by the constructed example of Figure 3(2). We may summarise the notation by saying that a number of variety-clusters are obtained, $_1$VC, $_2$VC, . . . containing a specified number of highly

similar speakers, ${}_1VC^7$, ${}_2VC^{17}$,... for each of which we derive diagnostics (D), ${}_1D$ for ${}_1VC$, representing specified criteria, ${}_1D_{150}$, for criterion 150, and having a particular value within a cluster, ${}_1D_{150}(0.73)$. D values range from 0 to 1 (in theory). Values close to either pole indicate a key diagnostic. [N]

[N] A value of 1.0 would indicate that that criterion in that cluster is a (sample-based) necessary one, but of course it may not be a (population-based) sufficient one.

We also obtain clusters of informants who are similar in terms of social attributes (${}_1SC$, ${}_2SC$,...) each having a specified number of members, ${}_1SC^{12}$, ${}_2SC^{16}$,....

Consider this situation (see Fig. 3(2)). We have a social cluster, ${}_1SC^{12}$, consisting of twelve informants, $i_{1,2,3,....,12,}$ and we ask what is the social specificity of D_{150} with respect to this SC. Only when there is a complete match between the constituents of a VC and the constituents of the SC in question, can the answer be a simple one. (See Figure 3(2),for the visual equivalent of what

follows.)

That is, if ${}_3SC^{10} = i_{1,2,3,...,10,}$ and ${}_4VC^{10} = i_{1,2,3,...10,}$, then the social specificity of ${}_4D_{150}$ to ${}_3SC^{10}$ is simply its value, ${}_4D_{150}(0.80)$. Diagnostic values, it will be recalled, are cluster based, not sample based. An inherently more likely situation, when one considers the relativity which is imposed upon the two multidimensional spaces by the particular coefficients and sorting methods which are used, is that any given SC will be dispersed across more than one VC, and, conversely that any given VC will be represented in more than one SC.

Thus ${}_1SC^{12}$ might be dispersed as follows:

$i_{1,2,3,4,5}$ in ${}_1VC^{7}$,

$i_{6,7,8}$ in ${}_2VC^{17}$,

$i_{9,10,11,12}$ in ${}_3VC^{4}$.

(Read 'Five of the informants in a social cluster of twelve members also appear in a linguistic cluster which has seven members' etc.)

The social specificity of a linguistic diagnostic (${}_1D_{150}$ to ${}_1SC^{n}$)

must therefore be some function of the average diagnostic value for all the VCs represented in the SC. The problem, of course, is to define this function. Three elements which might usefully be incorporated in the function spring to mind:

(i) the number of VCs which are represented in the SC (in this case 3),

(ii) the fractions of the SC represented in these VCs (in this case 5/12, 3/12, 4/12),

(iii) the fractions of each relevant VC which this SC incorporates (in this case 5/7, 3/17, 4/4).

There are, however, other, less determinate elements which must play a part. For instance, if we had $_1D_{150}(0.75)$ (in the above example), and $_2D_{150}(0.75)$, even though these ($_1VC$, $_2VC$) fractions cover 2/3 of $_1SC$, nevertheless the fact that $_3VC$ is completely represented in $_1SC$ involves us in the consideration that the value which <u>it</u> contributes (in the form of $_3D_{150}(x)$) ought to be positively weighted, but in a way which is not at all clear. This problem is, apparently, a novel and interesting one in taximetry (Pellowe 1973,

Sneath 1972). However, from what I have been able to discover (Bobisud & Bobisud 1972, Gower 1973, Malone 1975, n.d.), there is, in the foreseeable future, unlikely to be a theoretical (mathematical) solution to this problem, let alone an algorithmic one.

Naturalness & Hearer Judgement Tests (HJTs)

I have referred, at various points, to different kinds of adequacy - of criteria, of classification, of informants, of mapping functions, of the analytic objectivity of linguists, of the model itself. Evaluation procedures fall into two broad classes: internal and external.

Internal evaluation depends upon the contrastive comparison, at various levels, of the different VSps we can generate by variations of input speakers, variations of criteria, variations of the definition of similarity, variations of the technique for forming groups, variations of the method of obtaining diagnostic features.

External evaluation would depend on the nature of the responses made in hearer judgement tests of various kinds. Clearly

these two may be fused by designing the judgement tests to match the particular output of any generated VSp (cf. Fig. 3(1)). We reiterate that such a general, controlled generation of constructs is necessary because

(a) we have no idea of the general mathematical properties of linguistic variation (distribution, continuity etc.);

(b) we do not know at what level the intersection between the two multivariate distributions (linguistic, x-linguistic) is most significant (in either a causal or a resonance sense).

A proposal for hearer judgement tests (HJTs) constitutes an attempt to reconnect the abstraction of the products of the VSp and the SSp to the behaviour of actual hearers, even in the face of our certainty that $_{a}$Vs and $_{h}$Vs cannot ever be persuaded to converge.

What, then, could HJTs show? In what follows I merely present an extremely programmatic sketch of the possible content and interpretation of such tests.

Even in the face of test situations and test materials of different kinds, one would expect different real Hs to judge the same speaker differently, either because of their different norms of

linguistic comparison, or because of their different norms of world view, or (most likely) because of a combination of these two. Some of the kinds of question one might be looking for answers to are:

(1) Is the speaker a Tynesider ?

(2) Is the speaker the same kind of speaker as the hearer believes himself to be ?

(3) Are these two speakers the same kind of speaker as each other ?

(4) What <u>kinds</u> of x-linguistic information can hearers derive ?

(5) How much linguistic material is required to derive each <u>type</u> of x-linguistic information ?

(6) How specific can hearers be in attributing a piece of information to particular, or types of, linguistic variants ?

(7) How certain are hearers of their judgements concerning the x-linguistic information inhering in some stretch of speech ?

(8) What promotes a hearer to revise his judgement about (some characteristic of) the speaker ?

Of course there is an infinity of such questions, and it is important to stress that whatever answers were obtained to these questions,

regardless of the type of material to which they referred, they would not necessarily be easy to interpret, nor to group together in coherent ways.

Broadly speaking I envisage two types of situation in which material is presented to hearers:

(i) a situation in which the hearer is in mechanical control (by means of a pause pedal or some such device) of a lengthy tape of a speaker (or a dialogue), and is encouraged to stop the tape at any point and discuss his or her inferences about information concerning the speaker (or speakers). In this kind of task one would be concerned, amongst other things, to determine the extent to which hearers felt the need for new metalanguage;

(ii) a situation in which the tester is in control of the material which might take the form of very short extracts (one word or phrase), or of mutilated (reversed or inverted tape) extracts from 'real' material. In these situations the hearer might be expected to respond on some <u>scale</u> of judgemental possibilities decided in advance by the tester.

In either case, the idea that there could be anything like a

"degree of accuracy of hearer response" is rendered implausible by the following considerations.

(1) The H's judgement about the speaker may be complicated by the H's inference of his own social and/or linguistic proximity to the S.

(2) It is likely that the social scales upon which the H judges the S are not independent, and that there is a one-many relationship between social and linguistic variants.

(3) It is possible that there will be a correlation between the H's own social profile and those parameters which he uses to judge other Ss.

(4) It is impossible to isolate one of the cooccurrent (sets of) features (semantic, syntactic, prosodic, segmental phonological) which may be promoting some judgement.

In spite of all this, it is necessary to try to see how far one can get - even if the metalinguistic education of the hearer has to be undertaken - in establishing the extent to which hearers may or may not be able to operate with notions (or surrogates of them) such as 'linguistic profile', '$_{a}$V', 'VC', 'key diagnostic', 'diagnostic', 'positive/negative diagnostic', 'in- out- group', 'diasystem',

'linguistic subsystem', 'linguistic variant', '$_{h}$V distance', '$_{h}$V direction', and so on, and so on ....

Problems of modelling

I am aware of several general problems in empirical modelling processes such as this; I shall briefly discuss five of them here.

1.The encapsulation problem

This is a serious methodological problem which has major consequences for the interpretation of results. It is often caused by the particular model which is adopted. I shall give one example from the TLS and one from the Labovian model (Labov, 1966).

From a statistical point of view the problem may be thought of as arising from the fact that one is dealing with at least a part of two intersecting hypermultivariate distributions, and that there is a consequent attributional problem. For example, in the TLS, it might reasonably be asked what would happen if (instead of dispersing informants independently in the two different spaces (VSp & SSp), and then looking for a mapping function to relate the clusters in one space to those in the other) we established just one space whose

dimensionality involved both linguistic and x-linguistic characters?

If (for the two space model) the VSp projects discrete clusters and the SSp projects compositionally <u>different</u> discrete clusters (which we know is the case), then a 'simultaneous' social/variety space (SVSp) would presumably represent the individuals as weakly dissociated groups showing extensive 'chaining' or straggliness. In interpretive terms, the single space (SVSp) model would be unduly restrictive since it would always tend to uniquely associate a particular pattern of linguistic variants with a particular pattern of social attributes (and conversely). It would constitute another of Bazell's (1966) correspondence fallacies (see also below, Chapter 4).

In other words the encapsulation problem can be seen, in many cases, as the ways in which a certain subset of the variables is pre-empted as to its role by the model. The Labovian case is different but exhibits some of the same general properties. The occurrence of casual style for Labov is signalled when at least one criterion from each of two lists (channel cues and situational cues) is conjointly satisfied by a given passage of speech. The channel

cues are: increase in tempo, increase in breath rate, laugh, overall change in pitch range, overall change in mean pitch (usually upwards). It so happens that these features could be quite legitimately considered as linguistic criteria in the sense of this model. If they were so considered, then clearly the discrimination of styles would be confounded.

2. The relativity problem

Replication, one of the cornerstones of experimental psychology, does not play much of a role in the linguist's (or the sociologist's) life. Sociolinguists, who are centrally concerned with variability, should be more willing to examine the extent to which they are unable to remove themselves from their own linguistic experience as speakers and hearers, or from beliefs about, and precepts for, that experience, or from their folk-linguistic views however subtle. But it is possible to build this kind of relativity amongst analysts into the constructs, by classification, of the informants (see below, Appx. A, Section 6; Appx. B, Section F). The general problem must be true for pragmatic, semantic, lexical, syntactic and prosodic

criteria as well as for phonological ones (Ladefoged 1960, Pellowe et al. 1972b).

In the model sketched above, these differences are conceived of as contributing topological deformations to the relevant subspaces of the variety space. It may or may not be reasonable to assume that the sum of these topological distortions gives a regular space (given that there were a wide enough range of analyst differences); but the only way of testing this will be to produce different classifications, partialling out a different analyst's contribution on each occasion.

3. The levels problem

One of the most crucial issues in sociolinguistics in terms of the results obtained and the conclusions drawn on <u>any</u> set of data which includes linguistic and extra-linguistic variables, is levels of abstraction which are set as relevant for the variables concerned. This matter has received <u>very</u> little attention. Garvey and Dickstein (1972), in an important contribution, show how in terms of sex, s-e status, and race, the correlational behaviour of a complex

possessive construction varied widely in terms of the level at which it was analysed. Pellowe et al. (1972), in a critique of part of Labov's model, claim that the reverse is also true, that is, for a given level of analysis of a linguistic variable, changes in the level of analysis of the social or other extra-linguistic variables will also radically alter the correlational pattern. Hearers are capable of operating at different 'levels' in the above sense. This problem merits a great deal of work.

4. The reticulation problem

Independent of the 'levels problem', though interacting with it importantly, is the question as to how much structure, or, in another dimension, how much detail, variables should be coded with. This, like all the other problems I have outlined, is <u>partly</u> a matter of purpose, but often also, it is a matter of invention (the structure of the variable <u>as a variable</u> has not been worked out before). For example, the detailed structure of the localised Tyneside ($_{L}$T) pronoun system (see Appx. C), of $_{L}$T double modals (see Appx. C), of

$_{L}$T prosodic systems (see Appx. B), or of $_{L}$T reduced vowels (Appx. A) were simply not known until we wanted them as dimensions of the VSp (Pellowe et al.1972a).

The problem is analogous to the process which Halliday called delicacy, but it is complicated by the fact that the features are being focussed upon as variables (for correlation, indexing, clustering, etc.), and hence their number and structure will directly affect central statistical parameters (variance, kurtosis etc.).

5. The retrievability problem

All coding systems throw information away. There is probably a rather small number of ways in which this can be done optimally, and they are almost certainly all expensive. Having one's data in a form which will satisfy all demands upon it in the future, is not possible, but there are generally established principles about data structure in both sociology and linguistics which ought to guide one to a certainty about what needs aiming for. The TLS has often wanted to be able to afford to have a linear, nested coding system. This would enable one, for instance, to retrieve computationally,

rather than by hand, the co-occurrence of any character (say epenthesis) with any other(s) (say pitch range features and nasalisation) at any given structural level (say syllable). Such a form of coding, though expensive in terms of storage and retrieval, would be immensely useful.

ALL MISSING PAGES ARE BLANK

IN

ORIGINAL

Chapter 4

Consequences and comparisons

"There is a common quality among all beings and things . . . [which] is *impermanence*, 'anicca', a constant decaying and changing which is uncontrollable" (An unnamed Buddhist scholar in Wat Sraket, Bangkok, quoted by Hamilton-Merritt (1979: 55)).

"The order that our mind imagines is . . . like a ladder, built to attain something. But afterward you must throw the ladder away, because you discover that even if it was useful, it was meaningless . . . The only truths that are useful are instruments to be thrown away" (Eco1984: 492).

"All 'explanations' in language are circular and leave the most essential things unexplained and undefined . . . Where do I begin and end in space? I have relations to the sun and air which are just as vital parts of my existence as my heart" (Watts 1987: 45-6).

Abstract.
This chapter gives a summary account of the methods and theoretical positions which I have made use of to handle linguistic variation and indicates how they relate to the methods and theories of others. There will be no attempt at closure or finality; rather, it will appear that a framework within which linguistic variability could be properly considered has barely begun to be imagined.

Largely from the point of view of the ecologist-hearer specified at the end of Chapter /, I shall deal here with the following matters: /2, p.92ff.

(1) the economic and epistemological utility of 'results' in investigations of linguistic variability;

(2) classification as a hearer process and as a research method in the interpretation of linguistic variability;

(3) the reasoning behind including and excluding linguistic criteria and informants in surveys of linguistic variability;

(4) variation in prosodic systems/provider/the prime and grimmest test of both sociolinguistic method and theory; (as the, of)

(5) the inhomogeneity of observer/analyst effects;

(6) the psycho-social necessity of the notion of whole 'speech

variety';

(7) the utility of the ecological frame as a source of indeterminacy;

(8) the problem of theorylessness in studies of linguistic variability.

First however, it will be useful for us to have beside us a powerful but much neglected idea of Bazell's (1949, 1966). It is useful because in warning us against correspondence fallacies Bazell is actually adumbrating the bases for a quantum linguistics.

The reason why I do not believe that most efforts in variationist linguistics and in sociolinguistics are directed at the problem of limitless variability in languages (Ch. 1) is precisely that they fail to handle linguistic variability <u>in its own terms</u>. And this failure operates at the ontological, the theoretical, the methodological and the interpretive levels. To <u>some</u> extent the problem may be caused by terminological blinding. (Some such chain of association as: linguistic variability is the sum of

linguistic variations, and these variations must be variations of something invariant; the sum of these invariants is a series of systems, so linguistic variability arises from, depends on, is subordinated by, linguistic systems.)

Belief in such a chain would constitute an illustration of the ontological failure. The theoretical failure is illustrated by all those cases in which linguistic variability is to be handled by an expansion and modification of a theory of linguistic systematicity (as, for instance, those which extend and weaken Chomsky's 'ideal speaker-hearer in a completely homogeneous speech community' (Bierwisch 1977, Hymes 1972, Labov 1966)). Methodological failure follows from the theoretical, for instance in the shape of the variable rule. And interpretive failure is to be found wherever a determinate meaning, in terms of speaker variables or group variables, is proposed for some specified linguistic variant.

However, a far more significant source than terminology for the failure to account for linguistic variability in its own terms is the failure of linguists to consider, let alone to try to work with,

the notion that individual persons both are all <u>different</u> and are all <u>creative</u>. Saying that sentence

'Individual persons are all different and are all creative'

enough times to begin to be able to create a meaning for it, and to begin to be able to reflect upon it, is a first step in coming to know what treating variability in its own terms might feel like.

A quantum linguistics, then, would be constituted on the mutually excluding complementarity of both

(a) a flux of overlapping continuities, and

(b) a system of categorial oppositions;

whether in respect of languages or of linguistic expressions.

The flux (variability) is necessary for the creation of her/his individuality by each individual. The system is necessary so that hearers can economise in their seeming avoidance of a strict solipsism. Of course, both variability and system are endlessly <u>re</u>created by successive individuals. Note that, according to this argument, the view of a language/linguistic expression as flux is the 'unmarked', or 'prior', state - the needs of the individual to

construct his individuality for himself necessarily precede his need or willingness to be an efficient creative hearer for others. (I return to this, only tangentially, in (8) below.)

Bazell's idea (1949, 1966) is simply that the postulate of solidarity between linguistic systems remains to be proved, and that, more generally, any <u>assumption</u> of correspondence (between any phenomena, states, categories, systems, etc.) will almost always be fallacious, or engender fallacies. [N]

[N] His initial sketch of the problem was in reply to Question 1, precirculated to participants in the 1949 International Congress of Linguists in Paris (the VIth), which asked

> "Existe-t-il des catégories qui soient communes à l'universitalité des langues humaines? Dans quelle mesure peut-on asseoir sur l'étude des catégories une classification structurelle des langues? Quelles corrections doit apporter en cette matière une étude diachronique aux conclusions de l'étude synchronique?" (Bazell 1949: 115).

Bazell pointed out (1949: 115) that the question presupposes an image of linguistic categories "as 'niches' capable of being filled or remaining vacant in one or another language [or variety JP]", whereas the potential types do not form discrete slots, but "overlap so that it is seldom or never feasible to identify a category in one language [or variety JP] with that in another". He goes on to say that <u>any given definition</u> of a category "will risk associating dissimilar facts and separating related facts". Any

particular opposition will cut across equally important instances of the general opposition between dependent and independent which is one which allows "infinite gradations, and each language [or variety JP] chooses different boundaries (with different boundary-signals) along the line of the same opposition" (1949: 115). Bazell goes on to say that such common linear oppositions enable one to compare, though not to identify categories in different languages [or varieties], and concludes that "structural classification must start from small systems and not from languages as a whole, since the postulate of solidarity, so far as it is not tautologous, remains to be proved" (1949: 116).

In his later paper, Bazell (1966) examines a large number of examples of the correspondence fallacy occurring in different treatments of various topics in structural linguistics. What he calls <u>the</u> correspondence fallacy is in fact

> "a whole family of fallacies which, though they may shade imperceptibly one into the other, often show little similarity in their most extreme form" (1966: 271).

The prototype for all the fallacies is the assumption that two distinct sets of criteria will, in the face of given data, necessarily lead to isomorphous analyses. In one of its more sophisticated forms, Bazell says, the fallacy occurs when, though it is recognised that there is no one-one relation between two units, it is <u>not</u> recognised that there is no necessary relation <u>at all</u>.

> "When taken as working principles rather than as postulates, such assumptions are still fallacious. But it is of course no fallacy to assume (at least as a working principle) that two units coincide <u>more often than not</u>. Nor is it a fallacy to assume that it is possible to devise, <u>a posteriori</u>, two sets of criteria which will lead to very similar results .... The fallacy lies in assuming a one-one relation between the results of criteria which have not been selected <u>with this end in view</u>" [emphasis added] (1966: 271).

The complementary fallacy is clearly the one which supposes

that since different sets of criteria lead to different results these different sets of criteria must necessarily be handled separately.

But, Bazell says (1966: 272), there are less sophisticated and more prototypical fallacies. One such supposes that some single, necessary and sufficient criterion will lead not to a unit fixed by some other criterion, but rather to some unit already recognised by field workers, tradition or the 'linguistic conscience'.

> "The complementary fallacy is sheer scepticism: the unit cannot be defined at all (Daniel Jones for the phoneme, A.S.C. Ross for the morpheme)" (1966: 272).

He argues that there will never be an end to the pseudo-discoveries in linguistics until it is realised that "the frontiers of a category are arbitrary until the specific purposes of the delimitation are known" (1966: 274); such a principle is often recognised, but it will frequently lead to erroneous conclusions of the type which says

> "since there is no one bounded category, serving all purposes, it is assumed that there must be several differently bounded categories, each necessarily deserving a name of its own" (1966:274).

The dissolution of these, as of other, correspondence fallacies must depend upon a recognition of marginality and of centrality, upon an understanding of the limits of applicability of criteria, upon an ability to see and admit overlap and gap, and upon a willingness to entertain the possibility of the unboundedness of units and categories. In short, we must be willing to operate **especially** in respect of our defined units and categories with a certain amount of slippage and uncertainty.

(And compare Popper (1966: 19) who is confident that this doesn't matter provided we "keep within the penumbra of vagueness"; and Lakatos (1976:100-102) "you never get out of falsehood . . . you never get out of vagueness . . . If you want [your subject] to be meaningful, you must resign of certainty. If you want certainty, get rid of meaning. You cannot have both.")

Clearly the whole family of correspondence fallacies is ubiquitous. Their occurrence is not restricted to treatments of phonetics, phonology, morphology and syntax in linguistics, any more than it is to linguistics rather than history, sociology, mathematics and biology.

We may conclude by quoting at length from Bazell (1977b: 1-2):

> "I can see no reason why (a priori) intonational sub-varieties should answer e.g. to segmental consonantal sub-varieties on Tyneside or anywhere else: to assume so would be a good instance of my correspondence fallacy, but of a kind not available to me when I wrote, since of course then Labov had not yet been heard of.
> It would of course be reasonable to expect a fairly good correlation between <u>some</u> varieties: it would be just as surprising to find it in <u>none</u> as it would to find it in <u>all</u>.
> Intonational and segmental phonology are not likely candidates for a very close relation, in the absence of a still closer relation between consonantal and vocalic segmental phonology."

(1) The utility of 'results' concerning variability

If one of the (mutually exclusive) ways of 'seeing' linguistic phenomena is as a limitless flux, then a new meaning will have to be attached to the word 'results' arising from any serious investigation of any part of that flux, since it will naturally permit the generation of many possible sets of results. As I shall suggest in the next section, there is every likelihood that there can be **no** external criterion for judging the best, or the most appropriate, or the most revealing set of results from all the possible sets of results. Because of that problem, there can be no additivity from one study to another. To assume that some chosen set of results from any such study could have useful implications for another such study is an example of the correspondence fallacy.

Thus, discussing urban dialects in the British Isles, Lesley Milroy (1984: 207) in a critical account of the Tyneside Linguistic Survey suggests that "an imbalance of interest in favour of methodology and theory has led to a relative weakness of results".

But such results as are available are recognised as "stimulating and

innovatory". (I presume that we are meant to unravel, and finally reject, the causative implicature in Milroy's first remark; we are meant to interpret 'weakness' as something quantitative - 'there aren't enough results', and reject 'jejune', 'insipid', on the grounds of the second remark.)

Similarly, the Final Report on Project 5490/1 to the Social Science Research Council was submitted in 1980 (and cf. Pellowe & Jones 1979b) and accepted by the SSRC. Several months later, however, a letter was received asking both for more results, and for simpler results, as if the quantity and simplicity of results were freely manipulable variants reflected by and in a person's report-writing style, rather than being something to do with the data gathered, the skills of its analysts, and the methods used to handle those data.

What these brief examples show is that professional people, especially academic ones, somehow manage to believe first that 'progress' in respect of some problem or topic is a possibility, and secondly that it is brought about by the piling up of 'results'. The

creative view of mind (Sampson 1980) casts grave doubt on the tenability of both of these beliefs.

Furthermore, in the model I am here trying to develop and test, the economic value to the ecologist-hearer of any given real-time processing of variability is evanescent. Its epistemic value may be a good deal longer lived, to be sure, but only after further processing and abbreviating - to give for example further detail to his $_{h}$VSp, or to add further conditions to the mapping functions between his $_{h}$VSp and his $_{h}$SSp, and so on.

(2) Classificatory methods in the handling of variability

We need to distinguish here between discussion of, on the one hand, processes which are hypothesised or believed to be those used by real individuals in their daily lives for the purpose of processing manifestations of their worlds and creating meanings for them; and,on the other hand, methods which are used in research which attempts to model those behaviours.

I shall deal with these in turn.

There is a vast amount of evidence in the psychological literature that classificatory processes of one kind or another ubiquitously underlie many, if not all, different types of human behaviour and human understanding (Bateson 1979,1985, Cattell 1966, Gregory 1970, McQuitty 1967c, 1968b, Minkoff 1965, Norman 1970, Popper and Eccles 1977). Johnson (1970) opines that, at least in euro-centric cultures (but see also Berlin et al. 1974, and Hunn1977), classification amounts to an obsession. Attempts to provide computing machinery with the same skills have used these same techniques (Bobrow & Collins 1975).

A particularly pleasing example is provided by Fillenbaum & Rapoport (1971). One of their methods of collecting information about the ways in which the lexicon varies from individual to individual is to get informants to group (by connecting) words according to their similarity. The method of grouping/connecting is open to the informant. My own replication efforts with various groups of students, with whom I was able to discuss their techniques, produced many of the different kinds of classificatory process discussed in Appx. D.

As to the processing of linguistic variability, LePage & Tabouret-Keller (1985: 153) feel that "the clustering process itself provided a reasonable analogue for what we supposed to be happening in real life. [And though] this view has since been challenged . . [it] remains our view". It is my view too. (See also (6) below.)

In spite of this view, McEntegart & LePage (1982) have doubts about the final usefulness of classificatory methods in research into linguistic variability. They say that even though "cluster

analysis may be useful, it should be viewed as an exploratory method" (1982: 121). Furthermore,

"even with the best possible data there are severe limitations on the usefulness of cluster analysis in sociolinguistics. Whatever linguistic variables are selected, if variants are socially marked clustering is bound to pay most attention to those factors which are most strongly marked" (1982: 123-4).

This negative generalisation seems to me to be a serious error.

The review and reanalysis of their data, and of its treatment by their clustering program, says something about the interaction between McEntegart & LePage's data and its primary (linguistic) and secondary (classificatory) analyses. Nothing in their remarks shows <u>any</u> connection with other classificatory treatments of other linguistic data collected and analysed in different manners.

The only principled basis there could be for such a generalisation would be the existence of **precisely** that which we lack in order to make an informed choice of classificatory method in the first place, namely an understanding of the distributional regularities underlying linguistic variability and its social representation.

For example, I do not know, nor does anyone else know, what the degree of continuity of each the linguistic criteria specified in Appx. A **is** in the Gateshead-Newcastle speaker population. As far as I can <u>imagine</u> any combination of types and degrees of continuity in the criteria across the population would be <u>possible</u>. Nevertheless, the point is that the classificatory processes **work**. <u>And they work in a way which fits them for their heuristic role.</u>

Appx. A, Section 7 and Appx. E, p. 558A ff. give, respectively, classifications of 52 informants (45 from Gateshead and 7 from Newcastle) in their VSp (for segmental criteria) and in their SSp (Jones 1978 (=1983)). It is quite clear that though the patterns are complex, they are interpretable. The classificatory methods which were used have properties (of sensitivity and robustness) which are fairly well understood (Wishart 1969, Sneath & Sokal 1973, Everitt 1974, Jardine & Sibson 1971a). The <u>same</u> methods were applied to a second sample - sample 2 - not reported in this thesis.

Sample 2 (Pellowe & Jones 1979b) comprises sample 1 from which were subtracted the 7 Newcastle informants (i.e. those

constituting K1 = KA = K_Y (see Appx. A, Section 7)), and to which a further 12 Gateshead informants were added. Sample 2 thus consists of 57 informants, all of whom were drawn by the same sampling process - a stratified random sample. The speech of all of these informants was analysed by the same analyst (i.e. analyst C of the calibration experiment (Appx. A, Section 6)).

[Computer programs (written by Val Jones - Pellowe & Jones 1979b) checked the raw data files of the 12 new cases to see if zero-variance variables eliminated in the case of sample 1 were now present. Ten such Sts were found. (Their codes in the Appx. A catalogue being : 00043, 00103, 00264, 00304, 00602, 00722, 00825.) Data files for the 'old' 45 informants then had to be reprocessed to re-incorporate these variables. The reverse problem also occurred, namely, that with the removal of the Newcastle 7, variables which had <u>not</u> been zero-variance variables in sample 1 <u>became</u> zero-variance variables. There were 26 such Sts.]

Changing the case list (informants) <u>and</u> changing the variable list (criteria) clearly completely alter the basis for the

classifications which will be obtained, even though most of both the dimensions (criteria) and the most of the contents (informants) of the VSp are the same.

We know that small changes such as these for the methods involved can cause chaotic alterations in the resultant classification (Minkoff 1965, Sneath & Sokal 1973 (& Appx. D)), but in the present case this did not happen. Of course there are changes but equally significant there are preservations, stabilities.

In respect of the interaction between these data and their primary and secondary analyses, McEntegert & LePage's generalised criticism is simply wrong. Classificatory processes have shown for these data that they contain structure which is partially stable in the face of small changes in the data. This indicates that linguistic variability **is** amenable to treatment by multivariate taximetric processes. (It now seems to me possible that a serious comparative investigation of such taximetric processes in respect of a given data set of linguistic variation could begin to yield some information on the underlying mathematical properties of such

linguistic material.)

In addition, the kinds of changes of result which were brought about by the changes of data look promisingly like the sorts of changes one would expect to be true for our real ecologist-hearer for similar kinds of different samples.

Indeed, the range and complexity of speech samples which hearers interpret, often in differing ways, encourages me to believe that this kind of modelling is not too wide of the mark from a cerebral (Good 1972) as well as from a linguistic point of view.

(3) Including and excluding variables and informants

The very success of Labov's (1966) tool as the basis for practical surveying matters in sociolinguistics has stopped people from thinking deeply about how and why the tool works. Compare the effects of Schrödinger's wave equation on the development of quantum mechanics (Gribbin 1985).

Labov (1966, 1972) makes three undefended assumptions which are examples of what Bazell (1949, 1966, 1977b) has called correspondence fallacies. The assumptions are:

(a) that examining the variation in a small number of structural items (phonemes) constitutes a suitable data base for a large scale survey of linguistic variation, and that the findings accruing from the examination of one such small set of structural items will pattern after the findings of any other such small set of items;

(b) that from the sociological point of view, the primary nexus between the social and the linguistic, in social/linguistic variation, is socio-economic class, and that an examination of the nexus at any other levels of either or both the social and the linguistic would

produce results which were reductively predictible from those prior results (Labov 1972);

(c) that from the interpretive point of view it is reasonable to attribute to a whole speech community patterns of productive/ receptive linguistic behaviour which have only been observed in a small sample prestratified in terms of linguistic belongingness and in terms of socio-economic class.

This rationale for choice of variables, by being locally selective, and by treating the variables atomistically (i.e. as totally independent of each other), both limits the proper generalisability of the method and pre-empts the resultant stratification in terms of its stereotyping characteristics. There is no evidence to indicate that stereotypes are the limit of interest in variety data, nor that a stereotypical interpretation of variation of this type is all, or is even the most important thing, that is produced by the mind's ear of the hearer.

Put another way, I eschew the constraints of the three assumptions sketched above, believing that the model presented

here will not only generate Labovian variables analytically (for a sketch of this see Chapter 3), but will indicate the extent to which, and reasons why, other parts of the linguistic structure are candidates for this role.

Any investigation which requires an adequate typology of speech varieties must have a maximal selection of variables which are representative of the population which is to underlie the typology and must have a selection of informants which overcomes the problem of type rarity. Any investigation which wishes to make inferences from its sampled informants to the underlying population at large must involve itself with random samples. It is specious, in respect of this last requirement, to claim (Labov 1972) that linguistic variability is distributed in such a way as to make random sampling unnecessary - when in fact the very methods Labov uses are such as to make it impossible for him to know anything about how linguistic variability is distributed.

(4) Variant prosodic systems as the grimmest test

In Appx. B I have tried to establish - a thesis within a thesis - that prosodic realisations vary lectally as well as being the carriers of affective, syntactic, semantic, and metalinguistic signs. But working out <u>how</u> they do this is not so easy. I used two methods. The first method attempted to establish useable dimensions for use in the VSp by means of mapping the co-distributions of various prosodic features in the speech of sample-survey informants. The second method attempted to converge on solutions of the first method by the use of experimentally manufactured spoken model sentences and their imitations. Some parallelism was discernible, but no real convergence. A good deal of further working out is required (Pellowe (in preparation, a)).

Why do I claim that prosodic variation constitutes the grimmest test for sociolinguistic theory and method? Not simply because I find it interesting and difficult! It is the hardest test because

(a) the variants suffer from low recognisability, and

(b) prosodic variation is <u>both</u> close to the idiosyncratic level of the individual's sense of identity <u>and</u> close to the largest group level an individual might construe himself to be in.

The problem of low recognisability is rather complex and is dealt with at length in Appx. B.

But any linguistic variant which represents an individual simultaneously at levels close to the idiosyncratic and close to the biggest group he can feel himself to be a member of is going to cause trouble for any sociolinguistic theory which tries to relate individuals to groups through variants.

(5) The inhomogeneity of observer effects

It is surprising that sociolinguistics does not treat the products of its own linguistic analysis in the same way as it treats the spoken products of its informants; that is, as being subject to variation which is distributed according to various co-occurrent variables.

The hearer basis of the model developed here certainly predicts that a linguist, just like any other speaker, will have a place in the overall VSp and hence will hear speakers in different 'directions' and at different 'distances' from himself in different ways from linguists who are in other, different places in the VSp.

Apart from Ladefoged (1960), Ringaard (1965) and Pellowe et al. (1972b), and more recently Kerswill & Wright (1989) - all of which are concerned with various correlates of variations in phonetic and phonemic analysis - nothing has been done.

But surely sociolinguistics should <u>expect</u> analysts to analyse differently ? It would be surprising if they did not (Bazell 1966, 1977b).

(6) The psycho-social nature of 'whole speech-variety'

Here we are concerned to some extent with the complement of Section (3) above. I have always been at pains to point out that sociolinguistics has to deal with varieties as <u>wholes</u>. There are both theoretical and methodological reasons for this.

Theoretically we should deal with varieties as wholes simply because the communicative competence of the speaker is realised through her whole variety, and not just the parts of it which vary in some way which interests an atomistic researcher. But when she speaks, a given speaker in her utterance-turn will not necessarily use all the possible variant slots which exist in her variety, and she cannot, perhaps, know in advance which subset of all those possibilities <u>will</u> occur in her utterance.

Not only this, but the communicative competence of the hearer, his creativity, can only work on a whole variety. The hearer's skill in making the meanings he does for the variants he hears, is very possibly partly dependent upon the order in which the variants arrive to him, amongst a host of other things, but more importantly

his created meaning will depend upon the <u>total pattern of co-occurrences</u> which he hears. And clearly the hearer cannot predict in advance what subset of all the possible variant slots he will be regaled with in any piece of speaking.

These things mean that the notion of a 'whole variety' is somewhat abstract since it can never be 'fully represented' in any given utterance-turn, and therefore can never be 'fully interpreted' from any given utterance-turn.

Methodologically we should deal with varieties as wholes because otherwise we can say nothing significant about co-occurrence between variants. It is simple minded, but it seems necessary to point out that

(a) if a variant usually co-occurs with another variant, then part of what the first variant, taken alone, means, extralinguistically, is an unseen result of that co-occurrence. (Absence of co-occurrence will usually lead to a different created meaning.) And

(b) if we do not have information on the occurrence of **x** and on the occurrence of **y** then we can never say anything about the

co-occurrence of x and y.

(7) The ecological frame

The usefulness of the ecological frame is manifold. By strict analogy with the best in botanical ecology for instance (Williams 1959, 1960, Lambert 1961, 1962) it demands from us answers to the following questions. What species are present in the area under consideration? What is the relative commonness and rarity of each of those species? What a prioristic probabilities of association, if any, might we expect between those species on the grounds of botanical characteristics and why? What is the range and type of niches available in the area under consideration? And what is their commonness and rarity, and a prioristic probability of association? What is the probability of association between any given niche and every available species? What is the probability of association between any given species and every available niche? Can a model of associative probability be constructed which is remotely successful predictively?

Here we are trying to transfer this question-set to the data of linguistic variability distributed in the personal and public infinity of social space. Our problem in doing so arises, in no small part,

from knowing exctly what conceptual material should be substituted 1a

for <u>species</u> and what for <u>niche</u> in the above questions. (The need for whole varieties is certainly, perhaps, further clarified.)

In case it is thought to be a trivial problem, let's recall Watson's (1976: 30) opinion that "it is as difficult to find a satisfactory definition of an individual as it is of a species". In what has gone before I have sat on the fence - or rather see-sawed uncomfortably upon it - about whether the $_{a}$V or the VC level of analysis was the level of 'species'. (The reader, rocking in this sea of uncertainty, may be clinging to the notion that <u>species</u> is a well defined notion in biology. I entreat her/him not to, since it isn't.) Thus when I have discussed conditions for an optimal VSp I have leaned towards the VC as something like a 'species' level (then the $_{a}$Vs in any VC would be equivalent to individual different plants or animals of that given species). But when I have discussed mapping processes from VSp to SSp - trying to find a function which uses the properties of its VC to predict the SC membership of a VC <u>member</u> - I have inclined, but only slightly perhaps, to treating the

$_{8}V$ as the 'species' level.

The same sorts of question, but in my own feeling, even more intractable, arise in connection with niche. Problems of continuity here are, if it is possible, even worse. What is the equivalent of niche in social space? Is it something like 'group affiliation' or some recognisable identity, or what?

Of course I do not have ready-made solutions to these problems. But the questions are valuable, and I prize meaningful questions which are difficult.

(8) Theorylessness

Within the range of what are known to be occurring variants (in some set of varieties) is there any such thing as an impossible variety? What are the forces which promote or depress the co-occurrence of particular sets of variants in the speech of some informant? The kinds of studies initiated by Labov cannot answer questions of this type because such studies are not actually interested in the **functions** of variability. These questions require a **theory** which proposes a <u>reason</u> for variability.

LePage offers such a theory (1980). He proposes that each individual creates his/her systems of linguistic behaviour so as to define him/herself both idiosyncratically and in relation to others. The linguistic systems thus created will resemble the linguistic systems of the group or groups with which at various times he/she wants to be identified.

Each individual can do this only providing that:

(i) he/she is able to identify/locate/specify the groups,

(ii) he/she has sufficient access to the group(s) and can analyse

their behaviour,

(iii) he/she has sufficient motivation,

(iv) the group(s)/society provides feedback indicating the chances of success,

(v) he/she can still modify his/her linguistic behaviour.

In this manner, both the existence and the functions of linguistic variation are predicted by LePage's theory, and hence permit the testing of more and more particular hypotheses (e.g. Rampton 1989).

The theory accounts for linguistic variability through acts of identity made by speakers. Limitless variability exists to permit both the expression of individuality and the expression of various kinds of belonging. Limited systematicity exists to permit the hearer to create meanings, relatively economically, for the need inherent in the speaker's expression.

But notice that the speaker's acts of identity are of no use unless they are complemented by the hearer's created meanings of them, the hearer's acts of identification. (This <u>may</u> be covered by LePage's fourth rider, above.)

Since the speaker <u>risks</u> something in performing an act of identity (see . . . Chapter 2 above), and the hearer risks nothing but <u>gives</u> something in according his act of identification to that act of identity, we may conclude that the hearer is in the socially and phenomenologically prior role. (Note that there is no guarantee, given what I established in Chapter 2, that there will be a one-one relation between the speaker's act of identity and the meaning which is created for it by the hearer, that is, his act of identification.)

And since what the speaker <u>does</u> in an act of identity is psycho-socially simpler (algorithmic) than what the hearer does to create a meaning for that speaker's utterance (heuristic), a theory based on the hearer's acts of identification can contain a theory of the speaker's acts of identity, but the reverse cannot be true.

Acts of identity are risks which, after all, do not have to be ratified, and worse still, may be ignored.

The hearer has power.

Paradoxically, the space in which he wields it is silent . . .

* * * * * * *

ALL MISSING PAGES ARE BLANK

IN

ORIGINAL

Index

of

conjectures

Conjectures here should be taken to include pre-naive and naive conjectures as well as theoretically motivated conjectures. I include also guesses, speculations and assumptions. Assumptions, as Lakatos (1976) has shown, are special background forms of conjecture. All of these different types of conjecture are simply numbered sequentially, without distinction as to type, in the text above, but subcategorised by chapter (1,C1; 2,C4; 3,C17; etc.) or appendix (A,C3; B,C1; F,C2; etc.). Here they are collected together by Chapter or Appendix and the page of their occurrence is given. The form of them, though not the substance, has in some cases been abbreviated, but in others augmented to compensate for the loss of context.

As Eco (1985: 57-8) says

"An abstract model of conjecturality is the labyrinth [of which] there are three kinds: . . . the Greek [which] does not allow anyone to get lost . . . The classical labyrinth is the Ariadne's-thread of itself. Then there is the mannerist maze: if you unravel it you find in your hands a kind of tree, a structure with roots and blind alleys . . . [It] is a model of the trial-and-error process . . . Finally there is the net, or .. "rhizome" [which] is so constructed that every path can be connected with every other one. It has no centre, no periphery, no exit, because

it is potentially infinite. The space of conjecture is a rhizome space. The labyrinth of my library [i.e. that of Eco (1984) JP] is still a mannerist labyrinth, but the world in which [the conjecturer who is trying to solve that labyrinth] realises he is living already has a rhizome structure: that is, it can be structured, but it is never structured definitively . . . It is [in other words] impossible for there to be <u>a</u> story."

1,C16	The <u>process</u> involved in the building of pre-understanding will be the noticing, and hence the collection and grouping, of recurrences of differences which make a difference (Bateson 1985). That is, it will be some **classificatory process.**	27
1,C17	An empirically and aesthetically convincing model of mind-functioning would be dangerous for mind-functioning.	28
1,C18	The distinctions between selfhood, memory, imagination, mind, consciousness, and language are not so distinct.	29
1,C19	Cultural transmission enables individuals to avoid conjectural recapitulation, but it does not bind individual minds (Sampson 1980).	30
1,C20	If each differently constituted individual creates the meanings of all the phenomena which have meaning for him/her, then there are **<u>no</u>** conditions under which we can assume that some particular phenomenon will have the same meaning for two different individuals.	31

Chapter 2

2,C1	Hearers create the meanings of utterances.	38
2,C2a	That most hearers are also speakers is not precluded by the conjecture (2,C1) nor does it constitute an attack on it.	39
2,C2b	The meanings of utterances are created freely; that is hearers can entertain any conjecture whatever about the meaning of an utterance, which is weakly consonant with the context.	39
2,C2c	The meaning of an utterance is its effective meaning, that given it by the hearer. The meaning of an utterance will always be, at least in part immanent, and can never be canonical.	39

2,C3	For a given utterance, different meanings will be created by different hearers.	39
2,C4	Meanings are not transparently resident in utterances.	41
2,C5	Speakers can never know what they have meant (done) as an immediate result of having spoken.	43
2,C6	The hearer creates the context.	44
2,C7	Utterances which have no hearers have no meaning.	55
2,C8	The meaning of a speaker's utterances can be created by himself-as-hearer.	56
2,C9	Participants aspire to be hearers rather than speakers.	57
2,C9a	Perhaps the only proper use of language is the conversational use, the inter-<u>active</u>, the inter-<u>personal</u>, the inter-<u>subjective</u> use (Firth 1957).	58
2,C10	Because the speaker knows 2,C1 - 2,C9a, she has no intentions.	61
2,C11	Some utterances neutralise the hearer's capacity to create their meaning.	75
2,C12	The context, and the meaning of the utterance, which the hearer creates, cannot be wrong.	78
2,C13	Utterance meaning and variation meaning are inseparable and mutually informing; hence they must be created more or less simultaneously, though not necessarily by the same processes.	90
2,C14	In being socially reflective the hearer is creating different spatial and conceptual urban sub-environments which seem significant to him.	93
2,C15	In being socially reflective the hearer is creating a number of the different manners of speaking of his co-citizens which seem significant to him.	93
2,C16	In being socially reflective the hearer is building pictures of those manners of speaking which are associated with a particular sub-environment, <u>and</u> of those different sub-environments which are associated with a particular manner of speaking.	93

Chapter 3

3,C7 The VSp is optimal if: (a(i)) the number of VCs represented by a single member is a minimum, however large the sample size; (a(ii)) without contravening (i) above, the number of VCs is a maximum; (b) each VC is well-represented, and has a relatively higher level of internal cohesion than would exist in clusters of either a uniformly or a randomly distributed population; (c) no VC has the same group mean profile on a high proportion of criteria as more than a very small number of other VCs. 137

3,C8 Insofar as a given selector under- or over-represents any delimitable subsets of possible criteria, he will be operating with a topologically deformed version of the VSp, and I shall assume that such deformations <u>are</u> systematic. 150 % o

Appendix A **Vol ii, p.**

A,C1 Rejecting sampled speakers on the grounds of some <u>a prioristic</u> assumption of 'typicality' begs theoretically important sociolinguistic questions. 5

A,C2 It seems clear that circumstances will never be ideal and that the universe of comparisons is, anyway, transfinite, or ℵ in Cantor's scheme of things. 10

A,C3 The definitions of OU, PDV, and the notion of partition do not, <u>in any manner</u>, constrain or prejudge the ways in which any particular variety may express, combine, borrow or overlap its particular partitions of particular lexical sets. 32

A,C4	If samples of speakers showed that the perceived regularities represented as partitions were chimerical, the classifiability of those speakers would not be compromised. This is because the notion of 'a partition' (and its size and commonness) is not one which is built into the classificatory method; it simply gives the reader a crude idea of the lexical patterning of the phonologies to be found on Tyneside.	40
A,C5	I do not preclude the possibility of a strong connection between criterion 196 and criterion 200, but, in line with other contexts of the same principle, I propose to keep them separate on the grounds that it is precisely these assumptions of co-occurrence which need to be documented rather than left in a state of suspended, even if plausible, assumption.	72
A,C6	This criterion measures the speaker's range of reduced forms in terms of the number of items in the list which are <u>typically</u> reduced, rather than measuring the exact frequency of reduction of each item in the set. Clearly, both are relevant measures for a sociolinguistic survey. But because the latter will tend to measure variety mixing of a certain kind, and the former will tend to express a varietal norm (because it throws away much of the presumed continuity of the distribution), we prefer to consider only the former, at least initially.	79
A,C7	In the case of both of these criteria (0210, 0212) we must allow multiple coding on single occurrences of lexical items. Of course this may be castigated as an inconsistency in the coding system. A more positive view would see that where the definiteness of the variable is itself in doubt it is appropriate that the coding system become a little flakey.	83

A,C8 Taking one occurrence of any state as sufficient for its coding is a necessary method of representation because of our socio-perceptual ignorance of the distributional consistency of the variants of these criteria. 91

A,C9 At least <u>some</u> of the variation attributed to informants must have its real source in variation arising amongst their analysts. One presumes that analysts do not want to be believed to be less, or more, human than their subjects. 97

A,C10 One can easily <u>imagine</u> an informant saying something which one has never heard **anyone** saying. For instance in OU {ei}, 0106 PDV //ɛ//, I have only **heard** <u>make</u>, <u>take</u> as {mɛk}, {tɛk}; nevertheless, {bɛk}, <u>bake</u>, may be imagined fairly readily; {rɛk}, <u>rake</u> (vb.), requires a good deal more effort, and {lɛk}, <u>lake</u> (n.) seems to be somewhere beyond unlikely. Any of these, doubtless, <u>could</u> be produced by a native speaker on the basis of analogy, but under what conditions and with what probabilities I guess that we shall never know. 99

A,C11 One might imagine that as the number of investigators increased, and hence as the number of contributory deformations of the dimensionality increased, one would find that their conflation led to a **regular** VSp. That is, that there might be a cancelling effect among such skewings of selection. At present it is impossible even to establish a likelihood for this speculation. 100

A,C12 Periods of communal training will not <u>eradicate</u> the different experiences of analysts both as hearers and as speakers, but will <u>overlay</u> them. Between training sessions my guess is that there will always be a drift of their auditory habits away from the communally established norms and back to their 'natural' hearer-speaker experience. This drift will clearly be different for different analysts, it may be different for a single analyst on two separate occasions, and the end 'location', after the drift, may be different as a result of the repeated training. 101

A,C13 Transcriptional disparities between analysts will be a function of both (a) the relative differences between the analysts' <u>own</u> positions in the VSp, and (b) the relative differences between the analysts' positions and those of the informants in respect of whose speech their transcriptions are being compared. 103

A,C14 We know that many relationships are semi-metric, and therefore I reject neither of the depicted relationships amongst the transcribers, but will behave as if they were in some perfectly natural resonance with each other, albeit a resonance which can neither be heard, nor made use of for argumentative ends. 108

A,C15 If either of these measures - the range of CV disparity or the overall mean CV disparity - is an optimum one for constraining the variability amongst analysts, it is not at all clear which of them it is. 109

Appendix B

B,C13	Stable criteria might best be defined on the relations between pairs of terms from different systems.	190
B,C14	It is possible that the replacement of ØR tones by DR tones is a result of idiosyncratic affect.	193
B,C15	Given prosodic and paralinguistic systems as defined, and cooccurrence patterns amongst their terms in educated BrE, then marked differences in the frequency or cooccurrence distributions of those terms in Tyneside Englishes should give a satisfactory criterion.	199
B,C16	The low recognisability of prosodic and paralinguistic variants is predicted by the poverty of folk-linguistic terminology for terms in the <u>more</u> linguistic of those systems.	202
B,C17	The rejection of an expectation without surprise or reluctance is an indication of the general unknownness of the phenomena about which it was entertained.	205
B,C18	What do I guess this criterion would sound like in the mouth of x, or y, or z?	208
B,C19	I would expect to find curvilinear dispersions of a range of varieties (from NL to L to NL) on terms from the systems of tension and voice quality, rather than on those from the systems of tone and pitch range.	217
B,C20	If we define 'the % difference betwee F tones and R tones' as a dimension, we might expect speakers with high values to be less (Tyneside) localised than those with low values.	228
B,C21	Differences between one variety and another in respect of the distribution of one tone may be reflected in other parts of the tonic system.	230
B,C22	There is **no** reason for imagining that all patterned variation will correlate with things such as socio-economic class, or, indeed, with anything at all. (Cf. Eliot's Gyre, §1, Appx. F.)	240

B,C23	In respect of the continuum of "states" (i.e. % points) of some complex quantitative prosodic criterion, a set of varieties otherwise believed to be homogeneous as to locality may range <u>continuously</u> from one pole to the other.	244
B,C24	Depictions in which Level tone is given explicit dimensional power represent varieties which may be the most $_{L}T$ on a continuum which is perpendicular to that representing non- and less- localised varieties.	245
B,C25	Different dimensions are not <u>expected</u> to group people in the same way. This is a response to the assumption that left-hemispheric conscious analysis [sc. for right-handers] can **never** match right-hemispheric non-conscious 'analysis', or pattern 'apprehension', or the creation of 'gestalts'.	258
B,C26	I have no plausible guess as to why people should use 11% more levels with boosters (% levels) than rises with boosters (% rises), <u>regardless of the relative proportions of levels, rises and falls</u> used by the various speakers.	261
B,C27	A highly significant basis of much inference, though one not often owned, is, for the present case, the importance of being able to establish that features which co-occur have the capacity for different, unassociated distributions in a sample of speakers, since if they did <u>not</u> have this capacity, we would be unsure about the significance of associated distributions between those features when they <u>did</u> show themselves.	262

B,C28 Acknowledging no positive (but some negative) evidence for it, and that in such cases other things very rarely <u>are</u> equal, I assume that the tonic functions to be realised in an informal interview will be more or less similar, as to range and frequency of occurrence, amongst different speakers from the same sampling stratum. 275

B,C29 I assume that it's important in any part of an investigation to be able to declare either that one has <u>no</u> plausible interpretation of a pattern in the data, or that the data don't seem to show <u>any</u> of the expected patterns. 279

B,C30 I embrace the possibility that all apparently stable variations are created upon fractions of chaos each of which is necessarily unique. 280

B,C31 It is tempting to speculate that the prosodic behaviour of these women (having negative values on the dimension % difference between Fs. & Rs) is in the opposite direction from that assigned to them by Trudgill (1972). 283

B,C32 The women appear to be the guardians of a behaviour which is either <u>locally</u> prestigious, or not prestigious <u>at all</u>. 284

B,C33 In equations of the type $L = \gamma(L-R)$, $R = \tau(L-R)$, and $F = \theta(L-R)$ predictions of values for γ and τ will always be better than those for θ. 293

B,C34 My assumption that the spoken output of experimental volunteers is less likely to vary in unwanted ways than interview material begs the question of whether anyone can ever know exactly what all the unwanted manners of varying <u>are</u>, and <u>why</u>. 309

B,C35	One might argue that insofar as a given substantial feature in a model is **not** agreed upon (i.e. repeated) by an informant, or a group of informants, just so far we may attribute a different preferential pattern of realisation to those informants in respect of that type of feature, in that frame, in that environment.	318
B,C36	Clearly, insofar as the range of differences amongst informants' own underlying systems increases, so far also different informants may be expected to have wideningly different agreement rates with some particular feature of a model.	319
B,C37	Rates of agreement and disagreement with features of a model have only to do with the degrees of contrastivity of those features in those contexts under the strict assumption that the intonational systems of the speaker of the model and the intonational systems of the informants responding to the model are the same.	319
B,C38	I make the assumption that the range and type of agreements and disagreements which a given informant characteristically has in respect of the various features in several models is a reflection of her preferential realisation patterns as a speaker under normal circumstances, and that these preferential realisation patterns are reflections of her underlying intonational systems.	320
B,C39	Informants whose patterns of agreement and disagreement with models are similar, have similar intonational systems.	320
B,C40	Informants whose patterns of agreement and disagreement with models are different have different intonational systems.	320
B,C41	Informants showing a generally high level of agreement with some model or models have similar intonational systems to those underlying the model or models.	320

B,C42	Informants showing a generally high level of disagreement with some model or models have dissimilar intonational systems from those underlying the model or models.	320
B,C43	If the low predictability [of a TU in a fronted verbless adjunct] is to be understood as equatable with high information value (<u>species</u> Shannon), then it is surprising that such a wide range of tonic selections is possible. If low predictability is is to be understood as equatable with low information value (marginality), then it is surprising that so many modellers choose to delimit it as a tone unit.	331
B,C44	High agreement amongst many imitators of several models suggests that the differences between underlying tonic systems expresses itself least in mandatory tonicities having very low marking function. If this is the case it will be important to distinguish tonic distributions according to their tonicities.	353
B,C45	The polarity of any pair of tones must be related in some way to two measures: the **means** and the **ranges** of agreement with model tones by imitators.	366
B,C46	On the assumption that the index of tonic alteration is a reflection of the difference between the underlying tonic system of the imitator and the underlying tonic system of the modeller, imitators are similar to each other insofar as they share similar patterns of values - in respect of the different models - for the index of tonic alteration.	375

B,C47 Variant prosodic systems are grouped in terms of greater or lesser degrees of a **continuous** measure of similarity. And the basis of the similarity concerned can vary considerably (e.g. whether it takes account of tonicity, whether it is in respect of a single model or the sum of models, etc.) This does not mean that one or another type of similarity is spurious. On the contrary, it means that the structural and distributional relationships between variant prosodic systems are hierarchic and that no level of that hierarchy is one-one predictive of the other. 376

Appendix C

C,C1 Those things which one admits as facts are shaped by and dependent upon only and precisely one's conjectures. 398

C,C2 If there **were** a single best base of measurement of the occurrence rate of variants about which next to nothing is known, then that <u>best possible</u> base could not be known in advance of experimenting with <u>several possible</u> bases. 398

Appendix D

D,C1 That things (entia) should not be multiplied beyond necessity (non sunt multiplicanda praeter necessitatem - Occam) surely applies just as importantly to data as it does to analytical categories, or concepts or hypotheses. 430

D,C2 A criterion which is invariant in one subsample may not be invariant in another, and therefore it is probably safe to include invariant criteria provided: (a) that there are not too many (say no more than 1 in 20), and (b) that they are removed from account in the construction of diagnostic keys. 437

D,C3 There is a general empiricist Catch-22 of one's needing to know all of everything-in-general before one can know any something-in-particular. 443

D,C4 It is an erroneous assumption that the ab initio weightings which are felt to be deserved by the criteria in some classification are collectively equivalent to the diagnostic key which we would obtain post hoc from the groups to be formed. 444

D,C5 A more permissive attitude towards the inclusion of negative matches between OTUs in the computation of similarity would be both inferentially and statistically advantageous. 450

D,C6 It is known that when expressing relationship by distance coefficients, decreasing values of the coefficient (i.e. expressions of greater similarity) are directly proportional to decreasing confidence in those values; but this is surely exactly the opposite of our experience as classifying creatures in daily life. 462

D,C7 I presume that the profitability or utility of a final group or class depends on the interaction between the mental constructs of the interpretation of those final groups and of the original purpose(s). 474

D,C8 The tendency, in intuitive or 'hand' classification, to emphasise close relative relationship and to de-emphasise distant relative relationship will become more marked as the number of OTUs rises, leading to substantial between-group vagueness. 522

D,C9 There is no reason why taxonomies of biological OTUs and of non-biological OTUs must be different in kind. 523

D,C10	There is a serious possibility that the stability of a classificatory algorithm is inversely proportional to its classificatory power.	527

Appendix E

E,C1	Presumably there is an end to one's patience or persistence ?	546
E,C2	The obviousness of the banale is at least recognisable as human.	546

Appendix F

F,C1	The never-ending accumulation of 'results' which are required to underpin the unlikelihood of 'progress' are, very probably, hopeful fictions of the self-important.	560
F,C2	Any empirical attempt is misrepresentative of what it purports to show because of the indeterminacy of its sampling methods, and because of the absence of population boundaries, and because of the transfinite nature of any property space.	571
F,C3	Apart from being wearyingly lengthy, any attempt to model right-hander right-hemisphere processes by left-hemisphere analysis will be thoroughly misleading.	571
F,C4	The 'real' nature of hearer inference must mostly arise from the **complete** cluster of the hearer's needs and interests which arise and weave and dissolve moment by moment.	571
F,C5	Of a given set of objects, it is possible to generate an **infinite** number of classifications amongst which there can be no externally valid criterion for choosing 'the best'.	571

F,C6 There is far more to the world of spirit than any 'meaning', in the sense of effability, can adhere to. 572

F,C7 The very <u>possibility</u> of refutation disappears since the refuter's case can never make contact with that which it seeks to refute. 572

References

Superscript numbers attached to dates refer to an edition other than the first.

Abbreviations

A.C.M.	Association of Computing Machinery
Am./Amer.	American
Anthropol.	anthropology/anthropologist
Appl.	applied/application(s)
Assn.	association
Biol.	biology/biological/biologists
Brit.	British/Britain
Bull.	bulletin
Comm.	communication(s)
Cong.	congress
Ed.	editor/edited
Educ.	education/educational
Foun.	foundation(s)
Gen.	general
H.M.S.O.	Her Majesty's Stationery Office
IEEE	Institute of Electrical & Electronic Engineers
Intl.	international
J.	journal
Ling.	linguistic(s)
Lg.	language(s)
Math.	mathematics/mathematician/mathematical
Meas.	measurement
n.d.	no date
n.p.	no place
p.c.	personal communication
Phil.	philosophy/philosophical/philosophers
P.	philology/philological
Phon.	phonetic(s)
Proc.	proceedings
Psych.	psychology/psychological/psychologists
Rev.	review
Sci.	science(s)

Soc./S.	social/society/sociology
Stat.	statistics/statistical
Symp.	symposium
Theoret.	theoretical
T.	transactions
U.(P.)	University (Press)
Zool.	zoology/zoological/zoologists

Al-Amadidh, D.G. (1989) The unreality of quantitative figures, **York Papers in Linguistics,** 13, p. 33 ff.

Anderson, A. (1957) A semigraphical method for the analysis of complex problems, **Proc. National Academy of Sciences,** 43, p. 923 ff.

Anderson, L. (1986) Hearing you in my own voice: woman as listener and reader, IN McGregor, G & White, R.S. (eds.) **The art of listening.** p. 73 ff.

Annand, J.B. (1977) (ed.) **Education for self-discovery.** London: Hodder & Stoughton.

Assagioli, R. (1973) Psychosynthesis : a technique for the use of intuition IN Ornstein, R.E. (ed.) **The Nature of human consciousness.** San Francisco: Freeman. p. 336 ff

Atkin, R.H. (1974a) An approach to structure in architectural and urban design. 1 Introduction and mathematical theory, **Environment and Planning B** , 1, p. 51 ff.

Atkin, R.H. (1974b) An approach to structure in architectural and urban design. 2. Algebraic representation and local structure, **Environment and Planning B,** 1, p. 173 ff.

Atkin, R.H. (1974c) **Mathematical structure in human affairs**. London : Heinemann.

Atkin, R.H. (1975) An approach to structure in architectural and urban design 3. Illustrative examples, **Environment and planning B,** 2, p.21 ff.

Atkin, R.H. (1976) A general mathematical approach in physical, urban and social systems, **J. Institute of Mathematics & Its Applications,** p. 305 ff.

Bailey C.-J.N. (1973) **Variation and linguistic theory.** Washington: Center for Applied Linguistics.

Bailey, K.D. (1982) Clusters as systems, **Classification Soc. Bull.**, 5(2), p.18 ff.

Bannister, D. & Fransella, F. (1971) **Inquiring man: the theory of personal constructs.** London: Penguin.

Bannister, D. (1983) p.c.

Barley, N. (1986) **The innocent anthropologist: notes from a mud hut.** London: Penguin.

Barr, A. (1957) Differences between experienced interviewers, **Applied Statistics** , 6, p. 180 ff.

Bartlett, F.C. (1932) **Remembering.** Cambridge: C.U.P.
Bateson, G. (1979) **Steps to an ecology of mind.** London: Paladin.
Bateson,G. (1985) **Mind and nature: a necessary unity.** London: Fontana (Flamingo).
Bateson, G. & Bateson, M.C. (1988) **Angels fear: towards an epistemology of the sacred.** London: Bantam.
Bayes, T. (1763 [1958]) An essay towards solving a problem in the doctrine of chances (reprinted with a biographical note by G.A.Barnard), **Biometrika,** 45 (3), p.293 ff.
Bazell, C.E. (1949) Réponses à la question 1 IN Lejeune, M. (ed.) **Proc. 6th. Intl. Cong. Ling.** Paris: Klincksieck p.115 ff.
Bazell, C.E. (1966) The correspondence fallacy in structural linguistics IN Hamp, E.P., Householder, F.W., & Austerlitz, R. (eds.) **Readings in Linguistics II.** Chicago: Chicago U.P. p.271 ff.
Bazell, C.E. (1977a) p.c.
Bazell, C.E. (1977b) p.c.
Bazell, C.E., Catford, I., Halliday, M.A.K., & Robins, R.H. (1966) (eds.) **In memory of J.R. Firth.** London: Longmans.
Becker, H. (1950) **Through values to social interpretation.** Durham, N.C.: Duke U.P.
Beckett, S. (1938) **Murphy.** London: Calder & Boyars.
Beckett, S. (1965) **Krapp's last tape & Embers.** London: Faber.
Beckner, M. (1959) **The biological way of thought.** N.Y.: Columbia U.P.
Beckner, M. (1964) Metaphysical presuppositions and the description of biological systems *IN Gregg, J.R. & Harris,* F.T.C. (eds.) **Form and strategy in science: studies dedicated to J.H.Woodger.** Dordrecht: Reidel. p. 18 ff.
Beers, R.J., Fisher, J., Megraw, S. & Lockhart, W.R. (1962) A comparison of methods for computer taxonomy, **J. Gen. Microbiology,** 28, p. 641 ff.
Beers, R.J. & Lockhart, W.R. (1962) Experimental methods in computer taxonomy, **J. Gen. Microbiology,** 28, p. 633 ff.
Bell, A. (1984) Language style as audience design, **Lg. Soc.,** 13 (2), p. 145 ff.
Benn, T. (1982) **Arguments for democracy.** London: Penguin.

Berge, C. (1962) **The theory of graphs.** (Transl. A. Doig.) London: Methuen.

Berger, J.(1979) **Pig Earth.** London :Writers and Readers Publishing Cooperative.

Berger, J. & Mohr, J. (1982a) **Another way of telling.** London: Writers & Readers Publishing Cooperative.

Berger, J. & Mohr, J. (1982b) **A seventh man.** London: Writers & Readers Publishing Cooperative.

Berger, P.L. & Luckmann, T. (1967) **The social construction of reality.** London: Lane.

Berlin, B., Breedlove, D.E., & Raven, P.H. (1974) **Principles of Tzeltal plant classification: an introduction to the botanical ethnography of a Mayan-speaking people of highland chiapas.** N.Y.: Academic.

Bertalanffy, L.von (1952) **Problems of life.** N.Y.:Wiley.

Berthoff, A.E. (1986) "Reading the world . . . Reading the word": Paulo Freire's pedagogy of knowing IN Newbirk, T. (ed.) **Only connect: uniting reading and writing.** Upper Montclair, N.J.: Boynton Cook. p. 119 ff.

Bibby, R. (1980) (ed.) **Northumbriana: Northumberland's own magazine (No. 19).** Morpeth, N'mblnd: Westgate House.

Bickerton, D. (1971) Inherent variability and variable rules, **Foun. Lg.**,7, p. 475 ff.

Bickerton, D. (1975) **Dynamics of a creole system.** London : C.U.P.

Biddle, B.J. & Thomas, E.J. (1966) **Role theory: concepts & research.** London: Wiley.

Bierwisch,M. (1977) Social differentiation of language structure IN Bever, T.G., Katz, J.J., & Langendoen, D.T. (eds.) **An integrated theory of linguistic ability.** Hassocks, Sussex: Harvester. p. 271 ff.

Bing, J.M. (1985) **Aspects of English prosody.** London: Garland.

Black, M. (1956) Definition, presupposition and assertion IN Hook, S. (ed.) **American philosophers at work.** N.Y.: Criterion. p. 84 ff.

Bladon, A. (1986) Phonetics for hearers, IN McGregor, G. (ed.) **Language for hearers.** p. 1 ff.

Bloom, D. & Hays, D.G. (1978) Designation in English IN Hinds, J. (ed.) **Anaphora in discourse** (Current enquiry into Language & Linguistics 22) Edmonton: Linguistic Research Inc. p.1 ff.

Bly, R. (n.d.)(ed.) **News of the universe: poems of twofold consciousness.** San Francisco: Sierra Club Books.

Bobisud, H.M. & Bobisud, L.E. (1972) A metric for classifications, **Taxon,** 21, p.607 ff.

Bobrow, D.G. & Collins A. (1975) (eds.) **Representation and understanding: studies in cognitive science.** London: Academic.

Bohr, N. (1958) **Atomic physics and human knowledge.** NY: Wiley.

Bok, S. (1980) **Lying: moral choice in public and private life.** London: Quartet Books.

Bolinger, D.L. (1961a) Contrastive accent and contrastive stress, **Lg.**, 37, p. 83 ff.

Bolinger, D.L. (1961b) **Generality, gradience and the all-or-none.** The Hague: Mouton.

Bolinger, D.L. (1965) **Forms of English: accent, morpheme, order.** [Abe, I. & Kanekiyo, T. (eds.)] Tokyo: Hokuou.

Bolinger, D.L. (1972) **Degree words.** The Hague: Mouton.

Bolinger, D.L. (1973) Truth is a linguistic question, **Lg.**, 49, p. 539 ff.

Bolinger, D.L. (1977) p.c.

Bolinger, D.L. (1983) Affirmation and default, **Folia Linguistica,** XVII, p. 99 ff.

Bolinger, D.L. (1985) p.c.

Bolinger, D.L. (1986) **Intonation and its parts: melody in spoken English.** Stanford, Calif.: Stanford U.P.

Booker, H.S. & David S.T. (1952) Differences in results obtained by experienced and inexperienced interviewers, **J. Roy. Stat. Soc. A,** 115, p. 232 ff.

Botha, R.P. (1973) **The justification of linguistic hypotheses.** The Hague: Mouton.

Boughey, A.S. (1969) (ed.) **Contemporary readings in ecology** Belmont, Calif.: Dickenson.

Bradac, J., Friedman, E. & Giles H. (1986) A social approach to

propositional communication: speakers lie to hearers, IN McGregor, G. (ed.) **Language for hearers.** p. 127 ff.

Bradley, R. & Swartz, N.(1979) **Possible worlds: an introduction to logic and its philosophy.** Oxford: Blackwell.

Bradshaw, J. M. (1990) p.c.

Brandis, W. (1970) An index of social class, IN Brandis, W. & Henderson, D. (eds.) **Social class, language & education.** London: Routledge. p. 130 ff.

Brazil, D. (1975) **Discourse intonation.** (= Discourse analysis monographs 1.) Birmingham University ELR: Birmingham.

Brazil, D. (1978) **Discourse intonation II.** (= Discourse analysis monographs 2.) Birmingham University ELR: Birmingham.

Brazil, D., Coulthard, M., & Johns, C. (1980) **Discourse intonation and langauge teaching.** London: Longman.

Brown, R., (1986) Aspects of the speaker's role in speaker recognition, IN McGregor, G. (ed.) **Language for hearers.** p. 37 ff.

Brown, R. & McNeil, D.S. (1966) The tip-of-the-tongue phenomenon, **J. Vbl. Learning & Vbl. Behav.**, 5, p. 325 ff.

Bruner, J.S., Goodnow, J.J., & Austin, G.A. (1956) **A study of thinking** . N.Y.: Wiley.

Caianiello, E.R. (1961) Outline of a theory of thought processes and thinking machines, **J. Theoret. Biol.**, 2, p.204 ff.

Capra, F. (1982) Buddhist physics IN Kumar, S. (ed.) **The Schumacher Lectures** . London: Abacus. p.121 ff.

Capra, F. (1983) **The turning point: science, society & the rising culture.** London: Fontana.

Carmichael, J.W. & Sneath, P.H.A. (1969) Taxometric maps, **Systematic Zoology**, 18, p. 402 ff.

Carroll, L.(1889) **Sylvie and Bruno** IN Woollcott, A. (1939) (ed.) **The complete works of Lewis Carroll.** London: Nonesuch.

Cartwright, D. & Zander, A. (1968)[3] **Group dynamics: research & theory.** London: Tavistock.

Carvell, H. & Svartvik, J. (1969) **Computational experiments in grammatical classification.** The Hague: Mouton.

Cattell, R.B. (1944) A note on correlation clusters and cluster search methods, **Psychometrika**, 9, p. 169 ff.

Cattell, R.B. (1949) r_p and other coefficients of pattern similarity, **Psychometrika**, 14, p. 279 ff.

Cattell, R.B. (1962) Group theory, personality and role : a model for experimental researches. IN Geldard, F.A. (ed.) **Defence psychology**. Oxford : Pergamon. p. 209 ff.

Cattell, R.B. (1966) (ed.) **Multivariate methods in experimental psychology**. Chicago: Rand:McNally.

Cattell, R.B. & Coulter, M.A. (1966) Principles of behavioural taxonomy and the mathematical basis of the taxonome computer program. **Br. J. Math. & Stat. Psychol.**, 19 (2), p. 237 ff.

Cattell, R.B., Coulter, M.A. & Tsujioka, B. (1966) The taxonometric recognition of types and functional emergents IN Cattell, R.B. (ed.), p. 288 ff.

Catford, J. C. (1957) Vowel systems of Scots dialects, **Trans. Philol. Soc.**, p. 107 ff.

Chomsky, N. (1968) **Language & Mind**.N.Y.: Harcourt Brace.

Chomsky, N. (1978) Interview with Magee, **The Listener**, 99, *2554, (6 April), p. 435 ff.

Chomsky, N. & Halle, M. (1968) **The sound pattern of English**. NY: Harper & Row.

Chomsky, N. & Miller, G.A. (1963a) Introduction to the formal analysis of natural languages, IN Luce, R.D., Bush, R.R. & Galanter, E. (eds.) **Handbook of mathematical psychology, Vol. II**. NY: Wiley. p. 269 ff.

Chomsky, N. & Miller, G.A. (1963b) Finitary models of language users, IN Luce, R.D., Bush, R.R. & Galanter, E. (eds.) **Handbook of mathematical psychology, Vol. II**. NY: Wiley. p. 207 ff.

Cicourel, A.V. (1973) **Cognitive sociology**. London: Penguin.

Cleverdon, C. & Keen, M. (1966) **Factors determining the performance of indexing systems: Vol 2 Test results**. Cranfield Aslib Project.

Cochran, W.G. (1968[2]) **Sampling techniques**. NY: Wiley.

Cochran, W.G. & Hopkins, C.E. (1961) Some classification

problems with multivariate qualitative data, **Biometrics**, 17, p. 10 ff.

Cochrane, G.R. (1959) The Australian English vowels as a diasystem, **Word**, 15, p. 69 ff.

Cohen, P.S. (1968) **Modern social theory**. London: Heinemann.

Cole, A.J. & Wishart, D. (1970) An improved algorithm for the Jardine-Sibson method of generating overlapping clusters, **Computer J.**, 13(2), p.156 ff.

Collins, H.M. (1985) **Changing order: replication & induction in scientific practice**. London: Sage.

Colodney, R.G. (1983) (ed.) **Beyond the edge of certainty: essays in contemporary science & philosophy**. London: U. P. of America Inc. (Reprint of 1965 issue by Prentice-Hall.)

Conant, R. C. (1972) Detecting subsystems of a complex system, **IEEE T. Systems, man & cybernetics**, p.550 ff.

Connolly, B (n.d.) **The jobbie weecha**. N.p.

Cruttenden, A. (1981) Falls and rises: meanings and universals, **J. Ling.**, 17, p. 77 ff.

Cruttenden, A. (1986) **Intonation**. Cambridge: U.P. [See also Nolan (1987).]

Crystal, D. (1966) **Studies in the prosodic features of educated British English with special reference to intonation**. Unpublished Ph.D. thesis , University of London.

Crystal, D. (1969) **Prosodic systems and intonation in English** . London: Cambridge U.P.

Crystal, D. (1970) p.c.

Crystal, D. (1971) Prosodic and paralinguistic correlates of social categories. IN Ardener, E.(ed.) **Social Anthropology and Language**. London: Tavistock (=ASA Monograph 10). p.185 ff.

Crystal, D. (1975) **The English tone of voice: essays in intonation, prosody and paralanguage**. London: Arnold.

Crystal, D. & Davy, D. (1969) **Investigating English style**. London: Longmans.

Crystal, D. & Quirk, R. (1964) **Systems of prosodic and paralinguistic features in English**. The Hague: Mouton.

Da Free John (1980a) **The four fundamental questions.**

Clearlake, Calif.: Dawn Horse.

Da Free John (1980b) **Scientific proof of the existence of god will shortly be announced by the White House.** Clearlake, Calif.: Dawn Horse.

Da Free John (1983) **God is not a gentleman and I am that one.** Clearlake, Calif.: Dawn Horse

Danes, F. (1960) Sentence intonation from a functional point of view, **Word,** 16, p. 34 ff.

Davidson, P.O. & Costello, C.G. (1969) (eds.) **N=1: Experimental studies of single cases: an enduring problem in psychology.** N.Y.: Van Nostrand Reinhold.

Davis, M. (1958) **Computability & unsolvability.** N.Y.: McGraw Hill.

Defoe, D. (1928) (ed. Cole, G. D. H.) **A tour through England and Wales.** London: Ladybird.

Deikman, A.J. (1973) The meaning of everything IN Ornstein, R.E. (ed.) **The nature of human consciousness.**San Francisco: Freeman. p. 305 ff.

Derrida, J. (1976) **Of Grammatology.** Baltimore: Johns Hopkins U.P.

Derrida, J. (1982) **The margins of philosophy.** (Transl. with addl. notes by Alan Bass.) Chicago: U.P.

Derwing, B.L. (1973) **Transformational grammar as a theory of language learning.** London: Cambridge U.P.

Devonish, H. (1989) Language variation theory in the light of co-occurrence restriction rules, **York Papers in Linguistics,** 13, p. 129 ff.

DeWitt, B. & Graham, N. (1973) (eds.) **The many-worlds interpretation of quantum mechanics.** Princeton: Princeton U.P.

Dirac, P. (1982) **The principles of quantum mechanics.** London: Oxford U.P.

Dobson, Scott (1969) **Larn yersel Geordie.** Newcastle: Frank Graham.

Donne, J.W. (1939)[5] **An experiment with time.** London: Faber.

Douglas-Cowie, E. & Cowie, R. (1989) Speech disorder as a sociolinguistic problem, **York Papers in Linguistics,** 13, p. 155 ff.

Durbin, J. & Stuart, A. (1951) Differences in response rates of experienced and inexperienced interviewers, **J. Roy. Stat. Soc. A**, 114, p. 163 ff.

Durrell, L. (1962) **The Alexandria Quartet** . London: Faber. (One volume edition.)

Eco, U. (1984) **The name of the rose.** London: Pan (Picador).

Eco, U. (1985) **Reflections on The name of the rose.** London: Secker & Warburg.

Everitt, B (1974) **Cluster analysis.** London: Heinemann.

Ellis, A.J. (1889) **On early English Pronunciation Vol V.** London: Trubner.

Fant, C.G.M. (1963) Comment on the motor theory of speech production, IN **Proceedings of the Speech Communication Seminar.** Stockholm: Speech Transmission Lab., Roy. Inst. Tech. p. 30 ff.

Feigenbaum, M.J. (1984) Quantitative universality for a class of nonlinear transformations, IN Hao, B.-L., (ed.) **Chaos.** p. 158 ff.

Ferguson, C. A. & Gumperz, J.J. (1960) (eds.) **Linguistic diversity in South Asia.** Bloomington, Indiana: Rsch. Centre in Folklore, Anthropology & Linguistics Publ. 13 (= **I.J.A.L.** 23 (3) Pt III)

Ferguson, C.A. & Gumperz, J.J. (1960) Introduction IN Ferguson, C.A. & Gumperz, J.J. (eds.). p. 1 ff.

Feyerabend, P.K. (1983) Problems in empiricism, IN Colodny, R.G. (ed.), p. 145 ff.

Fillenbaum, S. & Rapoport, A. (1971) **Structures in the subjective lexicon.** N.Y.: Academic

Fingarette, H. (1969) **Self-deception**. London: Routledge & Kegan Paul.

Firth, J.R. (1957) **Papers in linguistics 1934-1951**. Oxford: U.P.

Fischer, D.H. (1970) **Historians' fallacies.** NY: Harper & Row.

Fischer, R. (1979) On the remembrance of things future: the psychobiology of divination, **Diogenes**, 108, p.17 ff.

Fischer, R. (1981) Matter's mastermind: the model-making brain, **Diogenes**, 116, p.18 ff.

Fischer, R. (1985) Deconstructing reality, **Diogenes**, 129, p. 47

ff.
Fischer, R. (1987) Emergence of mind from brain: the biological roots of the hermeneutic circle, **Diogenes**, 138, p. 1 ff.
Fishman, J.A. (1966) **Language loyalty in the United States.**The Hague: Mouton.
Fleck, L. (1979) **Genesis and development of a scientific fact.** Chicago: U.P.
Fleiss, J.L. & Zubin, J. (1969) On the methods and theory of clustering, **Multivariate Behavioural Research**, 4, p.235 ff.
Forrester, J.W. (1961) **Industrial dynamics.** Cambridge, Mass,: M.I.T.
Foucault, M. (1983) (transl. & ed. Harkness, J.) **This is not a pipe,· with illustrations and letters by René Magritte.** Berkeley: California U.P.
Foster, R. (1983) p.c.
Fransella, F. (1965) **The effects of imposed rhythm and certain aspects of personality on the speech of stutterers.** Unpublished PhD thesis, University of London.
Freire, P.(1972) **Pedagogy of the oppressed.** London:Penguin.
French, P. & Local, J.K. (1983) Turn-competitive incomings, **J. Pragmatics**, 7, p. 17 ff.
Freud, S. (1953-1966) **The standard edition of the complete psychological works.** London: Hogarth.
Friedman, H.P. & Rubin, J. (1967) On some invariant criteria for grouping data, **Am. Stat. Assn. J.**, 52, p. 1159 ff.
Fry, D.B. (1947) The frequency of occurrence of speech sounds in southern English, **Archives Néerlandaises de phonétique expérimentale**, 20, p. 103 ff.
Galtung, J. (1981) Literacy, education, and schooling for what? IN Graff, H.J. (ed.) **Literacy and social development in the West.** Cambridge: U.P.
Garvey, C. & Dickstein, E. (1972) Levels of analysis and social class differences in language, **Lg. & Speech**, 15, p.375 ff.
Gazdar, G, (n.d.) **Implicature & presupposition.** (Mimeo handout, n.p.)
Gazdar, G., Pullum, G.K., Carpenter, R., Klein, E., Hukari, T.E. & Levine, R.D. (1988) Category structures, **Computational**

Linguistics, 14 (1), p. 1 ff.
Gilbert, G.N. & Mulkay, M. (1984) **Opening Pandora's box: a sociological analysis of scientists' discourse.** Cambridge: U.P.
Giles, H. & Powesland, P.F. (1975) **Speech style and social evaluation.** London: Academic.
Gilmour, J.S.L. (1937) A taxonomic problem, **Nature,** 139, p. 1040 ff.
Glauser, B. (1984) **A phonology of present-day speech in Grassington (North Yorkshire).** Bern: Francke. [= The Cooper Monographs No. 32.]
Gleick, J. (1988) **Chaos: making a new science.** London: Cardinal (Sphere).
Goffman, E. (1959) **The presentation of self in everyday life.** NY: Doubleday.
Goffman, E. (1961) **Encounters.** NY: Bobbs-Merrill.
Goffman, E. (1963) **Behaviour in public places.** London: Collier-Macmillan.
Goldman-Eisler, F. (1961) Hesitation and information in speech, IN Cherry, C. (ed.) **Information theory.** [Fourth London Symposium.] London: Butterworth. p. 63 ff.
Good, I.J. (1953) The population frequency of species and the estimation of population parameters, **Biometrika,** 40, p. 237 ff.
Good, I.J. (1961) Weight of evidence, causality and false alarm probabilities IN Cherry, C. (ed.) **Information theory.** [Fourth London Symposium.] London: Butterworths. p.125 ff.
Good, I.J. (1962) Botryological speculations IN Good et al. (eds.) p. 120 ff.
Good, I.J. (1965a) A categorisation of classifications IN Medical Research Council (ed.) **Mathematics and computer science in biology and medicine.** London: H.M.S.O. p.115 ff.
Good, I.J. (1965b) **The estimation of probabilities: an essay in modern Bayesian methods.** Cambridge, Mass.: MIT.
Good, I. J. (1972) Food for thought, IN Nicholson, J.P. (ed.) **Interdisciplinary investigation of the brain.** London:

Plenum.
Good, I.J., Mayne, A.J. & Smith, J.M. (1962) (eds.) **The scientist speculates.** London: Heinemann.
Goodall, D.W. (1966) Numerical taxonomy of bacteria - some published data re-examined, **J. Gen. Microbiology**, 42, p. 25 ff.
Goodman, L.A. 1949) On the estimation of the number of classes in a population, **Ann. Math. Stat.**, 20, p. 572 ff.
Gower, J.C. (n.d.(a)) **Maximal predictive classification.** Harpenden, Herts.: Rothamsted Experimental Station (pre-publication mimeo).
Gower, J.C. (n.d.(b)) **A general coefficient of similarity and some of its properties.** Harpenden, Herts.: Rothamsted Experimental Station (pre-publication mimeo).
Gower, J.C. (1967) A comparison of some methods of cluster analysis, **Biometrics**, 23, p. 623 ff.
Gower, J.C. (1969) A survey of numerical methods useful in taxonomy, **Acarologia**, XI, p. 357 ff.
Gower, J.C. (1971) Statistical methods of comparing different multivariate analyses of the same data, IN Hodson, F.R. et al. (eds.) **Mathematics in archaeological and historical sciences.** p.138 ff.
Gower, J.C. (1973) p.c.
Graham, P. (1982) p.c.
Gray, P.G. (1956) Examples of interviewer variability taken from two sample surveys., **Applied Statistics**, 5, p. 73 ff.
Gray, P.G., Corlett, T. & Jones, P. (1951) **The proportion of jurors as an index of the economic status of a district.** London: The Social Survey.
Gregory, M. (1967) Aspects of varieties differentiation, **J. Ling.**, 3, p. 177 ff.
Gregory, R.L. (1970) **The intelligent eye.** London: Wiedenfield & Nicholson.
Gribbin, J. (1985) **In search of Schrödinger's cat.** London: Corgi.
Grice, H.P. (1968) Utterer's meaning, sentence meaning and word meaning, **Foun. Lg.**, 4, p. 225 ff.
Grice, H.P. (1969) Utterer's meaning and intentions, **Phil. Rev.**,

78, p. 147 ff.
Grice, H.P. (1975) Logic and conversation, IN Cole, P. & Morgan, J.L. (eds.) **Syntax and semantics Vol III, Speech Acts.** London: Academic. p.132 ff.
Grimm, R.H. (1977) **Nietzche's theory of knowledge.** Berlin: de Gruyter.
Grootaers, W.A. (1959) The origin and nature of the subjective boundaries of dialects, **Orbis,** 8, p. 355 ff.
Gumperz, J.J. & Naim, C.M. (1960) Formal and informal standards in Hindi regional language area, IN Ferguson, C.A. & Gumperz, J.J. (eds.) p. 157 ff.
Guy, G., Horvath, B., Vonwiller, J., Daisley, E. & Rogers, I. (1986) An intonational change in progress in Australian English, **Lg. Soc.** 15, p. 23 ff.
Hall, A.V. (1967) Methods for demonstrating resemblance in taxonomy and ecology, **Nature**, 214 (5090), p. 830 ff.
Hall, A.V. (1968) Methods for showing distinctness and aiding identification of critical groups in taxonomy and ecology, **Nature**, 218 (5137), p. 203 ff.
Hall, E.T. (1959) **The silent language.** NY: Doubleday.
Hall, J. & Jones, D.C. (1950) Social grading of occupations, **Brit. J. Soc.,** 1, p. 31 ff.
Halliday, M.A.K. (1961) Categories of the theory of grammar, **Word,** 17, p. 241 ff.
Halliday, M.A.K. (1966) Typology and the exotic, IN McIntosh & Halliday (eds.), p. 165 ff.
Halliday, M.A.K. (1967) **Intonation and grammar in British English.** The Hague: Mouton.
Halliday, M.A.K. (1990) p.c.
Hamblin, C.L. (1971) Mathematical models of dialogue. **Theoria,** XXXVII (2), p. 130 ff.
Hamilton-Merritt, J. (1979) **A Meditator's Diary: a western woman's unique experiences in Thailand monasteries.** Harmondsworth, Middlesex: Penguin.
Hao, Bai-Lin (1984) (ed.) **Chaos.** Singapore: World Scientific.
Harary, F. (1969) **Graph theory.** Reading, Mass.: Addison Wesley.
Harrah, D. (1963) **Communication: a logical model.** Cambridge,Mass.: M.I.T.

Herbst, P.G. (1961) A theory of simple behaviour systems I & II, **Human Relations,** 14, 1 & 3, p. 71 ff: & 193 ff.

Herbst, P.G. (1973) **Behavioural worlds: the study of single cases.** London: Tavistock.

Herdan, G (1964) **Quantitative linguistics.** London: Butterworths.

Hill, T. (1963) Phonemic and prosodic analysis in linguistic geography, **Orbis,** 7, p. 441 ff.

Hill, T. (1966) The technique of prosodic analysis IN Bazell, C.E. et al. (eds.) **In memory of J.R.Firth.** London: Longmans. p. 198 ff.

Hill, T. (1983) p.c.

Hockett, C.F. (1955) **A manual of phonology.** (=Memoir 11 of International Journal of American Linguistics).Waverly, Baltimore: Indiana UP.

Hockett, C.F. (1961) Grammar for the hearer IN Jakobson, R. (ed.) **Proc. Symp. in Appl. Math. XII (Structure of language and its mathematical aspects)** Providence, Rhode Island: Am. Math. Soc. p. 220 ff. [Also now reprinted in McGregor, G. (ed.) **Language for hearers.** p. 49 ff.]

Hockett, C.F. (1968) **The state of the art.** The Hague: Mouton.

Hodson, F.R., Kendall, D.G. & Tautu, P. (1971)(eds.) **Mathematics in archaeological and historical sciences.** Edinburgh: U.P.

Hodson, F.R., Sneath, P.H.A. & Doran, J.E. (1966) Some experiments in the numerical analysis of archaeological data, **Biometrika,** 53, p. 311 ff.

Hoenigswald, H.M. (1966) A proposal for the study of folk-linguistics IN Bright, W. (ed.) **Sociolinguistics : Proceedings of the UCLA Sociolinguistics Conference 1964.** The Hague: Mouton. p. 16 ff.

Hofstadter, D.R. (1980) **Gödel, Escher, Bach: an eternal gold braid.** London: Penguin.

Horvath, B. (1985) **Variation in Australian English: the sociolects of Sydney.** Cambridge: U.P.

Houck, C.L. (1969) Methodology of an urban speech survey, **Leeds studies in English (N.S.),** 2, p. 17 ff.

House, J.W. & Willis, K.G. (1967) **North region and nation: a**

short migration atlas 1960-1961. Newcastle upon Tyne: University Department of Geography.

Huddleston, R. (1988) **English grammar: an outline.** Cambridge: U.P.

Hughes, E.(1952) **North country life in the eighteenth century. The North-East, 1700-1750.** London: Oxford U.P. (Reprinted 1969).

Humphreys-Jones, C.E. (1986) Make, make do and mend: the role of the hearer in misunderstandings, IN McGregor, G. (ed.) **Language for hearers.** p. 105 ff.

Humphreys-Jones, C.E. (1987) **An investigation of the types and structure of misunderstandings.** Unpublished Ph.D. thesis, University of Newcastle upon Tyne.

Hunn, E.S. (1977) **Tzeltal folk zoology: the classification of discontinuities in nature.** London: Academic.

Hunt, E.B., Marin, J., & Stone, P.J. (1966) **Experiments in induction.** N.Y.: Academic.

Hymes, D. (1962) The ethnography of speaking, IN Gladwin, T. & Sturtevant, W.C. (eds.) **Anthropology & human behaviour.** Washington, D.C.: Anthropol. Soc. of Washington. p. 13 ff.

Hymes, D. (1964) Toward ethnographies of communication, IN Hymes, D. (ed.) **The ethnography of communication.** (Special publication of **American Anthropologist,** 66 (6(2)).)

Hymes, D. (1972) On communicative competence, IN Pride, J. & Holmes, J. (eds.) **Sociolinguistics: selected readings.** London: Penguin. p. 269 ff.

Hymes, D. (1990) Review of Dittmar, N. & Schlobinski, P. (1988) (eds.) **The sociolinguistics of urban vernaculars: case studies and their evaluation.** (Soziolinguistik und Sprachkontakt 1(1).) Berlin: de Gruyter **Lg. Soc.,** 19 (1), p. 117 ff.

Illich, I. (1973) **Celebration of awareness: a call for institutional revolution.** London: Penguin.

Irwin, R. (1978) **100 Geordie jokes.** Newcastle upon Tyne: Frank Graham

Jackson , D.M. & White, L.J. (1971) The weakening of taxonomic inferences by homological errors, **Math. Biosciences,** 10,

p.63 ff.
Janicki, K. (1989) A rebuttal of essentialist sociolinguistics, **York Papers in Linguistics,** 13, p. 167 ff.
Jardine, C.F., Jardine, N. & Sibson, R. (1967) The structure and construction of taxonomic hierarchies, **Math. Biosciences,** 1, p.173 ff.
Jardine, N. & Sibson, R. (1968a) A model for taxonomy, **Math. Biosciences,** 2, p.465 ff.
Jardine, N. & Sibson, R. (1968b) The construction of hierarchic and non-hierarchic classifications, **Computer J.**, 11, p.177 ff.
Jardine, N. & Sibson, R. (1971a) **Mathematical taxonomy.** N.Y.: Wiley.
Jardine, N. & Sibson, R. (1971b) Choice of methods for automatic classification, **Computer J.**, 14(2), p. 404 ff.
Jarman, E. & Cruttenden, A. (1976) Belfast intonation and the myth of the fall, **J. Intl. Phon. Assn.**, 6, p. 4 ff.
Johnson, L.A.S. (1970) Rainbow's end: the quest for an optimal taxonomy, **Systematic Zool.**, 19(3), p.203 ff.
Jones, D. (1976)[9] **An outline of English phonetics.** London: Cambridge U.P.
Jones, D (1972)[4] **The pronunciation of English.** London: Cambridge U.P.
Jones, V.M. (1978) **Some problems in the computation of sociolinguistic data.** Unpublished Ph.D. thesis, University of Newcastle upon Tyne.
Jones-Sargent, V.M. (1983) **Tyne bytes: a computerised sociolinguistic study of Tyneside.** Frankfurt am Main: Lang. [= Jones, V.M. 1978]
Joos, M. (1950) Description of language design, **J. Acoust. Soc. Amer.**, 22, p. 701 ff.
Joos, M. (1957) (ed.) **Readings in linguistics.** Washington, D.C.: American Council of Learned Societies.
Katz, J.J. & Fodor, J.A. (1963) The structure of a semantic theory, **Lg.**, 39, p. 170 ff.
Kaur, Daljit (1984) **Determine the extent to which different hearers assign different meanings to a given remark.** (Academic exercise submitted in partial

fulfilment of the requirements for the degree of B.A.Hons. in English Lang.) Singapore: National University Dept. of English.

Kelly, G.A. (1955) **The psychology of personal constructs.** N.Y.: Norton.

Kelly, J. & Local, J.K. (1980) **Is creole pulse-timed?** Preprint.

Kendall, D.G. (1971) Construction of maps from "odd bits of information", **Nature**, 231, p. 158 ff.

Kendall, M.G. (1957) **A course in multivariate analysis.** London: Griffin.

Kendall, M.G. (1966) Discrimination and classification IN Krishnaiah, P.R. (ed.) **Multivariate analysis.** N.Y.: Academic. p. 165 ff.

Kendall, M.G. & Stuart, A. (1967)^2 **The advanced theory of statistics Vol II.** London: Griffin.

Kendall, M.G. & Stuart, A. (1968)^2 **The advanced theory of statistics Vol III.** London: Griffin.

Kendrick, W.B. (1964) Quantitative characters in computer taxonomy IN Heywood, V.H. & McNeill, J. (eds.) **Phenetic and phylogenetic classification.** London: Systematics Association. p. 105 ff.

Kendrick, W.B. & Proctor, J.R. (1964) Computer taxonomy in the fungi imperfecti, **Canadian J. Botany,** 42, p. 65 ff.

Kerswill, P.E. (1983) **Style shifting in the speech of Durham City adolescents & adults: research proposal.** Mimeo.

Kerswill, P.E. (1987) Levels of linguistic variation in Durham, **J.Ling.**, 23, 1, p. 25 ff.

Kerswill, P. & Wright, S. (1989) On the limits of auditory transcription: a sociophonetic approach, **York Papers in Linguistics**, 14, p. 35 ff.

Kierkegaard, S. (1959) **Either/ Or, Vol II** (transl. Lowrie, W.) N.Y.: Anchor.

Kingdon, R. (1959) **The groundwork of English intonation.** London: Longman.

Klima, E. (1964) Relatedness between grammatical systems, **Language,** 40, p. 238 ff.

Knowles, G. (1978) The nature of phonological variables in

Scouse IN Trudgill, P. (ed.) **Sociolinguistic patterns in British English.** London: Arnold. p. 80 ff.

Kohler, K.J. (1967) Structural dialectology, **Zeitschrift für Mundartforschung, 34,** pp. 40 ff.

Kreckel, M. (1981) **Communicative acts and shared knowledge in natural discourse.** London: Academic.

Kruskal, J.B. (1964a) Multidimensional scaling: a numerical method, **Psychometrika,** 29, p.1 ff.

Kruskal, J.B. (1964b) Multidimensional scaling by optimizing goodness of fit to a nonmetric hypothesis, **Psychometrika,** 29, p.115 ff.

Kurath, H. (1939) **Handbook of linguistic geography of New England.** Washington, D.C.: Am. Council of Learned Societies.

Labov, W. (1964) Phonological correlates of social stratification IN Gumperz, J.J. & Hymes, D. (eds.) **The ethnography of communication.** Menasha, Wisc.: Amer. Anthrop. Assoc. (= Special Publ. **Amer. Anthropol.**, 66, 6 (2)) p.164 ff.

Labov, W. (1966) **The social stratification of English in New York City** . Washington,D.C.: Center for Applied Linguistics.

Labov, W. (1972) Some principles of linguistic methodology, **Lg. Soc.**, 1, p. 97 ff.

Ladd, D.R. (1980) **The structure of intonational meaning.** Bloomington: Indiana U.P.

Ladefoged, P.(1960) The value of phonetic statements, **Lg.**, 36, p. 387 ff.

Laing, R.D. (1967) **The politics of experience** and **Bird of Paradise.** Harmondsworth: Penguin.

Laing, R.D. (1970) **Knots.** London: Penguin.

Laing, R.D. (1982) What is the matter with mind? IN Kumar, S. (ed.) **The Schumacher Lectures.** London: Sphere. p. 1 ff.

Lakatos, I. (1976) **Proofs & refutations: the logic of mathematical discovery.** (Eds. Worrall, J. & Zahar, E.) London: C.U.P.

Lakoff, R. (1972) Language in context, **Lg.**, 48 , p. 907 ff.

Lance, G.N. & Williams, W.T. (1966a) A generalised sorting strategy for computer classifications, **Nature,** 212, p. 218.

Lance, G.N. & Williams, W.T. (1966b) Computer programs for

hierarchical polythetic classification ("similarity analyses"), **Computer J.**, 9, p. 60 ff.

Lance, G.N. & Williams, W.T. (1966c) A general theory of classificatory sorting strategies: I Hierarchical systems, **Computer J.**, 9, p. 373 ff.

Lance, G.N. & Williams, W.T. (1967) A general theory of classificatory sorting strategies: II Clustering systems, **Computer J.**, 10, p. 271 ff.

Lance, G.N. & Williams, W.T. (1971) A note on a new divisive classificatory program for mixed data, **Computer J.**, 14, p.154 ff.

Langendoen, D.T. (1968) **The London School of Linguistics: a study of the linguistic theories of B. Malinowski and J.R. Firth.** Cambridge, Mass.: M.I.T.

Langendoen, D.T. & Postal, P.M. (1984) **The vastness of natural languages.** Oxford: Blackwell.

Langer, S.K. (1967) **Mind: an essay on human feeling Vol I** Baltimore: Johns Hopkins U.P.

Langer S.K. (1972) **Mind: an essay on human feeling Vol II** Baltimore: Johns Hopkins U.P.

Lass, R. (1984) **Phonology.** Cambridge: U.P.

Lawrence, D.H. (1981) **Apocalypse.** (Ed. Kalnins, M.) London: Granada.

Lazarsfeld, P. (1962) Philosophy of science and empirical social research IN Nagel, E., Suppes, P., & Tarski, A. (eds.) **Logic, methodology and philosophy of science.** California: Stanford U.P. p. 463 ff.

LePage, R.B. (1976)**The multidimensional nature of sociolinguistic space, illustrated from the Sociolinguistic Survey of Multilingual Communities Stage I (Cayo District, Belize) and II (St. Lucia).** [Abstract]. York: University Department of Language.

LePage, R.B. (1980) Projection, focussing, diffusion, **York Papers in Linguistics,** 9, p. 9 ff.

LePage, R.B. (1989) What is a language ?, **York Papers in Linguistics,** 13, p. 9 ff.

LePage, R.B., Christie, P., Jurdant, B., Weekes, A.K., & Tabouret-Keller, A. (1974) Further report on the

sociolinguistic survey of multilingual communities: Survey of Cayo District, British Honduras, **Lg. Soc.**, 3, p. 1 ff.

LePage, R.B. & Tabouret-Keller, A. (1985) **Acts of identity: Creole-based approaches to language and ethnicity.** Cambridge: U.P.

Lewin, K. (1936) **Principles of topological psychology.** NY: McGraw-Hill.

Li, T.-Y. & Yorke, J. (1984) Period three implies chaos, IN Hao, B.-L. (ed.) **Chaos.** p.244 ff.

Liberman, A.M., Cooper, F.S., Shankweiler, D.P. & Studdert-Kennedy, M. (1967) Perception of the speech code, **Psych. Rev.**, 74, p. 431 ff.

Lieberman, P. (1967) **Intonation, perception, and language.** Cambridge, Mass.: MIT.

Lim Tong Li, C. (1983) **What arguments can be marshalled in favour of the view that hearers are creative ?** (Academic Exercise submitted in partial fulfilment of the requirements for the degree of B.A Hons. in English Lang.) Singapore: Nat. Univ. Dept. of English.

Lin, P.J. (1977) **A translation of Lao Tzu's Tao Te Ching and Wang Pi's Commentary.** Ann Arbor: University of Michigan Center for Chinese Studies (= Michigan Papers in Chinese studies No. 30).

Ling, R.F. (1972) On the theory and construction of k-clusters, **Computer J.**, 15(4), p. 326 ff.

Local, J.K. (1978) **Studies towards a description of the development and functioning of children's awareness of linguistic variability.** University of Newcastle upon Tyne, unpublished PhD thesis

Local, J.K. (1979) Modelling intonational variability in children, **York Papers in Linguistics**, p. 61 ff.

Local, J.K. (1981) **Making a transcription: the evolution of A.J. Ellis's palaeotype.** Preprint of article for Jnl. Intl. Phon. Assn.

Local, J.K. (1982a) **Modelling intonational variability in children's speech.** Preprint.

Local, J.K. (1982b) **How many vowels in a vowel?** Preprint.

Local, J.K. (1982c) **Patterns and problems in a study of**

Tyneside intonation. Preprint.
Local, J.K. & Brown, K. (1981) Interpreting phonetic variability in children's speech, **Work in Progress (Edinburgh University, Dept. Linguistics),** 14, p. 39 ff.
Local, J.K. & Pearson, M. (1979) Some physical properties of three English tones, **York Papers in Linguistics,** p. 207 ff.
Local, J.K. & Wells, W.H.G. (1981) **You don't have to resort to syntax.** Preprint.
Lock, G. (1988) **Variation, norms and prescribed standard in the Mandarin Chinese spoken in Singapore.** Unpublished Ph.D. thesis, University of Sydney.
Lockhart, W.R. & Hartmann, P.A. (1963) Formation of monothetic groups in quantitative bacterial taxonomy, **J.Bacteriology,** 85, p. 68 ff.
Lorenz, E.L. (1984) Deterministic nonperiodic flow, IN Hao, B.-L., (ed.) **Chaos.** p.282 ff.
Loy, D. (1988) **Nonduality: a study in comparative philosophy.** New Haven: Yale U.P.
Lucas, J.R. (1973) **A treatise on time and space.** London: Methuen.
Lumsden, R.A. (1983a) p.c.
Lumsden, R.A. (1983b) Riggs, IN **Focus.** Singapore: Nat. Univ. Literary Soc.
Lumsden, R.A. (1984) Appointment by parable, IN Nicholson, C. & Chatterji, R. (eds.) **Tropic crucible: self and theory in language & literature.** Singapore: U.P. p. 347
Lumsden, R.A. (1989) **Deconstruction, the bare bones.** T/s.
Lumsden, R.A. & Pellowe, J. (1986) The nature of listening in reading poetry: a conversation, IN McGregor, G. & White, R.S. (eds.) **The art of listening.** p.102 ff.
Lyons, J. (1962) Phonemic & non-phonemic phonology, **Intl. J. Amer. Ling.,** 28, p.127 ff.
Lyons, J. (1977) **Semantics Vols. I & II.** Cambridge: U.P.
Macafee, C. (1989) Qualitative insights into working-class language attitudes, **York Papers in Linguistics,** 13, p. 191 ff.
Macbeth, N. (1974) **Darwin retried.** London: Garnstone

Malone, J.M. (1975) p.c.
Malone, J.M. (n.d.) **Toward a general method of assessing salience of explanatory factors.** (N.p.) Mimeo.
May, R. (1984) Simple mathematical models with very complicated dynamics, IN Hao, B.-L., (ed.) ***Chaos.*** *p.149 ff.*
McCormick, K. (1989) Unfiltered talk - a challenge to categories, **York Papers in Linguistics,** 13, p. 203 ff.
McCullers, Carson (1975) **The mortgaged heart.** London: Penguin.
McEntegert, D. & Le Page, R.B. (1982) An appraisal of the statistical techniques used in The Sociolinguistic Survey of Multilingual Communities, IN Romaine (ed.) , p. 105 ff.
McGregor, G. (1983) Listeners' comments on conversation, **Lg. & Comm.,** 3 (3), p. 271 ff.
McGregor, G. (1986) (ed.) **Language for hearers.** Oxford: Pergamon Press.
McGregor, G. & White, R.S. (1986) (eds.) **The art of listening.** Beckenham, Kent: Croom Helm.
McIntosh, A. (1952) **An introduction to a survey of Scottish dialects.** Edinburgh: Nelson.
McIntosh, A. & Halliday, M.A.K. (1966) (eds.) **Patterns of language: papers in general, descriptive & applied linguistics.** London: Longmans.
McNaughton-Smith, P. (1965) **Some statistical and other numerical techniques for classifying individuals.** [No. 6 in Home Office series Studies in the causes of delinquency and the treatment of offenders]. London: H.M.S.O.
McNeany, V. (1971) **Unpublished data and analysis.** (To establish prosodic criteria for a classification of speech varieties. Cf. Pellowe (1970d).) M/s.
McNeany, V. (1972) **Vowel reduction in localised Tyneside and R.P. speech.** Newcastle upon Tyne: University School of English (mimeo).
McQuitty, L.L. (1966) Similarity analysis by reciprocal pairs for discrete and continuous data, **Educ. & Psych. Meas.,** 26 (4), p.825 ff.
McQuitty, L.L. (1967a) A mutual development of some typological theories and pattern analytic techniques, **Educ. & Psych.**

Meas., 27 (1), p. 21 ff.
McQuitty, L.L. (1967b) Expansion of similarity analysis by reciprocal pairs for discrete and continuous data, **Educ. & Psych. Meas.**, 27 (2) p. 253 ff.
McQuitty, L.L. (1967c) A novel application of the coeffiecient of correlation in the isolation of both typological and dimensional constructs, **Educ. & Psych. Meas.**, 27 (3), p. 591.
McQuitty, L.L. (1968) Improving the valididty of crucial decisions in pattern analytic methods, **Educ. & Psych. Meas.**, 28 (1), p. 9 ff.
McQuitty, L.L. & Clark, J.A. (1968a) Clusters from iterative intercolumnar correlational analysis, **Educ. & Psych. Meas:**, 28 (2), p. 211 ff.
McQuitty, L.L. & Clark, J.A. (1968b) Hierarchical classification by reciprocal pairs of course selections in psychology, **Educ. & Psych. Meas.**, 28 (3) p. 659 ff.
Meyerson, E. (1930) **Identity and reality.** (Transl. Loewenberg, K.) London: Allen & Unwin.
Milroy, J. (1982) Probing under the tip of the iceberg: phonological 'normalization' and the shape of speech communities, IN Romaine (ed.), p. 35 ff.
Milroy, J. (1989) The concept of prestige in sociolinguistic argumentation, **York Papers in Linguistics,** 13, p. 215 ff.
Milroy, L. (1982) Social network and linguistic focusing, IN Romaine (ed.), p. 141, ff.
Milroy, L. (1984) Urban dialects in the British Isles, IN Trudgill, P. (ed.), p. 199 ff.
Milroy, L. (1989) Gender as a speaker variable: the interesting case of the glottalised stops in Tyneside, **York Papers in Linguistics,** 13, p. 227 ff.
Minkoff, E.C. (1965) The effects on classification of slight alterations in numerical technique, **Systematic Zool.**, 14, p.196 ff.
Minsky, M. (1963) Steps toward artificial intelligence IN Feigenbaum, E.A. & Feldman, J.F. (eds.) **Computers and thought.** N.Y.: McGraw Hill. p. 406 ff.
Minsky, M. (ed.) (1968) **Semantic information processing.**

Cambridge, Mass.: M.I.T.

Minsky, M. (1975) A framework for representing knowledge. IN Winston, P.H. (ed.) **The psychology of computer vision.** N.Y.: McGraw Hill. p. 211 ff.

Minsky, M. & Papert, S. (1969) **Perceptrons: an introduction to computational geometry.** Cambridge,Mass.: MIT.

Mitchell, T.F. (1975) **Principles of Firthian Linuistics.** London: Longmans.

Moore, G.E. (1939) Proof of the external world, **Proc. Brit. Academy,** XXV, reprinted IN Moore, G.E. (1959) **Philosophical papers.** London: Allen & Unwin.

Morgan, B.J.T. (1973) Cluster analysis of two acoustic confusion matrices. **Perception & psychophysics,** 13, p.13 ff.

Morris, R.N. (1968) **Urban sociology.** London: Allen & Unwin.

Morton, J. (1964) A preliminary functional model for language behaviour, **Intl. Audiology,** 3, p. 216 ff.

Moser, C.A. & Kalton, G. (1971)[2] **Survey methods in social investigation.** London: Heinemann.

Moser, C.A. & Scott, W. (1961) **British towns: a statistical study of their social & economic differences.** Edinburgh: UP

Moss, W.W. (1971) Taxonomic repeatability: an experimental approach, **Systematic Zool.,** 20, p.309 ff.

Moulton, W.G. (1960) The short vowel systems of Northern Switzerland, **Word,** 16, p. 155 ff.

Needham, R.M. (1961a) **Research into information retrieval, classification and grouping.** University of Cambridge PhD thesis (mimeo).

Needham, R.M. (1961b) **Theory of clumps II.** Cambridge: Cambridge Language Research Unit. (Rept. M.L. 139 (mimeo).)

Needham, R.M. (1965a) Computer methods for classification and grouping, IN Hymes, D. (ed.) **The use of computers in anthropology.** The Hague: Mouton. p. 168 ff.

Needham, R.M. (1965b) Automatic classification: models and problems, IN Medical Research Council (ed.) **Mathematics and computer science in biology and medicine.** London: H.M.S.O. p. 111 ff.

Needham, R.M. (1965c) Application of the theory of clumps,

Mechanical Translation, 8 (3/4), p. 37 ff.
Needham, R.M. (1967) Automatic classification in linguistics, **The Statistician,** 17 (1), p. 45 ff.
Needham, R.M. & Sparck-Jones, K. (1964) Keywords and clumps, **J. Documentation,** 20, p. 5 ff.
Neumann, J. von (1958) **The computer and the brain.** New Haven, Conn.: Yale U.P.
Nixon, G. (1970) **Unpublished MA dissertation.** University of Newcastle upon Tyne.
Nixon, G. (1972) Measuring corpus adequacy, **Archivum linguisticum,** 3 (N.S.), p.101 ff.
Nolan, F. (1987) Review of Alan Cruttenden **Intonation** Cambridge: U.P. **J. Ling.** 23 (2), p. 455 ff.
Norman, D.A. (1970) (ed.) **Models of human memory.** NY: Academic.
Norwich, K.E. (1983) An entropic theory of perception, **J. Theoretical Biol.,** 102, p. 175 ff.
NRPC (1967) **Mobility and the North, Vols 1-3.** Newcastle: North Regional Planning Committee.
O'Brien, F.(1967) **The third policeman.** London: MacGibbon & Kee.
O'Toole, S. (1970) **Confessions of an American scholar.** Minneapolis: Minnesota U.P.
Oller, D.K. & Eilers, R.E. (1975) Phonetic expectation and transcription validity, **Phonetica,** 31, p. 288 ff.
Oppenheim, A.N. (1966) **Questionnaire design and attitude measurement.** London: Heinemann.
Ornstein, R.E. (1972) **The psychology of consciousness.** San Francisco: Freeman.
Ornstein, R.E. (1973) (ed.) **The nature of human consciousness.** San Francisco: Freeman.
Orton, H. & Dieth, E. (1962) **Survey of English dialects: introduction.** Leeds: E.J. Arnold.
Pahl, R.E. (1968) (ed.) **Readings in urban sociology.** London: Pergamon.
Pahl, R.E. (1970) **Patterns of urban life.** London: Longmans
Palmer, F.R. (1968) (ed.) **Selected papers of J.R. Firth 1952-1959.** London: Longmans.

P'ang Yun (1971) **A man of Zen: the recorded sayings of Layman P'ang, a ninth century Zen classic.** (Transl. from Chinese by Sasaki, R.F., Iriya, Y., & Fraser, D.R.) N.Y.: Weatherhill.

Pankhurst, R.J. (1970) A computer program for generating diagnostic keys, **Computer J.**, 13(2), p. 145 ff.

Parker-Rhodes, A.F. (1978) **Inferential semantics.** Hassocks, Sussex: Harvester.

Paul, H. (1891)[2] **Principles of the history of language.** (Transl. Strong, H.A.) London: Longmans.

Payne, S.L. (1951) **The art of asking questions.** Princeton, N.J.: U.P.

Pears, D. (1975) **Questions in the philosophy of mind.** London: Duckworth.

Peitgen, H.-O. & Richter, P.H. (1986)(eds.) **The beauty of fractals.** Berlin: Springer Verlag.

Pellowe, J. (1967) **Studies towards a classification of varieties of spoken English based on specimens collected in Jesmond, Newcastle upon Tyne.** Unpubished M. Litt. thesis, University of Newcastle upon Tyne.

Pellowe, J. (1970a) **Establishing speech varieties of conurbations: I Theoretical position.** Newcastle: University School of English. [Also reprinted IN McGregor, G. (ed.) **Language for hearers.** p. 25 ff.]

Pellowe, J. (1970b) **Establishing speech varieties of conurbations: II Criteria & sampling.** Newcastle: University School of English.

Pellowe, J. (1970c) **Establishing speech varieties of conurbations: III Varieties as constructs.** Newcastle: University School of English.

Pellowe, J. (1970d) **Establishing some prosodic criteria for a classification of speech varieties.** Newcastle: University School of English.

Pellowe, J. (1973) A problem of diagnostic relativity in the Tyneside Linguistic Survey. **Classification Soc. Bull.**, 3 (1), p. 2 ff.

Pellowe, J. (1974a) **Unpublished data and analysis.** (To

calibrate the agreements and differences between prosodic analysts. M/s; now lost.)

Pellowe, J. (1974b) **Tyneside Linguistic Survey - Abstract.** [for: Colloquium on empirical work in Sociolinguistics LSE 27 April 1974]. Newcastle: University School of English.

Pellowe, J. (1976) The Tyneside Linguistic Survey: aspects of a developing methodology IN Viereck, W. (Hrsg.) **Sprachliches Handeln - Soziales Verhalten ein Reader zur Pragmalinguistik und Soziolinguistik.** Munchen: Wilhelm Fink. p. 203 ff. & p. 365 ff.

Pellowe, J. (1980) Establishing variant intonational systems, **York Papers in Linguistics,** 8, p. 97 ff. (First circulated in mimeo Jan. 1978.)

Pellowe, J. (1984) For our selves we are silent IN Nicholson, C. & Chatterji, R. (eds.) **Tropic crucible: self and theory in language and literature.** Singapore: U.P. p. 37 ff.

Pellowe, J. (1986) Hearers' intentions IN McGregor, G. & White, R.S. (eds.) **The art of listening.** p. 5 ff.

Pellowe, J. (1990) Who is context ? IN McGregor, G. & White R.S. (eds.) **Reception and response: hearer creativity and the analysis of spoken and written texts.** London: Routledge. p. 69 ff.

Pellowe, J. (in preparation, a) **Intonational variation: a sociolinguistics for individuals in groups.**

Pellowe, J. (in preparation, b) **The challenge of 'the good listener' to linguistics.**

Pellowe, J. & Jones, V. (1977) **On intonational variability in Tyneside speech.** Newcastle: University School of English.

Pellowe, J. & Jones, V. (1978a) On intonational variability in Tyneside speech IN Trudgill, P. (ed.) **Sociolinguistic patterns in British English.** London: Arnold. p. 101 ff.

Pellowe, J. & Jones, V. (1978b) **Representing and intepreting the structure of linguistic variation.** (Unpublished paper delivered at Vth International symposium on Literary & Linguistic Computing, University of Aston, Birmingham, UK, April 3-7,1978.)

Pellowe, J. & Jones, V. (1979a) Establishing intonationally

variable systems in a multidimensional linguistic space, **Lg. & Speech,** 22(2), p. 97 ff.

Pellowe, J. & Jones, V. (1979b) **Interim report to Social Science Research Council on Project HR 5490/1: Tyneside Linguistic Survey Phase 4.** Typescript.

Pellowe, J., Nixon, G., & McNeany, V. (1972a) **Defining the dimensionality of a linguistic variety space.** Newcastle: University School of English.

Pellowe, J., Nixon, G., & McNeany, V. (1972b) Some sociolinguistic characteristics of phonetic analysis IN Rigault, A. & Charbonneau, R. (eds.) **Proc. VII Intl. Cong. Phon. Sci.** (Montreal 1971). The Hague: Mouton. p. 1172 ff.

Pellowe, J., Nixon, G., Strang, B., & McNeaney, V. (1972) A dynamic modelling of linguistic variation: the urban (Tyneside) linguistic survey. **Lingua,** 30 (1), p. 1 ff.

Penfield, W. (1975) **The mystery of mind: a critical study of consciousness and the human brain.** Princeton: Princeton U.P.

Penfield, W. & Jasper, H. (1954) **Epilepsy and the functional anatomy of the human brain.** Boston: Little Brown.

Percy, Walker (1987) The Loss of the Creature IN Bartholomae, D. & Petrosky, A. (eds.) **Ways of reading.** NY: St. Martin's. p. 394 ff.

Perec, G. (1988) **Life: a user's manual.** (Transl. Bellos, D. from (1970) **La vie: mode d'emploi,** Paris: Hachette.) London: Collins Harvill.

Personal Counselors Inc. (1982)[4] **Fundamentals of co-counselling manual.** Seattle, Washington: Rational Island Publishers.

Peters, R. (1956) **Hobbes.** London: Penguin.

Phillips, E.M. & Pugh, D.S. (1987) **How to get a PhD: managing the peaks and troughs of research.** Milton Keynes: Open University.

Pielou, E.C. (1969) **An introduction to mathematical ecology.** N.Y.: Wiley.

Pilch, H (1977) Intonation in discourse analysis, **Phonetica,** 34, p. 81 ff.

Pittenger, R.E., Hockett, C.F. & Danehy, J.J. (1960) **The first**

five minutes. New York: Martineau.
Polanyi, M. (1958) **Personal knowledge: towards a post-critical philosophy.** London: Routledge.
Popper, K.R. (1959) **The logic of scientific discovery.** London: Hutchinson.
Popper, K.R. (1963) **Conjectures and refutations.** London: Routledge & Kegan Paul.
Popper, K.R. (1966)[5] **The open society & its enemies: Vol II The high tide of prophecy: Hegel, Marx, and the aftermath.** London: Routledge & Kegan Paul.
Popper, K.R. (1972) **Objective knowledge: an evolutionary approach.** Oxford: Clarendon.
Popper, K.R. & Eccles, J.C. (1977) **The self and its brain.** Berlin: Springer.
Postal, P. (1968) **Aspects of phonological theory.** N.Y.: Harper & Row.
Postal, P. (1972) The best theory IN Peters, S. (ed.) **Goals of linguistic theory.** Englewood Cliffs, NJ: Prentice Hall p. 131 ff.
Preston, F.W. (1962) The canonical distribution of commonness and rarity, I & II, **Ecology,** 43, p. 185 ff & p. 410 ff.
Pribram, K.H. & Broadbent, D.E. (1970) (eds.) **The biology of memory.** New York: Academic.
Prigogine, I. & Stengers, I. (1985) **Order out of chaos. Man's New Dialogue with Nature.** London: Fontana.
Proctor, J.R. & Kendrick, W.B. (1963) Unequal weighting in numerical taxonomy, **Nature,** 197, p. 716 ff.
Proust, M. (1971) **On Reading.** (Transl. from **Sur la lecture** by Autrec, J. & Burford, W.) London: Condor.
Pulgram, E. (1964a) Proto-languages as proto-diasystems: proto-Romance, **Word,** 20, p. 373 ff.
Pulgram, E. (1964b) Structural comparison, diasystems and dialectology, **Linguistics,** 4, p. 66 ff.
Putnam, H. (1983) A philosopher looks at quantum mechanics, IN Colodny, R.G. (ed.) p. 75 ff.
Quirk, R. (1960) The Survey of English Usage, **T.Phil.Soc.** p. 40 ff.
Quirk, R. (1965) Descriptive statement and serial relationship,

Lg., 41 (2), p. 205 ff.
Quirk, R. & Crystal, D. (1966) On scales of contrast in connected English speech IN Bazell, C.E., et al. (eds.) **In memory of J.R. Firth.** London: Longmans. p. 359 ff.
Quirk, R., Svartvik, J., Duckworth, A.P., Rusiecki, J.P.L., & Colin, A.J.T. (1964) Studies in the correspondence of prosodic to grammatical features in English IN Lunt, H. (ed.) **Proc. X Intl. Cong. Ling.** The Hague: Mouton. p. 679 ff.
Raine, K. (1985) **Defending ancient springs.** Cambridge: Golgonooza.
Ramanujan, A.K. (1973) (transl. & ed.) **Speaking of Siva.** London: Penguin.
Rampton, M.B.H. (1989) Group affiliation & quantitative sociolinguistics, **York Papers in Linguistics,** 13, p. 279 ff.
Rapoport, A. (1963) Mathematical models of social interaction, IN Luce, R.D., Bush, R.R. & Galanter, E. (eds.) **Handbook of mathematical psychology, Vol. 2.** New York: Wiley. p. 493 ff.
Renwick, E. (1972) p.c.
Reps, P. (1971) **Zen flesh, zen bones.** London: Penguin.
Rich, A. (1980) Women and honour: some notes on lying, IN Rich, A. (ed.) **On lies, secrets and silence: selected prose 1966-1978.** London: Virago. p. 185 ff.
Rijsbergen, C.J. van (1971) An algorithm for information structuring and retrieval, **Computer J.**, 14(4), p.407 ff.
Ringaard, K. (1965) The phonemes of an area perceived by phoneticians and by the speakers themselves IN Zwirner, E. & Bethge, W. (eds.) **Proc. V Intl. Cong. Phon. Sci.** Basel: Karger. p. 495 ff.
Roberts, C. (1974) **The scientific conscience: reflections on the modern biologist and humanism.** Fontwell, Sussex: Centaur.
Robson, B.T. (1969) **Urban analysis: a study of city structure.** London: Cambridge U.P.
Rogers, D.J. & Tanimoto, T. (1960) A computer program for classifying plants, **Science,** 132, p. 1115 ff.
Romaine, S. (1981) The status of variable rules in

sociolinguistic theory, **J. Linguistics,** 17 (1), p. 93 ff.
Romaine, S. (1982) (ed.) **Sociolinguistic variation in speech communities.** London: Arnold.
Ross, J.R. (1975) Clausematiness, IN Keenan, E.L. (ed.) **Formal semantics of natural language.** London: Cambridge U.P. p. 422 ff.
Ruelle, D. & Takens, F. (1984) On the nature of turbulence, IN Hao, B.-L. (ed.) **Chaos.** p. 119 ff.
Sacks, O. (1982) **Awakenings.** London: Picador.
Sacks, O. (1986a) **A leg to stand on.** London: Picador.
Sacks, O. (1986b) **The man who mistook his wife for a hat.** London: Picador.
Saltarelli, M. (1966) Romance dialectology and generative grammar, **Orbis,** 15, p. 51 ff.
Sampson, G. (1973) The concept of 'semantic representation'. **Semiotica,** VII (2), p. 97 ff.
Sampson, G. (1979) **Liberty and language.** Oxford: U.P.
Sampson, G. (1980) **Making sense.** Oxford: U.P.
Sandvid, D. (1964) **Mair Geordie Taalks.** Newcastle upon Tyne: Harold Hill.
Sankoff, D. & Rousseau, P. (1974) A method for assessing variable rule and implicational scale analyses of linguistic variation. IN Mitchell, J.L. (ed.) **Computers in the humanities.** Edinburgh: U.P. p. 3 ff.
Sapir, E. (1921) **Language.** N.Y.: Harcourt Brace.
Sapon, S.M. (1953) A methodology for the study of socio-economic differentials in linguistic phenomena, **Studies in linguistics,** 11, p.57 ff.
Sapon, S.M. (n.d.) **An experimental verification of some aspects of socio-linguistic theory.** (Paper to general phonetics section, M.L.A. of America, Dec1956.) N.p.; mimeo.
Sartre, J.-P. (1957) **Being and nothingness: an essay on phenomenological ontology.** (Transl. with an intro. by Hazel Barnes.) London: Methuen.
Sartre, J.-P. (1962) **Imagination: a psychological critique.** Ann arbor: Michigan U.P.
Schank, R. (1973) Identification of conceptualizations underlying natural language IN Schank, R. & Colby, K.M.

(eds.) **Computer models of thought and language**. San Francisco: Freeman. p. 187 ff.

Schwarz, J. (1985) **Superstrings.** Singapore: World Scientific.

Sekida, K. (1977) **Two zen classics: Mumonkan and Hekiganroku.** NY: Weatherill.

Shaw, M.L.G. (1980) **On becoming a personal scientist.** London: Academic.

Shepard, R.N. (1963) Analysis of proximities as a technique for the study of information processing in man, **Human Factors**, 5, p.33 ff.

Shields, A.F. (1974) **Work on judgements made by hearers.** Newcastle: University School of English. (Mimeo.)

Shields, A.F. (1975) **Notes from a fortuitous data file.** Newcastle: University School of English

Sibson, R. (1972) Order invariant methods for data analysis, **J. Royal Stat. Soc. (B)**, 34. p.311 ff.

Silverman, D. (1970) **The theory of organisations.** London: Heinemann.

Simon, H.A. (1970) **The sciences of the artificial.** Cambridge, Mass.: M.I.T.

Simpson, G.G. (1980) **Why and How: some problems and methods in historical biology.** Oxford: Pergamon.

Skeat, W.W. (1911) **English dialects.** Cambridge: U.P.

Slagle, J.R. (1971) **Artificial intelligence: the heuristic programming approach.** NY: Prentice Hall.

Sledd, J. (1983) In defense of the *Students' Right*, **College English,** 45, p. 667 ff.

Smith, M.J. (1975) **When I say No I feel guilty: how to cope using the skills of systematic assertive therapy.** London: Bantam.

Smith, N.V. (1982) (ed.) **Mutual knowledge.** London: Academic.

Sneath, P.H.A. (1957a) Some thoughts on bacterial classification, **J.Gen.Microbiology,** 17, p.184 ff.

Sneath, P.H.A. (1957b) The application of computers to taxonomy, **J.Gen.Microbiology,** 17, p. 201 ff.

Sneath, P.H.A. (1967) Trend surface analysis of transformation grids, **J. Zool. (London),** 151, p. 65 ff.

Sneath, P.H.A. (1972) p.c.

Sneath, P.H.A. & Sokal, R.R. (1973)[2] **Numerical taxonomy: the principles and practice of numerical classification.** San Francisco: Freeman. (A much transformed Sokal & Sneath 1963.)

Sokal, R.R. (1962) Typology & empiricism in taxonomy, **J. Theoret. Biol.**, 3, p. 230 ff.

Sokal, R.R. & Michener, C.D. (1958) A statistical method for evaluating systematic relationships, **U.Kansas Sci. Bull.**, 38, p. 1409 ff.

Sokal, R.R. & Michener, C.D. (1967) The effects of different numerical techniques on the phenetic classification of bees of the hoplitis complex (Megachilidae), **Proc. Linnaean Soc. London,** 178, p.59 ff.

Sokal, R.R. & Rohlf, F.J. (1966) Random scanning of taxonomic characters, **Nature (Lond.)**, 210, p. 461 ff.

Sokal, R.R. & Sneath, P.H.A. (1963) **Principles of numerical taxonomy.** London: Freeman.

Sparck Jones, K. & Jackson, D. (1967) Current approaches to classification and clump-finding at the Cambridge Language Research Unit, **Computer J.**, 10, p. 29 ff.

Sparck Jones, K. & Needham, R.M. (1968) Automatic term classifications and retrieval, **Information Storage & retrieval**, 4, p. 91 ff.

Stein, G. (1971)(ed. Meyerowitz, P.) **Look at me now, and Here I am: writings and lectures 1909-1945.** London: Penguin.

Stein, G. (1988 [1935]) **Lectures in America.** London: Virago.

Stevens, K.N. (1960) Toward a model for speech perception, **J. Acoust. Soc. Amer.**, 32, p. 47 ff.

Stevens, W. (1954) **The Collected Poems.** NY: Knopf.

Stevens, W. (1972) (ed. Holly Stevens) **The Palm at the End of the Mind.** NY: Vintage.

Straight, H.S. (1986) The importance and irreducibility of the comprehension/production dialectic, IN McGregor, G. (ed.) **Language for hearers.** p. 69 ff.

Strang, B.M.H. (1968) The Tyneside Linguistic Survey. **Zeitschrift fur Mundartforschung,** p. 788 ff.

Strang, B.M.H. (1969) p.c.

Strevens, P.D. (1964) Varieties of English, **English Studies,** 45, p. 20 ff.

Svartvik, J. (1966) **On voice in the English verb.** The Hague: Mouton.

Taylor, T.J. (1986) Do you understand ? Criteria of understanding in verbal interaction, IN McGregor, G. (ed.) **Language for hearers.** p. 91 ff.

Thelander, M. (1982) A qualitative approach to the quantitative data of speech variation, IN Romaine (ed.), p. 65 ff.

Thompson, E.P. (1980) **Writing by candlelight.** London: Merlin.

Thompson, W. D'Arcy (1942)[2] **On growth and form.** Cambridge: U.P.

Toulmin, S. & Goodfield, J. (1965) **The discovery of time.** London: Hutchinson.

Trudgill, P. (1970) p.c.

Trudgill, P. (1972) Sex, covert prestige and linguistic change in the urban British English of Norwich, **Lg. Soc.**, 1, p. 179 ff.

Trudgill, P. (1974) **The social differentiation of English in Norwich.** London: Cambridge U.P.

Trudgill, P. (1978) (ed.) **Sociolinguistic patterns in British English.** London: Arnold.

Trudgill, P. (1984) (ed.) **Language in the British Isles.** Cambridge: U.P

Tulving, E. & Donaldson, W. (1972) (eds.) **Organisation of memory.** London: Academic.

Voegelin, C.F., Voegelin, F.M., Wurm, S., O'Grady, S. & Matsuda, T. (1963) Obtaining an index of phonological differentiation from the construction of non-existent minimax systems, **Intl. J. Amer. Ling.**, 29 (1), p.4 ff.

Voegelin, C.F. & Yegerlehner, J. (1956) The scope of whole-system ('distinctive feature') and subsystem typologies, **Word,** 12, p. 444 ff.

Warburton, R.E. (1967) The purposes of classification, **Systematic Zool.,** 16, p. 241 ff.

Ward, J.H. (1963) Hierarchical grouping to optimize an objective function, **J. Am. Stat. Soc.**, 58, p.236 ff.

Wathen-Dunn, W. (1967) (ed.) **Models for the perception of speech and visual form.** Cambridge, Mass.: MIT.

Watson, Lyall (1976) **The Romeo error: a matter of life and death.** London: Coronet.

Watson, L., Williams, W.T. & Lance, G.N. (1966) Angiosperm taxonomy: a comparative study of some novel numerical techniques, **J. Linnaean Soc. (Botany)**, 59, 380, p. 491 ff.

Watts, A. (1987) **The wisdom of insecurity.** London: Century.

Weinreich, U. (1954) Is a structural dialectology possible? **Word,** 10, p. 388 ff.

White, P. (1959) **Voss.** London: Readers Union, Eyre & Spottiswoode.

White, P. (1974) **Riders in the Chariot.** London: Penguin (Modern Classics).

White, R.S. (1979) p.c.

White, R.S. (1982) **Innocent victims: poetic injustice in Shakespearean Tragedy.** Newcastle: Tyneside Free.

White, R.S. (1986) Shakespeare and the listener, IN McGregor, G. & White, R.S. (eds.) **The art of listening.** p.124 ff.

Whitehead, A. N. (1929) **Science and the modern world.** Cambridge: U.P.

Whitrow, G.J. (1981)^2 **The natural philosophy of time.** Oxford: Clarendon.

Williams, Heathcote (1988) **Whale Nation.** London: Cape.

Williams, W.T., Dale, M.B. & MacNaughton-Smith, P. (1964) An objective method of weighting in similarity analysis, **Nature**, 201, p. 426 ff.

Williams, W.T. & Gifford, H.T. (1971) On the comparison of two classifications of the same set of elements, **Taxon**, 20(4), p. 519 ff.

Williams, W.T. & Lambert, J.M. (1959) Multivariate methods in plant ecology: I Association-analysis in plant communities, **J.Ecology**, 47, p. 83 ff.

Williams, W.T. & Lambert, J.M. (1960) Multivariate methods in plant ecology: II The use of an electronic digital computer for association-analysis, **J. Ecology**, 48, p.689 ff.

Williams, W.T. & Lambert, J.M. (1961) Multivariate methods in plant ecology: III Inverse association analysis, **J. Ecology**, 49, p. 717 ff.

Williams, W.T. & Lambert, J.M. (1962) Multivariate methods in

plant ecology: IV Nodal analysis, **J. Ecology**, 50, p. 775 ff.
Winograd, T. (1983) **Language as a cognitive process Vol I Syntax.** London: Addison-Wesley.
Wirth, L. (1938) Urbanism as a way of life, **Amer. J. Sociol.**, 44, p. 1 ff.
Wishart, D. (1969) **Fortran II programs for 8 methods of cluster analysis (CLUSTAN 1).** (=Computer contribution 38.) Lawrence, Kansas: State Geological Survey, Kansas University.
Wittgenstein, L. (1961[1922]) **Tractatus logico-philosophicus.** [Transl. Pears, D.F. & McGuinness, B.F.] London: Routledge & Kegan Paul.
Wittgenstein, L. (1958)^2 **Philosophical investigations.** [Transl. Anscombe, G.E.M.] Oxford: Blackwell.
Wittgenstein, L. (1969) **On certainty.** [Eds. Anscombe, G.E.M. & Wright, G.H.; transl. Paul, D. & Anscombe, G.E.M.] Oxford: Blackwell.
Woods, W.A. (1970) Transition network grammars for natural language analysis, **Comm. A.C.M.**, 13 (10), p.591 ff.
Wright, J.T. (1966) Urban dialects: a consideration of method, **Zeitschrift für Mundartforschung**, 33, p. 232.
Yates, F. (1948) Systematic sampling, **Roy. Soc. Phil. Trans.**, A 241, p. 345 ff.
Yates, F. (1960)^2 **Sampling methods for censuses and surveys.** London: Griffin.
Zadeh, L.A. (1965) Fuzzy sets, **Information & Control**, 8, p. 338 ff.
Zadeh, L.A. (1972) A fuzzy-set-theoretic interpretation of linguistic hedges, **J. Cybernetics**, 2 (3), p. 4 ff.
Zadeh, L.A. (1973) The concept of a linguistic variable and its application to approximate reasoning, IN Fu, K.S. & Tou J.T. (eds.) **Learning systems and intelligent robots.** N.Y.: Plenum. p. 1ff.
Zadeh, L.A. (1977) Fuzzy sets and their application to pattern clarification and cluster analysis, IN Ryzin, J. van (ed.) **Classification and clustering**. N.Y.: Academic p. 215 ff.
Zadeh, L.A. (1979) Fuzzy systems theory - a framework for analysis of humanistic systems, IN Cavallo, R.E. (ed.)

Recent developments in system methodology for social science research. Amsterdam: Martinus Nijhoff p. 96 ff.

Zwicky, A.M., Salus, P.H., Binnick, R.I. & Vanek, A.L. (1971)(eds.) **Studies out in left field: defamatory essays presented to J.D.McCawley on the occasion of his 33rd or 34th birthday.** Edmonton, Canada: Linguistic Research Inc.