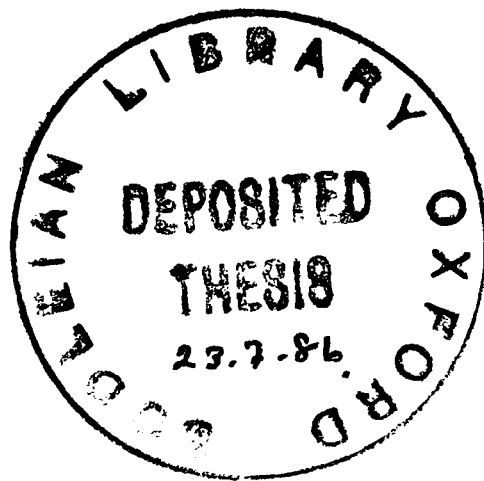


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**A Comparative Study of Phonetic Sex-specific
Differences Across Languages**



A thesis submitted to the University of Oxford,
in fulfilment of the regulations for the degree
of Doctor of Philosophy

by

C.G. Henton, B.A., M.Phil.

December 1985 [12/1986]

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A Comparative Study of Phonetic Sex-specific Differences Across Languages

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ABSTRACT

Extensive reviews of phonetic and phonological investigations into sex-related differences reveal a mottled history. The investigations suffer from methodological and theoretical deficits: the most serious being the misrepresentation of the interaction between variables, a lack of homogeneous data and its misinterpretation, and the widespread neglect of women's speech.

Existing phonetic databases are shown to be inadequate and poorly-controlled, admitting too many unwanted variables. A very tightly-controlled database, constructed for this research, contains data for eighty female and male speakers of two accents of British English. This contribution is regarded as important per se. Digital acoustic analysis of the data permits quantification of the phonetic divergence shown by the sexes in British English.

Previous attempts to normalize the acoustic effects of speaker-sex on vowels have been largely unsuccessful. Here, the application of an innovative auditory normalization procedure reflects how perceptual normalization may be achieved. It further demonstrates that male/female phonetic differences remain after normalization, which cannot be accounted for by anatomy, but are accountable by social-role conditioning (i.e. learned). These differences are statistically significant. Speaker-sex and gender are thus shown to interact at the phonetic level.

Extending this technique to five other languages/dialects corroborates the central hypothesis that the degree to which the sexes diverge phonetically will vary from speech-community to speech-community. Exploration of the possibility that contoids will reveal similar systematicity shows this to be unlikely across languages. The examination of suprasegmental sex-associated differences, however, merits further pursuit.

Implications of these experimental findings are discussed for, inter alia, speech technology, language-planning and medical aids. Using sex-linked differential voice quality as a springboard, it is suggested that sex-appropriate norms are required in speech pathology. The need for socio-phonetics to be recognized as an important new discipline is thus underlined.

EXTENDED ABSTRACT

Faculty of Medieval and Modern Languages

The main objectives of this thesis are:

1. Solutions for the phonetic normalization of female and male vowels across languages/dialects.
2. The construction of an adequate methodology and database from which to examine phonetic and phonological differences in female and male speech.
3. The establishment of the need for a **socio-phonetic** approach to the description of not only sex-linked speech differences but also normative phonetic data in general.

The first issue revolves around attempts to model the way in which the listener transforms a diversity of incoming phonetic tokens (from, for example, different phonetic environments or different spoken occasions, or from a male or a female) into a single phonetic quality comparable across languages or dialects. This is termed the **normalization** process. Previous attempts, using articulatory and acoustic procedures, are shown to be inadequate because their solutions are either non-unique or non-uniform across vowels.

This research thus describes, implements and validates an innovative **auditory normalization algorithm**, which is based on the assumption that it replicates closely the perception of a sound, i.e. how it is analyzed by the ear and brain. The procedure is applied firstly to data from closely-controlled homogeneous populations of speakers of two accents of British English (Received Pronunciation and Modified Northern). All informants were selected with great attention to possible sociolinguistic variables, including age, education, domicile, occupation, state of health, stature and, of course, sex. They were recorded in sound-treated conditions, without the object of the investigation being made known to them. The sex of the investigator (a possible influential variable) was kept constant throughout. In the two recording locations, technical specifications were exactly duplicated. Original data is now stored to avoid all degradation and forms a unique database for further empirical investigation. The instrumental analysis of this data is also innovative in that it departs from conventional spectrography and uses, by preference, digital narrow-band spectral analysis and computational techniques. The improvement in accuracy is duly pointed out, while certain residual (and vowel-specific) problems are also acknowledged.

Analyses were obtained for the formants and fundamental frequency of 880 vowels in citation context, and 560 sentence-context forms. These were stored on computer files and the means and standard deviations were then calculated and converted from Hertz to the Bark scale: the scale which is understood to represent critical auditory bandwidths. The normalization algorithm was then applied, and the results evaluated. Conventional non-parametric statistical tests are used to establish the significance of the results. These include analyses of variance, t-test and coefficient of variation.

The results indicate that among speakers of Modified Northern there is an outstanding lack of parity after normalization. This imbalance is construed to be constrained by conditions outside the physiological, i.e. as originating in socially-determined role differences for the sexes. The other accent (RP), however, reveals statistically insignificant inter-sex differences after normalization. To a limited extent we are thus able to support the notion that RP is the accent which incorporates the **fewest sex-specific traits** (a notion which has not been tested phonetically before). Essentially, then, we are now able to give precise quantified account of the perceptually-valid amounts by which the sexes differ in two accents of British English.

In addition to these central findings, there are important disclosures about the greater variability of female speakers compared with males; the use of one vowel as a **socio-phonetic indicator of degrees of accentual accommodation**, and warnings about the possibilities of data-collection and measurement-induced artefacts. Preliminary results are also presented concerning the influence of context on vowel production in British English. Initial hypotheses are made furthermore concerning sex-specific behaviour in the control of the fundamental frequency of vowels. These limited results indicate fruitfulness for their further empirical examination, but these lie outside the scope of the present work.

When the same auditorily-based normalization process was applied to data for General American, English, Standard and Utrecht Dutch, and Parisian French, a diversity of results appeared. It was found that the disparity between the normalized females' and males' vowels varies from language-to-language and from accent-to-accent. Support was thus firmly supplied for the original research hypotheses that (i) a difference between females and males would remain after the application of the normalization procedure, and (ii) male/female speech differences differ from speech-community to speech-community. Stemming from these cross-language results, there is greater phonetic exploration of Labov's (1972) description of female speakers being more 'open-mouthed' than males. The conclusion on this matter

was that the term must be accent-specific and cannot be used universally.

The second main concern of this thesis was the establishment of an improved methodology from which to make phonological, phonetic and socio-phonetic inferences. Detailed compilations of phonological and phonetic studies of sex-linked variants establish that there is manifold evidence for differing female/male speech behaviour at these levels. An extensive discussion of the literature devoted to phonological and phonetic sex-linked variation, shows however that there are frequent and serious shortcomings in both the early (pre-Labovian) and later (post-1960) work. In the phonological paradigm, there was often too great a reliance in the early work on introspection, intuition, phonetic imprecision, and a Eurocentric and androcentric bias. Because much 'evidence' was reported by non-linguists, the question also arose of whether the differences were phonemic or allophonic, sex-exclusive or sex-preferential. Phonetic investigations suffer from general inattention to the influence of social and biographical factors, viz. socio-economic status, occupation, age, race, sex and accent of the informants. The majority of 'representative' accounts of phonetic inventories are based on male speakers and are obtained from too small a number of subjects. This research attempts to highlight the importance of female speakers' data and indicate that this neglect is not an automatic consequence of the research tools employed.

Three specific attempts are made to **improve the methodological deficits** by (i) emphasizing that phonological variant-investigation must be much more sensitive to culture-specific norms; (ii) arguing that women currently cannot be assessed using sociolinguistic criteria for SES; (iii) setting out specific criteria for phonetic databases (which also take account of the international requirements of speech technology), and (iv) constructing the British English database accordingly. These contributions are regarded as central to the originality of this thesis. En route, we also engage in particular exploration of Labov's (1972 inter alia) assertions that middle-class women are the most active in the process of language change, while at the same time are more prone to conservatism in language and hypercorrection. The certainty and cross-cultural applicability of these widely-accepted beliefs are challenged, and alternative interpretations are offered. Two new concepts are introduced to better describe the linguistic behaviour of females: the **chameleon model** and women's **active** participation in conversation rather than being mere prey to passive accommodation.

In the latter parts of this thesis, the viability of the auditory normalization procedure is tested further by considering its application to segments other than vocoids.

Contoids are subsequently shown to be less amenable to normalization (in general, and not only using an auditory technique), due to a variety of linguistic and acoustic reasons. Many suggestions are made however for the positive expansion of sex-linked investigation at the **suprasegmental level**, especially with regard to speaker pitch and intonation.

Further experimental work illustrates that there is good reason to suspect **sex-specific differential use of voice quality**, at least in British English. A study of breathiness in the two accents of British English indicates that the diagnosis of pathological or healthy voice requires a socio-phonetic set of criteria. Such results should also be heeded by those engaged in synthesizing female voices.

The third strand of research was the argument for the necessity of **socio-phonetics**. Throughout this thesis, there are numerous instances of the **gender** of the speaker being ignored or considered irrelevant. We argue however that that not only should phonetic data collection proceed with the incorporation of speaker-specific variables, but it should also take specific regard of the effects of social expectations made on the role of the particular speaker's sex, i.e. their gender. Foregoing linguistic and psychological research has been at pains to tease these two concepts apart: the derived clarification of the terms is offered at the outset of the thesis. By the end of this investigation, however, we hope to have indicated that **both sex and gender** should be regarded as important variables in phonetic research.

The validity of this research is additionally borne out by the numerous and varied applications which are exemplified in the last chapter. The usefulness of these socio-phonetic results may be incorporated into frameworks for language teaching/learning. Language-planning may benefit too, with further underpinning of the idea that women and men may vary in their degrees of bilingualism, of conservatism and innovation, and in code-switching behaviour. Medical applications include the refinement of diagnoses of psychological illness based on phonetic evidence, and sex-specific aids for the handicapped. Commercial exploitation in advertising strategies is also foreseen. The most immediate area of extrapolation from these results is that of automatic speech recognition. Voice-actuated devices clearly have to take sex-of-speaker into account: the indications are that an auditorily-based solution would be rewarding. Sex-linked phonetic constraints would further help in these recognition tasks.

The study of sex-specific speech differences is disseminated among many different disciplines. The focus here has been on the sociolinguistic, the phonological and the phonetic. Within these areas, degrees of explanatory success are

examined. In order to demonstrate the need for a more vigorous empiricism, many of the shortcomings of previous investigations, from the historical to the recent sociological, have been discussed. The division of sex (biological given) and gender (social role) has been especially underscored at the phonological and phonetic levels.

Socio-phonological studies have, on the whole, eliminated women because of classificatory elusiveness. Compared with other linguistic strata, phonetic research has been guilty of concealing women more than most other 'levels' of analysis. Instrumentally troublesome, they were consigned to dialectal 'obscurity'. What is needed now is a precise matrix for the assignation to women of a real identity for purposes of academic description. This pre-empts the notion that women can be allotted a general character on the basis of anatomical attributes alone. It is a dichotomy in culture which gives rise to these differences, but there is no reason not to wish for an androgynous unmarked reality.

For the phonetic investigation undertaken here, women were brought into the glare of daylight. Ensuing results reveal that there is impressive experimental evidence for a learned, socially-determined element in speech. Moreover, it can be shown that shifts of the order of those occurring in womens' vowels are often large enough to yield phonological category changes. The position of women in the processes of language change is thus central.

Sex-linked differences, it can be predicted, will vary from culture to culture, but the main point is that they should be analyzed, acknowledged and acted upon. A phonetic approach offers useful results: it is now the responsibility of further research to amplify the "distant voice in the darkness."

In brief, we may state that this research has successfully borne out the initial research hypotheses, has implemented original experimental methodological techniques and theories, has challenged previously-accepted phonological and phonetic dogma, and that the applications foreseen have fully justified the investigation.

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Chapter 1

Preliminaries, the past and proposals

This thesis takes as goals four main issues. Firstly, it seeks to establish an adequate methodology from which to proceed in the observation of sex-based phonetic and phonological variants. Secondly, it argues the need for a recognized socio-phonetic discipline. Thirdly, it describes the exact phonetic nature of some female-male differences of pronunciation. Fourthly, it indicates how such findings as those presented here are important to the advance of applied phonetics and linguistics.

This first chapter clarifies some essential preliminary issues, provides a background to the study of sex and language, exemplifies some previous work and delineates my precise research hypotheses.

1.1. Sex- or gender-linked language variation?

This terminological statement should not consume much space, since the issues have been argued clearly elsewhere (Bodine, 1975; Key, 1975; Kramarae, 1981; Cameron, 1985b). My decision to use the term **sex-related** rather than **gender-related** nevertheless needs a brief explanation.

Until the twentieth century, reports on language varying according to whether the speaker was female or male referred unequivocally to the sex of the speaker. This usage implied that it was biological fact which influenced the speakers' utterances. Slowly, though, it became apparent that the qualities of female-ness and male-ness were intricately bound to social structures, and that cultures construct

societal norms for sexes which go beyond biological differences (Mead, 1949: 8). Thus the concept of gender, and, in turn, gender-linked variation, was introduced. Gender then refers to the social expression and presumptions of feminine and masculine behaviour.

This new labelling was not entirely unproblematic. The term gender, when used in the context of linguistic discussion, can be too easily conflated and confused with **grammatical gender**, i.e. categories used for the analysis of word classes displaying such contrasts as masculine/feminine/neuter; animate/inanimate. Various debates (revealing remarkable bias) have brewed with regard to there being any/no connection between grammatical gender and sex: these are evaluated and criticized by Cameron (1985b). The obfuscation of the two is summarised by Miller and Swift (1976: 68-69):

When grammarians use the word gender they know they are referring to a convention that may overlap sex or may have nothing to do with it. When psychiatrists use the word gender, they, too, assign it a specific meaning that can match sex, although it does not necessarily do so. At the risk of over-simplification, sex in both contexts is a biological given: gender is a social acquisition. Yet most people, including most grammarians and psychiatrists, occasionally confuse the two terms and by using them as though they were synonymous, semantically blur a biological given with something that is socially induced.

Jan Morris (1974), (herself a well-known exemplar of the sex/gender confusion) elucidates the situation:

...to me gender is not physical at all... Male and female are sex, masculine and feminine are gender, and though the conceptions obviously overlap, they are far from synonymous.

So, sex is a matter of anatomy and gender is "destiny" (McConnell-Ginet, 1979: 67), reflecting cultural views about the importance of a person's sex.

This thesis is concerned with aspects of male/female differences in pronunciation. The differences will be influenced by two factors:

- (a) the vocal physiology of the speaker, which, in adults, is determined by their biological sex and size;
- (b) the cultural expectations of female and male behaviour in certain linguistic societies.

We thus need to take account of **both** sex and gender. The experimental results of this investigation argue that while sex (as it determines the anatomical dimensions of the vocal tract) is of course influential in speech production, it is **not a sufficient explanation** for phonetic variation in females' and males' speech behaviour: therefore, gender must also be implicated. Our discussion focusses on both physiologically-induced differences, and societal expectations; but because phonetic work conducted hitherto has taken into account only a speaker's sex (and even that is infrequent), it is 'sex' which will be used throughout. The reasons for this choice are founded on accuracy, brevity and precedent, but one certain objective of this thesis is to demonstrate that gender must also be incorporated into phonetic methodological practice and theory.

1.2. Speech by, about, or associated with females and males?

The expansion of the study of sex-associated language differences has taken four main directions. The most straightforward (but, ironically, most neglected) area of research is into productional differences in the speech of females and males. This has been mostly the concern of linguists and educational psychologists, and involves the analysis of actual speech samples obtained under a variety of conditions. As Smith (1979: 110) says from his social psychologist's standpoint:

... the heuristic value of sex as an explanatory variable in sociolinguistics is ... an empirical issue, which involves a close examination of the processes whereby our ideas about social groups originate, stabilize and evolve.

Other researchers, however, have been anxious to obtain insights into social behaviour, by examining peoples' views **about male and female language**. This should, of course, be recognized as differing from language used **about males and females**, which again has provided interesting information relating more to the way in which females and males are evaluated per se, and the origins of sexism in language, rather than about language use. Finally, many psychologists have concerned themselves with the **perception** of male/female speech characteristics: whether listeners' reactions to certain voice qualities are consistent, whether they extend across languages, and whether **stereotypes** reflect reality as well as cultural attitudes (see Kramer, 1977, for a review, and Smith, 1979: 137, calling for more investigation into

the relationship between markers and stereotypes). All four strands of sex-linked language studies are fully referenced in the excellent annotated bibliography in Thorne, Kramarae and Henley (1983), and the sources relating to aspects outside my specific research need not be duplicated here. A review of the literature pertaining to differences in phonetic and phonological behaviour of females and males appears as appropriate in the course of this thesis.

The research reported here was conducted in the one area of the four which is conspicuously undersupplied with quantifiable endeavours: namely, speech production by females and males. The phonetic results for seven languages/dialects are duly presented in Chapters 6 and 7. Further inferences about female and male language will benefit from such data because the experimental base will be firmly secured. That stage of investigation lies outside the scope of this thesis, however.

1.3. Sex-linked variation: types of explanation

Four approaches in the history of sex-related pronunciation differences may be recognised:

1. Subjective judgement (influenced by contemporary prejudice)
2. Connection to taboo
3. Connection to the economic relations of the sexes in a society
4. Equation with physiological differences between the

sexes.

The first three approaches may be seen to be linked: the authors who provide such analyses as the first are likely to be influenced not only by superstition about the sexes overtly expressed in tribal societies, but also by the attitudes about the comparative roles and statuses of the sexes current in their own society (i.e. the third type of analysis). The three types still need to be separated, nevertheless, because the third generally arrives at such a link after consideration of experimental data, while the first takes the cause for variation as an a priori given, and the second is not always as explicit as the third in Western societies. The fourth type is self-explanatory, and is amplified in Section 1.7. A clear understanding of these approaches is vital because, as McConnell-Ginet (1975:44) explains:

...to distinguish actual, imagined, and desired sex-differences in verbal behaviour is essential if one is to avoid the common error of conflating reality with myth, observation with ideal.

Let us next discuss further the first class of explanation.

1.4. Early studies: misconceptions and misogyny

This section offers some historical sources for remarks on sex-specific pronunciation, so that we may demonstrate by the end of this thesis that (a) distinct empirical advance has been effected, but, at the same time, that (b) traces of the early misconceptions may still be found in our contemporary literature.

The shared factor in all the early reports is that they were made by Western white males: it is thus probable that they reflect an androcentric bias. To add to their deficiencies, they were often introspective and vague, lacking in systematic documentation, and founded on preconceived ideas about which qualities were **desirable** in language.

Chronologically, the first set of sex-linked observations about language derive from accounts by various ethnographers and travellers, who were concerned with producing taxonomies of 'exotic' languages. Indeed, Furfey (1944: 218) virtually pre-empts the examination of languages 'nearer to home' by claiming that sex contrasts "were barely discernible in the familiar languages of Europe". The accuracy of this assertion (which is echoed by Smith, 1985: 9) will be critically scrutinized in the course of this thesis. Jespersen (1922) provides a selection of early references to sex-related differences in languages, which include Smith (1567), Mulcaster (1582), Gill (1621), Swift (1735), Lord Chesterfield (1754) and Greenough and Kittredge (1901). These commentators referred however to lexical and grammatical differences and are thus excluded from focus here.

The most famous (?infamous) of the sources listed by Jespersen (1922: 237), which does cover apparent phonological differences, is that by the Dominican missionary, Wilhelm Breton (1664). Breton's preliminary account of 'sex dialects' among the Carib Indians is

expanded by Rochefort (1665). While this case is usually cited (see Kraus, 1924; Bodine, 1975, inter alia) as the earliest allusion to sex-linked languages, the same set of facts has suggested several different analyses, all of which can be usefully related to linguists' implicit ideology of gender. Rochefort (1665) attributed the split between the male and female languages on the island to the extermination by the invading Caribs of the male native inhabitants (the Arawaks). The Arawak females were spared however for progenitive purposes. The two sexes then passed on their respective original language to their offspring, with mothers speaking Carib to daughters and fathers Arawak to sons. Now, this may have been true; but, with hindsight, it is more plausible to posit that the different 'languages' (which were, in fact, more like two registers of the same language) evolved not in connection to biological sex, but more to **taboo** and **gender role**, i.e based on the division of labour. The relationship to taboo will be discussed in Section 1.5, and gender-influenced interpretations are the subject of Section 1.6.

Before illustrating more rational explanations for sex-related language differences (Section 1.5 and following), we should include some further indication of the subjective comments mentioned above. Apart from reporting other authors, Jespersen also made significant contributions of his own to the linguistic misogynistic literature. Firstly, he placed his chapter "The Woman" in among other sections

like "The Foreigner", and "Causes of Change", with the implication that women's speech is similarly 'outside' 'normal' language. More particular barbs include calling women's language the "small change" of talk, and stating that (1922: 247):

Men will certainly with great justice object that there is a danger of the language becoming languid and insipid if we are always to content ourselves with women's expressions, and that vigour and vividness count for something.

Furthermore, of the languages known to Jespersen,

English is the most positively and expressly masculine...(with) very little childish or feminine about it.

Linguistic anthropomorphism and downright silliness apart, Jespersen was also responsible for many notions which have slid unquestioned into expectations and beliefs about female language. For example, women are more conservative and more innovative, although Jespersen fails to remark on the paradox: see discussion in Chapter 3. Jespersen ricochets between women being prisoners of language, conservative because they mix less with outsiders and other cultures, and women being less educated, less exposed to written forms (which were deemed to be the standard to be upheld), and therefore likely to invent language. Either way, the result is a Batesonian double-bind, for the language of women is compared to the assumed norm, viz. male language.

Women, according to Jespersen, use more intensifiers and euphemism; shy away from 'coarse' expressions; exploit more parataxis rather than hypotaxis; due to their "flighty

nature", they speak more readily than men, with less precision of thought; and, finally, have a less extensive vocabulary. Jespersen's stereotypes have been taken up directly by Bernard (1972); Farb (1973); Key (1972) and Pei (1973), among others. Subsequent empirical testing has proved most of these assertions to be founded on prejudice rather than fact, but still the stereotypes flourish. Indeed, Lakoff (1975) may be seen as a worthy successor to Jespersen, since she has argued from introspection about several patterns in women's speech. The ideas were not exceptionally fresh in the first instance, nor do they resist close scrutiny, and they consistently have not been empirically corroborated (e.g. Dubois and Crouch, 1975; Erikson et al., 1977; Edelsky, 1979a). In fact, we can but only agree with McConnell-Ginet's thorough demolition of Lakoff's 'intuitions', and most particularly with her statement (1975: 44), that:

...using oneself as informant...is inadequate for studying systematic linguistic variation among the speakers of a large and diverse speech community.

Ultimately, Jespersen is too easy a target to continue sniping at, and we should turn to other early sources of sex-linked linguistic comment. Swift (1712), for example, determined that, phonetically, women are the more mellifluous speakers:

...if the Choice had been left to me, I would rather have trusted the Refinement of our Language, as far as it relates to sound, to the Judgement, of Women, than of illiterate Court-fops, half-witted Poets, and University Boys. For, it is plain, that Women, in their Manner of corrupting Words, do naturally discard

the Consonants, as we do the Vowels...more than once, where some of both Sexes were in Company, I have persuaded two or three of each to take a Pen, and write down a Number of Letters joined together, just as it came into their Heads; and upon reading this Gibberish we have found that which Men had writ, by the frequent encountering of rough Consonants, to sound like High-Dutch; and the other by the Women, like Italian, abounding in Vowels and Liquids. Now, although I would by no means give Ladies the Trouble of advising us in the Reformation of our Language; yet I cannot help thinking, that since they have been left out of all Meetings, except Parties at Play, or where worse designs are carried out, our Conversation hath very much degenerated.

The totally unsatirical flavour of this directive cannot be eschewed, but we will note with irony in the course of this thesis that women have continued to be "left out" of phonetic data as recently as 1982 (c.f. Chapter 4). The tenor of Swift's remarks is duplicated in comments by Michaelis (1768, and cited by Morgan, 1979), who believed that women had:

those pretty mouths, which the graces seem to animate, and whose every word meets with echoes, delighting to repeat it.

It is tempting to dismiss these early comments as historically quaint and retrospectively amusing. This thesis will show, however, that such a judgement would be flippant, and we shall recall the words of Swift, Michaelis and Jespersen, noticing how frequently the same sort of tone is found in contemporary work. Despite the early reporters' ill-qualifications to make linguistic inferences, they were responsible for some 'received wisdom' which pervades sociolinguistic theory today. Such comments imply largely unstated principles such as:

- (i) disparities in the speech of females and males may occur due to **competence** (rather than performance) differences between the sexes
- (ii) women are conservatives, who uniformly use **standard language** and prestige forms more than males
- (iii) the object of linguistic/sociolinguistic research is **language spoken by males**
- (iv) the **standard language** is that spoken by **males**.

Therefore females' speech was/is **differently coded**, conservative and standard-preserving, but (at the same time!) non-standard, and somehow deviant or abnormal (see further comments in Chapters 3 and 4).

The first of the 'principles' has, fortunately, withered in credibility, although one still comes across lingering prejudiced allusions to women not being able to understand or express certain notions. Vestiges still appear of the Victorian physicians' belief that 20% of women's creative energy (brain work) was diverted by their physiological functions, and indeed anthropologists writing at the same time thought that women were more stupid than men because the frontal lobes of male brains were heavier and more developed than females' (Showalter, 1981: 187). It may be possible that certain rules are never acquired by/present in by one sex or the other, but it has not been empirically demonstrated.

Following Jespersen's vacillation and unrealized paradoxes

about women being both conservatives and innovators, the early accounts portray women not only as conservatives, but also supply women's 'playfulness' as a reason for innovation (Lasch, 1907: 100):

Daneben ist auch des Spieltriebes nicht zu vergessen, welcher namentlich beim weiblichen Geschlechte das Kindesalter häufig Überdauert und bei einigen Frauen-sprachen sicher das Hauptmotiv abgegeben hat.

Later, we have an explicit report by Keenan (1974) that Madagascan women are norm-breakers, i.e. sever tradition, break rules and express anger openly (c.f. (ii) above). In Keenan's work, it is revealed that the stereotype overrides nevertheless, and despite women's speech being recognized as more straightforward, men's speech is still regarded as more skillful.

What we may ultimately infer is that the simple division into conservatism or innovation is simplistic (see Chapter 3), and whatever behaviour women exhibit, they will be marked for it, solely because they are women.

Evidence of the belief in type (iii) can be found in Labov (1972: xiii):

There is a growing realization that the basis of intersubjective knowledge in linguistics must be found in speech - language as it is used in everyday life by members of the social order, that vehicle of communication in which they argue with their wives, joke with their friends, and deceive their enemies.

Sociolinguistics may have benefitted greatly from Labov's pronouncements, but as an emerging discipline thirteen years ago, it was still content to assume that the subjects of interest were males. Hence, Kramer (1977:151) judged that:

The questionableness of research stemming from such assumptions and principles has served as a major research impetus for the study of actual sex-based differences in speech behaviour...

Previously, Conklin (1973) noted astutely that women's vernacular had not received any specific study, and indicated the need for special examination of the dialect of all-female interactions. Her suggestion has been rapidly adopted in the area of conversational interaction (see large number of entries in the bibliography in Thorne, Kramarae and Henley, 1983) but not at all in phonetics.

The deviant characteristic (iv) was made explicit by Sapir (1915: 181), who, in describing Yana and Nootka, placed speech in the presence of women firmly in the **abnormal category**, among the types employed to:

...children, unusually fat or heavy people, unusually short adults, those suffering from some defect of the eye, hunchbacks, those that are lame, left-handed persons and circumcised males.

This kind of categorization is compounded by such references as are cited by Key (1975: 23), where women are grouped with dogs in Islamic notices, and elsewhere, with the mentally disturbed, minors and convicts. Chapter 3 takes the discussion of type (iv) much further. Certainly it needs to be said at the outset that the ignoring, and classification of females as abnormal in phonetics, was "a major impetus" in the genesis of this thesis.

This section has portrayed the historical background of sex-linked studies, and indicated pervasive stereotypes and research attempts based on them. More serious investigation

of sex-linked language variation arose as part of the civil rights movement in the United States, of feminism, and of the growth of sociolinguistics as a separate discipline (but see comments above). This renewed interest occurred in the early 1970s, and since then the area has burgeoned, with contributions from linguists, anthropologists, psychologists, sociologists, educationalists, biologists and lettrists (see volumes by Key, 1975; Thorne and Henley, 1975; Miller and Swift, 1976; Dubois and Crouch, 1976; Eakins and Eakins; and, most informative, Thorne, Kramarae and Henley, 1983, for an impressive overview of the literature, by subject). The common question posed in these studies is 'what are the implications for society if women and men indeed speak differently?' Many researchers have attempted investigative replies, but remarkably few contributions have appeared from **phoneticians**. Generally, as Chapter 4 shows, women have been not only excluded, but ignored in both descriptive and experimental phonetics: this is because they have been accepted as the unusual, the abnormal.

1.5. Sex-related language and taboo

The women say, the language you speak poisons your glottis tongue palate lips.

Monique Wittig Les Guerillères

The second type of connection which was mentioned at the beginning of this chapter, was between sex-linked language and taboo. Superstition, taboo and kinship were (and

perhaps are?) the prime motivators in the creation of women's languages. That such a tie exists is not surprising, since social taboos revolve around fears and inexplicable events or objects, e.g. death, failures, sex, kinship, power and money. Originally, women were regarded with superstition and fear because of their child-bearing ability - lack of understanding being translated into suspicion and then oppression. They were also constantly equated with witchcraft and the ability to mysteriously affect events with their deeds and words. Although effectively cordoned off in society by their reproductive abilities, it was still necessary to restrict their 'magical' verbal powers. Kraus (1924: 301) offers four explanations for the origins of secret languages connected to taboo, viz: historical, social, religious and psychological. Predictably, she concludes that all four are influential, but because of the influence of Freud at the time of her writing, she argues primarily for the last reason.

Women, so the literature reveals (Lasch, 1907; Jespersen, 1922; Kraus, 1924;), also qualify frequently to be included in the list of taboo subjects. For example, Frazer (1900: 360) reports that some Australian aborigines had:

...a dread of passing under a leaning tree or even under the rails of a fence (because) a woman may have been upon the tree or fence, and some blood from her may have fallen on it and might fall from it on to them.

Many other cross-cultural horrors relating to menstrual

blood are cited by Frazer. Other taboo subjects linked to women's bodies are conception, pregnancy, childbirth, and anatomical parts per se.

From these biologically-conditioned taboos evolved linguistic taboos. Thus, one's fortunes were not only damaged by coming into contact with a woman (for fear of contamination), but superstitions also developed about women's speech. A fear of the mention of gods (who were largely perceived as male) was extended to the mention of men and their activities. Frazer (1900: 413-414), in his extensive account of 'Reluctance to utter Names of Relations', reports for example that:

...among the Caffres of South Africa a woman may not publicly pronounce the birth-name of her husband or of any of his brothers, nor may she use the interdicted word in its ordinary sense...a Caffre wife is forbidden to pronounce even mentally the names of her father-in-law and of all her husband's male relations in the ascending line...

Predictably, this outlawing has occasioned an almost distinct language, or 'women's speech'. The taboo is not limited to the Caffres, but appears in women from Northern Nyasaland (now Malawi); the Barea and Bogos of East Africa; the Kirghiz, Ojebways, Dacotas and many more tribes listed by Frazer (p.413 ff.), together with the Nuer from Sudan (Evans-Pritchard, 1965). Should phonemes or syllables occurring in a taboo name occur in any other word, then women must employ synonyms, elision, metathesis, paraphrase and euphemism. Jakobson and Waugh (1979: 208-209) provide some language-specific instances of phonological alternation

dictated by taboo. This linguistic censorship was even carried over to the mention of the words for food by the female inhabitants of Nkondeland (Lasch, 1907: 99) and 'house, garden, bed, poison, tree, sun, moon, sea and earth' by the Caribs (Jespersen, 1922: 238). Thus worldwide women have been forced to create secret languages to discuss the simplest subjects, because their tongues have been designated poisonous and dangerous. A classic case of virtue out of necessity is thus produced, if women are hailed as 'creative/innovative' in language use.

A further and predictable taboo area giving rise to dichotomy is sexual references and parts of the body. Lasch (1907) describes a 'Geheimnissprache' in East Africa employed by women especially for the discussion of "women's troubles/matters". The similar language employed among women in the West may not be secret, but it is a code of tangential reference. The secret languages of the Wasuaheli and Wasaramo tribes, and the women of Brunei are formed either by lexical substitution, or by morphological inversion or expansion: much like the 'secret' languages (of which 'Pig Latin' is the best known) formulated by schoolchildren, though the latter are motivated by a spirit of elitism, not shame.

The important connection to be highlighted between taboo and sex-linked language differences in **Western** languages is that taboo is a **form of control** in society: women are restricted by its use and are forced to employ alternatives which are

not construed as dangerous to males. While Western societies do not overtly state the alternants to be adopted, women are subtly expected to be soft-spoken, high-pitched, avoid expletives, use euphemism and even **exploit a different phonological inventory** from males. Separate and secret languages are most easily maintained in closed tribal systems, where kinship is the paramount linguistic organizer (Gumperz, 1972: 231), but diluted forms still exist in non-tribal (Western) societies: we may thus understand taboo to still operate. The form taken resembles more special speech styles defined by status, rather than by kinship.

Taboo is therefore a linguistic confinement which is all the more difficult for Western women to break out of because it is (a) often tacit, and (b) perpetuated by linguists who are too often male. Farb (1974: 50) considers that:

Nowadays young women use words that were formerly taboo for them with as much freedom as young men use them.

This may be true, but the **stereotypes** (e.g. that coarse male language contrasts with euphemistic female language) are still maintained. Key (1975: 58) suggests that one means of escape is for males to realize that they, too, are subject to taboo, are discriminated against in language (albeit less frequently than women), and should work towards eliminating the social causes for the dichotomy. There is little sign, however, of this route being followed by linguists, yet.

1.6. Connection of sex-related pronunciation to economic relations in a society

...a theory of culture incorporates ideas about woman's body, language and psyche but interprets them in relation to the social contexts in which they occur.

Showalter (1981: 197)

Jespersen (1922: 254) is the prime elaborator of the theories which link social role and dichotomous language use for the sexes. Both Lasch (1907) and Jespersen rely heavily on the accounts of Carib so-called separate languages for their separate hypotheses about reasons for the origins of specifically female phonologies and lexicons; but, more importantly, many much later writings develop the implications of Rochefort's theories. The phenomenon of women being 'guardians' of language, more likely to retain archaic forms, recurs in the work of Haas (1964) and Lakoff (1975). For Lasch, however, the evolution of two parallel languages based on reproductive roles seemed unlikely, and a more plausible thesis focused attention on the differing areas of concern for women and men. Jespersen used the concept of *primaeval* society to make the same point: men were hunters, using spasmodic, intensive bursts of energy only to collapse torpid at the end of the day. The language used in their activities was sparse and precise, giving orders and co-ordinating movements, and they were too exhausted to engage in general domestic banter after a hard day's bison-slaughtering. Meanwhile the women, left to agricultural and domestic duties had more opportunities to indulge in idle social chatter, and more energy to do so

because their tasks were less onerous. This simplistic analysis is easy to ridicule, but the facts of role-division and language variation remain.

As Cameron (1985: 47) remarks:

Jespersen here sets a high standard for sexist non-explanation.

The notion that sex-related language differences are connected to different roles is nevertheless an important one, and one to which we now turn.

While hunting versus domesticity is far too crude an image for the effects of sex-specific languages in the twentieth century, the fact that dominant socio-economic relations affect female and male language has been signalled by many authors. Trudgill (1972) was one of the first to indicate that women may use more 'correct' speech in an attempt to compensate for their 'inferior' social position. This is echoed by Abrahams (1972; cited by Kramarae, 1981), who suggests that less-powerful groups evaluate verbal skills more carefully so that they can be sure to portray themselves in an unoffensive verbal manner, and (from a Marxist viewpoint) by Rowbotham (1973), who claims that supremacy is preserved by 'superior' people through language. Bodine, (1975), Key, (1975), Lakoff (1975), Kramarae (1981) and Showalter (1981) are among many others who have developed the socio-economic-linguistic arguments further.

A 'nutshell' explanation (for that is all we can afford

here) is that women are constrained by the patriarchal power-system, which, in turn, constrains both their access to and use of language. The Ardeners (1975) have distilled this situation in a cultural anthropological discipline, and named it the 'dominant/muted group' theory, wherein women are 'excluded' from communication (see Kramarae, 1981: 3-4, or Showalter, 1981: 199-201, for essence). The basic assumptions are that female and male perceptions of the world are different because their activities are different; males are both politically and perceptually dominant, and 'impede' females' expression; women therefore have to accommodate to, and accept, the male perceptual and expressive model if they want to be 'full' social and linguistic participants.

The strongest proponent of this type of argument is Spender (1981), who in titling her book Man Made Language implies that women cannot or have not participated in either the construction or usage of language. Now, the Ardeners' argument assumes that social reality is reflected in language: a tenet with which we do not wish to disagree (although, for criticism of the nebulous parts of the model, see Cameron, 1985a: 102-105). Spender, however, explicitly invokes Sapir-Whorfian determinism and argues that men create and control language and, through it, women. She claims that society is divided into the 'Namers' and the 'Named'. She also proposes that women should use alternative modes of self-expression. We may judge the

efficacy of this approach by the success of attempted neologisms such as pronominal reform (e.g. per, tey and this as neutral pronouns); the resilient markedness of chairperson, and ultimately, the pejoration of Ms. and feminist (see Cameron, 1985a: 82).

It is not appropriate to discuss determinism and control further in this thesis (but, for extended comment, see Cameron and Henton, 1981). An understanding of their existence is however important and relevant when we examine our empirical results (Chapters 6 and 7). Female speakers are not phonetically different just because they are female, but because they are female speakers in societies which harbour (often subtle) expectations about the linguistic behaviour of the sexes. Kramarae (1981: xiv-xv) points out that females are encouraged to talk softly and adopt non-assertive intonation patterns. They are largely 'encouraged' in this behaviour by men.

1.7. Possible physiological causes for sex-related pronunciation differences

We commence this section with another provoking comment by Showalter (1981: 189):

Ideas about the body are fundamental to understanding how women conceptualize their situation in society; but there can be no expression of the body which is unmediated by linguistic, social and literary structures.

The mediation of anatomical basics, in connection to speech production, is central to this thesis. Firstly, however, it

should be established just what these anatomical differences comprise. The primary anatomical difference affecting male and female speech lies in the dimensions of the larynx and of the vocal tract. The main disparities comprise (a) the length and thickness of the vocal folds and (b) the comparative length of the pharynx in relation to the oral cavity. It is the laryngeal differences to which we will attend first. Then we move on to work which has explored the comparative roles of laryngeal and supra-laryngeal frequencies in speaker-sex identification, and how early sex-differentiation may be acquired.

Generally, males' vocal folds are both longer and greater in volume than females'. This growth is associated with the secondary effects of sexual dimorphism which occur at puberty. Larger vocal folds will produce a lower fundamental frequency (F0). So this might be expected to be the most salient, indeed perhaps only, differentiator between the vocal productions of males and females. Investigations into the role of F0 as distinguishing between female and male voices have revealed the situation to be more complex, however. Schwartz (1968) and Ingemann (1968) showed that listeners could identify speakers accurately from certain isolated voiceless fricatives (c.f. also Chapter 8), i.e. with no F0 information present. Their results were corroborated by Schwartz and Rine (1968) and Coleman (1971), who extended the test segments to include whispered vowels, and vowels produced with an

electronically-generated F0. This latter experiment by Coleman suffers from a central methodological fault, however, which could have biased listeners' judgements quite seriously: for elaborated criticism, see Henton (1984b). In further investigations, Coleman (1973a, b) attempted to explore the comparative contributions of F0 and the vocal tract resonance frequencies (formants) to the identification of speaker sex. His technique of using a laryngeal vibrator is somewhat suspect, because of the values he selected for F0. More interesting for the purposes here is Coleman's comment (1973b:21) that:

It is also possible that the glottal source in females differs from males in some basic way besides simply that of pitch.

While Fujisaki and Kawashima (1968), LaRiviere (1975) and Sekimoto (1983) found that both F0 and formant frequencies contribute to speech identification judgements approximately equally, Lass et al. (1976) (spotting a set of variables which had not previously been juxtaposed in his and other colleagues' many speaker-identification experiments), used six vowels in three conditions to conclude that other experimenters were correct: the laryngeal fundamental provides a more salient cue to speaker sex than do formants. They thus agree largely with Coleman, but do not necessarily find support from the Schwartz studies or from Ingemannn, as Lass et al. infer they do. The essential conclusion on this issue is that both fundamental frequency and formant frequencies contribute to speaker identification, but with F0 being rather too consciously manipulable to be a reliable

measure.

If we accepted that vocal pitch were the prime sex-related divider, then it would follow that **childrens' pitch** would be undifferentiable, because children have not arrived at the point where the vocal folds begin to differ in size, and boys' voices start to 'break' (but c.f. Fairbanks et al. 1949, for breaking in pre-adolescent boys). Therefore, if childrens' sex is identifiable from their voices alone, then ceteris paribus it must be due to the behaviour of the formants (Weinberg and Bennett, 1971; Bennett and Weinberg, 1979).

Experimental results (Sachs, Lieberman and Erikson, 1973; Sachs, 1975) appear at first sight to support this supposition: female and male pre-adolescent children were reliably identified for sex. Unfortunately, though, the first study incorporated children of 14 years who may well have been pubertal, and the second concludes that other aspects of the speech signal (e.g. rhythm and lexical content) may have influenced judges' decisions. These perceptual experiments may not be regarded, then, as entirely conclusive with regard to the influence of formant frequencies as salient identificational cues.

Meditch (1975) attempted to identify when these apparent sex-linked patterns start to appear in children. Her rather surprising finding was that by the age of three both boys and girls have developed their respective patterns well

enough to be identified solely on the basis of speech. Boys were better identified by listeners than were girls and more incorrect judgements were made for older girls than for any other speakers. This aspect, of women's voices being generally less reliably identified, will receive brief notice in due course. The hypothesis offered by Meditch is that at some (indeterminate) age, all children speak a code which is not marked for sex. Then, they both lose features but the boys' loss is greater and they are therefore easier to correctly identify than girls. The older girls were difficult to identify because not only were they losing features (a male-associated trait) but also because they had not learned all the features of female-associated speech. They were thus in a state of transformation providing often conflicting features for the listeners to adhere to. In Sachs et al.'s work larger or older children were called boys, and smaller or younger children, girls. This feeds both the notion that boys break with the 'androgynous pitch' earlier and that of female-associated speech being immature or child-like. By the time these children mature, males are talking 'bigger' than vocal-tract size would predict, and females 'smaller'. The semantic consequences are well attested by the prolonged use of 'girls' and 'gals' for women, whereas men are far less frequently referred to as 'boys'. We demonstrate the phonetic consequences in Chapters 6 and 7.

Smith (1979: 124) reveals that the image created by Meditch

may be less well-defined than it first appears, with geographical location of the study (c.f. Local, 1978), sex of the interviewer, social class of the child (c.f. Edwards, 1979), and formality of the interaction all possibly affecting the degree of confidence by which child sex may be identified.

1.7.1. 'Difficulties' in women's voices?

Although the identifiability of speakers' voices is not a facet of sex-specific language study which will be directly empirically addressed in this thesis, nevertheless it has been consistently found that females' voices are more difficult to identify than males' in conditions for normal adult voices (Lass et al., 1976); normal adults using electro-larynxes (Coleman, 1971); pre-adolescent speakers (see discussion above) and laryngectomees (Weinberg and Bennett, 1971). Bennett and Montero-Diaz (1982) take this tendency a step further, and report that children are less able to perceive female voices in a whispered condition, and indeed they may tend towards a male response bias. This weighting towards the male will be shown to be frequent in both phonological and phonetic empirical techniques (see Chapters 3 and 4), and is a frequently repeated reflection of the 'default factor' in society. Whether female voices are less intelligible (a different issue from identifiability) is a question which receives some attention in later chapters in this thesis.

1.7.2. The unreliability of speaker pitch

Our considered point of departure, then, was that while F0 does contribute to phonetic sex-specific behaviour, it is neither a steadfast indicator, nor the sole determinant of a speaker's sex in their voice. The formant frequencies of the vocal tract also supply substantial cues to speaker sex and are furthermore more likely to reveal subconscious sex-related patterns because they are least under the speaker's conscious control (unlike pitch). The formant frequencies therefore formed the primary source of investigation in this thesis. That pitch (F0) is, by contrast, under conscious control from a very early age has been demonstrated by Lieberman (1967: 45-46). He cites a study of a 10-month-old boy and a girl of 13 months, which found that the average F0 of the babies' vocalizations was 50-100 cps. lower when they were 'talking' to their fathers than to their mothers. Both of these fundamental frequencies were lower than that of solitary babbling or crying. Hints at the different modes of babies' communication are given by Lieberman, but not pursued:

The mimicry in speech perhaps represents a social use of speech, while the crying is egocentric since it still has an emotional reference.

Thus pitch and intonation can be seen to vary according to the sex of the addressee, and this variance cannot be ascribed wholly to physiological composition, because the vocal tract size of infants is equal.

1.8. Are sex-related differences so really "subtle and few"?

Smith's (1985) book is the most recent to appear in the rapidly-growing shelves of language-and-sex studies, so it is important to examine some of his assertions in detail in this thesis. A longer critique of his arguments relevant to phonology occurs in Chapter 2, and we acknowledge his improvements to the sophistication of terminology. There is though one remark which should not pass unheeded at the outset of this thesis. Smith (1985: 9) claims that:

...sex differences are subtle and few; indeed differences have sometimes not been found where they were expected (Labov, 1966; Fasold, 1972).

This is a rather negative outlook to express so early in his study, and one which prompts the reader to enquire how he has managed to spin out these allegedly "subtle and few" differences into a book-length examination. Not content with a priori demolition of his own subject, Smith repeats these remarks (virtually verbatim) in his discussion of methodological issues in determining phonological and grammatical sex differences. He says (1985: 81):

The authors of recent urban linguistic surveys have been careful to point out that the speech variables they are interested in are usually better predicted by SES, ethnicity and age than by sex. Indeed, differences have sometimes not been found where expected.

The factual accuracy of these statements is contested in Chapter 4 of this work. At this point, though, it is worthwhile summarizing our position as follows:

(a) there are many cases of well-attested

phonetic/phonological variation linked to sex, across languages. An example of an authority who supports our opposing stand is Hudson (1980: 20) who states:

As far as speakers are concerned, the commonest characteristic to be reflected in linguistic terms is sex.

Since sex is a primary organizing factor in our perception of other individuals, it is unlikely that it will be readily subsumed by other (less apparent) speaker characteristics such as SES or perhaps ethnicity.

- (b) To construe differences as subtle may be no more than a reflection of using clumsy methodology or insensitive instrumentation as an analysis tool.

In short, I wish to commence this thesis from the viewpoint that there are **numerous** sex-linked variants, and it is only due to methodological inadequacies that they have not been more obvious previously.

1.9. Exclusive or preferential; saturated or unsaturated usage

Among these preliminary statements and terminological distinctions, brief clarification is needed of the extent to which sex-connected behaviour is anticipated in this study. As has been said, the focus is on phonetic and phonological evidence. But how far do we predict the pronunciation of a variant to be due only to sex, or how far to sex covarying with other factors?

Bodine (1975: 131, 133) postulated two possible types of sex-related language behaviour. If a linguistic form is found to be used solely by one sex, it may be labelled **sex-exclusive differentiation**. In order to qualify, the correlation between speaker sex and a speech feature would have to be absolute: variation should be established as not arising from a link between sex and some other variable (e.g. race, age, SES, occupation). While it is conceivable that this kind of usage may occur in African and American Indian languages (see Frazer, 1900; Kraus, 1924; Haas, 1944 and Flannery, 1946) where it is related to taboo (see Section 1.5), it is not ascertainable that these authors were aware of any such distinction between exclusive and preferential uses, and thus they attributed only the stronger version to their evidence. It can be said today that sex-exclusive variation probably no longer exists.

Sex-linked language reported for European languages indicates a less direct link between sex and speech. Thus, if variation is manifested by one sex more or less frequently, or if it can be shown to correlate with a number of variables including sex, then this is **sex-preferential** behaviour. Such forms, according to Bodine (1975: 131):

... are less accessible to conscious awareness, and since they require assessment of relative probability of occurrence, are more difficult to describe accurately.

While her first comment seems accurate, the contention that such (subtler) differences present more difficulties in terms of description will be shown to be an overstatement,

in the light of the results of this research. In order to determine sex-preferential use, it is of course necessary to keep other variables as constant as possible: that fact is acknowledged in our research design and in the presentation of results (Chapters 6 and 7).

Sex-preferential forms can be seen to be closely connected with divisions of labour (occupations) in a society. Thus, for example, drill sergeants may employ very loud and often hoarse voices not because they 'prefer to' as men, but because the occupation of drill sergeant is uniformly male. Similarly, if men were child-rearers, they might also use 'motherese' (Cameron, 1985a: 47).

Another distinction has been drawn among types of sex-linked variation, according to whether they show **qualitative** differences (e.g. Koasati, as reported by Haas, 1944) or **quantative** differences (e.g. as reported in recent sociolinguistic investigations by Labov, 1972; Trudgill, 1972, etc.). This is a distinction drawn by Hudson (1980: 121) who argues that qualitative differences act as reinforcement of observable sex differences, whereas quantitative differences (such as those which occur in English) occur as a result of other linguistic motivations - such as females' purported greater approximation to norms. It is not clear that Hudson is describing with this division anything other than the same kind of distinction in sex-linked behaviour as is termed by Bodine (1975) and by Smith (1979, 1985) sex-exclusive and sex-preferential. We do

nevertheless agree with Bernard's (1973) comments about the approaches, namely that:

...in general the qualitative approach (which is said of course to give 'soft' data) is not as well respected these days as the quantitative approach.

One useful expansion of the exclusive/preferential dichotomy was however introduced by Smith (1980: 115, and 1985: 9) when discussing sex in terms of social marker theory. His important addition is to show that within preferential usage, there is also **saturated** and **unsaturated** use. Smith uses examples of enthusiastic discussions about football, and motherese. It is usually men who talk about football (sex-preferential use), but it is only some men who do this (unsaturated use). Similarly, all females may use the high-pitch, exaggerated stress and slowness of 'baby-talk', but in fact only women with children do so. A further illustration, which is furnished by this thesis, is the suggestion that academic women use a lower than average speaking pitch (though not all women will do so).

Smith's assertions of the last paragraphs have not been tested empirically, yet. Clearly, what is needed is a study of, for example, both mothers and non-mothers (including men) talking to babies. Speculation, taken together with the actual findings of Lieberman (1972 - see Section 1.7.2), would lead one to predict a **uniform raising** of pitch and increased loudness among all the adults. The concept of 'saturation' in this context may then become rather leaky.

The necessary distillation from the above discussion is this. The hypotheses of the present research incorporate expectations of **sex-preferential phonetic behaviour**. Degree of saturation probably cannot be established empirically, because it is a concept which is liable to be open to too much uncontrollable variation (such as individuals' personality, mood, SES and accent).

1.10. Socio-phonetics defined

Among the aims of this thesis which were stated in the first paragraph of this chapter was a contribution to the establishment of **socio-phonetics** as a new avenue of research. The empirical and theoretical directions taken in this work will convince the readers, I hope, of the need to incorporate social circumspection into experimental and descriptive phonetic and phonological enquiries. Just such a necessity has been indicated explicitly in the literature repeatedly in the recent past. A particularly pungent directive comes from McConnell-Ginet (1975:50):

The major theoretical issue that the linguistic usage of women raises is how to articulate adequate models of language, the minds of those who speak it, and the society in which they live. Such models must capture the interrelations among these three components of a living speech community and be able to show how la parole eventually transforms la langue and how cognitive categories form and are formed by linguistic categories.

From Knowles, it is more a request (1978: 90):

What we need to study variables properly is theory of **sociolinguistic phonetics** (my emphasis)...(which)...is concerned with the options open to the speaker at different stages in speech production, and the way these options can be used to convey sociolinguistic

information about the speaker.

And from Smith (1979: 132):

...the pronunciation variables influencing naive listeners' judgements of men and women have yet to be brought under phonological scrutiny, and it is the isolated variables of the phonetician which are the basis of sociolinguistic arguments about sex.

Lastly, in our battery of supportive quotations, we notice the conclusion by Vincent-Richter (1983):

It is not sufficient to account for sex-differentiated speech varieties with previous biological or innate conditions as the main reason. It is also not satisfactory enough to postulate as the origin of any discrimination the constant economical dependence of females.

...speech attitudes are determined exteriorly in terms of cultural role expectations in one given society at one given time.

Thus we may define our conception of **socio-phonetics** as the examination of speech production and perception behaviour in specific relation to the social expectations made in a certain society at a certain time. It is with this tenet held firm that this thesis proceeds.

1.11. Research hypotheses

The exact hypotheses explored in the body of this thesis are:

- 1.11.1. Females and males speak differently at the phonetic and phonological levels.
- 1.11.2. The degree to which they speak differently will vary in different societies (languages/accents).
- 1.11.3. When anatomical disparities between males and females are normalized, there will remain an

amount of productional difference which can only be accounted for in terms of social expectation of appropriate linguistic behaviour in the two sexes.

1.11.4. Previous attempts to normalize for male/female differences have not succeeded. The use of an auditory normalization procedure to normalize acoustic differences would be an improvement.

These hypotheses will be expanded on and specifically tested experimentally in Chapters 5, 6 and 7.

1.12. Conclusion

A great deal of necessary introductory scene-setting has been compressed into this first chapter. We have tried to present a background to the conception of this particular research, indicating, at the same time, the originality of its approach. It was furthermore essential to define certain terms unambiguously and illustrate their applicability to this work. Previous investigations into the area of sex and language were shown to follow four routes, and our own has benefitted from the shortfalls or insights of all four. It is thus hoped that the model constructed and presented in the ensuing chapters will in turn provide an improved platform for subsequent studies. The strongest hope is that the steady female 51% of the population will become equally represented, in percentage terms, in phonetic and phonological research in the future.

Chapter 2

Phonological variants: a curate's egg

Introduction

This chapter sets out to accomplish two objectives. The first is to provide an overview of previous studies which have incorporated investigation into sex-specific phonological variants and point out their shortcomings. The second is to augment that knowledge with more recent studies, and to provide indications of how and in which directions this type of work might proceed.

An examination of earlier studies of sex-specific phonological variation soon reveals an important centrepiece to the argument maintained by this thesis. The important disclosure is that (perhaps not unexpectedly) physiological differences between and within social groups generally, and the sexes specifically, are not significant to a degree that can account for observed phonological differences (García and Frosch, 1978). This finding, as has been mentioned in Chapter 1, is central to this work, and will receive particular attention from the phonetic angle in Chapters 5, 6 and 7. However, a review of prior phonological work in this area is soon confronted by the linguistic equivalent of the hydra.

One of the heads of the hydra may be seen as which (and how many) variables remain after the finding emphasized above has been accepted as a fundamental. Phonological variants may occur as a result of a host of possible variables other than sex (e.g. social class, age, geographic limitations,

religious affiliation and so on) which might provoke or catalyze differences.

A further hydra-head may be regarded as the influence of investigator-bias. Smith (1985:82), casting his customary cold eye, summarizes this problem well:

Sociolinguists must, by virtue of the immense cost of analysing phonological data, be very selective in their choice of linguistic variables. Generally speaking, this choice is based on the intuition of insightful observers, who may themselves be speakers of the varieties in question. This reliance on intuition increases the risk that the selection will be biased in favour of stereotypical linguistic indicators. Less obvious variables, perhaps distributed in quite different ways, go undetected. This selective bias could have serious consequences for conclusions about sex-differentiated usage, since the differences are generally small, and less consistent than differences predicted by other social variables....The difficulties raised by these considerations are by no means insuperable, and one must suppose for the moment that appropriate interpretative and methodological refinements would not entirely undermine the generality of conclusions about male-female phonological and grammatical differences.

So, the general intractability of the data per se, coupled with a reliance on the validity of an observer's intuitions presents a fairly shaky foundation on which to build theories of sex-specific usage. The precise ramifications of this problem are examined in the ensuing discussion of the cross-language examples assembled for this thesis. And, as Smith indicates in the more optimistic of his remarks, we hope to be able to present some indications for methodological refinements and act upon them in the collection of the data collected for this study.

This chapter faces the hydra, then, but does not guarantee

necessarily to perform the Herculean task of cutting off its heads, nor of not allowing others to grow in their place. We do, however, provide some constructive suggestions for future work, based on the lessons to be learnt from past misconstructions.

2.1. Methodological preliminaries: a cautionary note

The best-documented, and apparently most frequent differences between the languages of the sexes are at the phonological level. Smith (1985:79) provides an exhaustive list of such studies to date and particularly focusses on those conducted by sociolinguists, who were mostly set on establishing that women used more prestige forms than did men. It is not intended to replicate this taxonomy here, although some of the studies listed will receive specific attention in Chapter 3.

Whether the wealth of phonological studies reflects linguists' analytical preference (i.e. the data is at first sight fairly easily definable), or the reality of more variables actually occurring here rather than at the lexical, syntactic or suprasegmental levels, is not immediately answerable; nor is it within the scope of this discussion. One suspects, though, that 'ease of access' to the data, combined with an apparently well-honed descriptive and analytical paradigm, have been instrumental in generating more coherent (but, as we shall see, not necessarily correct) accounts of segmental phonological

variation than, say, suprasegmental or syntactic. Attempts to grapple with apparent sex-related suprasegmental differences will be discussed, nevertheless, in brief in Chapter 8.

From the outset of this section, it may be worth bearing in mind that any research which claims to provide definitive answers on the basis of phonological variation alone must be viewed with suspicion. If we recall Chomsky and Halle's (1965: 98) statement that,

The phonological component of a [generative] grammar relates the phonetic representation of an utterance to its syntactic structure

and further add that,

It seems that the underlying base forms and the linguistically significant abstract structures cannot be revealed by segmentation and classification but are, rather, related to phenomena only by intricate sets of ordered rules that violate the formal conditions presupposed in taxonomic methodology. (ibid., footnote 3: 101),

we may rapidly become aware that the 'truth' is unlikely to be revealed by examination of a phonological inventory alone.

A thorough critique of the shortcomings of phonological methods is not intended here. Nevertheless, analyses which only meet the levels of observational and descriptive adequacy and which fail to provide explanatory adequacy (that is, they do not show on what basis the device of the acquisition model or the linguist selects a grammar admitting one form rather than another) can be regarded as insufficient.

The realm of phonological theory swarms with internal strife and cyclic argument. We may be thankful, however, that there are some fundamental precepts which the factions would agree on, and which may serve to reveal fissures in investigations of a socio-phonological type. Of these undisputed points, one of the primary is that **the corpus of data should be homogeneous**. Specifically, it should be garnered from speakers from one speech community (see Gumperz, 1968, for what this might involve).

The desired homogeneity of this base becomes an immediate stumbling block in the consideration of much previous phonological work. Just as a phoneme only has relevance in relation to a system, the argument goes, so does a speaker only have identity in relation to a speech community (given that people and speech are 'social animals'). But the notion of a speech community is nebulous and vulnerable to multiple interpretation (see Labov, 1980 and Romaine, 1982, for a thorough discussion of the problem). Twaddell (1935: 69) perceived this definitional obstacle relatively early in the history of modern phonology:

Utterances are acoustico-articulatory events accompanying social situations and correlated to them. In terms of accompaniment and correlation, some utterances are alike, and some are different in a given speech community.

Twaddell thus highlights the naivety implicit in expecting data to be automatically homogeneous. This concern should not be interpreted as agonizing over type-tokenism, but rather as a reminder that much linguistic analysis in the

twentieth century has been not so much founded on sand but rather cloaked in untented assumption. Of the studies reviewed in the next section, it is doubtful whether any were originally conducted with the notion of the social homogeneity of their informants as paramount. Some of the innovative quality of the current work, incidentally, consists of emphasis on the importance of a closely-controlled homogeneous database on which to found phonetic and phonological deduction (see Chapter 4).

Hyman (1975:1) further echoed Twaddell's sentiments:

Since speech sounds are used to convey meaning, sound systems cannot be fully understood unless they are studied in a linguistic context.

I would want to assert here that a true 'linguistic context' must incorporate not only all so-called 'levels' of linguistics, but the subject-matter of many other disciplines as well. In her entry in an encyclopaedia, Vincent-Richter (1983: 570) has also given voice to this necessity:

...no study of sex-related speech varieties could be possible without taking into account an interdisciplinary standpoint involving linguistics, psychology, biology, sociology, anthropology, ethology, economics, politics etc.

We thus stand in opposition to Bloomfield (1926), when he dictated in his Postulates that,

... the psychological and acoustic description of acts of speech belong to other sciences than ours. The existence and interaction of social groups held together by language is granted by psychology and anthropology.

Bloomfield may be partly exonerated from what appears, with hindsight, blinkered absurdity because he was concerned, in part, to justify the establishment of linguistics as an autonomous subject. It is still lamentable, though, that many contemporary investigators incline towards his ideas. The point arising from (and antithetical to) Bloomfield's assertion, which needs stressing is that disciplinary isolationism is counter-productive in the formulation of linguistic explanations. Kramer (1974:85) has captured the essence of this dictum well:

Words, phrases and sentence patterns are not inherently strong or weak. They acquire these attributes only in a particular cultural context.

The verbal repertoire of the individual and differences in style are inexorably bound up with "cultural context", and the phonological is only one level at which they are manifest. This connection between phonology and sociolinguistics has not always been guaranteed in the studies outlined in the next section, and is one of the prime origins for criticisms emerging.

2.2. Previous phonological studies

The paradigmatic phonological studies of sex-linked variables are often considered to be Labov's (1966 and 1972 inter alia) descriptions of New York American English. In the following discussion, such studies will be called **socio-phonological**. They will form a separate subject for treatment in the next chapter. Socio-phonological works,

however, postdate many which had been conducted from a more traditionally 'pure' linguistic/phonological standpoint. To attempt, in this way, to separate the two types of phonological studies is not entirely accurate, though, since most of these early studies were not performed by linguists per se. We will describe the qualifications of the collectors in due course. It is, nevertheless, a useful preliminary to opt for the general bifurcation of pre- (= early) and post-(= after 1960) Labovian methodologies.

Rather than attempt to bring together too much information at once, and in order to portray in some detail the problems of both the linguistic and socio-phonological approaches, this chapter will limit itself to the early descriptions. Chapter 3 then, will be devoted to an outline of the post-Labovian (socio-phonological) studies.

Many pre-Labovian investigations are admirably summarized by Bodine (1975). Her collated examples are presented as Table 2.I. here. For the purposes of this study, a great deal more evidence for cross-language phonological sex-linked variation has been assembled. For instance, a considerable part of the first volume of Orbis (Bulletin International de Documentation Linguistique) was devoted to 'Le langage des femmes', in 1952. The relevant phonological remarks from these studies will be incorporated in Table 2.II, see later in this chapter. We shall not be concentrating much on this oeuvre, though, since the majority of the reports are somewhat anecdotal and focus on semantic and syntactic

Table 2.II Cross-cultural summary of sex-differentiation in languages (from Bodine, 1975)

DIFFERENCE IN:	BASED ON SEX OF:			
	A. SPEAKER	B. SPOKEN TO	C. SPEAKER PLUS SPOKEN TO	D. SPOKEN ABOUT
1. PRONUNCIATION	1A Bengali, Caraya, Cham, Chukchee, Creek, Eskimo, Gros Ventre (2A), Hitchiti, Kossati, Zuni (2A)	1B	1C Yana.	1D
2. FORM:	2A	2B	2C	2D
INTERJECTIONS	Gros Ventre (1A), Zuni (1A). Also, according to Flannery, Cree, Algonkun, Iroquois, Ojibwa, Sioux, Fox, Yokuts.	ONLY IN DIRECT ADDRESS		
PARTICLES	Japanese (2B), Thai.			
PERSONAL PRO- NOUNS	Japanese (2B), Thai. Yuchi? (See discussion).	Tunica, Semitic lan- guages, pre-modern Japanese (2A).	Chiquita-affixes and a few stems.	Widespread, but far from universal. Chiquita-only in M-M speech.
TITLES		Bengali.		Widespread, but far from universal.
KINSHIP TERMINOLOGY	Many languages of Amer. Indians, Asia, Africa, according to Dixon and Kroeber.	*See below.	Carib-stems, Chir- quita-usually stems, probably Yana-affixes.	Generally used in all kin- ship terminologies for at least some relatives.
NOUNS	Many of these also dif- fer on basis of sex of spoken about. Yuchi? (See discussion).	*See below.	Carib-stems, Chir- quita-affixes, probab- ly Yana-affixes.	Probably all languages have sex differentiation in some nouns. Different affix in some languages, as in gender, different stem in other lan- guages. Chiquita-gender only in M-M version.
VERBS		Semitic languages- affixes.	Probably Yana-affixes, Biloxi-only in direct address.	Gender concord often manifested in verbs.
NOUN MODIFIERS		*See below.	Yana-affixes.	Gender concord often manifested in noun modifiers.
		*Differentiation in any part of language (titles, kinship, nouns, verbs, etc.) based on sex of spoken about automat- ically implies differen- tiation when these forms are used in direct address.		

differences. Table 2.II will also incorporate Bodine's information from Table 2.I, but will add to it the results of subsequent bibliographic scouring.

2.2.1.

Bodine's essay begins by highlighting a few of the incipient problems which can influence the eventual analysis of a language in terms of speaker sex initiated variants. The essence of these problems will become apparent below. By way of illustration she provides a very useful matrix which captures much of the cross-linguistic evidence available at the time for sex-differentiated languages (her Table I: 134-135; reproduced for convenience here as Table 2.I). The x-axis of the chart is divided into the four categories 'Speaker; Spoken to; Speaker plus spoken to; Spoken about'. The y-axis consists of the linguistic categories 'Pronunciation; Interjections; Particles; Personal pronouns; Titles; Kinship terminology; Nouns; Verbs; Noun Modifiers'. Thus it is possible, with the aid of the matrix, to read off that, for example, Semitic languages incorporate different verb affixes according to the sex of the person spoken to.

While the 'higher level' occurrences of sex-specific linguistic behaviour (as illustrated in Table 2.I) are not under direct investigation here, it is nevertheless interesting to observe how widely these manifestations appear across the various speaker-interactional categories.

Smith (1985:79) comments on the paucity of evidence of sex

differences in patterns of grammatical combination; but this notion does not appear to receive much support from the picture presented by Table 2.I. It is perhaps more the case that Smith has not extended his survey beyond the English-speaking world. It is also worthy of note that in the other linguistic 'form'-based areas, the sex of the person spoken to, that of the speaker plus the spoken to and the spoken about, may all influence the structure of the utterance. Seemingly, then there is a comparative barrenness of 'pronunciation' differences across the board. Possible reasons for the concentration of evidence listed down one column are dealt with in the following paragraphs.

In the 'Pronunciation' row in Bodine's chart there is a marked emptiness in all the cells apart from that of the 'speaker'. The overwhelming majority of phonetic/phonological variation illustrated in Bodine's grid derives from the sex of the speaker. This arises primarily because it is this type of differentiation which is most widely reported, and not (as we proceed to argue) necessarily because it is the most common. Would it be justifiable to infer from this picture, then, that in many cultures, speakers are anxious to establish/maintain their individual sex-related identity on an active basis by using obviously marked pronunciation? This may be an appealing deduction, and receives some underpinning from the existence of single-sex secret languages, but it will be shown in Section 2.4.7 to be on the whole not only simplistic but also leaky.

2.2.2.

It may be possible to propose that some of the speaker-initiated differences may have emerged from single-sex secret language (see Chapter 1, Section 1.5) becoming less ritualistic and being used in ordinary contexts. One could extend the hypothesis further and suspect that secret languages would also take the sex of the person spoken to or about to be influential. Specific diachronic evidence is not available, though, and so this inference must remain speculative.

2.2.3.

Bodine provides some detailed discussion of the nature of the pronunciation differences, which may be condensed here. Speakers have several tactics at their disposal by which to separate speech sounds (and I use this term advisedly because of the reservations to be expanded in Section 2.4.5.). Those noted by Bodine are:

- (i) one sex omits one or more speech sounds which the other sex uses. Language examples: Chuckchee; Caraya; and Muskogean languages.
- (ii) both sexes employ the same number of speech sounds, with the same place of articulation, but with a different manner of articulation for one or more sounds, for each of the sexes. Language examples: Koasati; Bengali; Eskimo.
- (iii) the reverse of (ii); i.e. both sexes employ the same

number of speech sounds with the same manner of articulation, but for one or more sounds the place of articulation may differ for male and female speakers.

Language examples: Gros Ventre; Zuñi.

Obviously, this is not an exhaustive account of the various means available to speakers to distinguish speech sounds. These differences are limited to those which occur in the data summarized in Table 2.I. As will be shown in Chapter 8, the other modes of phonetic/phonological sex-related distinctiveness available to speakers may include the exploitation of an alternative phonological inventory according to the speaker's sex, and, indeed an overall difference in 'vocal setting' (i.e. the habitual overall articulatory and phonatory posture adopted for speech).

2.2.4.

Concerning the lacunae in the other cells possibly affected by pronunciation differences, Bodine has little to offer of a positive, interpretative nature. She notices absences, but has no suggestions for the cause of those absences. One obvious explanation which immediately leaps to mind, however, is that investigators who have noticed speaker-engendered variants in abundance simply have not been looking for (and, by deduction, not noticed) variants which may be occasioned by other, extrinsic factors. Social, communicative accommodation theory (see Giles and Smith, 1979, inter alia) is, after all, a relatively recent

concept. Stated briefly (for it will receive more detailed description in Chapter 3.2.), accommodation theory holds that speakers will (unconsciously) adapt to each others' speech 'mode': they will emphasize similarities in their speech in order to make the conversation more co-operative. A linguistic researcher is unlikely to escape this effect, since it is governed by a desire for social acceptance, even approval. Obviously, the most effective way to assemble phonological data is for the researcher to be pleasant, to co-operate with the informant. So, the informant and the researcher are both likely to be affected by accommodation theory, with the informant producing forms due to the sex/status/age etc. of the researcher. The researchers at the time, however, were not aware of this phenomenon and so did not perceive differences in their informants' speech which were occasioned solely by the 'doing' of an interview. The ethnographers and missionaries who were responsible for the assembly of much of the early sex-related phonological examples are unlikely to have been constructing their descriptions according to late twentieth-century sociology of language explanations. Thus variants conditioned by factors of a larger interactive conversational setting, than simply the sex of the speaker, were not recorded.

2.2.5.

So, while gaps occur (possibly more as a result of not being suspected rather than simply not existing at all) there is no apparent reason why they should not exist.

Differentiation based on the sex of the person spoken to, is perfectly sociolinguistically possible, even though phonological examples are not attested for any language in Bodine's grid. There are, for example, Sapir's (1915) instances from Nootka, where the physical traits of the interlocutor are incorporated in the speech of the speaker.

From Bodine's work, it at first appears that only in Yana (a North American Indian language spoken in California and as described by Sapir, 1929, and Dixon and Kroeber, 1903) does the sex of the speaker plus the sex of the spoken-to interact to produce different pronunciations. Parallel examples, of the speech of the speaker being modified to the peculiar qualities of the interlocutor are nevertheless easy to adduce: 'baby talk' (both that used to address babies and the variety used by some speakers in intimacy) and speech addressed to the deaf are two obvious cases. These latter two categories will be shown to be capable of explicit expansion in Chapter 8, Section 8.3.

There is no doubt that further research is needed to establish empirically that the sex of the hearer may play an important role in the productions of the speaker, and the pinpointing of this phenomenon is surely only a matter of time. A little research has been conducted already into the influence of the ethnicity of the interviewer on conversations with Mexican-American women (Tixier y Vigil and Elsassser, 1978). Furthermore, interest has been expressed explicitly in the possible effects of the sex of

the interviewer when assembling linguistic data (see Bodine, *ibid*: 147) but, at the time of writing "the influence of interviewer sex (and other interviewer characteristics) on sex differences in phonology remains of unknown quality and quantity" (Smith, 1985,:81-2).

Given that a great deal of phonological theory has been built on the isolated utterances of one (usually male) individual in one interactional context with one (usually male) investigator, this scepticism is not misplaced. Smith (1985:81) catalogues the surveys which have been carried out by women, and lists three of a post-Labovian type, and three of the same type which have been carried out by a team of a woman and a man. In the same inventory, there are ten studies conducted by men. In pre-Labovian days, this 3:1 male-to-female ratio was undoubtedly worse. Indeed, of the sources provided in Bodine's bibliography, a mere five entries come from female authors from a total of twenty-six dealing specifically with sex-related language differences (ratio = 5.2:1 !).

Projecting a little, investigations into the effect of the sex of the interviewer will probably run into difficulty in trying to separate out the effects of sex from that of the relative status of the interlocutors, or the interview-situation in a broad sense, as affecting speaking style. The untangling of the influence of these and other variables, however, must remain matters for future research, lying as they do outside the scope of the present work.

2.2.6.

It is perhaps less surprising that there is apparently no instance of a pronunciation difference based on the sex of the person **spoken about**. This type of variation is clearly possible (c.f. Sapir's examples from Nootka again, where the physical traits of the person spoken about alters the phonology of the speaker). This is, nevertheless, only one so-called 'exotic' instance, and further examples from other languages do not appear to have been documented. A tentative explanation for the non-occurrence of this type of marker stems from the argument in Section 2.2.4. above. If early researchers were not generally aware of variants influenced by factors outside the immediate elicitation procedure (which, in itself, would be more accurately labelled 'metaconversation' than conversation proper), then they most likely did not recognize any variants produced according to the constraints of this fourth category.

2.3. Objectivity in analysis

Before moving on from a description of the broader aspects of Bodine's exposition (to a critique in Section 2.4.), it is irresistible not to repeat her most pungent example of the problems besetting studies of sex-based phonological variants. It demonstrates just how strongly the sex of the investigator may influence the supposedly objective reporting of variants.

Bodine cites the studies of Chatterji (1921) and Das (1968),

who both report on Bengali. Das' work focusses on forms of address in Bengali, and need not detain us in detail here. He deserves mention, however, because he forms half the partnership which Bodine considers "unique among investigators" (p.147). The two authors' infamy stems from their status of being both investigator and informant. This situation strikes Bodine as unusual and provokes comment per se. From a broader linguistic perspective, this is not a particularly remarkable state of affairs. Indeed, there has always been a disturbing proportion of linguistic 'evidence' which has been founded solely on linguists' personal introspection on their own usage. This instance is nevertheless particularly pithy of its type:

As linguist and informant, Chatterji makes an automatic, apparently undiscussed, undefended equation of the Bengali language with men's speech and reports that Bengali initial /l/ is often pronounced as /n/ by women, children and the uneducated classes. Although he gives no numerical counts or estimates of either speakers or usage frequency, it would appear that women, children and the uneducated constitute the great bulk of the speakers of Bengali and perhaps the description should read, "Bengali initial /n/ is sometimes pronounced as /l/ in pretentious speech, particularly that of status-conscious men". (p.147).

Now, it is not certain that Bodine's re-analysis of Bengali is necessarily correct either, but it does serve to emphasize how easily a so-called "simple phonetic statement" can be contorted by implicit (and apparently unconscious) social evaluation.

2.4. Further critique and expansion of Bodine's survey

The wide coverage and critical insights of Bodine's survey notwithstanding, there remain rather more basic assumptions which she fails to question. These questions should be raised about any phonological study. Therefore, an updated overview of phonological studies which incorporate sex-attributable references, has been assembled for this thesis. It includes both pre- and post-Labovian studies; examples which have been added to the catalogue since 1975 and those early examples which Bodine omitted. This new synthesis is presented as Table 2.II.

It would no doubt be fascinating to discuss the exact phonetic nature of the differences documented in Table 2.II, but such an undertaking would be too space-consumptive and somewhat tangential to our current purposes. A precise list of the phonetic segmental differences in the languages/dialects in the table was extracted from the literature, however, and forms the basis of further discussion in Chapter 8.

Table 2.II An updated cross-language summary of **segmental** phonetic/phonological sex-differentiation (1712-1985). Language names are followed by sources, which are listed in chronological order. * shows studies conducted with a socio-phonological outlook. £ indicates a language or dialect which is mentioned in further detail in this thesis.

A. PRONUNCIATION DEPENDENT ON SEX OF SPEAKER

Alemanic (Swiss)	Hotzenköcherle (1934), as reported by Bultot (1952)
* Arabic (Cairene)	Khan (1975) Ahmed (1979)
Bengali	Chatterji (1921)
Caraya (= Karajà) (S.A.I.-Brazil)	Ehrenreich (1894: 23) Fortune and Fortune (1984)
Cham (S.E. Asia-Vietnam)	Blood (1962)
Chinese (Mandarin)	Grootaers (1952)
Chukchee (N.E. Siberia)	Bogoras (1901; 1922) Bouda (1953: 33)
Creek (N.A.I.)	Haas (1944)
Danish	Brekke (1881:17), as reported by Jespersen (1922: 244)
Darkhat Mongolian	Capell (1966: 101)
*£ Dutch: Standard	Koopmans-van Beinum (1973)
*	Brouwer and van Hout (1984)
*£ Utrecht	Koopmans-van Beinum (1973)
English: American - East Coast	Fischer (1958)
*£	Labov (1972)
£ - General	Peterson and Barney (1952)
*	Shuy, Wolfram and Riley (1967)
*£	Fasold (1968; 1972)
£	Schwartz (1968)
£	Schwartz and Rine (1968)
*£	Wolfram (1969)
*	Berryman (1980)
*	Hartford (1978)
*£ - Mexican	Labov (1966; 1972)
*	Levine and Crockett (1966)
*	Anshen (1969)
*	Nichols (1978)
*£ Australian	Mitchell and Delbridge (1965)

Table 2.II continued

A. PRONUNCIATION DEPENDENT ON SEX OF SPEAKER

* English: Australian	Shopen (1978) O'Kane (1984)
*f English: British - Belfast	Milroy (1976); Milroy and
*f	Milroy (1978) <u>inter alia</u>
*f - Modified Northern	Bladon, Henton and Pickering (1984)
*	Henton and Bladon (1985a;b) Henton (1985)
*f - Norwich	Trudgill (1972; 1974)
f English: British - Received Pronunciation	Jones (as reported by Jespersen, 1922: 245)
*f	Henton (1983)
*f	Bladon, Henton and Pickering (1984)
*f	Henton and Bladon (1985a;b)
*f - Scottish	Romaine and Reid (1976)
*f	Macaulay (1978)
*	Romaine (1978)
* - Southern	Hudson and Holloway (1977)
*	Cheshire (1978)
* South African	Lanham and Macdonald (1977)
Eskimo (Baffinland)	Egede (1818), as reported by Frazer (1900) Boas (1911:79)
f French: Parisian	Alemand (1818) and Grimarest (1712), as reported by Jespersen (1922:244)
*	Mettas (1979)
*	Lonchamp (1986)
* Canadian Swiss	Sankoff and Cedergren (1971) Gauchat (1905) Hermann (1929)
Gimi (Papua New Guinea)	As reported by Gregersen (1979: 16)
Gogo Yimidjir (Australia)	de Zwaan (1969)
f Gros Ventre (N.A.I.-Montana)	Flannery (1946) Taylor (1982)

Table 2.II continued

A. PRONUNCIATION DEPENDENT ON SEX OF SPEAKER

Hitchiti (N.A.I.)	Haas (1944)
Italian	Merlo (1952)
Japanese	Jespersen (1922: 243) Bodine (1975)
Kazakh (Turkic-Central Asia)	Zelinin (1929-30: 142)
Koasati (N.A.I.-Louisiana)	Haas (1944)
Koryak	Stebnickij (1934: 58)
Latvian	Ruke-Dravina (1952)
Metaco (S.A.I.)	Harrington (1944: 108)
* Persian	Jahangiri (1980)
Rumanian: Albania	Capidan (1930), as reported by Pop (1952)
Moldavian	Rankin (1976), as reported by Taylor (1982)
Russian and Ukranian dialects	Šerech (1952)
* Russian	Capell (1966: 101) Shapiro (1968: 20)
Spanish: Granada	Salvador (1952)
Castilian	Lorenzo (1966: 24)
* Panamanian	Williams (1983) Cedergren (1970)
Sumerian	Diakonoff (1974)
£ Swedish	Fant (1966; 1979)
Urmi	Garbell (1965)
Yao	Meinhof, as reported by Jes- persen (1922: 256)
Yana (N.A.I.-California)	Dixon and Kroeber (1903) Sapir (1929)
Yukaghir (N.E. Asia)	Capell (1966: 101)
Zuñi (N.A.I.)	Bunzel (1933-38)

Table 2.II continued

B. PRONUNCIATION DEPENDENT ON SEX OF PERSON SPOKEN TO

Chichimeca (Mexico)	Driver & Driver (1963: 103)
Yana	Sapir (1929)

Few explicit but possible: e.g. 'baby talk'; speech to the deaf; whispered and shouted speech and, by inference, in Nootka (Sapir, 1915). Implicit in every social linguistic interaction? See Chapter 8, Section 8.3.

C. PRONUNCIATION DEPENDENT ON SEX OF SPEAKER PLUS PERSON SPOKEN TO

Yana	Sapir (1915)
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D. PRONUNCIATION DEPENDENT ON SEX OF PERSON SPOKEN ABOUT

None reported, but possible, c.f. Nootka again

KEY	N.A.I.	North American Indian
	S.A.I.	South American Indian

Those languages marked with 'f' will be described in some detail in the course of this thesis. Of the remainder, there is either too little evidence from which to form a coherent argument, or the evidence is too anecdotal to merit serious consideration. These examples are included nevertheless because they have been remarked on and they help to form a picture of how widespread sex-specific pronunciation differences may be in global terms. An asterisk indicates results of a post-Labovian, socio-phonological or socio-phonetic viewpoint and will appear, summarized, again (for ease of reference) in Table 3.I. Full references to the sources in Table 2.II appear in the Bibliography. The approximate geographic location or language family of the less well-known languages is supplied for convenience in parentheses after the name of the language.

The daunting length of Table 2.II demonstrates clearly that there is no shortage of studies of phonological sex-differentiated segmental variation, and even this list does not claim to be exhaustive! What is not determinable from the table, however, is the quality of the studies. Many (e.g. those listed for Bengali; Caraya; Cham; Darkhat Mongolian; Japanese; Koryak and Yana) derive from qualitative studies and anecdote, while others attempt greater rigour in their empiricism but only record a minimum number of informants (e.g. Arabic and Swedish). Indeed, it is regrettable to conclude that most of the arraignments of

Section 2.4. may be aptly applied to most of the pre-Labovian studies at least in one respect or another. And the post-Labovian studies have their own intrinsic problems too, as will be disclosed in the following paragraphs, and, from theoretical standpoints, in the next chapter. Table 2.II can nevertheless be seen as having both a retrospective critical value and another value which is, by inference, constructively predictive.

We now continue with our critique, and with points which relate mostly to the early studies.

2.4.1.

A primary question, which was forecast in Chapter 1, concerns the reliability of the linguistic expertise of the collectors of the original data. The great majority of language examples listed by Bodine as containing evidence of sex-specific usage were documented in the late nineteenth and early twentieth centuries. Indeed, the chronological range falls mainly into the years 1818-1944, with only three of the pieces of evidence being published post 1960. From a list which comprises twenty-six separate sources, it is easy to see how preponderant is the evidence provided by these early studies.

While not wishing to open-handedly dismiss the validity of linguistic research prior to the latter half of this century, a certain caution should definitely be exercised in accepting the findings wholesale.

2.4.2.

These early researchers were commonly missionaries, geographic explorers and anthropologists. They were unlikely to have had any rigorous training in the techniques of the description of a language. Hence they would be relying probably on either Eurocentric grammatical description as a baseline for the analysis of all other languages, or (the dreaded) 'objective observation' coupled with the 'native speaker' intuition of the informants. It need not be belaboured that these 'techniques' are unlikely to produce the most accurate or unbiased description of a language and the phonological variants apparent in that language (c.f. Chatterji's (1921) work cited above, and the comments quoted from Smith (1985) in the Introduction to this chapter).

2.4.3.

Phonological field techniques employed at the time of these reports did not include the benefits of modern technology. There were no audio-recordings made of the informants. Data was probably recorded manually, using an ad hoc and idiosyncratic transcriptional convention. Any reconstruction from this type of record is obviously open to misinterpretation not only by a second analyst, but also by the original analyst on a subsequent occasion. The lack of permanency of records is a problem afflicting much phonological work as a whole, and supposed sex-specific findings do not escape this shadow of lack of credibility.

2.4.4.

The supposed differences may well have been noticed precisely because they were 'exotica', compared with the language systems known to the researchers. Now, just because a phenomenon is apparently unusual at first sight, it does not necessarily mean that that 'unusualness' has any significance intrinsically in the system under observation. So, the early documenters may have been seduced by the relative novelty of the segments they were hearing into describing them as distinct phonemes. This point is taken up again in the next of the criticisms given here.

2.4.5.

It cannot be ascertained that the speech segments annotated as belonging exclusively in usage to one sex or the other were actual distinctive **phonemes** or merely **allophones**. That is to say, we have no way of knowing now whether the sounds heard occurred in free variation or were in actual complementary distribution (i.e. phonemes), according to the sex of the speaker. Attempted replication of data-gathering would serve little purpose, for we would not now, of course, have access to a similar synchronic version of the language(s). Nor could we hope to duplicate the particular frozen social structure, or the frozen moments of 'conversation' which took place.

2.4.6.

Does the 'evidence' of apparent sex-specific segments arise only as a result of the androcentric bias of the investigator? One prime example of placing the male cart before the female horse has already been described in detail. Others are easy to assemble: indeed, from the sources listed by Bodine, only three of the ethnographers paused for reflection on which form of the language, male or female, was basic and which was derived. The sensitive three were Sapir (1915) (surprisingly) with his description of Nootka; Haas (1944) with the Muskogean languages Koasati, Creek and Hitchiti, and Flannery (1946) with Gros Ventre. Now, this widespread insensitivity does not exactly undermine the **evidence** of sex-specific segments, but rather the analysis of it, once observed. The stance of the observer is not to be underestimated, however, and may lead to a language being reported as having one prevalent sociolect and one minor one, when, in fact the reverse is the case. Flannery's (1946) remarks on this issue are particularly interesting, and deserve a more detailed exposition, in the next section. As a result, the original comfort to be drawn from them may be revealed to be rather full of thorns.

2.4.7.

Can the information supplied by native speakers in the first place be relied on? Bodine (1975:145-146) perceives this

difficulty, and provides evidence from Flannery's (op. cit.) study of Gros Ventre. We now show how speakers of Gros Ventre (albeit unconsciously) were capable of leading Flannery around the mulberry bush.

Flannery claims that there are explicit sex-determined lexical forms in Gros Ventre, such that speakers can state unambiguously what these forms are and can quote them reliably. However (and here's the capricious rub), when a woman quotes a man she uses female pronunciation, but when a man quotes a woman, he uses male pronunciation. Flannery then tells us (1946:139) that the speakers cannot state the difference between the two forms of speech. Well, that may not seem very noteworthy as such, because the phenomenon may be attributed to metalinguistic descriptonal insufficiency on the Gros Ventre speakers' behalf. Hardly their fault, one might add, as they may never have occasion to 'talk about talk'. Our suspicions are nevertheless aroused because (a) Flannery has already committed herself to there being such sex-specific **observable** differences, and (b) there is only one (at least according to Flannery) pronunciation difference between the sexes in Gros Ventre! This difference is a very simple change of place of articulation, with manner kept constant, e.g. women use a velar articulation while men use alveolar and palatal places of articulation for fricatives. The native speakers may well not be able to express the difference in precise phonetic terminology, but since it is so consistent and

universal in the language, it is then surprising that the informants could not apparently describe this difference to the investigator, Flannery. Or is it the case that they would not describe it? Several possible explanations spring to mind:

- (i) the speakers of Gros Ventre considered this variation an insignificant detail of their language which they did not judge important enough for the researcher to be interested in (even though she was).
- (ii) there existed some kind of antagonism within the Gros Ventre society and between the sexes about the use of these distinct forms. This antagonism may have been deemed 'ingroup trouble', which they did not want to highlight for the researcher and the outside world in general.
- (iii) the sex of the interlocutor played a role in determining which pronunciation was used, in a rather more complex way than was perceived by Flannery. The social determinants of this variation may have become convoluted enough such that it was actually syntactically, semantically, lexically and in terms of pronunciation impossible to explain the fine points of 'a woman quoting a man' to a woman (i.e. the investigator)! Given the effort involved to merely sort out the permutations from a tentative English gloss, it is not difficult to project that the grammatically-entangled informants could not free themselves from the language system and describe the

sex-related differences to Flannery.

- (iv) the alternations in pronunciation were simply not perceptually different to the Gros Ventre speakers. This situation is often described in cases of child language acquisition (where it is often called the 'fish' syndrome for short), and of second language acquisition by children or adults. Thus there are examples where a child will pronounce /fis/ for /fiʃ/, but when the 'correction' is made the child will respond with 'I didn't say /fis/, I said /fis/'. Thus the child is clearly perceiving a difference which is either too minute for an adult to notice, or exists as a perceptual reality for the child but not an articulatory one. This is clearly a case of 'over-perceiving' (i.e. a failure to lose some non-distinctive features). On the other hand, there are the familiar and well-documented cases of a learner of a foreign language failing to perceive a phonemic distinction in the target language. As a consequence, they either produce one phoneme in all the places where the two should be contrastive, or substitute a phoneme or phonemes of approximate similarity from their first language. Bearing these possibilities in mind, it may have been true that the Gros Ventre speakers simply did not perceive alveolar, palatal and velar fricatives as being distinctive. The fact that they had acknowledged sex-determined differences to Flannery may have been the result of the speakers perceiving solely lexical

variation (an example of which Flannery includes as an anecdote), and not specific variants in pronunciation.

Probably we will not ever know the solution to this conundrum, as there are too few speakers left of this language. Even at the time of Flannery's description, forty years ago, English was rapidly replacing Gros Ventre, and many young speakers spoke only English. Taylor (1982) nevertheless located a scattered handful of remaining speakers (although not necessarily with Gros Ventre as their first language any more) and produced the following deduction (p.304), among others:

I asked...why he was "talking like a woman". He was startled, but he replied without hesitation that he was talking in that way in order to make it easier for me. It occurred to me then that what I was recording was possibly not 'female' speech so much as Gros Ventre 'foreigner talk'. What Flannery had called 'men's and women's speech' - though it is certainly that in the main - was apparently something more complex, something that correlated at times with more elements of the speech context than the gender of the speaker. (My emphasis).

Taylor proceeds to conclude eventually that gender (sic) alone,

...is too trivial a difference to justify a thorough going linguistic difference. (p.305)

From the preceding points of this discussion, the present writer is largely in accord with these remarks by Taylor. While the detail which the description of Gros Ventre has been accorded may appear to be somewhat unbalancing at first sight, it is hoped that the reader may recognize a general critical stance emerging from the spotlighting of this

particular language. For the impatient, the essence of the critique is extracted in the conclusion to this chapter (Section 2.5.).

2.4.8.

Just how frequent are the pronunciation differences in the early reports? Since none are quantified, we have no means of knowing how numerous are the occurrences, or whether the 'sample' was large enough upon which to construct a theory of specific male and female languages. It is not inconceivable that what is often reported as general sex-specific 'variation' is merely a few isolated instances, or even the idiolectal idiosyncracies of one speaker.

2.4.9.

How far are the differences reported sex-exclusive and how far are they sex-preferential? As the argument in Chapter 1, Section 1.9 illustrates, this distinction is not only an important one to generally take on board, but in this instance it may lead to a complete re-working of previous phonological analyses. The problem we encounter in trying to apply this distinction to studies which antecede the exclusive-preferential dichotomy, is that we simply do not know whether usages reported are exclusive or preferential. No threat is posed if the former is true, for then the picture from the data remains clear (or as clear as it can be, given the muddiness engendered by the other criticisms in this section). If, however, the usages turn out to

be/have been preferential then we may flounder. The term sex-preferential, while providing a broad category of differentiation, brings us no nearer to a precise account. We cannot tell whether a form is used by all the speakers some of the time, some of the speakers all of the time, or the majority of the speakers most of the time. Once more, then, we are forced back to the need for objective and quantitative study rather than its rather-too-subjectively based antitheses which have been represented so far.

2.4.10.

One last extrapolation which may be made from a close analysis of the languages sources in Table 2.II, is the construction of a table illustrating the major phonetic categories (e.g. plosives, affricates etc.) in which sex-specific segmental phonological behaviour has been recorded. Such a table has been calculated and appears as Table 8.I. It forms the basis of hypotheses of a more directly phonetic nature, concerning the directions which sex-specific speech might follow.

2.5. Conclusion

There are four main points which emerge from the foregoing:

1. Previous ethnologically-based studies should be examined very closely to determine whether sex-related differences exist at all, or whether they are an artefact of the investigator's cultural stance.

2. Very few of these early studies rest on a reliable database, in terms of either homogeneity of speakers or quantifiability of techniques.
3. Manifold phonological evidence appears nevertheless to exist across languages, which would support the continuation of research into the possible reasons for these variants.
4. This research should be firmly couched in an interdisciplinary perspective, taking into account such possible independent variables as status, (both socio-economic and social), ethnicity, age, education, health, stature and so on. It should also be conducted by trained linguists, using a corpus of data which can be replicated (as far as possible) across languages.

Specific criticism of the construction and content of phonetic databases (and how these may affect phonological analyses), is reserved for Chapter 4. As a consequence, some guidelines for the improvement and standardization of phonetic and phonological corpora are made. For the moment, however, we should take significant notice of the overall leakiness of previous phonological studies, as demonstrated by the foregoing criticisms, and view their findings askance.

We have found the majority of previous phonological investigations to be less than totally convincing. Do the social insights of Labov improve those studies carried out since the 1960's? This question is examined in Chapter 3.

Chapter 3

Labovian and post-Labovian socio-phonological studies

Introduction

In the 1960's William Labov (1963; 1966) ensured the progress of phonological studies of variation from sheer linguistic taxonomy (often for its own sake) to a discipline which now acknowledges ethnology, sociology and politics as influential upon the realizations of phonological variants, and which is also committed to addressing major questions of linguistic theory. Vincent-Richter's clarion-cry which was quoted in Chapter 2, Section 2.1. seems then to not have been totally unanswered.

Smith (1985: 7-8) provides an excellent précis of Labov's model. It is expedient to reproduce his distillation of the main innovations of that model here. The new paradigm included:

1. The choice of a non-exotic urban population.
2. The premeditated sampling of the population on the basis of several social parameters (age, sex, socio-economic status) simultaneously.
3. The observation of each informant in several different speech situations (in this case ranging from formal to informal), over which some degree of control was exerted to ensure uniformity or standardization of the procedure.
4. Premeditated, precise measurement of several dependent phonological variants simultaneously.
5. The wedding of linguistic, sociological and situational dependent variables in a single research design.

The tightening of sociolinguistic methodology as a whole consequently affected the examination of sex as a variable, and of phonological variants in turn. Great improvement was thus claimed for this model over the previous techniques which were liable to admit all too easily serendipity, subjectivity and legerdemain (see Chapter 2, Conclusion).

Felicitous though Labov's original model was at the time, it has not remained unfortunately the universal hydra-slayer that it at first appeared to be (the need for which was implied in the Introduction to the preceding chapter). Nevertheless, the work was pioneering and has been subsequently hugely influential.

This chapter outlines the main findings of Labov's work, and indicates some ambiguities and problems. It then characterizes how post-Labovian enquiries have attempted to tailor the model to their own needs (and, more often, have not succeeded). Extensive coverage is given to a constructive critique of Labovian methodology and we are forced to conclude that much remains to be done to successfully hone the application of the model to the varying sociolinguistic needs of speech communities other than New York in 1966 or Martha's Vineyard in 1972 (as examined by Labov).

Problems emerging from a wholesale import of a Labovian model will be discussed, particularly as they have appeared in phonological analyses of British English. The lessons to

be learnt by phonologists and phoneticians alike are then indicated, before we proceed to show, in Chapter 4, how these lessons have either been ignored or adopted piecemeal by phoneticians.

3.1. Labov's results: a short overview

Parsimony dictates that all Labov's findings concerning sex-specific differences cannot be inspected in detail here. We will thus restrict ourselves to brief exemplification, together with signalling Labov's evidence for women's and men's speech being significantly different.

The most cogent of the findings to appear from Labov's (1966) interviews in New York City was that much phonological variation could be accounted for by the convoluted interactions of four independent variables. These variables were socioeconomic status (SES), ethnicity, sex and age. A further, dependent, variable was added in the form of social context, and this again interacted with the speaker characteristics. The most reliable indicator of variation, it turned out in this study, was SES (and this will receive specific notice in Sections 3.5 to 3.7). Sex, age and ethnicity were nevertheless also influential and served to predict some phonological consistencies.

In the course of monitoring five phonological variables as produced in the speech of women and men from the Lower East Side in New York, Labov (1966) found specific sex-linked variation in the following areas:

- 3.1.1. As demonstrated best by the variable /θ/, women most frequently pronounced the standard ('correct') form. Men more often produced /t/ or other stigmatized, socially unfavoured forms in words such as think. This pattern was echoed to a lesser extent by the variable /ð/.
- 3.1.2. Women **hypercorrect** more than men; i.e. they show more changes in their pronunciation from formal to informal contexts, (specifically where the variant chosen is phonologically the wrong one - see Chapter 6, Section 10.4). This feature was first highlighted by Labov's examination of the behaviour of the vowels /æ/ and /ɑ/, where higher incidence of the prestige forms in formal language was noted. The quality of hypercorrection by the women speakers in the British English data will receive further attention in Chapter 6.
- 3.1.3. While women from all SES-defined groups showed similar stylistic variation patterns to males, the style shifts among **middle** and **lower middle-class women** were more extreme than those of the males, and included more incidence of hypercorrection.

Now, while Labov's particular phonological variables are supplied here for accuracy, it is immediately apparent, for the purposes of a general discussion, that exactly which variants are at stake is largely irrelevant. There have been no arguments for a universal variable theory based on

sex preferences and one is not about to be expounded. The crucial points for our argument throughout this thesis are listed in sub-paragraphs 3.1.1 to 3.1.3. above. The recurrence of the general pattern of these New York City results across languages will be shown to be impressive, but not without room for scepticism in the ensuing sections of this chapter. Issues arising from these central discoveries are now discussed more fully in Sections 3.2 to 3.7.

3.2. Women as conservatives or innovators?

In both Labov's major works, overt reports of linguistic insecurity were recorded as being much more prevalent among women than men. That this insecurity gives rise to greater adherence to norms and can lead to hypercorrective behaviour is the subject of this section. We are thus tracing how Labov's first (3.1.1.) and second (3.1.2.) conclusions are directly capable of being linked.

The many similar studies which are presented in Section 3.8. have all given apparent support to Labov's first conclusion that, especially in formal situations, women adhere more closely to the perceived standard linguistic behaviour than do men. There seems to be a wealth of evidence from societies as disparate as French Canadian (Sankoff and Cedergren, 1971); Belfast (Milroy, 1980) and South African English (Lanham and Macdonald, 1977) to support this general finding. Smith (1985: 79) provides a whole page of study-citing, which it would be wasteful to repeat here. It would

therefore appear to be appropriate to label women as **conservatives** and standard-protectors rather than forgers of new phonological patterns.

A closer look at the foundations for this assumption reveals however more sand than rock, simply because Labov shifts his ground in the course of the 1972 exposition. Sociolinguistic Patterns (Labov, 1972) is often regarded as the seminal sociolinguistic work on speaker sex as a variable (the 1966 work being a research report, and not focussing particularly on theoretical issues). Among Labov's conclusions, the main finding of note for current consideration is that (1972: 303),

...the rate of advance and direction of a linguistic change owes a great deal to the special sensitivity of women to the whole process.

Labov goes into greater detail and proceeds to state (p.304):

The sexual differentiation of speakers is not ... a product of physical factors alone, or of different amounts of referential information supplied by speakers, but rather an expressive posture which is socially more appropriate for one sex or the other.

Before accusing Labov of determinism (and of subtly echoing Michaelis', 1768 "pretty mouths") it must be said that he appears to be scrupulous in pointing out (ibid.) that,

...the correct generalization is not that women lead in linguistic change, but rather that the sexual differentiation of speech often plays a major role in the mechanism of linguistic evolution.

The specificity of this comment is undeniable, but the comment is often overlooked by linguists who claim that

Labov champions women as innovators. This oversight is not remarkable however, if we reveal that the statement above is refuted by Labov's own words in the same volume (1972:141):

Hypercorrectness is certainly strongest in women - and it may be the case that the lower-middle class mother, and the grade-school teacher, are prime agents in this type of linguistic change.

Labov's two-sided argument has great advantage as a defence against subsequent contradictory evidence. For example, Trudgill's (1972) results, (discussed in detail in Section 3.4.), seemingly contradict in part Labov's original conclusion (see above) because his analysis shows **working-class men** to have a role to play in linguistic change. On the other hand, the Norwich results are then not at odds if we invoke Labov's statement about "the sexual differentiation of speech" because it is both middle-class women and working-class men who are active in the processes linguistic change!

By highlighting the 'heads I win, tails you lose' nature of Labov's argument, we may perceive that no single answer to the question of 'who leads linguistic change?' is realistically offered. It seems that Labov was unable to see how sexual differentiation in language is connected to linguistic change, and his general model is too simple. Throughout the subsequent sections, it will furthermore become increasingly apparent that the expectation of single, unilateral answers to most of the sex-attributed or class-attributed queries raised by Labov is essentially unreasonable.

3.2.1. The chameleon model

Perhaps the most accurate single inference at this stage from Labov's work is that while female speakers may indeed maintain the standard, this behaviour may not always be as influential as other currents (perhaps generated sometimes by male speakers) active at the same time in the process of language change. Further, it is probably more sensible to portray women as linguistic Proteuses, changing the form of their pronunciation to suit the purposes of the linguistic moment.

Hence, a weaker version of the contention that women are norm-makers is required. A feasible characterization would be to call the action of women **chameleon behaviour**. Much of the origin for this kind of behaviour can be accounted for by **accommodation theory**, as propounded by Howard Giles et al. (1977 inter alia). I would like to propose that this behaviour is apparent in women in particular, for reasons which will become apparent below.

Giles' central proposition is that speakers constantly adjust their speech posture in order to minimize (or, under different circumstances, maximize) the differences between interlocutors. They may wish to convey social similarity or distance, approbation or criticism, or crave approval. So, the greater the need for approval, the greater will be the approximation, convergence of the speech of the seeker towards that of the approval-provider. Naturally, this

approximation may go either upwards or downwards on the 'social scale'. For further explication of this theory, see Giles and Powesland (1975) and Giles and Smith (1979).

The effect of accommodation theory on chameleon behaviour is that women (seeking approval) may well adopt the standard forms of a language more often than do men. Now, the phrase 'standard forms' is not unproblematic itself, and its facets are explored in Section 3.3. here. Meanwhile, we shall use the term as a convenient shorthand. To assume that women will necessarily adopt standard forms is nevertheless too simplistic. A more accurate theory would hold that women may accommodate their idiolect to whatever is perceived as the desirable accent/dialect of the communicative setting at large. The use of "perceived" is deliberate, since the nature of the prestige forms may vary from speech community to community; for this reason we use **perceived prestige form** throughout, to indicate the possible heterogeneity of these forms.

Accommodation might incur the incorporation of accentual features which occur in the phonological inventory of an appropriate prestige acrolect, **but this is not necessarily always the case**. Thus, if a speaker perceived the social/linguistic integration desired as being accessible through some local (**non-prestige**) pronunciation, then that is the phonology towards which she might gravitate. If this adoption of the new pronunciation is achieved well, then her insecurity will be lessened: she will perceive herself (and

be perceived) as being more centrally included in that particular society. If her attempts are over-strenuous, then she runs the risk of producing hypercorrect forms which instantly reveal her insecurity to the core speakers of the language/dialect. Further, if the speaker is attempting to accommodate to a perceived prestige form and, en route, overgeneralizes the rules as she has learnt them, and thus produces hypercorrection, then she is in danger of being labelled a 'social climber'. If accommodating 'downward' (i.e. from, say, a national prestige form which is native in her own idiolect to a local prestige form), then she is unlikely to be criticized as a 'climber', but still runs the risk of being called 'false' or ingratiating. The Batesonian double-bind is one which constantly recurs in this study; and this is an obvious instance of a situation where a speaker is 'damned if she does, and damned if she does not'.

Elements of this argument have been mooted by several other studies. For example, on a general note, Jean Aitchison (1981: 82) remarks,

Women are consciously striving to 'speak better', partly because of a certain social insecurity, partly because they are not aiming to sound 'tough'. They presumably encourage their children to talk in a socially acceptable way, and so aid in the changes in the direction of the standard language. Subconscious changes, on the other hand, may well be initiated by working-class men...We all know couples like Edna and Eric who were of working-class Liverpool stock, but who came south and moved into the fashionable 'stockbroker belt' on the outer fringes of London. Edna rapidly lost her working-class Liverpool accent, though Eric retained his for the rest of his life.

Little can be said in this thesis about women shaping the pronunciation of their children, although there is a growing amount of empirical interest in this area in general (see the extensive bibliography on 'Language between Adults and Children' in Thorne, Kramarae and Henley, 1983: 312-319). There remain, at the moment, no specific studies of mothers' commenting on children's pronunciation but it is reasonable to speculate that some such lucubration is currently under way (see Snow and Ferrguson, 1977, for some preliminary indications). The effect of working-class males on the overall direction of language change will be discussed, with reference to Trudgill's results, in Section 3.4. Of most immediate relevance, though, are Aitchison's comments concerning the stereotyped husband and wife. She does not extend her consideration to arrive at the 'Catch 22' described above, but one could easily imagine a last sentence in her cameo: 'Edna was called pretentious and Eric "genuine"'. Most cogently, though, Aitchison emphasizes the malleability of women's speech. She also provides perfect arrowing to the results which are revealed in Chapter 6, describing the comparative degrees of modification practised by female and male speakers of a Northern English accent who have subsequently moved to the south of England.

The chameleon model receives further, precise underpinning from Levine and Crockett (1966: 97-98), who report that women are more likely than men to change phonological

patterns as the formality of the speech context changes. In correspondence to Cheris Kramarae, Patricia Nichols has called this phenomenon "linguistic flexibility" (Kramarae, 1981: 104).

We are led, then, to an amended, non-binary view of women's role in the process of linguistic change, and one which Labov (op. cit.) at times implied. Lakoff (1975) may be frequently misguided in her linguistic interpretations, but she does make an important point in connection to this particular issue: that a group may be conservative in one aspect of language use, radical in other. These two tendencies are by no means as simple as they might appear, and Lakoff's reading is quite plausible: that speakers exploit phonological forms to suit their purposes and the situation **at the time**. We can thus re-iterate our expectations of multilateral 'explanations', and show Labov's first conclusion (3.1.1.) to be rather over-generalized.

3.2.2. What is wrong with being sensitive = what is wrong with being female

Lacking in all the foregoing is an evaluation of women's labile linguistic practices in **positive** terms. Nowhere is it commended how well women manipulate their speech to exploit the communicative situation as fully as possible, and, as a result, achieve more successful 'communication' more quickly from either a co-operative or personal standpoint. Labov may allude to women's "special

sensitivity" (1972: 303; see Section 3.2 for full quotation), but he does not expand about why exactly this trait may be an advantage. One suspects that, only slightly beneath the surface of Labov's studies, lies the belief that the male form of speech is basic (see Chapter 1, Section 1.4), and therefore it is the female form which is different, abnormal and (as stated overtly in many early remarks, but lingering covertly in later accounts) inferior. Were women valued more positively socially, then their language would not be described in terms of either 'conservation' or 'innovation', but merely regarded as the norm (see cutting comments to this end in Anshen, 1979). Labov's terms may not have been consciously chauvinistic, because his major theoretical interest was in linguistic change, and he needed the dichotomy to fit major parameters of variation into a theory involving innovation/conservatism. The contrast has been adopted more widely, however, with the result that women are cast as linguistic 'push-me pull-you's', vacillating in their speech and buffeted by a constant desire for social approval. Rohrlich-Leavitt et al. (1975: 569) have stated this in quite cynical terms, saying that women have "the special sensitivity that members of subordinate groups must ... develop to those who control them."

The characterization of women as chameleons is, on further consideration, also insufficient in that it implies some lack of self-determinism in their behaviour. It creates an

image of women being 'at the mercy' of the social and linguistic situation. It would be a further act of subtle derogation to maintain the impression of **passivity** in women's behaviour in this way. Picking up the point made in the previous paragraph, there should also be available a positive, commending description of women's **active** role in communicative flexibility. Examples of this active manipulability of the linguistic moment by women are easy to adduce. When women choose to use forms which sound 'sweet', they mark the speech-act as amusing and social rather than 'informative'. Likewise, 'babytalk' and imitation of ethnic accents also indicate playfulness. As Lakoff (1977: 303) states:

... it would be to a woman's advantage to use phonological forms that made her sound 'cute' and non-serious - for the same reason that it is to her advantage to have in her lexicon adjectives like 'charming', 'divine' and 'adorable'.

Key (1975: 105) also has an interesting insight about why it may be a misrepresentation of women's phonological usage to call it passive and malleable:

...dialect differences and unusual forms of speech may be difficult to elicit from women who are more socially conscious of being denigrated. Language is one way in which females can better themselves, even if only in their own image.

Other possibilities which have been suggested variously for the labelling of women as active 'norm-breakers' (for the coining of the terms 'norm-makers versus norm-breakers' see Keenan, 1974) are that they are less well-educated; and as passive 'norm-keepers' they are 'arbiters' of etiquette and

respectability, resisting change, linguistic or moral. The latter is echoed from Lakoff's well-worn argument but it is en passant saddening to notice that what provided Swift (c.f. quotation in Chapter 1) with an explanation in the eighteenth century still receives credence for the description of the linguistic behaviour of women today. A more satisfactory description would hold that in different interactional settings, women may be **both** active and passive, alternately.

These last two sections have indicated that (1) a static, binary view of the role of women in the processes of linguistic change is insufficient, and (2) that to describe women's communicational acts as essentially passive is an unjust misrepresentation. Labov's first two conclusions (3.1.1. and 3.1.2.), then, cannot be wholeheartedly supported from this standpoint. In the next section, it is shown that other underlying assumptions in Labov's work cannot be accepted with confidence. In particular, the implications of the phrase "standard ('correct') form" are examined.

3.3. Heterogeneity of 'norms' and of 'standard'

As was adumbrated in Section 3.2., to expect to uphold one particular phonology as a national standard at all times, in all situations (both social and geographical) and for all speakers is to chase after shadows. While 'standard' language may be defined as that which does not contain so-

called 'ungrammatical' structures, it is not so simple to subscribe to the notion of a standard, immutable phonology. Crystal (1980: 329) enters his dictionary definition for standard as:

'Standard languages/dialects/varieties' cut across regional differences, providing a unified means of communication, and thus an institutionalized NORM which can be used in the mass-media, in teaching the language to foreigners, and so on.

Now, the focus of this argument is on the question of 'norms' rather than 'standard language', although it is not always easy to divorce the two, as the conflation by Crystal demonstrates. It is nevertheless interesting to note the weakness of the number of Crystal's examples as fairly indicative in itself. The reasons for establishing and maintaining a standard are not always easy to adduce or support, and inevitably lead to value-judgements of an insidious kind about the comparative merits of one accent/dialect over another (c.f. the well-known linguistic prejudice that Standard English is somehow better suited to the expression of complex ideas, as evinced intuitively by Honey, 1983, inter alia).

The defence of the argument that norms have to be maintained is not an issue that will be entered into here. There is, however, room for debate about what constitutes a phonological norm for whom and when. Perceptions of phonological norms are surely open to influence by speech style (register); age; ethnicity; political affiliation; SES; comparative status of the interlocutors (see J. Milroy,

1982: 40, who argues that there are "different patterns of prestige at different levels in the community"); setting and topic. There are, no doubt, more possible variables, but our point should be clear from these few examples. And, most importantly for the general thesis, the perception of a norm may be different for different people at different times, and for females than for males.

Overt enquiries into the latter division have not been reported, but there have been explicit reports of ethnic/geographical membership affecting the perception of standard or prestige forms. Smith (1985: 84-85) discusses some of the empirical evidence for the heterogeneity of prestige and standard speech norms. Two facts emerge most clearly:

- 3.3.1. Women and men from the same speech community differ in their evaluation of the comparative prestige of a variety of variables (Labov, 1966).
- 3.3.2. Women and men habitually over-report the number of prestige forms which they use in their own speech (as established by Labov's 'Self-evaluative Test', 1966). In Norwich (Trudgill, 1972), men under-reported their use of 'prestige' variants (giving rise to Trudgill's term 'covert prestige'), and women over-reported.

Taking these points into consideration, I must concur with Smith, and conclude that normative standards are seldom uniform and "do not warrant the assumption underlying the

conclusion about women's more prestigious speech" (1985: 85). Further pessimism intrudes when we also incorporate the evidence supplied by 'matched guise' tests for the different evaluation of regional accents in terms of prestige. The structure of such tests is outlined below.

3.3.3. How regional norms differ from one community to another

The comparative perceived characteristics of RP against other regional British English accents will be discussed in greater detail in Chapter 6. In matched-guise experiments, listeners are played recorded extracts spoken in various accents and/or dialects spoken by apparently different speakers who read the same 'emotionally neutral' passage. The listeners are then asked to judge the samples they hear according to impressionistic (and often subjective) rating scales. E.g. is the speaker with accent/dialect A more authoritative/ sympathetic/ successful/ trustworthy than speaker B (with a different accent/dialect)? The catch is that the 'speakers' are, in fact, one person alone (usually a trained phonetician), assuming different realistic 'guises' of the accents/dialects being studied. The technique has two main advantages: firstly, it attempts to overcome the effects of the speakers' individual voice characteristics (e.g. pitch, loudness, rate etc.) and secondly, it appears to show more about listeners' reactions to speakers than asking directly about attitudes. For further explanation of this method, see Elyan, Smith, Giles

and Bourhis (1978).

Results from such experiments are important to demonstrate that regional background can, and does, influence the listener-speaker's perception of a norm. Certain artefactual reservations apart, these experiments have shown that in different speech communities in various locations in the British Isles (e.g. Lancashire and South Wales), perceptions of what is the prestige accent/dialect vary. Indeed, in some areas, high status speakers have **several** choices available: in Belfast, for example, some MC accents approach English, while others sound Irish (L. Milroy, personal communication). We shall return to this issue in the course of depicting the so-called 'androgeny' of RP female speech (in Chapter 6, Section 6.16); but, for the moment, our interest lies in the lack of uniformity of the representation of norms. Indeed, and as we shall see in Section 3.9., one of the common faults of sociolinguistic studies is the expectation of social and perceptual homogeneity where it simply does not exist.

To conclude this section, then, we are forced to state that the case for women as guardians of norms is far from proven, because of the false assumptions of the methodologies employed up to now. We need to re-examine the research procedures, and probably re-design many, before pursuing the investigation of female and male connotations of prestige and non-prestige forms of speech. The former demand is met in Chapter 4, where a much-needed blueprint for the conduct

of quantitative socio-phonetic research is presented. The definition of separate male and female norms in different societies, however, lies outside the direct scope of this thesis. In essence, it is easier to describe 'avoidance of a vernacular' (as did the Milroys, 1978), for one person's 'prestige' may be another person's 'pretention' or 'slang' and the position of norms should be regarded as mobile rather than static.

3.4. Are middle-class women really "spearheading the march towards the national norm"?

At a first glance, Labov's findings are roughly corroborated by a great number of subsequent studies. They number among them those studies already listed in Section 3.2., together with more by Shuy, Wolfram and Riley (1967); Fasold (1968); Anshen (1969); Romaine and Reid (1976) and Hudson and Holloway (1977). Further details of these and other generally supportive reports are listed by Smith (1985: 79), and do not require replication here.

From an overview of these studies, it would no doubt be satisfying to be able to establish it is generally true that middle class women are the spearheads in language change, since this neatly connects two variables as interacting. Such precise matching of any two variables is, however, rare, and when it does seem to occur, we should be wary of the premises on which the results are based. Peering closely, we find three major faults in the premises, both

arising from the messiness of social reality compared with the somewhat utopian idealism of sociolinguistic methodology. These faults are:

3.4.1. Labov's two-sided argument (cited earlier in Section 3.2) means that subsequent studies may be seen as corroborative, whatever their results. The argument's Janus nature makes it unrealistic to accept straightforwardly that women are essentially important in the process of language change, but not necessarily the prime motivators. While we are not seeking monomorphous 'explanation' for language change, there is evidence that Labov believes in a deterministic link between hypercorrection, women and the middle classes. The link is apparent in the statement which has already been quoted, but which is repeated here for convenience (1972: 141):

Hypercorrectness is certainly strongest in women - and it may be the case that the lower-middle class mother, and the grade-school teacher, are prime agents in this type of linguistic change.

The unwarranted certainty of the first comment (viz. "Hypercorrection is certainly strongest in women") has been discussed in Section 3.2. above. The main objection here is to the implied uniformity of explanation, especially as we have shown that so-called uniformity to have ambiguous origins.

In this section, the focus is on the other two faults which can be unearthed in Labov's conclusions. These are:

3.4.2. The SES of women cannot be described adequately while continuing to employ traditional sociological tools.

3.4.3. Later studies (i.e. post-dating Labov's work) do not indicate that MC and LMC women are the sole "prime agents" of types of linguistic change.

We will now turn to the influential combination of SES and women which Labov so clearly connects, and delineate some problems arising from that partnership. It will be shown how, with hindsight, the third conclusion (3.1.3.) by Labov, that middle class (comprising UMC and LMC) women showed greater stylistic variation than did men, does not receive unanimous support.

The next two sections concern themselves with the substantiation of these criticisms. And, when their contents have been assimilated, it will be seen that the brief answer to the question posed in the title to this section is not a straightforward affirmative.

3.5. The 'class-lessness of women'

Illustration of the second of the points above (3.4.2) is provided by focussing on the problem which emerges in Trudgill's account of urban British Norwich English (1972).

The advisability of the direct transfer of Labov's technique to Britain will be examined in general later, in Section 3.11. Aware of some possible incommensurability between the societies of New York State and Norfolk, though, Trudgill did take care to use differing criteria for the selection of the 'classes' (= SES) of his subjects. Their SES assignation was based on income, education, dwelling type, location of dwelling, occupation and occupation of father.

The variable under consideration was the morpheme -ing, realised phonetically potentially as either /ɪŋ/ or /ən ~ n/. The styles investigated also tied closely to Labov's, viz. (in descending order of supposed 'formality') word-list style; reading passage style; formal speech and casual speech. After analysis Trudgill concluded broadly that women are more status-conscious than men because they consistently produce forms which more closely approach those of the standard language or which signal higher prestige. Women remain protectors of convention. His further evaluation is that in a 'covert' way, working-class men value non-standard speech as a signal of masculinity and group solidarity.

Now, the foundations for Trudgill's main deduction (that, in short, women are more prestige-conscious) has so far been shown to be insecure in Sections 3.2 and 3.3. The second conclusion (that WC men may exhibit signs of 'covert prestige') will receive attention in Section 3.7. and also in connection to our own empirical results, presented in

Chapter 6.

More importantly, though, within his own extrapolations, Trudgill (pp. 91-92) points to a primary social and linguistic stumbling block:

Men in our society can be rated socially by their occupation, their earning power, and perhaps by their own abilities - in other words by what they do. For the most part, however, this is not possible for women. It may be, therefore, that they have instead to be rated on how they appear.

His observation is correct, but he ignores the obvious obstacle to the acceptance of his own work, with the result that it could be regarded as syllogistic. If women cannot be evaluated by their occupation (the most common factor taken into sociological class-labelling) then how did Trudgill himself decide on their class in the first place? He explicitly states that occupation was one of the means by which he assessed SES, and yet here he admits that it is unusable for the assignation of women. The linguist hoist by his own interpretative petard.

The 'unclassifiability' of women undermines the credibility of many post-Labovian studies. It may, indeed, contribute to some of the lack of expected results, or to the disparate findings in societies which supposedly share some linguistic features or norms. The implications of the 'class-lessness' of women are far-reaching, and extend into areas which affect society much more directly than the results of sociolinguistic research. It is well-known (and too passively accepted as a fact) that government figures for

unemployment, claimants of supplementary benefit, and labour-division according to sex are inaccurate and thus misleading because of the invisibility, lack of existence of women. Statistics for unemployment percentages would be more credible if it were admitted that (a) the position of housewife/mother should be recognized as a full-time job, and (b), if women who had been in full-time employment were not deluded into believing that it is not worthwhile or is likely to affect their husband's claim if they too register as unemployed. Hence thousands, possibly millions, of the jobless are hidden because they are women.

On a similar scale, women cannot be classified in social stratifications according to present sociological method because the cornerstones are so frequently occupation for social class, and the family for the primary social unit. Nearly all socio-phonological studies follow this practice and use the occupation of the head of the household as a major index of a family's social class. So what does one do with unmarried women and widows? Typically they are eliminated from studies because of the familiar theme of 'classificatory problems'. Yet by the year 2025, 20% of the world population will be aged over 60 (data from the New Internationalist issue on Ageing, July 1982). Of that percentage, the proportion of men to women will be 7-10. Thus one can deduce that not only is the 51% female-to-male division swelling, but it is incorporating more old women who cannot be defined in current sociolinguistic surveys.

Are they to be conveniently swept under the carpet too?

Other solutions proposed have been to assign widows the occupation of their dead husband (risible), and to either give single women their own occupation (laudable, but these are typically low-status) or that of their father! Not only Adam's rib, but Adam's occupation too.

Married women, perceived as having no autonomous status if they do not have 'a proper job', are ascribed to their husband's class. There is considerable reason to doubt this general assumption (see Acker, 1973). In fact, Wells' (1985) study in Bristol showed that, in families, women were characteristically of slightly higher class status (i.e. came from families of such a status) than their husbands. Therefore, in order to be able to 'classify' women, the original research described in this thesis took great care to ensure that parity in the **occupations** of the female and male subjects was maintained (see Chapter 4, Section 12.).

3.5.1.

An even more fundamental objection is to the definition of class itself. Lesley Milroy (forthcoming) has explored this problem with insight. She indicates how, for example, different investigators use different indicators for social class on different occasions. She states:

This arbitrariness is one of the principal problems in the use of a social class index, and the implications of attempting to transplant the procedure from one kind of society to another are not always clear.

Agreement with this view has already been shown at the beginning of this section. Milroy then proceeds to expose the confusion which frequently arises in the separate interpretations of **class**, as distinct from **status**. These are disambiguated for linguists' use by Halsey's (1978:20) distinction:

...In short, classes belong to the economic, status groups to the social...

Milroy correctly perceives from this that linguists' interest is primarily in **status** and not in **class**. Using this argument, we can then remedy the position of women somewhat. While they still may be **class-less** (because class is linked to economic facts and occupation), women are **not status-less**.

We are still not entirely out of the trees, though. For a start, the crucial term **SES** conflates both class and status, by definition. Also, Milroy's further comments show that if class and status are combined, there still remain cross-linguistic non-uniformities:

To the extent that occupational groups overlap with status groups sharing similar prestige and life-style, we can view the two variables as co-extensive and not worry too much about distinguishing them. But the problem for linguists is that the interrelationship between status and class appears to vary in different communities, with the result that attempts to transplant a stratification scale which was sufficient for its purpose in New York City may not always be successful.

Two more impediments to the adoption of a one-dimensional use of the term class remain. The first occurs because of

the variability of social structures (Milroy):

...persons of the same class may have very different statuses if they live in cities with different class structures.

The second impediment lies in the differential amounts of mobility of classes in different communities. Milroy indicates that in Britain, for example, mobility is less than in Australia or the United States.

The cumulative effect of these problems may be seen as rather stultifying. Milroy does not provide any water-tight solutions, but concludes by directing attention to the need for circumspection when using the class variable because of possible ramifications for results. The task here, too, is to indicate pitfalls rather than necessarily produce repairs or remedies. In my own study, the variable of class was not under investigation and so the question of finding solutions for the definitional problems was sidestepped.

3.6. Middle class women as one of the agents, but not the sole agent, of linguistic change

It has thus far been shown that analysis of the SES of women is problematic. It still remains for us to answer fully the question we posed as a heading to Section 3.4. Namely, is it specifically **middle-class women** who are "spearheading the march towards the national norm"? We will now proceed to explain why the third general point of Section 3.4.3, that middle-class women cannot be portrayed as the only agents of linguistic change, can be made.

In America (according to Levine and Crockett, 1966) the vanguard of national norm-approximation comprises middle-aged middle-class women (and the young), but in Norwich (Trudgill, 1972: 102):

...there appears to be a considerable number of young working-class men marching resolutely in the other direction.

So, apparently middle-class women are approaching the norm in one society, while in another young working-class men are moving away from the norm. Here, it does not matter that the norms are bound to be phonologically remote, for the point is that it is **not only women** who are contributing to language change. This is precisely the implication by Labov that should not be allowed to remain unchallenged. Despite the faults which have been indicated in Trudgill's study, this diversity of movement in the linguistic amoeba is successfully noticed. Such remarks help to underpin Lakoff's (1975) suppositions and demonstrate that the quest for a singly-attributable cause for linguistic change is not only fruitless but misguided.

Levine and Crockett's (1966: 98) metaphor referred to in the title of Section 3.4 may be arresting, but it is too simplistic, indeed unilinear in its assumption. While language change, incorporating the approximation to or distancing from norms, certainly takes place all the time, it should not be assumed that it takes place constantly in one direction, or that it originates in one sex or one social group alone. A more accurate metaphor for the

movements involved in such fluctuation is perhaps that of the amoeba: forever altering its outline, stretching one boundary while retracting another. Using this image, it is possible to envisage more than one motivator for change at one particular instance.

3.7. The greater 'mobility' of the middle classes

If a successful connection can eventually be made between SES and women, and if that connection upholds the predominance of the LMC (as Labov imputes) as active in linguistic change, then a simple reason for this behaviour could be posited. A good starting point is Labov's (1972: 132) pronouncement:

The great fluctuation in stylistic variation shown by the lower middle class, their hypersensitivity to stigmatized features which they themselves use, and the inaccurate perception of their own speech, all point to a high degree of linguistic insecurity for these speakers.

Now, the members of the lower middle class are neither at the top nor the bottom of the social 'heap': they are tucked in the middle, but only just. Their insecurity is linked to the ease with which they could 'slip' to become the lowest class if they did not monitor their own behaviour closely enough (hence the hypersensitivity). In general, it has been observed that the speech of the uppermost and lowest classes are relatively stable. The highest-ranking group shows the "greatest linguistic security" (Labov, 1972: 134), and has little or nothing to gain by altering its speech behaviour. Similarly, the respective social 'station' of

the lowest class is relatively immutable. As Labov (ibid.) says,

The working-class respondents might be expected to follow the same general guidelines of behaviour as the lower middle class, but to a less pronounced degree. Only the lower class would be largely immune from the tendency to follow the latest prestige norms.

Precisely why the working-class is less prone to adopt prestige forms is not spelled out by Labov. His claim is rescued, however, by the revelations from Trudgill (1972), which were cited above. Among the working classes (and particularly among men), there appears to be a need for a greater demonstration of group solidarity than among the lower middle classes. One way open to the WC to preserve this solidarity is through their patterns of speech, and (in the Norwich example) occasionally actively discard accepted norms because they are redolent of another class. British sociolinguists have no direct correlate for Labov's 'Lower class' (see Labov, 1964: 170, for description of the individuals who comprise his two lower classes), just as Labov does not include a sample of what would be called in Britain the 'upper classes'.

These incommensurabilities apart, there does seem to be a consistent picture in the behaviour of the lowest classes (WC and LC) across communities. Labov (1972: 134) is consequently able to produce a table of relations between the older and younger groups in his four classes, with regard to their use of prestige forms. The square brackets in Table 3.I. are presumably (for he does not give direct

explanation) Labov's attempt to indicate that the behaviour of the working-class respondents is generally the same as the lower middle class, but exhibited to a "less pronounced degree". Similarly, we must interpret from Labov's text that the entries 'higher' actually mean 'greater usage than high'.

Table 3.I. Scheme of usage (low versus high frequency) of prestige forms, according to age and SES. Reproduced from Labov (1972: 134).

	Lower class	Working class	Lower middle class	Upper middle class
Younger	low	[lower]	lower	high
Older	low	[higher]	higher	low

Members of the lower middle class, in contrast to the relative stability of the highest and lowest classes, are often active in seeking upward social mobility. They have no particular solidarity to demonstrate: indeed, the very nature of social ambition dictates that any possible disadvantageous links should be severed. So, while the UMC and the LC may both be regarded as having 'nothing to gain' by adopting prestige forms, the LMC can be seen as having, conversely, the least to lose. Also, according to the Milroy (1980) model, the LMC has no dense networks to impose pressure on it. In rapidly adopting prestige forms, the LMC may risk the 'social climber' derogation, but, they may also 'get away with it' and their aspirations may never be detected overtly and will provoke no comment. Either way, the LMC member is still not moving downward.

It has already been said that one of the best ways in which a woman may improve her social standing, or at least her own perception of that standing, is through her speech (c.f. the quotation from Key, 1975, in Section 3.2.2.). If the LMC is the most susceptible to prestige forms, and the older members have the highest usage of prestige (see Table 3.I) then it is indeed **middle-aged** (and older) **middle class women** (for Levine and Crockett conflate the two LMC and UMC) who could then be the most active (than the other classes). It is particularly the women who are most influential because they, out of all the classes and sexual-subsets, have the very most to gain in terms of social approval and advancement (if only in their own eyes) by altering their stylistic repertoires. Whether this activity can be described as a "spearhead" is perhaps still open to debate because there are other factors at work, as we have already stated.

The argument remains rather cyclic nevertheless if we pause to ask whether it is the women's socio-economic status which is more influential in the process of language change, or the fact that they are women and appear to be more socially and linguistically insecure in the first place? Furthermore, does age not play a significant part in the rate at which stylistic variability occurs among women? From looking at Table 3.I, it is apparently older WC and LMC women who are the most prestige-conscious. But, patterns and societies change, and perhaps we should now be inspecting the behaviour of younger female speakers because

they are ambitious?

All these considerations are vital: attempts to incorporate them have been made in our assembly of data, but the provision of an immediate escape from the sex-versus-SES versus the sex-and-SES loop remains for the moment unsuppliable. Indeed, Labov (1972) was forced to come to the same inexplicable halt (p.302):

Why do women do this? It cannot only be their sensitivity to prestige forms, since that explains only half the pattern. We can say that they are more sensitive to prestige patterns, but why do they move faster in the first place? Our answers at the moment are not better than speculations...

One final indication of women's Protean habits, which is perhaps less well-known than the Labovian derivatives cited above, is Shopen's (1978) work in Canberra, Australia. Again it was the variable -ing which was scrutinized. Shopen did explore a slightly diverse aspect, however, and found (not surprisingly, by now) that speech adheres more closely to the standard norm in the presence of women (when either the speaker, the addressee, or both, are women). He also disclosed that most formality was exhibited in speech when women friends were speaking to each other. That women's insecurity became most apparent when in potential judgement by their own sex is a very interesting discovery. It adds support to arguments for women being more prestige-conscious in general; but it is not, however, a finding which we can afford further exploration in this thesis. It is thus an aspect of sex-specific language which requires further, future enquiry.

In the last few sections, it has been shown that Labov's original deductions were, on the whole, rather too overgeneralized. The issues of approximation to norms, of the concept of a norm itself, and of SES with regard to women have all been shown to be much more multi-faceted than Labov originally implied. It was not so much the case that he was wrong, but more that he was not right enough. In the following sections, however, we will see in general terms (and with some specific examples) just how tendentious some satellite studies have since been.

3.8. Other studies in the Labovian paradigm

The most effective way of presenting the scope of the numerous studies in the Labovian mould which have proliferated since 1963 is in a table, which is derived in turn from the overview version (Table 2.II.) in Chapter 2. Thus Table 3.II. shows a largely-representative number of socio-phonological studies carried out in the last twenty years or so. We will also indicate in the table, where possible, exactly which phonological variants were examined. Full references are provided in the Bibliography.

Table 3.II. A partial cross-language summary of socio-phonological studies conducted with methods similar to those of Labov (1966). The actual phonological variants under investigation are listed on the right of the table.

LANGUAGE	INVESTIGATOR(S)	PHONOLOGICAL VARIANT
AMERICAN ENGLISH:		
East Coast	Fischer (1958)\$ Labov (1972)	/ɪŋ/ Vowels;diphth- ongs;dental fricatives & stops; postvo- calic /r/
General	Shuy, Wolfram and Riley (1967) Fasold (1968) Cook (1969) Fasold (1972) Wolfram (1969)	Vowels;/ɪŋ/ Vowels (rhotac- ized) Postvocalic /r/ /ɪŋ /;/ð/; Final consonant clusters
Mexican New York	Berryman (1980) Hartford (1978) Labov (1966) Labov (1972)	/ɪŋ / Dental fricat- ives; vowels See above
Southern	Levine and Crockett (1966) Anshen (1969)	Postvocalic /r/ Postvocalic /r/ /ɪŋ /;/ð/
AUSTRALIAN ENGLISH	Shopen (1978)	/ɪŋ /
BRITISH ENGLISH:		
Belfast English	Milroy (1976) Milroy and Milroy (1978) Milroy (1980)	Vowels ditto Vowels and /ð/
Modified Northern English	Bladon, Henton and Pickering (1984) Henton and Bladon (1985) Henton (1985)	Vowels (monophthongs) ditto ditto
Norwich English	Trudgill (1974; 1974)	/ɪŋ /; /j /; vowels

Table 3.II. continued

LANGUAGE	INVESTIGATOR(S)	PHONOLOGICAL VARIANT
BRITISH ENGLISH:		
Received Pronunciation	Henton (1983)	Vowels
	Bladon, Henton and Pickering (1984)	(monophthongs)
	Henton (1984;1985)	ditto
	Henton and Bladon (1985)	ditto
Scottish	Romaine and Reid (1976)	/t/ versus /2/
	Macaulay (1978)	/r/; vowels
	Romaine (1978)	Postvocalic /r/
Southern	Hudson and Holloway (1977)	Vowels;
	Cheshire (1978)	/h/-dropping
South African	Lanham and Macdonald (1977)	Vowels
FRENCH: Parisian	Mettas (1979)	Vowels (oral)
	Lonchamp (1986)	Vowels
Canadian	Sankoff and Cedergren (1971)	/l/
PERSIAN	Jahangiri (1980)	Vowels
SPANISH: Argentinian	Lavandera (1975)	
	Castilian Williams (1983)	
	Panamanian Cedergren (1970)	/r/;/s/;/tʃ /

\$ Fischer's study pre-dates 1963, but used, nevertheless, quite similar criteria as those later formalized by Labov. It is thus included in this table.

From a glance, it is obvious from Table 3.II. that the great majority of Labov-influenced studies have been carried out in English-speaking communities. Now, this may in part be a reflection of my bibliographical resources, but a concentration of material from the researcher's own (fairly widely-defined) culture is usual in any survey. The prime reason for the large population of studies of English-speaking communities appears nevertheless to be the unquestioned assumption that the Labovian model could easily be employed to illuminate variation in communities other than those of New York or the East Coast of America, but sharing a common language. This assumption has been shown to be not only erroneous, but to have led to unjustifiable conclusions about other linguistic societies. This latter point is developed further in the next section.

3.9. A critique of some over-generalizations of the Labovian model

From Table 3.II. we can see that Labov's model has been used as the cast for explorations in many other language societies/communities. What is also apparent is that in many cases, researchers have chosen to examine exactly the same phonological variables as Labov, too! It is naive to expect separate linguistic communities to exploit the same variants as indicators of difference, whether based on SES or sex, or any other variable. Investigators nevertheless doggedly pursued the familiar variants of postvocalic /r/; word-final /ŋ/ and a variety of vowels. This is the first

instance, then, of Labov's methods being used in an over-generalized and uncritical manner. Furthermore, difficulties often arose precisely because of the different phonological structures of the languages investigated.

While other researchers often found replication of Labov's findings, occasionally (predictably) they did not (Fasold, 1972). One suspects that negative findings have actually been more frequent than the literature indicates; negative results, however, tend traditionally to go un-reported. There is also the question of whether 'replication' can be said to exist in any two communities, or even one community at two different points in time. Phonological systems are intrinsically different from community to community, and from language to language. So to hypothesize that the variable postvocalic /r/ is used as a consistent indicator of perceived prestige in two different communities would be over-simplifying the phonological case, and over-stating the sociolinguistic one, too.

Secondly, there may be many more sex-indicative segments in the dialects/languages already explored, which simply eluded the researchers' notice. Just how many more phonologically-different variables might occur as indicators of sex-related speech will be shown in Table 8.I.

Clearly, the substance of the phonological drag-net has to be inspected from two angles: firstly, is it the right net to be using in that particular linguistic water, and,

secondly, is it trawling for the correct variants (i.e. the ones which might show preferential usage according to whatever dependent variable was under investigation)? Posing these questions in retrospect reveals that, quite often, the answers supplied in the studies in Table 3.II are negative to one or both of the queries. The main problem lies in the adoption of a **random sampling** technique, where in fact **judgement** sampling would probably have provided more tractable results. Lesley Milroy (forthcoming) has elucidated the difference between these two techniques well. A brief outline of the difference between them is given in our discussion of preferred data-gathering techniques, in Chapter 4.

Taking the foregoing account into consideration, it is possible to understand why Smith's comments (1985: 80) are so negative. His reasoning leading to these comments, though, is not entirely sound. Smith states that, considering the extremely wide range of possibilities available for phonological and grammatical sex-based variation, differences which have been attested are "subtle and few". He then goes further to weaken the claims for sex-specific phonologies by reporting (p. 81):

The authors of recent urban linguistic surveys have been careful to point out that the speech variables they are interested in are usually better predicted by SES, ethnicity and age than by sex (e.g. Labov, 1970). Indeed, differences have sometimes not been found where expected. Labov (1966), for example, did not discover much difference between men and women in their pronunciation of postvocalic /r/, or of word-final /ɪŋ/ in his New York study.

(I have, incidentally, amended Smith's disturbingly random representation of graphemes as phonemes, and inserted the appropriate phonemic symbols).

There are several points of fact or deduction by Smith which are illogical and should not be allowed to stand unchallenged in his statements. To begin with, it is inaccurate to claim that **few** differences have been found. Tables 2.II and 3.II refute that. If, with a charitable interpretation, Smith means that the differences found have been uniform in their phonemic classes, then he should (a) make his intention unambiguous and (b) consider whether that lack of variety is actually evidence or effect. As will become transparent in Chapter 8, it is probably the case that many more sex-based phonetic variants exist than have been documented. Their absence is not due, however, to their 'lack of opportunity' for occurrence (although, phonetically, this may be a reason) but merely because investigators have not thought to look for them (c.f. remarks concerning types of difference in Chapter 2, Section 2.2)! While it is initially attractive and convenient to attempt inter-study comparability by examining the 'same' phonemic variables, this endeavour has given rise to ill-considered hypotheses and blinkered conclusions.

A second, broader issue arises from Smith's comments in his section called 'Methodological issues'. In the course of the section, it becomes increasingly clear that Smith expects that somewhere, at some time, a simple one-to-one

relationship will fall out of any socio-phonological study which correlates with one variable alone. The only reason that studies have so far failed to produce such a neat result is because they have not been sufficiently scrupulous in their exclusion of possible biasing factors. The naive optimism of this assumption has been dealt with previously in this thesis. In the end, though, Smith seems able to not only describe these pills, but can then swallow them, without realizing their bitterness, and still conclude that (ibid.):

These considerations notwithstanding, to the extent that the differences have been found are recurrent and consistent, even if only on a probabilistic basis, the generalization about sex-based phonological ... differences is warranted.

Perhaps the effort involved in unravelling the syntax is not so rewarding after all, for what Smith then concerns himself with is the relative influence of the sex of the interviewer on the results to date.

There is yet a third illogicality to appear in Smith's paragraph. He indicates that speech variables "are usually better predicted by SES, ethnicity and age than by sex." This may be true, but Smith fails to realise that these variables in turn have their failures, and that they too do not always find differences where expected (see, for example, Helfrich, 1979, for an overview of studies into age as a social marker in speech).

There are more, wide-reaching problems involved with the use

of Labovian models on a large scale, and these Smith does not pursue. Here, though, we will consider the wider consequences in a few other studies.

Nichols (1978) provides another strong criticism which relates to the data in Table 3.II for age and population distribution. There are several socio-phonological studies which take no care to match age and sex, nor to balance proportions. Labov (1966) included far more women than men in his New York City study (117 women as compared to 81 men; although some of these were later rejected, and in the 1972 version there appears to be no mention of how the total number used breaks down into females and males); his lowest class contained almost twice as many women as men, while the reverse obtained in the highest class. It is not surprising, then, that Labov is led to conclude that LMC women are among the most influential in the process of language change.

In the same study, Labov also unfortunately provides contradictory evidence for the age at which the patterns of variation are established. In the later, expanded work, though, (Sociolinguistic Patterns, 1972) he is much more dogmatic about the factor. The control of subjects' ages is necessary for both SES-based (see Section 3.7.) and physiologically-based deductions, as we indicate in connection to our own research documented in due course.

Trudgill (1972) carefully used equal numbers of women and

men, but provides no figures on their distribution by social class because, one presumes, of the foregoing class-classificatory dilemma about women. The language of women must be regarded in the context of their social roles, but meanwhile linguists are frequently hamstrung by sociologists' failures. In an attempt to avoid this dilemma, the data corpus gathered for this current study was scrupulous in collecting equal numbers of men and women from a narrow age range; it also arrived at a working SES analysis of the female informants both by pre-selectional criteria and by their answers to biographic questionnaires.

3.9.2. Race-specific as well as sex-specific variants?

Much of the preceding discussion was concerned with the exclusion and excludability of women from sociolinguistic studies. It is also noteworthy too, that the majority of phonological studies seeking sex-differences have focussed on the white middle-classes.

Wolfram (1969) is one of the handful of investigators who took ethnic membership into account. He found that for the black speakers of Detroit, females within each social class used forms closer to standard norms than did males. Females of all classes had fewer /f/, /t/ or $\emptyset$ realizations of /θ/ or /ð/ and they pronounced final consonant clusters more often, along with the postvocalic /r/. On the other hand, another investigation into Black American English (Fasold's, 1972 evidence from Washington D.C.) revealed either no difference

between the sexes in the use of standard forms, or even slightly more use by the males in the working-class black population. This is clearly a sharp indication that the keeping of some 'external' variables constant (i.e. class and race) will not generate uniformity even in different urban environments in the same country! Taking Anshen's (1969) and Nichols' (1978) studies of Black American English into account, together with the two studies of Mexican American (Hartford, 1978, and Tixier y Vigil and Elsassser, 1978), these studies remain nevertheless in a minority compared with the investigations undertaken into white speakers' speech. It seems, then, that the popular description of the 'ideal speaker-hearer', in Western linguistics at least, as white, middle-class and male (and, according to some, is actually Archie Bunker) is not so inaccurate after all.

3.10. Breaking out of the 'Observer's paradox': a newer approach to socio-phonological data garnering

Recognizing the pitfalls of an unmodified adoption of the Labovian model, a more positive move has been effected by some linguists who, perceiving the need for 'action anthropology', have attempted to become integrated members of the community under observation. As has been indicated in Section 3.5., class groups arrived at using combined indices do not have any objective, or even inter-subjective reality. 'Class' is a vague and, for women to a large degree, invalidated term. The separation of 'class' from 'status'

also needs to be achieved, and we will return to this topic in Chapter 4.

The 'community' provides more attraction for the observation of phonological variety. Foreseeably, the definition of a 'community' is another viper's nest. The term, however, can usually be safely construed as describing (Milroy, 1980):

...cohesive groups to which people have a clear consciousness of belonging...Unlike more abstract social classes, these groups have a strong territorial basis and so one can equate Labov's term 'categories of local identity' with community.

As soon as differences in speech habits within a community are not regarded as 'free variation', progress can be made. Blom and Gumperz's (1972) study of code-switching in Norway emphasized the importance of the effect of community membership upon the individual's speech habits. Their observations in Hemnes illustrate precisely the specific situational conditions under which speakers shifted from one code to the other in the bidialectal community. They demonstrate that not only do rules vary from one community to another, but that residents of the same town are segmented into groups or networks who do **not** in fact share the same linguistic norms (c.f. earlier comments in Section 3.3.1.). The social meaning carried by the dialect is different for each group. In so doing, Blom and Gumperz of course demolish Labov's definition of a speech community, and it falls because Labov does not examine situational variables as closely as possible, nor does he show in detail the manner in which language choice is linked to a local

values system.

How, then, does the 'action anthropologist' avoid the constraints of labelling 'classes' as such and yet still arrive at a working stratification of a society? Nichols (1978), for example, obtained an unpaid job in an elementary school, while Lesley Milroy (1980) penetrated Belfast communities by working on the 'friend of a friend' principle. Milroy's subsequent data gains credence because of her awareness of the position of the linguist as the ultimate 'lame'. Access to the vernacular without intrusion on normal interaction is the researcher's goal, but what is encountered is paralyzing irony, as formulated by Labov (1972: 209)

We are left then with the 'Observer's Paradox', the aim of linguistic research in the community must be to find out how people talk when they are not being systematically observed - yet we can only obtain these data by systematic observation.

Aspects which are bound to affect the performance of the individual speaker have been variously proposed. Milroy's (1980) contention however is that it is position in a **social network** which can be most influential. A social network is defined as referring (1980: 174)

... quite simply to the informal social relationships contracted by an individual.

She claims that this is capable of universal application and is less ethnocentric than, for example, notions of 'class' or 'caste'. Individuals in a "high density personal network structure" are more affected by that network as a norm-

reinforcement mechanism.

This approach is redolent of Labov's (1972; Chapters 5 and 7) findings in relation to "Lames" in the Harlem "communities". One community, the Jets, is broken down into core, secondary and peripheral members: social links which are established by "reciprocal namings". The reasons why lames lie outside this hierarchy vary from parental government, to ill-health, to non-participation in 'hanging'out' or 'shooting-up'. The relevance now becomes clear: linguists must ensure that they are in possession of "members" (i.e. fully-integrated participants in the local community) and secondly, that this is why women are discarded in most studies. Women are not 'members' because the core consists of men: therefore the speech of women can still be judged 'abnormal' because it is not belonging to the core, 'normal'.

Milroy's description of female phonological variables is not jeopardized by the above deduction, however. In the three areas of Belfast (Ballymacarret, Clonard and Hammar) which she examines, there are varying degrees of high unemployment. When disturbed by, for example, geographical mobility or high levels of male unemployment, the relationship between language and network is less close. Thus the variables which appear to be sex-specific can be governed by the unpredictability of the social tide. This is neatly encapsulated in the following (1980: 164):

... even though men use vernacular pronunciations more,

women are capable sometimes of using variables particularly associated with men to symbolize their integration into local networks.

To consider the 'sex' marker as exhibited in phonological variables in isolation is, then, clearly misdirected. In fact, dense, multiplex network ties do seem to be particularly associated with low-status groups, men and adolescents and these speakers approximate more closely to urban vernaculars than high status groups, women and middle-aged speakers. Milroy's findings do not overturn the applecart, but the way in which they were developed is highly innovative and productive for the assembly of data collected since her indications. The individual, and a description of her/his language, may be in danger of being strangled by ever-multiplying concentric circles of analysis, but this is greatly preferred to biased generalization based on stereotype.

3.11. Other British studies: where the Labovian model fits where it touches

It is the concentration on the individual rather than the group which cordons British from American sociolinguistic studies, according to Suzanne Romaine (1980). She observes that Trudgill (1972) and Macaulay (1978) fail to produce adequate 'group' analyses for Norwich and Glasgow respectively because their selection of speakers, environments and numbers were statistically unconvincing. It is revealed that Trudgill's choice of 'wards' in Norwich, and that of ten children to join the original fifty adults

was hardly 'random'. The spatial distribution of the social groups in urban areas can seriously affect results and Trudgill, Macaulay and Labov are all guilty of ignoring this factor to a greater or lesser degree.

As we have said, some of the British investigators' failure can be attributed to attempting to import the Labovian model wholesale. For example, in Macaulay's analysis of Glaswegian, it led to an assumption about the standard norm and a subsequent over-emphasis of the importance of RP and the indication of a unilinear model of sociolinguistic structure. It may well be agreed in New York City that one style is the prestigious one, but in Glasgow and Belfast there exist several contenders for that norm (c.f. Section 3.3.). Thus Milroy (1980) can argue for the creation of what may be thought of as 'intermediate' (although no continuum as such is implied) norms in Belfast: in moving away from the vernacular, speakers may not necessarily be moving towards 'prestige' RP. In the description of our own data, there are also indications of a possible 'intermediate norm' among the speakers of the 'modified Northern' accent: see Chapter 6, Section 6.10.

Romaine signals the necessity of correlating linguistic variables with a number of social indicators, each in turn. If one overlays this proposal with the sexual dichotomy, then the ensuing matrices may be disturbingly complex, but the rewards would be so much richer. One also has to do a great deal of preliminary work in situ, and possibly review

the facts of historical phonology, before one can come to any decision about which items have alternants synchronically, otherwise the situation may arise (as it did in Macaulay's 1978 data) that items are incorporated for assessment which do not have alternants. Unfortunately the total membership of a phonological set can rarely be predicted, nor do speakers themselves have reliable intuitions, so the linguist must approach the corpus with eyes open and as much circumspection as possible.

The strongest point to be made is that frequently linguists are attempting to deal simultaneously with different levels of abstraction in both linguistic and social analysis, coping with syntax, intonation and phones as well as the individual networks, communities, etc. No miraculous solution is proposed here, except that great benefit is to be had from regarding the data from several vantage points.

The differentiation of speech styles within society as a whole and the individual as a member of that society is central to sociolinguists. Examinations of sex as a style-determiner have been shown to be too isolationist. Those with phonological variables as their linchpin can only be evaluated in the larger perspective of the state of current sociolinguistic methodology. It does not seem overreaching to surmise that a large number of the sex-and-phonological variable studies conducted to date have been either founded on false premises or are methodologically far from perfect.

3.12. Conclusion: is the linguist the eternal "Lame"?

After this review of the various avenues of escape from the "Observer's paradox", the discovery of a few false turns and cul-de-sacs, there still remains with us the problem of how to make the 'lame' linguist walk. The linguist is cursed with worse than the riddle of the sphinx, for it appears that she/he is denied even the possibility of four, two or three legs and is condemned to hop from foot to foot in a perpetual circle. The answer to the question above, though, is an optimistic 'not quite'. There is a proposal to help cure the linguist's lameness: it consists of Milroy's network solution. An increased refinement of phonological and phonetic methodologies, and their extension to become truly socio-methodological is also certain progress, but does not actually avoid the paradox itself.

What then are the implications of these revelations for linguistics and phonology and sex-typed characteristics in particular? If a functional view of language change (as opposed to the substratum theory or borrowing) is adopted, then the real catalysts are social factors: factors which exploit a weak point, or potential imbalance within the system, which in turn may create further weak points. Natural phonology has fed this approach to change, with Hooper (1976: 85) stating that:

...innovation is the failure to suppress an innate, universally natural process (and that) all 'new' rules come from a finite set of phonetically motivated processes.

Certainly any theory which attempts to account for change (and innovation must be regarded as change, albeit temporary) and the possible significance of sex should incorporate phonetic likelihood and motivation. Nevertheless it is not sufficient. Only a hypothesis which embraces social position and is critical of linguistic theories would appear to have a chance. The immediate trigger for differences, including sex-based ones, must be looked for **alongside** the propensities of the language concerned, and of language in general. Hence, my explicit formulation and adoption of a socio-phonetic methodology.

Speaker-sex as an explanatory variable must be regarded for the time being as a plausible heuristic. Even using this heuristic, this chapter has in general depicted an overall lack of credibility of findings. This lack can on the one hand be attributable to inconstancy in variables but on the other to serious methodological oversights. It is essential that these two problem areas are teased apart. Our hope is that such a separation has been achieved in the socio-phonetic research into British English described in ensuing chapters.

Where **actual** differences occur (as contrasted to speech stereotypes which become associated with, and expected of, males and females), is a matter for empirical research. With such a process of constant methodological and experimental refinement, it is hoped that the research conducted for this thesis will not be so prone to weaknesses.

Chapter 4

Pre-requisites for a socio-phonetic analysis of speaker normalization

The last two chapters have provided an extensive critique of previous studies of **phonological** sex-determined variation. We have indicated that both the 'linguistic' (i.e. pre-Labovian) and the Labovian approaches have serious lacunae in their methodologies and hence provoke results of sometimes dubious validity. Iconoclasm per se, is not, however, very constructive. The challenge 'what have you got to put in their place?' may be justifiably levelled.

Bearing in mind this need for re-construction, in this chapter I move towards the definition of an improved socio-phonological and **socio-phonetic** methodology by providing explicit guide-lines for a sufficient data-base. These may, of course, be beneficially adopted (where appropriate) for general linguistic research across the board. Then, holding firm the propositions of this manifesto, there follows a critique of the data-gathering techniques frequently employed by phoneticians, working across languages. The androcentrism of the tools of analysis is also pointed out, in Section 4.11. A delineation of a stricter paradigm in phonetic data-collection is then presented, as exemplified by the selection of informants and recording methods used in the research conducted towards this thesis.

4.1. A manifesto for the construction of an adequate cross-language data-base

For too long, sociolinguists and phoneticians have been assembling pockets of data for different dialects/languages in isolation. For the purposes of cross-language

comparability and replication of experimental procedures, it is obvious that methodological standardization is desirable. Before detailing my own suggestions, other contributions to the field of database-standardization should be mentioned.

Previous proposals for assembling databases often focus on the demands of automatic speech recognition. They include a contribution by Pallett and Baker (1983), which gives suggestions for the assessment of speech recognition performance. Their paper does not however actually make recommendations for standardization as such, but rather shows how to optimize the limitations of existing hardware. A further, less than satisfactory, database for ASR purposes has been described by Leonard and Doddington (1983). While the database aims to be dialectally 'balanced' for American English, it includes on average only 5 males and 5 females for each dialect, but 34 boys and 36 girls from Dallas, Texas. The corpus of the database is also very limited, comprising solely digit sequences.

Recently, Carré et al. (1984) have produced a format for "defining, planning and recording a large database". This is specifically tailored to the French language, and, unfortunately, "neither the recordings in the base, nor the analysis results are available to foreign laboratories." The database itself is liable to biasing, though, because the ten speakers of 'Standard' French were chosen from speech laboratory staffs. Firstly, they are too few in number (5 speakers of each sex is simply not a sufficient

sample number - see later comments in this chapter); secondly, they are not naive speakers (see Section 4.5, following). However, the French GRECO group do intend to expand the database to incorporate a further ten speakers, but these will have a marked regional accent so the insufficiency problem remains. Carré et al.'s attention to 'phonetic balancing' in their recorded material is commendable, and they have included an impressive range of stylistically-diverse corpora.

For Australian English, O'Kane et al. (1984) have published a set of guidelines for the design and collection of a spoken database. There is a wide variety of styles, spanning word lists to conversation. Great detail is given concerning the exact phonetic contents of the more formal corpora. Unfortunately, though, one minute seems to be the maximum duration for which any subject is expected to speak at any one time. For measuring long-term average speaking fundamental frequency, for example, this duration may prove too short. It is nevertheless good progress to have obtained such material of the type that Labov (1972) calls "speech outside the formal interview", in 'clean', controlled recording conditions. Another positive feature is the inclusion of precise bio-data and of women speakers who, in Australia (as in many other countries/languages - see critique later in this chapter), have been excluded from most phonetic studies. On the whole, O'Kane et al.'s proposals are very sound, but there is a fundamental laxity

in recording speakers of "either General Australian or Cultivated Australian" (1984: 15). The pooling of dialectally diverse data cannot be done with impunity (see Section 4.10).

Benefitting from these previous attempts and with the intention of improving upon them, there is outlined below my conception for a blueprint of what an adequate database should contain. It has been drawn up with the needs of phoneticians and phonologists generally in mind, and will clearly serve the requirements of a socio-phonetic investigation into sex-differentiated speech patterns most satisfactorily. The specification of this blueprint has also emerged from the assembly of the data for this thesis and from later consultation with the International Working Group on Vowel Normalization. Further information concerning the group's aims may be obtained from a letter published in Speech Communication, 3 (1984: 169). I am grateful for their comments and contributions.

This set of guidelines has furthermore been influenced by the needs of collaborative national (e.g. the Alvey Advanced I.T. programme in speech research in Britain) and international (e.g. the ESPRIT programme in E.E.C. countries) research enterprises in speech recognition. Thus, while all the properties listed in the following pages are desirable, some of the twenty points have been included with the international demands of both pure (socio-) phonetic research and of applied speech technology in mind.

It will be seen, therefore, that they were not all necessarily utilized for the research described in Section 4.12. and Chapters 6 and 7.

Properties of an ideal socio-phonetic database

Speakers

- 4.1.1. Data should be collected from as large as possible sample of informants. A minimum number would be 10 adult females; 10 adult males; 10 girls and 10 boys (i.e. children who have not reached puberty). So, the sample should be numerically equal for any two groups. Naturally, the selection of these groups would depend on the variable to be investigated. The research described here, for example, did not include children and therefore none were incorporated into the database.
- 4.1.2. The age-range of any subset of informants should be controlled. Suggested ranges for adult speakers are 14-18; 18-25; 25-40; 40-60; 60-80. These parameters are based on findings by Hollien (1972; 1982 inter alia): for further discussion of their foundations, see Henton (1983).
- 4.1.3. Speakers should be 'naive': i.e. they should
- not know the purpose of the investigation
 - not be phonetically trained in the specific aspect of speech under investigation

Properties of an ideal socio-phonetic database continued

- not be professional or trained singers.

- 4.1.4. Speakers should not have suffered any hearing loss, or have a speech impediment. They should be in good health, and especially not be suffering from any infection of the chest or vocal tract. They should be asked if they are smokers, and rejected if they are heavy smokers - unless that is a variable of interest (see reason in 4.3.).
- 4.1.5. Speakers should fall into a normal distribution with regard to height and weight. Exceptionally short or tall, under- or over-weight people, together with women in the later stages of pregnancy, should therefore be excluded - unless their condition is a specific variable being studied.
- 4.1.6. Speakers should be assessed by trained phoneticians for homogeneity of accent/dialect. Where there is evidence of several sociolects within an accepted accent/dialect, care should be taken to ensure that the speakers all speak the same sociolect under the conditions of the experiment(s).
- 4.1.7. Speakers should come from the same socio-economic class. How this is interpreted is left, for the moment, to the investigators. Attention should nevertheless be paid to the warnings regarding the

Properties of an ideal socio-phonetic database continued

'unclassifiability' of women, in Chapter 3, here.

- 4.1.8. Biographic data should be elicited and recorded (by means of a questionnaire or pre-recording conversation with the investigator(s)) to be kept for reference and as a possible means of verification of accent/dialect.

Linguistic content/context

- 4.1.9. Each speaker should be recorded using several speech styles, e.g. listing (reading from material designed to investigate individual speech sounds and/or suprasegmental patterns); read continuous speech; controlled (but unscripted) continuous speech; casual speech (= 'free conversation'). The 'setting' of the 'free conversation' should be constrained as much as possible, without reducing the 'freedom' of the conversants to that of 'lab. speech'. At least one recording of each speaker should be made with a duration of at least 2 minutes: this length of time is needed to establish any long-term effects in the voice. Ideally, there should be controls too on the interlocutor: the same as outlined in point 4.1.18.
- 4.1.10. For the listed material, which contains isolated speech segments and 'carrier' words, a list should

Properties of an ideal socio-phonetic database continued

be used ideally that is common across accents/dialects. It may, however, prove necessary to hedge these desiderata when researching say, tone languages.

- 4.1.11. Similar word lists should be used as far as possible across accents/languages (i.e. attempt maintenance of the same consonant contexts when examining vowels, and vice-versa).
- 4.1.12. The scripted, elicitation material should be checked for containing all segments of interest in all possible phonotactic combinations. The suprasegmental repertoire (including different intonation patterns, pitch and loudness variation, and tempo) should also be explored. Range of **linguistic context** should be checked, i.e. to ensure that tokens are included in stressed/unstressed, initial/medial/final positions etc.
- 4.1.13. Ideally, speech should be recorded in normal ('modal'); loud (shouted) and soft (whispered) conditions. The full range is optional, of course, if different degrees of voice loudness are not of interest (see Section 4.8.).

Recording methods and conditions

- 4.1.14. Recordings should be made, as far as possible, in studio (sound-treated) conditions at sea-level.

Properties of an ideal socio-phonetic database continued

- 4.1.15. Subjects should be positioned a constant distance from the microphone. The wearing of a head-mounted microphone, such as the Shure M10A, offset by a uniform amount (say 2 cm.) to the side of the subjects' mouths is sufficient. Subjects' heads should be held in a comfortable, vertical position.
- 4.1.16. Material should be recorded digitally, the now widely-used standard being the Sony F1 PCM audio converter and videotape. This will ensure efficient storage, lack of degradation of signal and compatibility.
- 4.1.17. Recorded material should be digitized with 14 bits and 20 kHz. The alternative choice of 16 bits for digitization seems less acceptable, because it is valuable to retain 2 bits for error-checking.
- 4.1.18. The investigator conducting the recording sessions should remain the same throughout the data-gathering.
- 4.1.19. Close records and a detailed description of the data material should be kept. Anomalies in recordings (e.g. a subject omitting an item and having to repeat it; number of practice attempts; subjects' voice quality; background noise occurring on the tape, etc.) should be noted.
- 4.1.20. Recorded material should be kept in ideal

Properties of an ideal socio-phonetic database continued

conditions, with documentation about time, date, and place of recording kept with the tapes, together with any other vital information for the correct interpretation of the data (so that the material and the information do not become separated).

As has been indicated, some of the database specifications listed above require a word of explanation, although many are either self-explanatory or may be appreciated as simple common sense. The specifications arise often from the deficiencies of previous studies, and in the next sections, there is ample illustration of just how remote many acoustic phonetic studies have been from fulfilling these criteria.

4.2. Speaker samples in phonetic studies

The most frequent fault in experimental acoustic phonetic research is the failure to collect anything resembling a representative sample of speakers. The great majority of phonetic investigations into acoustic behaviour have used fewer than five (male) speakers. This often drops to only one or two speakers (the investigator her/himself and one other). Two examples of such studies, which are really only one experimental step away from self-introspection, are included in the forthcoming table as illustrations.

Charitably, one might argue that their appearance derives either because (a) little other acoustic data has been reported for Canadian French, or (b) perhaps native speakers of Shona are relatively rare. Nevertheless, if the only subject used is the researcher her/himself, then the work can hardly be termed 'objective' or 'representative': indeed the results can easily be prejudiced because the researcher's behaviour is likely to be conditioned by their knowing what it is they are expecting to find. As a result, this kind of data corpus runs the risk of being of the 'self-fulfilling prophecy' type.

Space precludes discussion of these 'minimalist' studies, but we can comment that this state of affairs is plainly unsatisfactory. Finding a larger number of subjects may present more time and effort, but it will surely provide a more reliable result upon which to base conclusions. This introduces an important consideration, namely, how do we assemble a **sample** of speakers? This issue is quite complex, and deserves further comment: this appears in Section 4.3.

Precise details of studies which have used larger numbers (and which **state** them!) representing a large selection of languages, is given later in this chapter, in Table 4.I. Most investigations which feature in the table have been concerned with the acoustic properties of vowels, as indeed is this current study in general. The overall picture in Table 4.I may be momentarily reassuring from a sheer

numerical viewpoint, but this is false security. For, if the requirements of sample numbers are satisfied, then, as we will discover (in Sections 4.6 and following), there is a plethora of other reasons why these studies should not be regarded as entirely trustworthy.

Table 4.I does not claim to be exhaustive, but three main shortcomings are apparent:

- 4.2.1. Fewer than half the studies which are included in the list (and were conducted prior to our own) regard females as 'suitable' or 'representative' subjects: they are, in fact ignored in 45% of these studies.
- 4.2.2. Very few studies have more than 10 speakers of each sex.
- 4.2.3. When 'mixed sex' samples are assembled, there are frequently fewer female speakers than males. Sample numbers for the two sexes are therefore unequal.

These discoveries are most discouraging, and their existence was a primary motivation for the need to construct a rigorous re-definition of the requirements of a socio-phonetic database. The other most frequent deficits found in judging whether these studies meet the criteria of the 'database manifesto' above include:

- failure to control for (or, if it was controlled, then failure to report) age range(s)
- failure to reject subjects in less than good health

- failure to monitor subjects' stature
- failure to control for accent/dialectal homogeneity of their subjects
- failure to control for the SES background of the subjects.

In providing a résumé of the failures, I have listed them in the same order as they appear as desirable in the manifesto: an order which does not, incidentally, imply a hierarchy of importance. For phoneticians, for example, it is essential that dialectal homogeneity is secured, but this does not occur in primary position in this listing.

The effects of these deficiencies on the reported phonetic results deserve separate comment. After confronting the sampling issue, we will make use again of the same ordering of points as appears in the manifesto.

4.3. Sampling a population

There are two basic approaches to assembling an adequate sample population, and as we mentioned in Chapter 3, Section 3.9, they are well exemplified by Milroy (forthcoming). The first, which was used by Labov and many of his followers, is random-sampling. The rationale for such an approach is admirable: it assumes that any individual appearing in a **sample frame** has a chance of featuring as a representative. These lists may be derived from, for example, electoral rolls or from library membership lists. Now, while the principle appears fairly sound, it is also easy to see how it might be eroded in practice. Milroy shows how a so-

called 'random' sample frame may be biased from the outset. Using the two sources of lists above, one can immediately object that only people over eighteen appear on electoral rolls and that (one assumes) illiterates will not necessarily be library-members, nor, indeed, will all members of a local population who are entitled to be because of difficulty of transport or lack of literary interest. These sorts of objection are virtually unavoidable for any publicly-available list of people. Secondly, certain randomly-sampled subjects may be unsuitable. They may, for example, be ill, non-native speakers, not naive about the purpose of the research, or unwilling to participate: they may even be dead! How does the researcher then set about filling these gaps? By **selecting** replacements, bias may be introduced (c.f. earlier comments, in Chapter 3, Section 3.11), although proper random sampling has a careful replacement procedure.

Milroy examines further problems, including the recommendations for a sufficient sample size, and the stratification of a sample. In discussing the former, she cites Sankoff (1980). Sankoff underscores the importance of ensuring that the researcher has characterized their interest group satisfactorily **before** sampling; the researcher should have furthermore stratified the sample correctly (e.g. in terms of age, sex, ethnicity, class, etc.); lastly, a sample size should be fixed. Labov, Yaeger and Steiner (1972) calculated that 4-5 informants to a cell

are, in general, sufficient to yield reliable sociolinguistic results. The study undertaken for this thesis had 20 speakers per cell, and it is hoped that it will be shown to meet Sankoff's other requirements (see Section 4.12).

Milroy also highlights the difficulties encountered in defining the **community**, and the **native-speaker** within that community. These topics have already attracted some notice in Chapter 3, and will receive further comment in the course of this chapter.

An alternative to random sampling is **judgement sampling**. The foundation for this technique is that a decision is taken a priori by the investigator about the types of speakers s/he wants to investigate. S/he then finds a number of speakers who fit the requirements. Milroy says:

A good judgement sample needs to be based on some kind of defensible theoretical framework; in other words, the researcher needs to be able to demonstrate that his (sic) judgement is rational and well-motivated.

The validation of such a technique is provided by Romaine (1975) and Romaine and Reid (1978). The difference between the two sampling techniques may be characterized as analogous to that between trawling and fly-fishing. Trawling uses a large net, and catches an unpredictable and very varied range of fish, some of which may be undesirable and have to be thrown back. Fly-fishermen, on the other hand, know what fish they want to catch from the start, and use an appropriate bait. Both methods can eventually produce a

good catch.

Given the theoretical framework which was outlined in Chapter 1, it was seen as appropriate in my case to use the technique of judgement sampling for this research. Precise details about the sample are given in Section 4.12.

4.4. The importance of subjects' age

There are two reasons why researchers should control the age-range of their informants. Firstly, as was pointed out in Chapter 3, subjects from different generations are likely to maintain different idiolectal 'styles'. Thus, broadly, older speakers will probably have a more 'conservative' pronunciation than the middle-aged, who, in turn, will be less innovative and changeable in their speech habits than the young. So, even if we were to operate using only these grossest age-divisions, care should be taken to ensure that the generational borders are not crossed within one sample-group. A second, and more vital reason for controlling for age, though, arises from physiological events.

Hollien and Shipp (1972) and Ramig and Ringel (1983) have demonstrated that speakers' fundamental frequency (or 'pitch') varies according to the age of the speaker. It is of course easily-observable that childrens' voices are higher in pitch than adults'. Changes in vocal pitch are governed largely by the growth (in length and density) of the vocal folds, but there are other age-and-pitch related changes apart from the one at adolescence which is commonly

noticed and referred to as boys' 'voices breaking'.

Hollien and Shipp show that mean speaking fundamental frequency (SFF) **decreases** from pre-adolescence until sometime in the 40-50 year age-range (dropping, for their American male subjects, from 120 Hz to 107 Hz). Then SFF steadily **rises** until old age (reaching 146 Hz at 80-89 years). The steady **lowering** until middle age (1972: 158)

...reflects an increase in the vocal-fold tissues associated with rapid laryngeal growth during adolescence, and thereafter, perhaps, the long-term effects of sub-clinical trauma associated with the years of vocal-fold collision during normal use.

In another study, de Pinto and Hollien (1982) indicate that women speakers in Australia appear to manipulate their speaking fundamental frequency for reasons of perceived social desirability. This is particularly so for younger women: a phenomenon which had already received comment from Linke (1982) in the course of his investigating American womens' pitch. Hollien also points out that the effect of smoking is to lower pitch, an indication of which had already been provided by Stoicheff (1981).

Now, the essential distillation from these studies for our argument is that the **acoustic attributes of speakers' voices change significantly with age** (and may also be changed consciously by women for purposes of social approval). If fundamental frequency is lowered, then this in turn will tend to 'pull down' the spectral envelope, and the first two formants in particular. Hence, the acoustic analysis of vowels produced by the same subject at age 20 will be diffe-

rent (all things being equal) from the analysis at age 50.

Investigators should, then, be scrupulous in defining a 'safe' and consistent age-group for each sex from which to take their sample. It would clearly skew results if data were collated from speakers ranging between 20 and 50, or from males of 45-60 and females 20-30 years old. A suggested 'safe' subset would be one spanning fifteen years in adulthood, before the onset of Hollien's middle-aged 'rise' for males.

4.5. Only 'naive', and 'normal' speakers need apply

4.5.1.

To safeguard the authenticity of the results of any experiment by ensuring that the subjects do not know the exact purpose of the investigation is clearly common sense. Further security of intent should be exercised however, if experimenters are using as subjects colleagues who work in the same field (c.f. the GRECO problem cited in Section 4.1.1). It would not be difficult for co-workers to find out the nature of the research to which they are asked to contribute and (albeit unconsciously) over- or under-react and thus skew the results. Even if the phonetically-aware subjects did not know the details of the experiment, they might easily deduce them from the structure of the experimental material: this is especially possible in perceptual tasks. Stringent effort should thus be made to guarantee that the purpose of the experiment is opaque.

For very similar reasons, subjects who use their voice professionally, or are teachers of various vocal skills (e.g. elocution or ventriloquism) should be excluded. From this perspective, the data assembled by Magdics (1969) for Hungarian and by Mahnken and Braun (1951) for Russian should be regarded askance. Magdics' subjects were Hungarian radio announcers, and Mahnken and Braun's three speakers "haben ausgedehnte Erfahrung im Sprachunterricht" (p.273). These types of speaker are certainly not likely to be representatives of a naive, 'natural' form of the language/accent.

4.5.2.

The exclusion of professional or trained singers from production tasks is necessitated by findings by Sundberg (1968: 3), who shows that,

...the articulation habits acquired during the voice training have some bearings on the speech articulation of singers.

As it is likely that singers have habitually lowered larynxes, they do not maintain a 'normal' speech posture, and will therefore produce articulatory and acoustic data which is not representative of the 'norm'. Specifically, their third and fourth formants are consistently lower in frequency than the values produced by non-singers.

4.6. Subjects' hearing

To ascertain that speakers do not suffer from hearing loss or a psycho- or pathological speech abnormality (Point

4.1.4.) is also obvious good practice. One suspects, though, that too often this confirmation is only sought from subjects' self-reporting. While speech abnormalities are relatively easy to pick up from recordings (either during, or afterwards, in the process of analysis), and to then reject from the corpus, hearing impairment is not so easy to detect. Subjects may not be aware themselves that they have any diminishment from optimal hearing, especially if the loss has been gradual. This presents a further reason for not incorporating older subjects in a general sample; unless geronto-phonetics is being explicitly explored, these older subjects are most likely to have some hearing loss, together with producing speech with more unusual vocal qualities.

Ideally, hearing should be tested before the subject is asked to participate: this would then provide an objective measure of the adequacy of their aural faculty. Naturally, this is a measure even more necessitated if perceptual tests are used, incorporating the presentation of test items either through head phones or through a strategically-placed loud-speaker. Specific remarks to the effect that such undertakings were carried out in the assembly of the phonetic databases listed in Table 4.I are, not surprisingly, entirely absent. Unfortunately, our own data is culpable of not being as thorough as the manifesto demands in this respect, although subjects were asked whether they knew they had impaired hearing. Hindsight would dictate that the procedures indicated in the foregoing

should be implemented in any expansion of our data. In self-defence, it should be noted that the immediately anticipated experimental tasks were solely of a productive sort, did not involve any perceptual tasks, and thus self-reported 'normal' hearing was deemed quite sufficient.

4.7. The necessity of good health

To state that subjects should be in good health may also appear self-evident. There do seem to be researchers, however, who are not particular about discarding unfit subjects. Karlsson (1985: 5) willingly volunteers that one of her female subjects was suffering from a sore throat at the time of the recording. As she also comments that another speaker had a "thin voice" and that there were obvious mis-fits between subjects' cheeks and the Rothenburg mask she was employing, the credibility (and replicability) of her results becomes somewhat minimal. The greatest irony is that the paper purports to describe the 'Glottal wave forms for normal speakers'! Karlsson does however give explicit account of her lack of experimental rigour, and it serves to emphasize the worry that other investigators either do not check that speakers are not suffering from any condition which may affect their speech or hearing, or, if they do check, then they simply disregard this impediment and incorporate the subject anyway.

4.8. Height and weight should also be 'normal'

The basis for point 4.1.5. in the manifesto lies in the fact

that the size of a speaker's head and larynx is directly correlated with their height. Thus, tall people have larger larynxes and short people smaller ones. This differential will naturally affect the properties of these people's speech, with the effect most immediately noticeable in vocal pitch. Vocal tract formants will also be affected. Thus, abnormally tall or short subjects (for their age, sex and SES) should not be included. Fatty deposits can furthermore increase the impedance of the vocal tract walls, and consequently dampen formants, with the reverse pertaining for abnormally thin subjects' vocal tracts. Statistics for average height and weight for the sexes, according to 'class' in Britain have been recently released (The Heights and Weights of Adults in Great Britain, a survey conducted by the DHSS). There are some very interesting revelations which have implications for a database. They include the following:

- men in higher social classes tend to be taller than those in lower classes
- the average height of men in social classes I and II is 5'9"; in classes III and IV it is 5'8": so the difference is not very large
- among men aged under 50, those who are in social class III (usually manual workers) tend to be the heaviest (but not the tallest)
- women in the lower social classes generally weigh more than those women in the higher social classes.

We may see, then, that it is important to control for speakers' SES for more than the one, primary reason (i.e. to not mix 'classes' of speakers), since SES, sex and stature can all interact. Problems involved in the assessment of height and weight are expanded in Henton (1983), and space precludes their repetition here. This brief discussion should indicate nevertheless the importance of maintaining an awareness of the average stature (in relation to sex and SES) in the selection of subjects.

4.9. Further comments on Manifesto points 4.1.6.-4.1.20.

Points 4.1.6. and 4.1.7. are self-explanatory. The practice of 4.1.8. has two benefits: it provides a permanent record of subjects' bio-data, which is useful if the same subjects are required again for replication of the experiment, or for subsequent different experiments. Secondly, it can serve as testimony to the accuracy of the researcher's classificatory abilities, in terms of the subjects' accents/dialects/idiolects. The information given in such questionnaires should, of course, be treated as confidential and subsequent reference to individual subjects should be codified.

Points 4.1.9. to 4.1.11. inclusive do not require expansion because they have either been explained in previous chapters, or they are self-evident. This does not, of course, guarantee that any or all of them have been observed by the assemblers of phonetic corpora listed in Table 4.I.

Pertinent criticism of several databases used primarily for the assessment of speech technology products has been produced by Bladon and Carlill (1985), who use point 4.1.12. as a yardstick. This paper is, however, in interim version form, and cannot be used for citation at the moment.

Concerning point 4.1.13., the need has arisen for a variety of speech amplitudes because of the demands made of speech databases by differing research groups. Those who serve defence need to investigate how speech alters in noisy conditions (e.g. an aircraft cockpit or 'battles'). Less invidious research, for example by those investigating speech pathologies, needs to monitor whether softer or louder speech will adversely affect existing pathologies, or will reduce accuracy of results by speech recognition systems.

The latter sentence in Point 4.1.15., concerning the position of subjects' heads during recording sessions, may seem superfluous at first, but Fant (1960: 111) reports that if the head is tilted back, then the behaviour of the first two formants will be exaggerated (specifically, F1 will increase, and F2 will decrease by a greater amount). The indication here, then, is not to position prompt-material on the ceiling!

Finally, and with regard to 4.1.18., it has been noted (Chapter 2, Section 2.2.5) that the sex of the interviewer can be influential on the productions of the subject. Giles

(1973) noted that the interviewer may accommodate their speech style to that of the subject, who may then also accommodate further to the interviewer's style. A law of ever-diminishing differences (and possibly, experimental returns) seems to be set in motion. Trudgill (1982) also observed that his style as an interviewer accommodated to that of his speakers, but there the process seemed to stop, because he did not notice any further accommodation by them to him. As it is likely that the comparative age, race and SES of the interviewer (or person conducting the recording sessions) will also affect subjects' performances, opportunities for these interactive variables to appear should be minimized by keeping the identity of the interviewer the same throughout.

A considerable amount of discursive detail has been devoted to the desiderata of our database manifesto. It is now appropriate to include Table 4.I., and thereafter illustrate how far its contents succeed in generally satisfying the foregoing demands.

Table 4.I.

Phonetic studies which have provided 'representative' acoustic
data for adult speakers of that language/dialect: a cross-
language survey

Language	Number of speakers	Sex of speakers	Source
Arabic (Cairene)	4	2f; 2m	Khan (1975)
	4	2f; 2m	Ahmed (1979)
American English (General)	61	28f; 33m	Peterson and Barney (1952)
British English (RP)	25	male	Wells (1962)
	6	male	Holtse (1972)
	40	20f; 20m	Henton (1985)
British English (Modified Northern)	40	20f; 20m	Henton (1985)
Danish	19	9f; 10m	Frøkjær- Jensen (1967)
	9	1f; 8m	Fischer-Jørg- ensen (1972)
Dutch (Standard)	20	10f; 10m	Koopmans-van Beinum (1973)
Dutch	25	female	van Nierop, et al. (1973)
	50	male	Pols et al. (1973)
Dutch (Utrecht)	20	10f; 10m	Koopmans-van Beinum (1973)
Estonian	10	4f; 6m	Liiv and Rommel (1970)
Finnish	16	8f; 8m	Suomi (1984)
French (Parisian)	?	?	Debrock and Forrez (1974)
		(as rep. by	Carton, 1976)
French (Parisian)	?	?	Landericy and Renard (1977)

Table 4.I. continued

Language	Number of speakers	Sex of speakers	Source
French (Parisian)	?	female	Mettas (1979)
	19	9f; 10m	Lonchamp (1986)
(Canadian)	1	male	O'Shaugnessy (1982)
German	3/6	male	Jørgensen (1969)
Gujarati	4	male	Dave (1977)
Hausa (Kano)	10	?	Lindau-Webb (1985)
Hungarian	5	2f; 3m	Magdics (1969)
Italian	50	25f; 25m	Ferrero (1968)
Japanese	7	male	Keating et al. (1984)
Korean	10	5f; 5m	Kim and Fuji- saki (1973)
Norwegian	10	male	Gamnes (1965)
Polish	3	1f; 2m	Jassem (1964)
Portuguese (Lisbon)	8	male	Delgado Mart- ins (1973)
(Brazilian)	9	male	Godinez (1978)
Russian	3	1f; 2m	Mahnken and Braun (1951)
	4	1f; 3m	Halle (1959)
Sardinian (Campidanian)	2	male	Contini and Boë (1972)
Serbo-Croatian	12	7f; 5m	Lehiste and Ivić (1963)

Table 4.I. continued

Language	Number of speakers	Sex of speakers	Source
Serbo-Croatian	5	male ("randomly chosen")	Purcell (1971)
Shona	1	male (the author)	Pongweni (1983)
Spanish (Peninsular)	6	male	Godinez (1978)
Spanish (Argentine)	4	male	Godinez (1978)
(Mexican)	6	male	Godinez (1978)
Swedish	14	7f; 7m	Fant (1966)
!Xhòõ	10	male	Ladefoged (1981)

4.10. Phonetic databases: how they measure up to the Manifesto

From the preceding review of desirable properties in a phonetic database, it is not unreasonable to say that the great majority of such databases compiled hitherto are imperfect. In fact, some should be re-assembled altogether, if they are to provide adequate portrayal of a cross-section of speakers in various accents/dialects/languages. We will now examine a chronological selection of those databases listed in Table 4.I. to exemplify this sorry state of affairs.

The material collected in 1952 by Peterson and Barney for the acoustic description of so-called General American was pioneering at the time because they attempted to describe the vowels of men, women and children, whereas previous studies had not crossed sex or age barriers. Their data is still used uncritically by many investigators in the United States and elsewhere. We will show, though, that their results suffer from several serious, eventually damning, faults.

Seventy-six speakers, including 33 men, 28 women and 15 children recorded a total of 1520 words. From the outset, then, there were large disparities in the number of speakers for each sub-set. Two of the speakers were born outside the United States (no information is given as to whether they were recent immigrants or had merely been imported as babies) and "a few others spoke a foreign language before

learning English" (1952: 120). Results obtained from these speakers should surely have been eliminated, but they were included and thus we cannot tell how far they skew the emerging picture. Further self-condemnation also emerges (ibid.):

Most of the women and children grew up in the Middle Atlantic speech area. The male speakers represented a much broader sampling of the United States; the majority of them spoke General American.

The notion of a speech community may indeed be hard to define, but this heterogeneity in a sample is totally unacceptable, and leads one to the simple, but death-dealing objection that many of the differences between male, female and children's speech apparently discovered by Peterson and Barney must be accounted for by **dialectal variation**. No mention is given to the age of the 'children', and indeed if pubescent and post-pubescent 'children' were used, then their formant frequencies would be quite dissimilar from 7-8 year olds.

The need for an accurate representation (as opposed to the melting-pot described above) of the General American accent is pressing, but at the time of writing, none is available. Perhaps, however, other accents/languages have been better documented? The brief answer is a depressing 'no'.

Wells (1963), exploring British English RP, tells us that 23 of his 25 **male** speakers were of university age, and two were university lecturers, who could be aged anything between 23

and 67! Apparently they were judged, to begin with, to be a fairly homogenous group of RP speakers, but according to what criteria and whose ears? Indeed, by page 37 of his thesis, Wells admits to insecurity about this homogeneity:

In the case of 5 out of the 25 subjects, there had been some doubt whether their speech was completely RP, that is absolutely free from regional influence. Speech of 3 of them seemed perhaps to have slight Northern or Midlands influence, and that of the other 2 to have slight London or Home Counties influence.

If 20% of his subjects were of doubtful qualification, then the strength of his results is seriously jeopardized.

Fant's data for Swedish males and females (1959, and reprinted, 1973) provides rather damning biographical information about his informants. Three of the fourteen were trained in speech production: one male was a professor of phonetics, another Fant himself and one female was a "well-known Swedish singer and voice specialist" (1973: 32). While all subjects had lived in Stockholm for a minimum of five years, five of them had been born elsewhere. Fant admits that one subject, born in Lappland, did not belong to the "fairly homogeneous dialect group". In view of these comments, Fant offers the deeply ironic judgement that (1966: 23)

(Peterson and Barney's) American English study and my Swedish study are the only reliable sources of data on both formant frequencies and amplitudes.

Material assembled for the acoustic description of tense and lax vowels in North German 'Hochsprache' by Jørgensen (1969)

is extraordinary in its origins. There are two sets of material discussed; the first set was analyzed from three male speakers. Spectrograms were made of one of the speakers from a gramophone record! This same subject (1969: 229)

... spricht eine sehr geschliffene Bühnensprache norddeutschen Gepräges.

MacCarthy (1975: 6) has some reservations about the use of 'Bühnensprache' as representative of standard German, which do little to help Jørgensen's case, namely:

Probably no single German speaks, or could speak, according to Siebs or Duden (the two prescriptive authorities on Bühnensprache - my brackets) for more than a few minutes at a time, if that. Ultimately, both stand for an artificial form of speech, an ideal perhaps, but one that it would be difficult to justify on any practical grounds.

Jørgensen's other subjects are as much of a mish-mash (1969: 229)

GR spricht Hochdeutsch auf einem ziemlich ausgeprägten berlinerischen Substrat. GJ... (zur Zeit der Aufnahme) hatte er sich, auf zwei Perioden verteilt, im ganzen etwa zwölf Jahre in Dänemark aufgehalten. Er spricht hochdeutsch mit südmitteldeutscher Färbung.

To quote the biographic details of Jørgensen's other six male informants would be excessive: they are all (if it is not oxymoronic to say so) consistently heterogeneous in their geographical origins and varieties of German spoken. It is impossible to ascertain the ages of these informants, but their dates of birth (1875; 1912; 1936; 1939; 1940) suffice to indicate that a staggering range of 65 years was incorporated! Ironical amusement is the only suitable reaction to such empirical 'methodology'.

Holtse's (1972) data for British English RP is even more suspect than that of Wells, in that Holtse only recorded six male speakers, who certainly did not comprise a homogeneous group. They were "judged by competent observers to speak a very close approximation to RP" (p.3). What, exactly, is 'a close approximation to RP'? The situation worsens further when it is revealed (ibid.) that of the six, three were university students, two were university lecturers and one a civil servant. Their ages consisted five speakers in the 20-30 year age group (acceptable) and one 71 year-old! Two informants were recorded in Edinburgh and four in Copenhagen, using different tape recorders and tape on both occasions. This data is not merely leaky: it is positively shot full of holes. Heterogeneity of data per se is not worthy of criticism, for the researcher may be interested in comparing two different age or SES-groups. The indiscriminate **pooling**, or averaging, of data recorded under completely different conditions, or derived from speakers with widely differing backgrounds or ages is what we are objecting to here.

The acoustic description of Danish short and long vowels by Fischer-Jørgensen (1972) suffers from similar deficits. Her data were assembled from one female speaker (herself, and therefore not naive) and nine male speakers. Her own description is sufficiently condemnatory (1972: 50):

...speakers of Standard Danish, born between 1905 and 1930. The speech of five of the informants can be characterized as Copenhagen standard, mainly on the basis of intonation features, one speaker has certain

Funish characteristics, and the speech of the remaining three subjects has hardly any local features.

The tolerance of the lack of parity of female and male speakers, the large age-range and the diversity within the 'Standard' renders the value of this study (and the many others like it) rather minimal.

Koopmans-van Beinum (1973) restricts geographical diversity in assembling the Utrecht and Standard Dutch data, but then records informants ranging in age from 25 to 65 years (two generations apart!) and pools their data. (See remarks in Section 4.4. concerning age).

The study by Godinez (1978) contains some particularly ironic remarks, which bear repetition and strengthen our current protests (1978: 7):

We are not interested, then, in the behaviour of an individual, but rather of how members of a group tend to behave as a whole. The experimenter will have to keep this in mind when selecting a representative sample from the parent population. Another question involves the control of variables such as a) the range in age of the subjects b) sex and c) the linguistic experiences of the subjects used in an experiment. This latter aspect includes, for example, such factors as place of birth, place(s) of residence and socioeconomic status.

Very laudable, we reply - he might even have foreseen the need for the Manifesto. Turning the page, however, to the description of the recordings made by Godinez himself, one might suspect that he suffers from some form of disability of short-term memory. For the four Romance dialects which he analyzes (viz. Mexican Spanish, Argentine Spanish, Peninsular Spanish and Brazilian Portuguese) he uses **solely**

male speakers. The number of the sample for each group (and, remember, "we are not interested in the behaviour of an individual") is 6, 4, 6 and 9 respectively: hardly a large step away from minimal sampling. Even the (comparatively impressive) 9 subjects for Brazilian Portuguese do not avoid Godínez's third type of variation, originating as they did from São Paulo, Rio de Janeiro, Espírito Santo and Bahia. All of which goes to show that what is preached cannot be relied upon to appear as practice.

Mettas (1979), investigating Parisian French, makes a good attempt at uniformity among her subjects and remarks on the power of speech to distinguish social categories. Although she analyzes "pronunciation distinguée" and "pronunciation de la bourgeoisie cultivée" separately, and bases this division on a socio-professional survey, she nevertheless ultimately omits to state how many informants she uses!

Finally, an assortment of particular dissatisfactions. For French males' data Carton (1976) and Landercy and Renard (1977) do not consider it important to reveal either the sex or the sample number of their informants, nor their dialect or sociolect! To make matters worse, the latter two authors appear to have rounded all their acoustic formant values (for formants 1-3) to the nearest 50 Hz. This is needlessly wide tolerance, since male formants can be measured on a spectrogram to an accuracy of 25 Hz. (Lindblom, 1967). Given also that both authors are at the University of Mons,

in Belgium, one cannot help but wonder whether the values are in fact for Belgian French, which has distinct dialectal differences from the (standard) Parisian variety. It is with relief that in this thesis we may now make use of the much-more closely controlled data for Parisian French recently collected by Lonchamp (1986) for the cross-language considerations in this thesis.

Suomi (1984: 201), in examining a 'whole spectrum' approach to vowel normalization in Finnish, blithely states "The dialect background of the subjects was not controlled." Lindau-Webb (1985) does not appear to perceive the sexual dichotomy which exists in life, and just describes her ten subjects as 'speakers', without ever telling her readers which sex they were. One may see, then, that blindness as to the possibility of men and women speaking differently is still alive and well at the time of writing. The worst recent offender on this score is Nartey (1982). In his study of fricative phones and phonemes from fourteen languages, he tells us (p.20):

My original plan was to analyse data from ten speakers each from each language (six male and four female), but due to insufficient number of speakers in a number of languages (and mostly for consistency), I ended up analysing five speakers each (3 male and 2 female). For Japanese and Korean I got more female speakers than males so I analysed data from three females and two males of each of these languages.

To begin by wanting fewer female speakers than males is bad (but not unexpected); to then analyse an even smaller number, with the sexes unevenly represented across languages

is worse, but to pool the data from the female and male speakers (which is what Nartey then did) finally makes his data totally incapable of replication, and the results very messy.

The problematic studies cited in detail above comprise a very large number in the cross-language list in Table 4.I. The others to which particular attention has not been given are by no means perfect, and we can only conclude that, in general, phonetic data-gathering has been insufficiently systematic and the results either unreliable, or more gross than they need be.

With an almost universal disregard by phoneticians for uniformity in speaker-sampling techniques and the ignorance of social and physical (stature and health) factors influencing their speakers' speech, it is most difficult to certify that the cross-language data published to date has in fact represented one accent, dialect or sociolect. The crucial problem this raises for work such as ours is that we cannot say exactly how much deviation in their results is due to actual male-female speech differences, and how much to spurious methodologies. We are forced to the unhappy conclusion that all these studies should be regarded warily, and that phoneticians have to monitor sociolinguistic homogeneity if their results are to be credible or cross-linguistically comparable.

Section 4.12. will depict how far we have gone to try to

remedy the ills of phonetic data-assembly for this study. Beforehand, though, there is another hurdle to the objective analysis of cross-sex phonetic diversity: this is outlined next.

4.11. Androcentric instrumentation: a womantrap

To add to the problems presented in the preceding sections, the disclosure that even the instruments of phonetic analysis are not unbiased is surely enough to invite despair. When Fant (1975) was attempting to employ a non-uniform normalization technique, it occurred to him that (p. 14)

Large deviations from the average female-male formant-frequency factors frequently occur in vowel studies, and it is uncertain to what extent these deviations are dialectal or systematically related to sampling and measuring techniques.

In view of the evidence presented in Section 4.9, we feel that that we can reply to Fant's query, and say 'to a very large extent'. We have already indicated that there are frequent possibilities for dialectal deviation to appear in a 'consistent' sample, and that sampling techniques are far from ideal. Here we turn explicitly to 'deviations' arising from measuring techniques.

Researchers have had ten years since the above remarks in which to try to improve the situation. Little, however, seems to have been achieved towards the goal of balanced sampling and the adequate representation of women's speech. In 1980 (pp.55 and 57) Fant is still to be found recounting the same facts:

...we lack reliable data on details of the vocal tract as well of essential differences between males and females...The lack of basic data is especially apparent for female and child speech ...Most studies have been concerned with male speech and vowels.

These observations are indeed still regrettably accurate another five years later. Fant himself does not seem to have done a great deal to appease his own dissatisfaction, nor, in fact, have many other phoneticians. This is why the acoustic data for females coming from two different accent-groups, which will be presented in Chapter 5, is a vital contribution to the progress of phonetic research in general.

It is not insignificant that Fant, together with being a most eminent experimental phonetician for the past twenty-five years, has also been extremely influential in the design of instruments employed in the experimental field. Thus if sound spectrographs, which have been the most widely-used tool in the acoustic analysis of speech, and other instruments (such as narrow-band spectrum analyzers) are still imperfect in analyzing female speakers' speech, then this surely reflects the androcentric reality of the obscuration of the speech of females. Specific criticisms of the problematicity of formant frequency determination, especially for female speakers, using spectrography has been voiced by Ladefoged (1967: 81), and by Ladefoged and Bladon (1982).

This issue has also caught the attention of Klatt (1982). When reviewing the efficacy of the use of spectrograms from

which to draw acoustic conclusions, he asserts that (p.181):

As far as speech perception research is concerned, it is not inconceivable that the sound spectrograph has had an overall detrimental influence over the last forty years by emphasizing aspects of speech spectra that are probably not direct perceptual cues (and in some cases may not even be resolved by the ear).

Klatt illustrates his argument with spectrograms of the same utterance produced by a man, woman and child. As he says (p.182),

The woman and child speak with a much higher fundamental frequency, have a more breathy voice quality, and also have shorter vocal tracts, implying higher formant frequencies...All of these differences leave the observer with the impression that either (1) the speech of women and children must be harder to understand than that of men, or (2) listening to women and children requires the invocation of different decoding strategies.

These are most apposite comments from Klatt, both for our present discussion and (as we will see later) for our theory concerning speaker normalization, which is expanded in Chapter 5. Klatt continues to say (p.183):

Few people could be convinced that the second alternative, markedly different perceptual strategies for women, is true either. In fact, it seems to be generally believed that the speech patterns of men and women could be made to look more similar if minor modifications were made to the sound spectrograph...Yet here we are, nearly forty years later, and the sound spectrograph machine essentially has not changed.

To have Klatt's criticisms as underpinning for our own dissatisfaction with the commonest piece of speech-analysis instrumentation is, of course, most gratifying. Echoes are further found in the introduction to an article by Johansson, et al. (1982: 117):

Comparatively little is known about the characteristics of the female voice as compared with the male voice. The background is the high fundamental frequency range

of the female voice which makes formant frequency estimates uncertain and, hence, information on the voice source unsafe.

The implication, which has been suggestively hovering around all these comments, namely that there is something intrinsically more difficult analytically about female voices, is finally made plain here. The assumption is incorrect, but too few authors have thought to blame the design of the technology rather than women for producing analytical problems. Women's voices only appear more analytically 'difficult' because of the limitations of present instrumentation. They are not more 'difficult' to the human ear: women speakers are not any less intelligible than men, and may actually be more so, although the evidence seems somewhat variable (see Klatt, 1982; Goldstein, 1980 and Chapter 7, Section 7.5). The insidious mysogeny which was so clear in the words of Swift and Jespersen (see Chapter 1) is still very much present in the mid-1980s.

It is thus from experience and from this argumentative platform that the use of the sound spectrograph as an appropriate tool for the analysis of my own female and male data was minimized. Those instruments that were used in preference are described in Chapter 6, Section 3.

We may infer, then, that a great deal of technological improvement could be made in the processing of those unfortunate speakers with higher fundamental frequencies, namely the 'unquantifiable' (see Chapter 3, Section 3.5)

female sex. For any analyst who has groaned and brow-beaten over a female vowel spectrogram, this is a cry from the heart. Unfortunately (but entirely in keeping with the distribution of women in scientific positions generally) few phoneticians are female and even fewer research technologists are. There is thus very little 'grassroot' motivation for the improvement of womens' analyzable lot. So, the myth that the speech of females is somehow more 'difficult' and 'deviant' (c.f. Chapter 1) is all too comfortably perpetuated by the continuing inadequacies of much equipment-design.

4.12. The collection of data for the description of male-female differences in two accents of British English

We have devoted a considerable amount of space to characterizing the deficiencies of other phonetic data-sampling techniques, and to highlighting shortcomings of acoustic phonetic experimental methodology in general. It is now appropriate to end this chapter by specifying exactly how the data for our own experiments were collected, and how this technique may be regarded as an innovation among phonetic data corpora.

To begin with, then, a very substantial recorded database for two accents of British English was assembled for this research, embodying controls of speaker characteristics which go far beyond those in most studies of this kind. In describing the data-collection techniques here, we are conscious of departing somewhat from a more usual

presentational sequence. It is not conventional to describe subject-selection and recording techniques **before** the main research hypothesis. This approach, I feel, is justified, however, by viewing this current study in the broader framework of trying to establish a new discipline of socio-phonetics. Thus the data-gathering techniques are intended as a methodological contribution in their own right. Having forged the link between phonetic data-collection and social structures, I will then, in Chapter 5, describe the central hypothesis and the way in which it was developed and explored.

4.12.1. Subjects

Eighty speakers of two accents of British English were recorded. The subjects comprised forty speakers (20 female and 20 male) of Received Pronunciation and forty (20 male and 20 female) of Modified Northern English. Explanation for the interest in these two particular accents will be given at the outset of Chapter 6.

While twenty speakers of each sex for each accent represents a most marked improvement on sample numbers characteristically used in phonetic studies, it is still a modest population. Given the constraints of this thesis proposal and the time available, though, this number was the maximum which could be thoroughly analyzed by one researcher.

The accent Received Pronunciation (RP) was determined according to Wells' (1982) and Gimson's (1984) criteria. Wells (1982: 279 ff.) draws distinctions between **mainstream RP**, **U-RP**, **adoptive RP** and **near RP**. The subjects recorded for this study were selected as speaking either mainstream RP or adoptive RP. According to Wells, the latter is defined as being spoken "by adults who did not speak RP as children" (1982: 283), but since "adoptive RP merges imperceptibly into mainstream RP" (p.284), there was little point in separating the two for our purposes. Speakers of U-RP were excluded because such an accent is "...popularly associated with, say, a dowager duchess" and "is not quite the same as mainstream RP" (p.280). U-RP is spoken by the upper class: a class which was not being investigated here. Lastly, speakers of near RP were eliminated quite simply because their accent, while not including many regionalisms, does not systematically contain the same phonological oppositions as mainstream RP (see page 297 ff. in Wells).

Gimson actually provides a phonological inventory of the vowels and consonants of present-day RP (1984: Table 3.1, page 47), which was checked, using phonetic controls, to be the contrastive system consistently used by all the informants recorded as speaking RP in this experiment. For example, subjects did not contrast the sounds in roll and role, nor did they fail to reduce glide clusters where expected. Gimson (1984: 48) further makes a worthwhile suggestion concerning the sampling of RP informants:

The most sensible solution restricts the informants to those of the middle age group 'general RP' speakers (Gimson, 1980: 92), avoiding the more conservative and obsolescent speech forms of older generations and the often eccentric and ephemeral innovations of the young.

The age group of my sample is thus supported from the 'stability' in their speech, together with the general physiological and pitch-related reasons mentioned in Section 4.4.

For the purposes of this research project, the accent 'Modified Northern' (MN) was defined to be that of speakers born and brought up within 16 miles of Leeds, Yorkshire, but who have subsequently moved away to the South of England and thus 'modified' their accents.

All speakers of RP were recruited and recorded in Oxford. The MN speakers were recruited from Oxford and Leeds jointly (the latter with the assistance of the University of Leeds Phonetics staff). The same criteria, however, were applied to these speakers whether recorded in Oxford or Leeds, viz. that they should have grown up and been educated in or near Leeds and that they should have then left Leeds (usually for educational or employment reasons) to live in the South of England for a period of at least three years. In the case of the Leeds subjects, they had then obviously returned to Leeds again (primarily to pursue a course as a mature student at the University of Leeds). In self-reporting, most speakers of Modified Northern English volunteered that they were conscious of the features of their accent having been accommodated to that of the prevalent accent to which they had been exposed (i.e. RP).

Great care was taken to maximize sociolinguistic homogeneity of accent (see Manifesto Points 4.1.6. and 4.1.7.) First, potential subjects were closely screened for accent by a trained phonetician (the author). When recruiting MN speakers, subjects were also asked whether they thought they fitted the criteria for a modified accent, according to their geographic origins and current place of abode/employment. After this initial selection procedure, subjects were invited to the Universities and asked to complete a questionnaire, an example of which is included as Appendix A. As a result, a subject pool was assembled all of whom were closely controlled for age (between 25 and 40 at the time of recording); height and weight (within a normal distribution); general health (good); education; occupation; general socio-economic status; geographical origin and (in the case of the Modified Northerners) amount of time removed from their place of origin. The subjects were all white. It was established that no potential subject suffered from any speech abnormalities or hearing impairment. A more general inference which could also be made from the questionnaires was that most subjects were 'middle-class', by the criteria of parents' occupations, the educational background of the subjects and their occupations. They were all 'academically employed': reasons for this control are provided in Section 4.12.2.

These extensive precautions mean that, compared with previous phonetic studies undertaken into sex-specific

differences, the subjects for this research are more closely controlled than any others described in the literature to date.

Lastly, and in connection to Manifesto Point 4.1.18., during all questionnaire completion times and all recording sessions, the researcher remained the same. This is important, since sex of the researcher has been shown to an influential variable on the collection of data (see Chapter 2, Section 2.2.5., and Section 4.9 here). The maintenance of uniformity in this respect was seen as a useful way to inhibit one possible source of variability.

4.12.2. Why determine an 'academic background'?

The relevance of desiring the subjects to have a shared academic background can be demonstrated with the assistance of remarks by Lakoff (1975). It is one of Lakoff's firmly held ideas that academic women perceive their roles as similar to those of men and thus have similar perceptions of the social and psychological environments they inhabit. There are simultaneous advantages and disadvantages. In her first discussion (1975:7) of women and academia, Lakoff describes the female student wanting to move in two directions at once. If a professor is a man (as is most likely) a female student knows that he will receive favourably comments which are scholarly, objective, and unemotional, and which will be couched in 'neutral' language. But if she knows that, as a man, he will respond

more positively to her at other levels (an often unavoidable truth) if she uses 'frilly' and 'feminine' 'women's language', she will become linguistically schizophrenic, the divided self. Incidentally, it has been noticed that female students frequently participate less in class discussions (conducted by men) than males; perhaps this linguistic indecisiveness is one reason why.

In the stronger version of her argument, Lakoff (1975: 85) claims that this blurring of distinctions is abnormal, and that it is academic men who appear to be adopting some of womens' prerogatives (and not the normal reverse situation) viz, nondirectness and expression of emotion. The result nevertheless is a general closing of the gap.

My choice of the words 'ideas' and 'claims' to describe Lakoff's work is marked and conscious. None of her comments are supported by objective experiments: they rely wholly on anecdote and casual, subjective observation. More empirical evidence is required for her views to be confirmed, but it does seem broadly reasonable to regard academic women as probably nearer to a male-norm than, say, non-academic women are to that norm. Therefore if the former still portray differences, then they will be likely to exist for most other occupational groups as well.

The basis of the selectional procedure employed here posited the following: if a sex-specific, phonetic discrepancy could be shown to exist between academic women and men (who have,

according to Lakoff, greater professional parity and therefore experience less of a social schism according to their sex), then the discrepancy is likely to be wider still among other occupational groups.

4.12.3. Recording procedures

After completing the questionnaires, subjects were asked to record the test material in sound-treated conditions (Manifesto, 4.1.14). In Oxford, these comprise a sound-treated room at ground level in the University Phonetics Laboratory. In Leeds, subjects were recorded in a sound-treated recording booth in the University Department of Linguistics and Phonetics at ground level.

Recording equipment employed in both locations was identical. A Ferrograph 7622H tape recorder was used in combination with hi-fidelity tape and a condenser microphone (AKG 451E). Subjects were seated at a distance of six inches from the laterally-offset microphone, with the material to be recorded supported vertically at a comfortable distance for the subjects to read (Manifesto, Point 4.1.15.).

Subsequent to the completion of all the recordings, the precaution was taken of copying the audio tapes digitally onto Sony Betamax videotape, using a Sony PCM audio converter (at 14 bits) and a Sony C20 video recorder. These digital recordings are, of course, protected from degradation (Manifesto, Points 4.1.16. and 4.1.17.). Had the audio converter been available at the start of the

collection of the data, then it naturally would have been used from the outset. Having commenced with hi-fidelity tape-recordings, though, there was a commitment to instrumental continuity throughout the research. The transfer to videotape, for digital storage (see Point 4.1.16.), was a safety-measure which will ensure the best maintenance of the material for further research which may be conducted from it.

The recording procedure was explained to the subjects, without the object of the investigation being made known (so as to preclude any prejudicial performances - Manifesto, Point 4.1.3.). The material recorded appears as Appendix B. It was devised (Manifesto, Points 4.1.9., 4.1.10. and 4.1.12.) to elicit the following types of monophthongal vowel token:

- unstressed tokens in sentence context (Exercise 1)
- stressed tokens with rising and falling intonation tunes (Exercise 2)
- stressed, isolated tokens with falling intonation (= 'citation forms') (Exercise 3).

Because the material had been hand-tailored to suit the purposes of the experiment, there was no chance that Manifesto Point 4.1.12. would not be fulfilled. The other points in that section (viz. those concerning linguistic content/context) of the Manifesto do not apply to the work described in this chapter. Their relevance will be

signalled, though, in the discussion of the cross-language data, in Chapter 7.

All vowels in the material for recording occur in the context /hVd/ (c.f. Manifesto Point 4.1.11.). This is widespread practice in assembling data for English: firstly, this formula provides meaningful words in all but one case (which is in fact avoided in Exercise 1) and secondly, the acoustic properties of /h/ and /d/ afford minimal unnecessary interference to the vowel when it is being analysed acoustically.

In order to disguise the nature of the exact tokens under investigation, subjects were asked to read Exercise 1 of the material first, without the remainder of the recording being explained to them. In Exercise 1, they were led to believe that the investigation was concerned with the way in which proper names are pronounced. To this end, they should stress those names. This therefore afforded the investigator the non-nuclear tokens of the vowels which were really under scrutiny. Having recorded this first exercise, subjects were then shown the second and third exercises, where it generally became apparent what the genuine material under investigation was. This realization by the subjects about the tokens of real interest was unavoidable, given the necessary structure of the material. However, subjects still did not know anything of the basic research hypotheses (see Chapter 5, for dénouement!).

Before the recording of Exercises 2 and 3, illustrations of some example material were spoken to the subjects by the researcher in order to ensure that subjects would use the required intonational tunes. While the great majority of subjects did pronounce the material with the required intonation tunes, in a few cases (especially in the word list), an unwanted 'listing' intonation pattern was occasionally used despite the efforts to control against it. Since, however, the isolated words were recorded twice, in different orders, it was always possible to select for analysis a word token which was free from the listing effect.

In Exercises 1 and 3, the occurrence of the words in the lists was randomized in order to avoid any other possible 'listing' behaviour according to some awareness of articulatory associations by any of the subjects. Exercise 2 was not randomized, but the tokens are not under investigation in this thesis.

All material was recorded with subjects using modal ('chest register') voice. There was no particular interest in this research in the effects on speech perception of a noisy background, or in whispered speech and so therefore subjects were not required to produce soft or shouted speech (c.f. Manifesto Point 4.1.13.). After the recording, before the subject was allowed to leave, the whole set of exercises was replayed, so that any unsatisfactory items could be re-recorded immediately.

Thereafter, the recordings were listened to by trained phoneticians. Any subjects' recordings which did not conform to the linguistic criteria for either accent were discarded and not included for analysis. Thus a collection of material was established, which had been scrutinized closely for any possible accentual variability, and which admitted only a minimum of idiolectal, allophonic variability within each accent.

4.13. Conclusion

This chapter has proposed a set of rigorous criteria for the construction (and evaluation) of phonetic database collections. The immediate necessity for such a measure has been established by the exposure of the various inadequacies of many separate cross-language phonetic databases. Their weaknesses have been shown to be so frequently fundamental that much acoustic data derived cannot be regarded as a sufficient account of the language(s)/accent(s) being described. Sampling and methodological faults are widespread, undermining results and giving rise to conclusions which cannot be relied upon.

With this background, it was incumbent on the present study to provide a model, as far as possible, of how to assemble a worthwhile database for the purposes of socio-phonetic research, particularly of the type using acoustic parameters. A manifesto for methodology has been presented, together with indications of pitfalls to be avoided. So, the

custom-made model has been described in considerable detail. The strenuous efforts taken to assemble a specific data collection in accordance with those model specifications have also been described.

It now remains to indicate exactly the purpose for which the data was designed and collected: this purpose is revealed next, in Chapter 5.

Chapter 5

Proposals for the solution of the problem of
speaker normalization

Research hypotheses

The last three chapters have concentrated on the design of an adequate methodological framework within which socio-phonetic hypotheses may be constructed. Now that a satisfactory base has been described, it is time to detail which particular theories are to be propounded, and how they make use of the custom-made methods described in Chapter 4.

As was stated at the outset, there are four essential hypotheses to this thesis, which we repeat for convenience and expand on here:

1. Females and males speak differently at the phonetic and phonological levels.
2. There are acoustic differences between the speech of men and women which cannot be accounted for solely by anatomical dimorphism, and which remain after the application of a normalization algorithm.
3. These differences will vary from language-to-language, accent-to-accent and speech-community-to-speech-community.
4. Previous attempts to normalize for the differences between male and female speech have not succeeded. An improved way of normalizing the acoustic differences is to use an auditory normalization algorithm.

This chapter will begin by covering the background to the fourth hypothesis. It will then give an account of an auditory theory of normalization. Chapter 6 will reveal the

results of applying the auditorily-based theory to the data for British English, the collection of which was particularized in the previous chapter. In Chapter 7, we will illustrate how well the auditory theory can be applied to further data for a selection of languages other than English. As a result of the cross-accent and cross-linguistic applications, we shall be able to give account of how much support there is for the three hypotheses stated here.

5.1. Why normalization is necessary

Before reviewing previous theories of normalization, it is expedient to outline not only the necessity for speaker normalization, but also what the phrase means. 'Normalization' is used as a telegraphic term to describe the attempts to answer the basic question of how do speaker-listeners, phoneticians, researchers in speech recognition decide that two tokens in speech are 'the same', when (acoustically) they are observably different? Thus, for example, the 'same' vowel spoken on two different occasions, or in two different linguistic contexts, or by two speakers of the same closely-controlled speech 'group' (a term which I use to include age, SES, ethnicity, language/dialect/accents etc.), or indeed by a female and a male will be manifestly different in its acoustic structure in the two tokens.

Until recently, this problem was either unrecognized,

unacknowledged, or, in respect of the sex-based part, non-existent because only male speakers were used as 'representative' of human speech. In the last thirty years, though, the presence of the female speech population has started to attract some notice. Their discovery, however, gave rise to such analytical difficulties that many researchers decided to bury their heads back in the sand, and continue to work only with male speech (see Table 4.I, and ensuing critique in the last chapter). Even this androfocal speech was not entirely 'communicative' however, because it was (inevitably) 'lab. speech'. That is to say, it was invariably produced under high-fidelity sound-treated conditions, with idealized phonatory control and under the expectant, possibly prejudicial gaze of acoustic researchers. Further criticism of this far remove of lab. speech from a 'communicative setting' appears in Bladon, Henton and Pickering (1984). At this juncture, the greatest concern, though, is with how acoustic theory has described and dealt with the differences between female and male voices.

Within the scope of this thesis it is impossible to enumerate and examine all the possibilities of cross-sex acoustic diversity. We limit ourselves, therefore, to the differences which may be catalogued between male and female vowels. The rationale for selecting vowels is threefold. Firstly, vowels carry more speaker-specific information, in terms of speaker-sex and accent. Secondly, vowels have

generally more consistent spectral structures than the majority of consonants (with a few exceptions, such as liquids and nasals: see discussion in Chapter 8). Thirdly, this acoustic finestructure is a feature of speech least under the speaker's conscious control. If analysis of these attributes turns out to reveal differences between female and male speech, which override the expected physiological products, it might be possible to infer that these differences are subconsciously programmed (c.f. Hypotheses 2 and 3). We might also assert that investigations of this sort may be seen as most likely to reveal hard evidence for sex-specific pronunciation differences: to be the 'investigative baseline', as I have called it elsewhere (Henton, 1984a). Lastly, in connection with the possible question 'why vowels?', it is vowels which have been most thoroughly investigated in the history of acoustic phonetics, i.e. over the last forty years.

If we now examine two acoustic analyses of 'the same' vowel, spoken by a female and a male, we can see that they are actually rather dissimilar in several respects. Figure 5.1 displays two [ʌ] vowels, both produced by a female and a male speaker of the same accent, using the same degree of stress and both on a falling intonational tone. They are, in fact, two tokens from the database described in the preceding chapter; so it may be appreciated how many precautions were taken to try to ensure that the vowels were not variable solely for reasons of accentual heterogeneity.

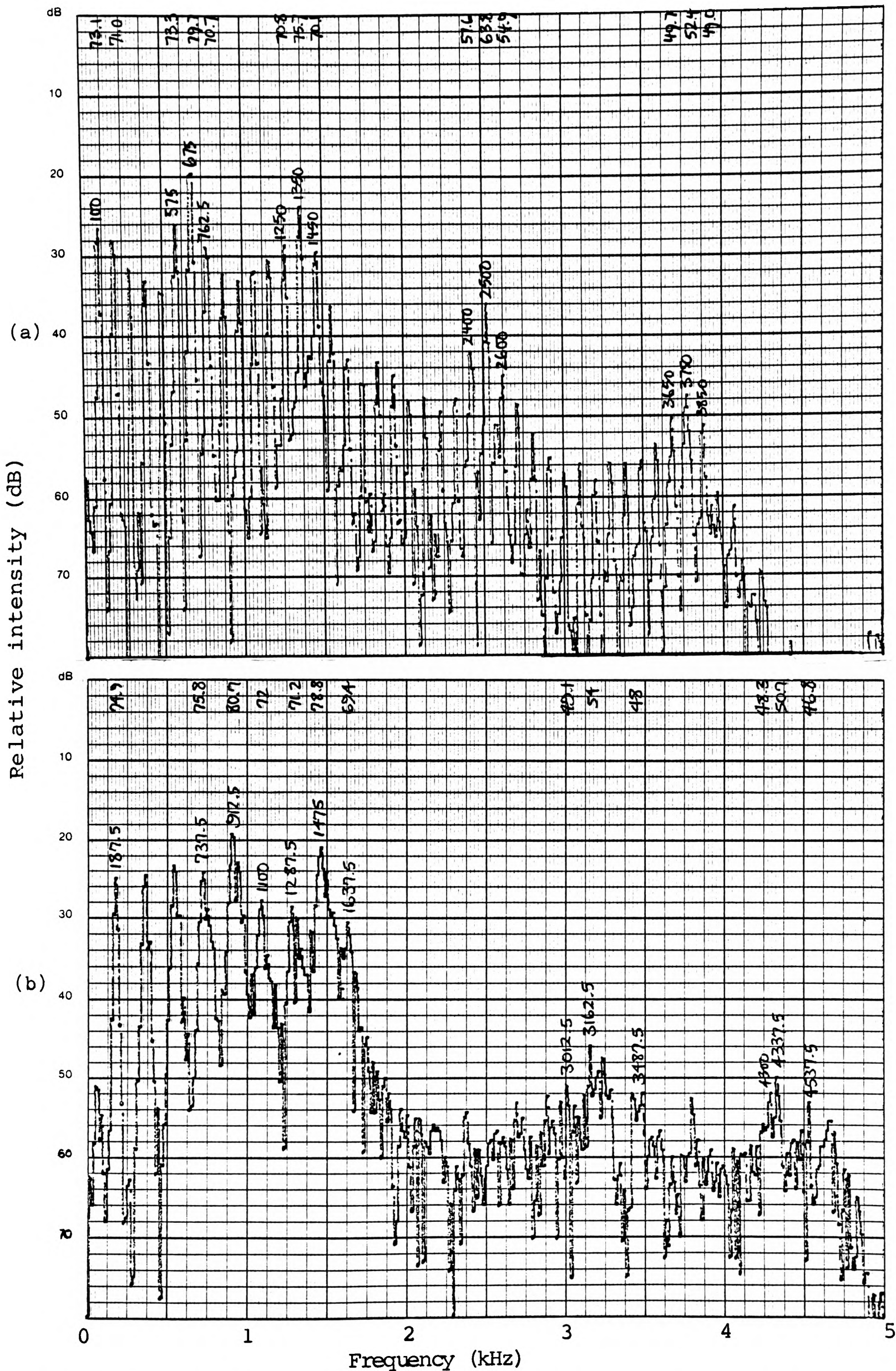


Figure 5.1 Narrow-band spectral analyses of two [ʌ] vowels, spoken by (a) an RP male and (b) an RP female.
 Full scale level: 100 dB;
 F.S. frequency: 5000 Hz.
 Speakers: PB and AJ.

Yet the dissimilarities are striking.

The most obvious difference between the two spectral analyses in Figure 5.1 is in the disparity of the location of the energy concentrations (formants) in the frequency domain: stated simply, the formant 'peaks' are in very different places. There are other points of nonuniformity which are noticeable in Figure 5.1, too. The male vowel has a lower fundamental frequency (F_0), giving rise to more harmonics in the spectrum. And, in general, male vowels apparently have narrower formant bandwidths, a first harmonic of lower amplitude and possibly a less steeply-sloping spectrum.

The details of these forms of variation need not concern us just yet. The first question which arises is 'how can we model the listener's (the brain's) ability to decide that two such differing patterns actually represent the same entity?' The specific answer supplied in this thesis appears in Section 5.7.

Naturally, answers have to be provided to the normalization issue if any progress is to be made in the area of speaker-independent speech recognition. But there are other needful areas of research, too, and some discussion of the applications of a potential answer will be provided in Chapter 9.

From a phonological and larger linguistic point of view, it is desirable and necessary to phonetically separate the

phonemic wheat from the personal chaff: i.e. to give acoustic representation of the vowels of a specific language or dialect which are representative of the **shared vowel targets** for that 'community'. It is not feasible to produce three sets of parameters for vowel production (for women, children and men) if one is attempting to, say, teach a foreign language: there must be an 'agreed' perceptual 'norm', for this is what the ear of any listener must possess. The need is thus increased: we must attempt a regularization for educational purposes and endeavour to represent an analog of the listener's behaviour when assigning a perceived vowel to a particular category.

The next section relates a few attempts which have been variously made to try to solve the normalization problem.

5.2. Attempts at cross-speaker normalization: an overview

On the whole, phoneticians who have noticed systematic differences in female and male speech, appear to have done so peripherally, starting from a quest for spectral shapes of vowels or models for the vocal tracts of growing children. It has seldom been the case that investigators have commenced with a sex-based hypothesis and set out to support it. This may well be because the majority of phoneticians have been male and not questioned the 'norm', using themselves as subjects and oracle (c.f. Chapter 4, passim). The tide is, however, turning.

One of the earliest studies to indicate the fact that, acoustically, children and adult females and males vary in their vowel production, was that by Peterson and Barney (1952). Stringent criticism has already been made of their methodology, but their analysis can serve a useful illustrative function in delineating the task of normalization theories.

Thirty years later, many of the findings of Peterson and Barney have been cast in shadow, but the research attempt being made at the time was novel. The authors indicated their intention as (1952: 118):

...to investigate in a general way the relation between the vowel phoneme intended by a speaker and that identified by a listener, and to relate these in turn to acoustical measurements of the formant or energy concentration positions in the speech waves.

Their results are presented as Figure 5.2. The presentation of acoustic vowel data in this form has now become commonplace, and what Figure 5.2 reveals is that vowel plots can show the disparity between the formant positions of vowels produced by women and men (and children). There are, however, some serious oversights which have been incorporated in Figure 5.2: ones which render it **emblematic** rather than fundamental for successive work.

Firstly, Figure 5.2 reveals exactly the sort of evidence that we might predict would appear from the uncontrolled scatter of their informants (see Chapter 4, Section 4.10), namely that there is not only overlap between vowel first formants, but there is overlap between separate phonemes for

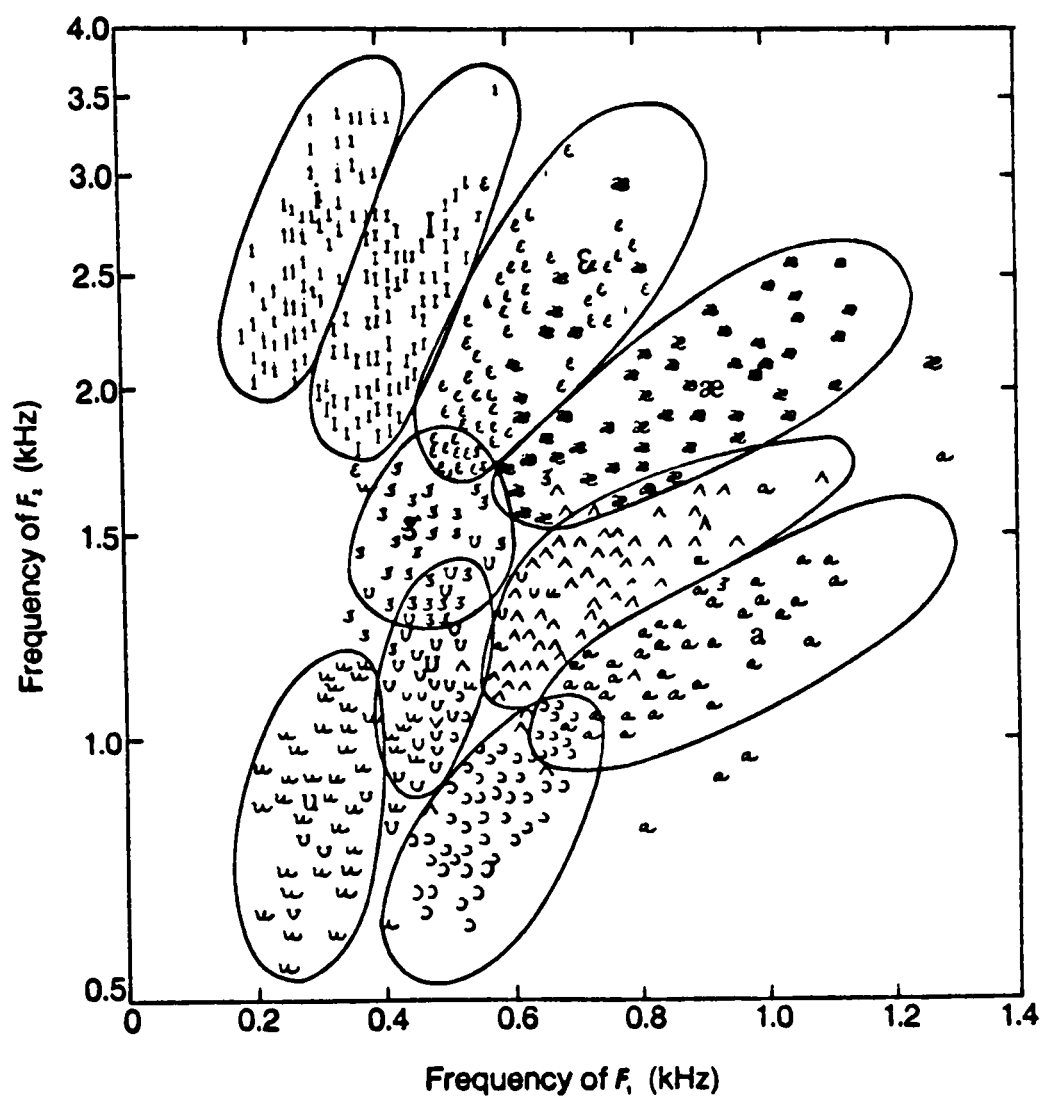


Figure 5.2 Frequency of the second formant versus frequency of the first, for ten vowels of General American, spoken by 76 male, female and child subjects. (Enlarged from Peterson and Barney, 1952).

some speakers. Secondly, the loops, which were drawn to include 90% of the tokens (see later rationale in Section 5.4), **incorporate values for all three classes of speaker!** This representation renders it impossible to detect how distribution varies according to sex or age.

The misleading qualities of Peterson and Barney's presentation of results aside, they did provide an eventual pertinent conclusion. They claimed that both production and identification of a vowel sound by an individual depend on previous language experience. We will return to the tenor of this insight frequently in the ensuing chapters.

Peterson and Barney's data reveal, then, that two speakers may produce vowels that sound the same although they may have very different formant values (vide the **size** of the loops in Figure 5.2), and speakers may also produce vowels which sound different, but which have remarkably similar formant values (vide the **overlap** of the loops). Some explanation for this overlap might be thought to lie in a language's use of phonological **length** for separating individual vowel phonemes. But for American English, length is of rather little distinctive value, in fact.

The problem besetting phoneticians, then, is how to combine the highly variable acoustic manifestations into one recognisable, distinguishable and distinct point or zone in phonetic space. It is possible by producing plots such as that in Figure 5.2, to give a phoneticians' representation

of precise vowel production, but in order to transform these diverse points onto a token with only the phonetic quality common to all speakers of a single language or dialect, normalization is necessary. With variance due to 'individual' characteristics (i.e. the male-female/child-adult anatomical differences) eliminated, it is possible to classify vowels as one entity, one extrinsic allophone, rather than a randomly-influenced collection of acoustic signals.

The final nub for one of the main arguments here (Hypothesis 2) is that **with these speaker-particular effects effectively eliminated from the data by the normalization technique**, then, if the normalization technique is satisfactory, **any remaining discrepancies must be due to the acoustic effects of learned production-behaviour**. In other words, what lies outside the normalized can only be attributed to socially-conditioned, sex-specific speech habits.

Before examining the viability of the second hypothesis, we should consider how far some specific attempts have succeeded in finding an optimal normalization.

5.3. Physiologically-based normalization techniques

A first hypothesis to account broadly for the size of the loops, in fact the acoustic disparities, such as those which appear in Figure 5.2, would be in terms of differences in speakers' vocal tract dimensions. Indeed, the majority of

previous efforts to confront the normalization problem have been couched in **articulatory frameworks**. They use as a basis measurements of, or inferences about, dissimilarities in the vocal tract dimensions of males and females.

We should be beware, however, that explanations of female-male acoustic disparity couched in terms of differences of anatomy alone may not be sufficient for various reasons. First, there is the **non-uniformity** problem. Starting from articulatory-acoustic bases, the anatomical scaling required between speakers is not straightforward, and seems to need different methods according to whether the values to be scaled are from a male, a female or a child speaker. This is because the ratio of pharynx/mouth dimensions is greater for males than for females or children; i.e. the female pharynx requires more scaling than does the female mouth. This lack of anatomical parity formed the basis for several previous formulations of **non-uniform normalization**. Both these and some uniform acoustic procedures are discussed in Section 5.5.

Secondly, articulatorily-based normalization techniques fail to allow for any possible **learned, role-determined influence** on speech behaviour by the sexes. Thirdly, the answers provided from a physiological platform are not convincing because they are **non-unique**: that is to say, any one (unchanging) vowel sound can be associated with a multiplicity of vocal-tract configurations (Ladefoged et al., 1972 and Linker, 1980).

Exemplification in detail of articulatorily-based normalization procedures will not be produced here because just such a review and evaluation has been executed impressively and recently by Disner (1980). Greater discussion will be allocated to previous normalization techniques, including those reviewed by Disner, in Section 5.5. In that section, it will also be shown how well the normalization procedures particularized by Disner perform. Meanwhile, it is important, as mentioned above, to recount the ways in which a normalization procedure may be evaluated. Section 5.4. provides that report.

5.4. Essentials for the evaluation of normalization procedures

There are three important and general considerations to be maintained when examining the efficacy of a vowel normalization procedure. The first concerns itself with the formal representation which is common to the data to be normalized, no matter what the technique. A cursory look back at Figure 5.2 reveals that the vowels appearing in the plots do so in an acoustic configuration which essentially reflects the same positions as those shown in the articulatory tongue hump diagrams employed by phoneticians for many years.

Formant specification is an account of some of the main features of the spectrum of a sound, therefore it is feasible to begin a comparison of vowels by displaying them graphically in terms of their formants. There is a great

deal of evidence (e.g. Delattre, 1951; Fant, 1959, 1960; Stevens and House, 1955) by many investigators which indicates that the frequencies of the first two formants are often the most important auditory characteristics of all vowels (the exceptions and their implications for difficulties arising in the data will be discussed in due course). Thus vowels are plotted in a formant space with formant one (hereinafter F1) along the ordinate and F2 along the abscissa. In most previous studies, the axes of such plots were scaled in Hertz. This study breaks with such use, for reasons which are expanded on later. Axes in previous studies may have been alternatively scaled proportionally to the *mel*, a unit which was shown by Ladefoged (1975) to provide distances which are good representations of the perceived distances in phonetic space. For my purposes, the *Bark* scale was adopted as an up-to-date improvement on the *mel* scale. The *Bark* scale is validated from psychoacoustic experiments (Zwicker and Feldtkeller, 1967; Zwicker and Terhardt, 1980) and, moreover, has been shown to have a direct physiological correlate: more will be said about this scale and its usefulness, in Section 5.7.2.

Within the vowel plot, the area occupied by each vowel is then delimited by means of an ellipse, a procedure which follows Labov et al. (1972) and Broad and Wakita (1975). For each cluster of vowel points, an ellipse with radii of 2 standard deviations is drawn along axes orientated along the

principal components. The ellipses thus enclose approximately 95% of the population along each axis. The ellipse program, as Disner points out, makes it possible to compare the scatter-reducing capabilities of the various normalization procedures with considerable precision. Thus, the first expectation of a normalization procedure is met by (a) a practical visualization in two dimensions; (b) the use of a perceptually-relevant scaling and (c) the use of ellipses as a statistically-based definition of a vowel's space. These points together provide a consistent formal presentational convention.

The second important consideration when regarding competing procedures and their success is related to parsimony. One must beware of deciding that the 'best' normalization procedure is the one which produces the smallest vowel ellipses when applied to a set of data. There is no reason not to adopt this as a criterion if the process is carried out on data from a single language, but it is a dubious yardstick if data is being compared from several languages, for the attainment of smaller ellipses may well ignore or sacrifice the linguistic differences between the languages involved.

Thirdly, normalization proposals must be shown to preserve the linguistic information of the data and this does not rely solely on the formants alone: there are characteristics of vowels which are attributable to the language itself,

rather than to the individual speaker. The defining characteristic of length in British RP, or American English, as mentioned above, is an apposite example. Broadly speaking, a normalization technique should not (in order to achieve phonetic parity) remove itself from the reality of linguistic systems.

One final remark deserves inclusion in this section. It concerns the scaling of formant frequencies to a uniform 'norm'. A 'norm' requires the selection of a reference value. For all studies of adults and children (including the present for reasons of continuity and further comparability) the reference has been the formants of vowels produced by male speakers. There is, of course, no logical reason why that set of references should not be female. To normalize towards values of children would be counter-productive, because their voices are (a) in a greater state of flux and (b) are not the voices which speech technology has to analyse in order to resolve such issues as ASR.

5.5. Some previous acoustic normalization algorithms

As has been said, Disner (1980) provides a thorough review of several normalization procedures. First, those she omits will be discussed here.

A first step towards **uniform, same-sex** scaling was taken by Nordström and Lindblom (1975). Their procedure consisted, in essence, of assigning a vocal tract length based on the relationship between its length and F3 of open vowels. Then

followed the calculation of a scaling factor for an underlying vocal tract and finally an adjustment of the observed formant-pattern along a trajectory to a position within the reference system (tentatively by multiplication by the scaling factor). In formulaic terms, male-to-male normalization was achieved thus:

$$F_{nV} \text{ scaled} = F_{nV} \times \frac{2485}{F_{3av}}$$

where F_{nV} is the observed (raw, unscaled) frequency of formant n of vowel V ; F_{3av} is the average F_3 of the subject's open vowels (those with F_1 greater than 600 Hz), and 2485 Hz is the average male F_3 value derived from speakers across a large cross-language sample.

A problem encountered with the Nordström and Lindblom procedure is that although in Swedish - the language which they were examining - open vowels may have a constant F_1 greater than 600 Hz, it appears that for British English, for example, F_1 cannot be relied upon to exceed this figure. Male voice values below 600 Hz for [æ], [ɑ:], [ɒ] and [ɔ:] are frequently observable in the data for the study undertaken here. Thus Nordström and Lindblom's suggestions concerning F_3 and vocal tract length may suffice for simulated (synthesized) Swedish male voices but there are difficulties in applying the resultant formula to human British ones. And, incidentally, no mention is made of average female values for the F_3 of open vowels. Furthermore, the 'adjustment along the trajectory' is far too

imprecise a manoeuvre when attempting to account for specific and precise anatomical or other acoustic correlates.

Departing from Nordström and Lindblom's conclusions, Fant (1975) attempted to provide a non-uniform method of normalization for female-male and child-male values. The method was non-uniform in the sense that it varied with both formant number and vowel quality. Fant uses as a basis from which to proceed the difference in pharyngeal and mouth ratio between males and females, and adds to it the information that the vocal tracts of children are like those of women in their relative dimensions.

Some of the specific points which Fant sets himself to address are:

1. Are there any universal vowel-category-dependent trends in the female-male and child-male formant frequency relations?
2. To what extent are these reflected in available data from various languages? In particular, is it possible to separate language and dialect-specific trends from speaker-category factors? What is the advantage of non-uniform normalization, if any?
3. How much of the "universal" trends can be explained from present insight in the vocal tract anatomy and acoustic-calculation techniques?

His aims are well-intentioned and within limits, Fant explores the various possible answers adequately. One

becomes defensive, though, when this announcement is made at the outset (p.3, and my emphasis):

Up till now very few attempts have been made to utilize formant data for objective comparisons of language or of dialectal variations within a specific language and **female speech will remain an obscure dialect in terms of the acoustic code.**

It appears that with blinkers strapped firmly in place, Fant will attempt to espy universal trends: it takes a great deal of tolerance to remain sympathetic to his investigative attempts with this pronouncement standing, especially repeating as it does the essence of the belief by Shuy (1970) that **"women are one of the mysteries of the universe."** Fant does nevertheless enlarge the field in his quest for universal trends. Together with data from his own Swedish investigations and that of Peterson and Barney's American English, he incorporates statistics from studies of Danish, Estonian, Dutch, Serbo-Croatian, Japanese and Italian. Fant states that his figures for F1, F2 and F3 were to be drawn from at least four speakers of each sex. It has already been shown, though, (Chapter 4) that dialectal and other types of homogeneity cannot be guaranteed for all those speakers. Thus, Fant's findings are not to be regarded as watertight by any means.

According to the purported differences in mouth and pharynx size of males and females, Fant sets about providing scale factors for each formant for each vowel under scrutiny (15 in all). They turn out to be highly non-uniform, as can be seen from Table 5.I.

Table 5.I. Scale factors (k) from Fant (1975). N.b. This tabulation is not intended as standard for IPA symbols and should be used with caution because of the subjective element inherent.

Vowel	k1	k2	k3	F1	F2	F3
u	6	1	23	310	760	2225
o	7	5	17	425	815	2375
ɔ	11	6	13	500	840	2470
ɒ	17	12	15	670	1045	2510
ʌ	18	18	16	640	1190	2390
a	25	15	15	735	1270	2480
æ	27	17	18	650	1670	2425
ɛ	19	18	20	480	1840	2455
e, i	11	22	18	360	2045	2580
i	7	21	13	275	2190	2950
y	0	19	17	265	1835	2225
ɥ	6	17	22	283	1633	2140
ø	5	16	16	375	1610	2185
œ	7	18	17	450	1390	2275
ə	-4	9	22	360	1225	2255
(Estonian)						
ə	-1	10	16	416	1070	2315
(Swedish)						
œ	7	17	16	525	1105	2430
(pre-r variant)						
ɤ	2	21	16	490	1350	1690
ʊ	3	12	18	440	1020	2240

With these k-factors from Table 5.I, Fant derives the full equation for a non-uniform scaling:

Female-male

$$F_{nV} \text{ scaled} = \frac{F_{nV}}{1 + \left(\frac{F_{3av}}{2485} - 1\right) \times \frac{k_{nV}}{17}}$$

where, as in Nordström and Lindblom's formula, F_{nV} is the observed (unscaled) frequency of Formant n of vowel V ; F_{3av} is the average F_3 of the subject's open vowels (using the criteria described by Fant above), and 2485 Hz the value arrived at (separately by Fant, in this case) for an average male F_3 derived from speakers across a large cross-language sample, but different from Fant's own sample. k_{nV} is the reference scale factor for Formant n of vowel V , selected from Table 5.I.

However, investigative curiosity and the search for further validation of the greater effectiveness of a **uniform normalization** procedure based on **perceptual criteria**, led us to explore the effect of plotting the Fant scale factors as a function of frequency on a Bark scale. From Figure 5.3, it is possible to conclude that there appears to be a strong correlation between the scaling factors and the formant frequencies themselves. However, this would seem to indicate that scale factors and formant frequencies are uniformly related, and that separating the formants achieves little. This diminishes Fant's argument for the remainder of his article.

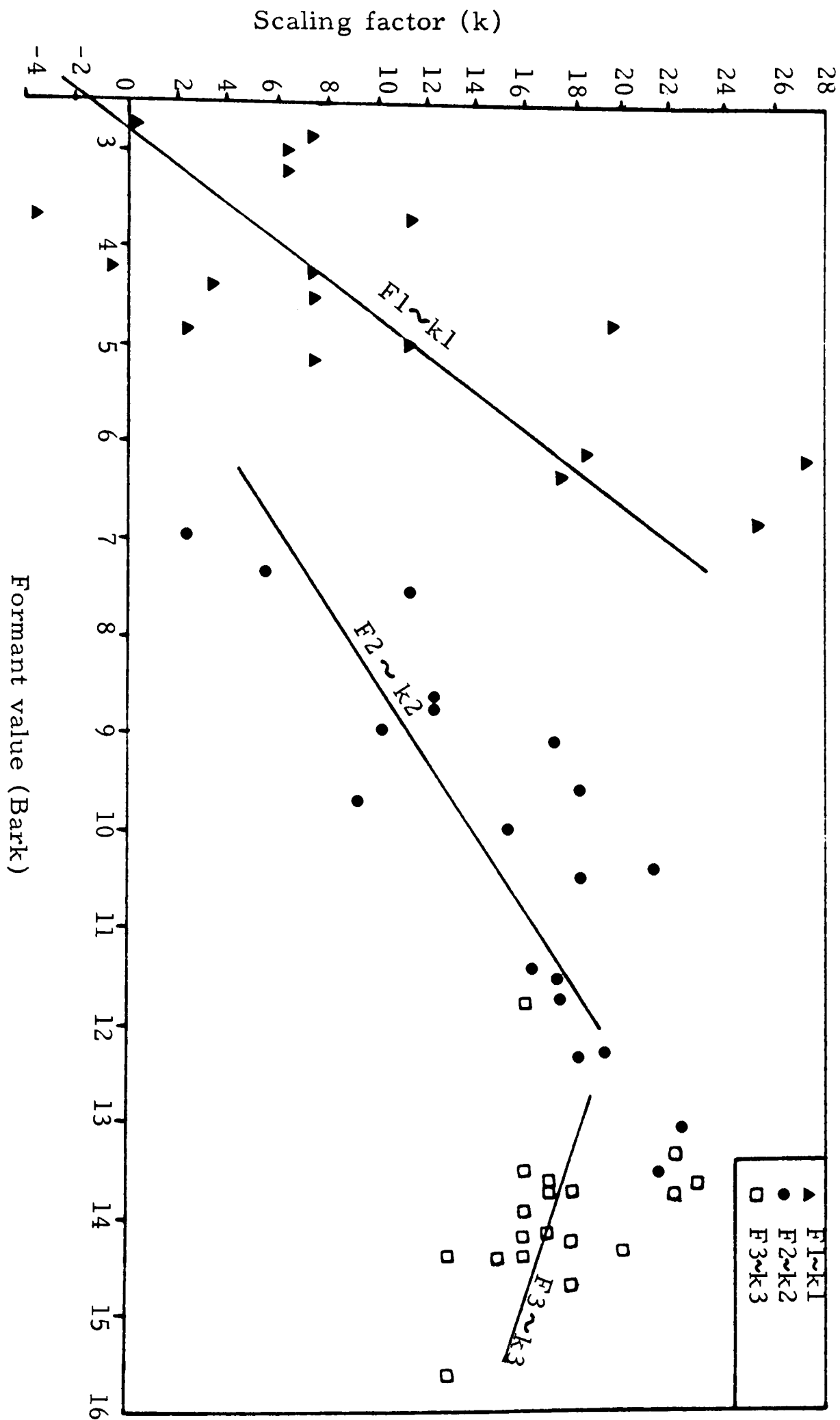


Figure 5.3 Formant frequencies (Bark) and scaling factors, after Fant (1975). Fant's claim is that his 'non-uniform' scaling factors k_1 , k_2 and k_3 are vowel-specific. Using a Bark representation, however, it becomes apparent that at least the scaling factors k_1 and k_2 are frequency-dependent (see correlation lines), and need not be regarded as vowel-specific.

The reduction of variance claimed by Fant may indeed be the case, but a successive remark (1975: 14), to which we have already drawn attention in Chapter 4, Section 11, casts further doubts on the overall efficacy of Fant's explorations in normalization:

Large deviations from the average female-male formant-frequency factors frequently occur in vowel studies, and it is uncertain to what extent these deviations are dialectal or systematically related to sampling and measuring techniques.

As we have already intimated, one of the reasons for female speech remaining the "obscure dialect" can be attributed to Fant's (and other phonetician's) lack of interest in making it anything else, either from a sampling or technological point of view.

Taking a few steps backward will enable us to show that Fant's work is capable of being criticised right from its inception. In the Abstract to his paper Fant conjectures that parts of the universal pattern of deviations from a simple scale factor can be ascribed to non-uniform scaling of vocal tract dimensions. He continues, however, (and this is what is really of interest) (1975: 1)

Other parts require the assumption of sex-specific articulation which may have developed to satisfy perceptual criteria.

The acknowledgement of sex-specific articulatory detail does seem at first to be a useful step forward. However, Fant leaves totally unclear what he means by "perceptual criteria". Does he mean individual speaker identification, male-female differentiation, female-child differentiation or

perceptual stereotyping of sex-linked speech habits?

Fant's final remarks (p.17, and my emphasis) are that,

...female-male formant-frequency relations are in part determined by anatomical constraints. In addition, we find specific compensations that appear to satisfy demands of perceptual invariance or contrast.

Fant does provide some insight into the morasse of conflicting information, but ultimately blurs progress by the limiting nature of a non-uniform normalization procedure and the lack of clarity as to the role of F3. Indeed his own words of conclusion (ibid.) indicate a certain persistent woolliness:

Finally, one cannot quite exclude the possibility of universal 'feministic' preference in vowel qualities which might have influenced the average.

Quite what he means by 'feministic' is as 'obscure' as the original 'dialect' itself.

The link made by Fant to a differential use of anatomy, however, cannot be denied. Indications of this likelihood were provided relatively early in the history of acoustic female-male speech difference investigations, by Mattingly (1966). Using Peterson and Barney's vowel-formant data, Mattingly revealed a low correlation between (a) the distribution of values for the formants of each of ten vowels for men, women and children and (b) vocal tract size. Mattingly concludes that the variation within speaker-class must be stylistic, not physical,

...and the differences between male and female formant values, though doubtless related to typical male and female vocal-tract size, is probably a linguistic convention.

The assertion in the latter part of Mattingly's statement may appear vague, but the inconclusiveness of phoneticians' attempts to explain such cross-sex "conventional" behaviour will become a familiar refrain when there are acoustic elements of the data left after phonetic analysis.

Mattingly's preliminary work was supported by evidence supplied by Sachs et al. (1973) from prepubertal boys and girls. Recordings of 14 boys and 12 girls played to 83 adult listeners provided reliable and valid (70-80% accuracy) identification of the sex of the children from their voices. Various proposals were offered as explanation for this super-anatomical differentiation (including, even, hormonal control over certain aspects of the motor output). No irrefutable claim could be made, however, because the judges may have based their decisions on cues in the childrens' speech other than formant patterns alone, e.g. rhythmical patterns and lexical content (c.f. Chapter 1, Section 7).

Thus a perceptual experiment of this kind cannot be conclusive because too many variables were admitted. It also supplies a reason for not using running speech for analysis, but opting instead for isolated items, as I did in the experiments for this thesis.

Nevertheless, as Sachs et al. (1973: 80-81) speculate, within the limits of their anatomy, speakers could be changing their formant patterns by inter alia changing the

configuration of the lips. Rounding the lips of course lengthens the vocal tract and lowers the formants. So, all the 'folk' linguistics which claims that women talk 'smilingly' may be some imprecise allusion to an attempt to shorten the length of the vocal tract to **raise** the formants and thus appear more stereotypically feminine (i.e. higher-pitched). For a perceptual examination of 'smiled speech', see Tartter (1979).

5.5.1.

Nordström expanded the normalization field further in 1977, with his attempt to simulate female and infant vocal tracts from male area functions. His springboard is that from Fant's work, neither length reductions only nor volume reductions in male area functions reveal formant patterns in complete agreement with observations of different speaker categories, and that earlier explanations of such differences must therefore be reconsidered.

Starting from Fant's findings on vocal tract dimensions and F-pattern nomograms derived from them, Nordström sets out to examine both length, (and this is the new departure) and **cross-sectional area scalings**. Primarily, he demonstrates that the effect of non-uniform scaling for the first three formants of the Russian vowel system (i.e. that used by Fant, with an additional 2 front vowels interpolated between [e] and [a] because these are not present in the Russian system) is very small. Thus, as has been previously

remarked, there is no great advantage in using a non-uniform over a uniform normalization method with regard to vocal tract length. The hypothesis of Fant was not substantiated and therefore Nordström introduced the consideration of volume (or cross-sectional area) scaling. The same scale factors were used as for his length-experiment, but when used on cross-sectional areas, the scale factors were squared.

Nordström comes closer to a better correspondence between scaling factors and the reality of male-female vowel disparities (see Figure 5.4), but he still produces the refrain (1975: 91) that:

..."anatomical" differences, between men and women/ children only explain part of the formant differences.

Acknowledging the further work being carried out by Fant, taking energy density along the vocal tract into account, Nordström points out that wall impedance of the vocal tract together with lip inductance had not previously been considered by Fant and his colleagues. Although the influences of these factors on the total results and the likelihood of their improving the similarity between observations and simulations may be slight, they should not be overlooked.

Nordström's final remarks are indicative of a frustration with acoustically-based investigations which has led to the new orientation of the present study. Nordström (1975: 91-92, and my emphasis) asserts:

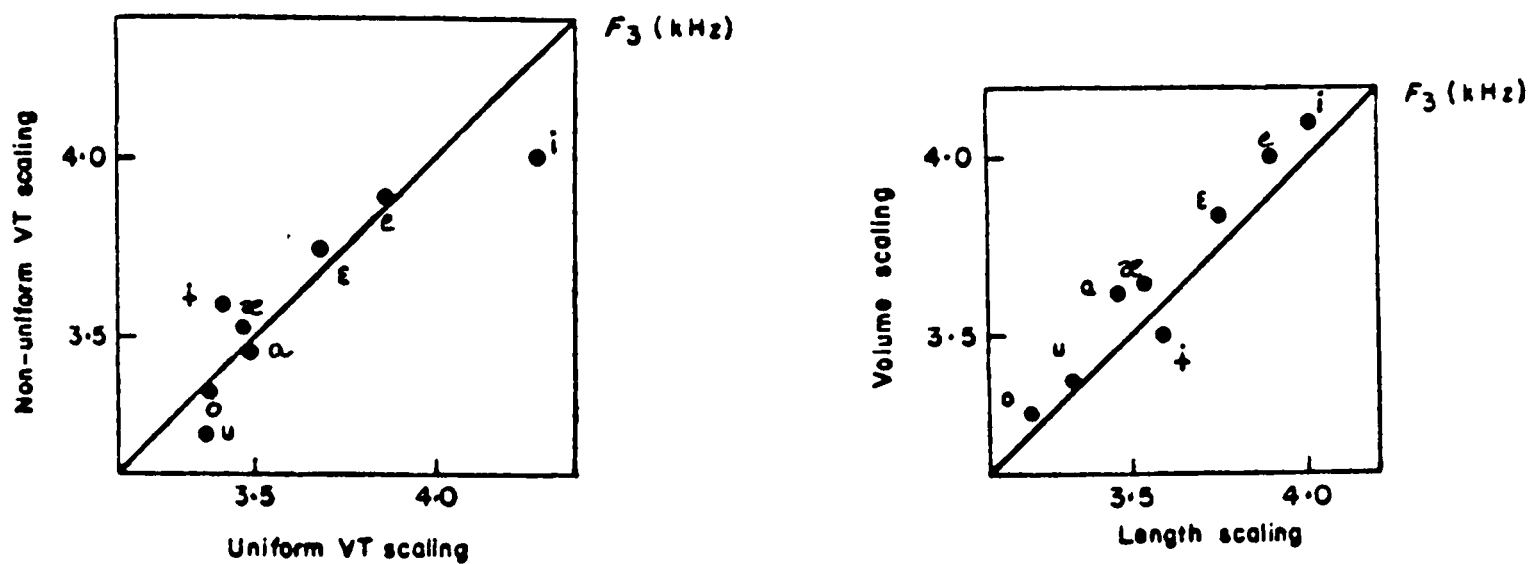


Figure 5.4 Graphs of (a) uniform versus non-uniform scaling for F_3 , and (b) length scaling versus volume scaling for F_3 (from Nordström, 1977).

... we find it probable that the vocal tract varies between men and women/children. Apart from establishing this experimentally, it is fundamental for our understanding of speech communication to find out the underlying reasons. An open-minded approach is essential, and it may be necessary to discard the formant-based explanation. **The all-important issue is how we perceive and how the auditory system performs the normalization between speakers.**

Even if we can describe the formant differences between men and women fairly well (Fant 1975), we are far from knowing the perceptual processing which presumably necessitates these differences. **The interplay between spectral envelope, the distribution of partials and in processing in our ears (and brains) is what must be mapped.**

The desirability of an auditory orientation to the speaker normalization issue still finds echoes seven years after Nordström's original statement. It is reiterated in Nolan (1982; my emphasis):

Describing (this) auditory phonetic normalisation constitutes a major task for phonetics - both theoretic and in the practical spheres of automatic speech recognition. In the acoustic domain progress has been made in for example male-female formant scaling; and now there is much interest in modelling more realistically the auditory processing at least of vowels (e.g. Bladon and Lindblom 1981); but to my knowledge there has not been a synthesis, yielding work on modelling normalisation in the auditory domain.

The necessity and urgency of such an approach, together with its obvious closer juxtaposition to what actually happens in speech/communication, is what prompted the current type of normalization procedure. Its structure will be described in Section 5.7.

5.6. How all normalization procedures cannot be applied to all data

Before amplifying the rationale for the adoption of an

auditorily-based normalization algorithm (Section 5.7), a few further remarks should be addressed to Disner's (1980) admirable survey. These will indicate the difficulties involved in both producing a universally satisfactory normalization algorithm. Obviously, duplication should be avoided, so this attention will be brief.

In order to assess previous proposals for normalization procedures, Disner selected the largely monophthongal vowels of five Germanic languages (the formant values for F1 and F2 for male speakers having been published) and applied several normalization procedures to them. For various reasons several other normalization procedures could not be applied to her data because of lack of available information (e.g. bandwidths or formant amplitudes). The techniques she does test are those of Gerstman, Harshman, Lobanov and Nearey. Full bibliographic references and detailed discussion of their procedure is available from her article. As will be recalled from Section 5.4, the heuristic for evaluation (on data from one language) was in overall scatter reduction, i.e. the minimization of the size of ellipses.

The overall result of Disner's testing was that, on the whole, a log-mean normalization procedure (as expounded by Nearey) is the most successful in reducing the scatter within each language. This is shown in Table 5.II, (reproduced from Disner, 1980: 256).

Table 5.II. Percentage of the scatter area remaining after normalization. Various normalization procedures applied to the data are listed along the ordinate (from Disner, 1980).

	German	Dutch	Norwegian	Swedish	Danish	English
Raw data	100%	100%	100%	100%	100%	100%
Point normalization (Gerstman)	77%	67%		100%	89%	110%
Partial PARAFAC (Harshman)	68%	67%	79%	65%	76%	75%
Mean-standard deviation (Lobanov)	32%	60%		59%	46%	61%
Log-mean (Nearey)	29%	30%		76%	39%	35%

It is important to note, that no one normalization is clearly the most effective for all languages and it may be that different data samples might render an alternate ranking. More important to remember is that scatter-reduction should not be the only criterion in assessing the success of such procedures. Preservation of linguistic information is also paramount.

With this second criterion in mind, most of the normalizations are found to be unimpressive. In fact Disner concludes (1980: 257) that,

...in numerous instances they obscure or even reverse the linguistic trends, which, in fact, are present in the raw data.

So it becomes obvious that different normalization procedures, if based on mean formant values, are required for different languages. Disner's next logical move, then,

is to examine contestants for cross-linguistic comparisons. Her conclusion is that a PARAFAC-based normalization (as first used by Harshman) is the most successful of the four normalizations considered for cross-language or cross-dialect comparisons. Its overall advantage is that it avoids unsupported assumptions about vowel quality across languages or dialects, which can definitely arise if mean formant values are considered as homogeneous.

Extensive coverage has been given to a survey in these sections of previous normalization techniques, using both an articulatory base and an acoustic one, and either a uniform or a non-uniform approach. The shortcomings of several algorithms have been enumerated, such that a different starting-point may be required. In the final section of this chapter, that new approach is characterized.

5.7. An auditorily-based normalization procedure

The reasons for approaching normalization from an auditory standpoint were two-fold. Firstly, as has been demonstrated, there appears to be much frustration with formant-based acoustic attempts because of the insecurity of both formant-cavity affiliations and the lack of transferability of mean values across languages.

The normalization process used for this thesis takes a very different point of departure from those catalogued above. It is based on the belief that the key to a solution to the normalization issue lies in the realization that **the human**

brain does not analyse like a sound spectrograph. We therefore need to simulate the processes of human auditory analysis in order to arrive at a clearer understanding of how a given sound is presented to the perceptual system. This, then is the second reason for adopting an auditory normalization procedure: it is firmly couched in psycho-acoustic reality.

5.7.1. An auditory model

For this investigation, I made use of a simple auditory model for steady-state sounds, consisting of a series of 'auditory transformations'. The model draws on knowledge garnered from both psychacoustic experiments and electrophysiological investigations into the response patterns of the auditory system. Several of the components of the model are relevant to the decision to adopt an auditory normalization algorithm. The transformations of the model were partly founded on findings by Schroeder et al. (1979), and are quite well validated (Bladon, 1981; Bladon and Lindblom, 1981; Bladon, 1985). Since it has been described in detail elsewhere (Bladon, 1985), its precise specification need not detain us here. Stages of the model's transformations are shown in Figure 5.5.

Figure 5.5. uses the vowel [y] as an example. In box (1) appears the acoustic power spectrum of the steady-state vowel, with $F_0=120$ Hz. The first transformation (box 2) is to convert the vowel's frequencies to the Bark scale: we

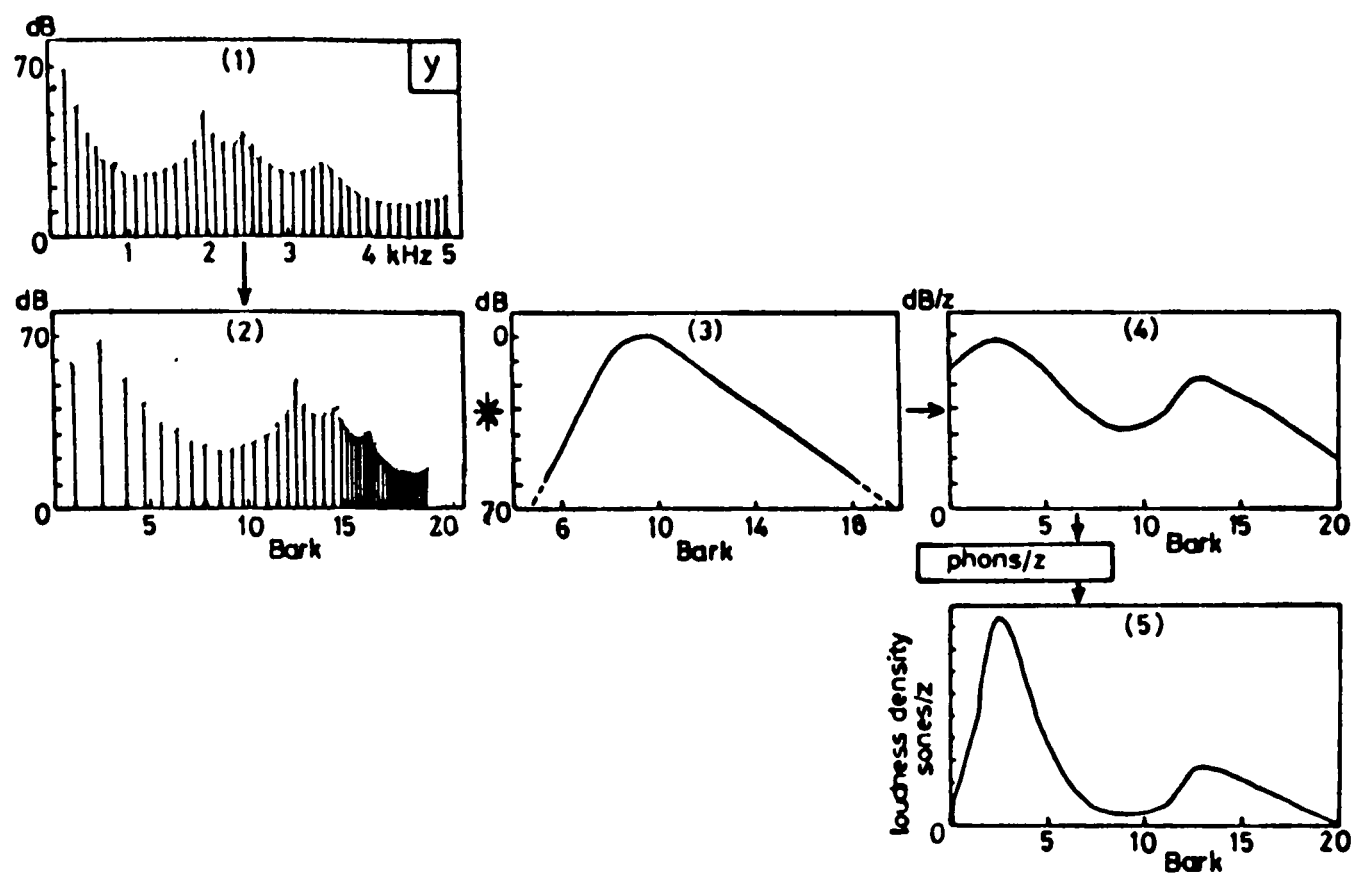


Figure 5.5 Steps in the acoustic-to-auditory transformation of a vowel (from Bladon and Lindblom, 1981). For explanation of each step, see text.

will discuss this scale further in Section 5.7.2. below. The third stage in the auditory model is the application of **masking** (whereby a component in the frequency spectrum will mask an adjacent component, particularly one higher in frequency). This masking (box 3) is achieved by means of a frequency-smearing function, which was derived by Schroeder et al. Box 4 illustrates the output of the masking filter - the highest formants (F3 and F4 in this [y] vowel) have virtually disappeared beneath the **masking skirt** of F2. The final steps in the model consist of a transformation of the intensity level scale in dB units to a perceptual scale of loudness, achieved via curves of equal loudness level (phons) to a scale of loudness density (sones/Bark). Box 5 shows the final result: this is how it is believed the original vowel [y] is represented as a pattern of excitation on the auditory nerve. Thus box 5 may be said to constitute a pseudo-auditory spectrum, i.e. how the vowel is heard by the ear and analysed by the brain.

5.7.2. The Bark scale

Experiments in auditory perception have clearly established the notion of **critical-band** as a property of the auditory analysis system (Fletcher, 1940; Zwicker and Feldtkeller, 1967; Zwicker and Terhardt, 1980), within which the loudness components of an incoming stimulus are integrated. The formant-like distribution of loudness across the frequencies, which is the familiar representation of vowel

quality, is likewise subject to auditory analysis in terms of critical bands. The critical bandwidth is not linear with respect to frequency, but could be reasonably approximated by one-quarter of an octave. The width of a critical band is defined as one Bark. The auditory range comprises some 24 Bark, of which 18 are important for speech. Figure 5.6 illustrates how the critical bandwidth is calibrated in Bark and varies with frequency (Hz). It may be seen how the Bark scale is close to a linear function of Hertz up to the 1000 Hz region (i.e. in the F1 region). Above that, it is close to a logarithmic function. It is important, moreover, that the critical-band concept, established by psychoacoustic experiment, has been found to have a measurable correlate in physiological terms, namely within the cochlea: the Bark scale is **proportional to distances along the basilar membrane** (1 Bark = 1.3 mm = 150 haircells).

An important influence on the rationale for adopting a Bark scale for this study was Potter and Steinberg's reasoning (which, in 1950, was well ahead of its time) that,

...within limits, a certain spatial pattern of stimulation along the basilar membrane may be identified as a given sound regardless of position along the membrane.

This must hold true from the displacement of the formant frequency pattern as observed in a sound (vowel) spoken by a woman, a man and a child. And this displacement is uniform on a scale of auditory critical bands.

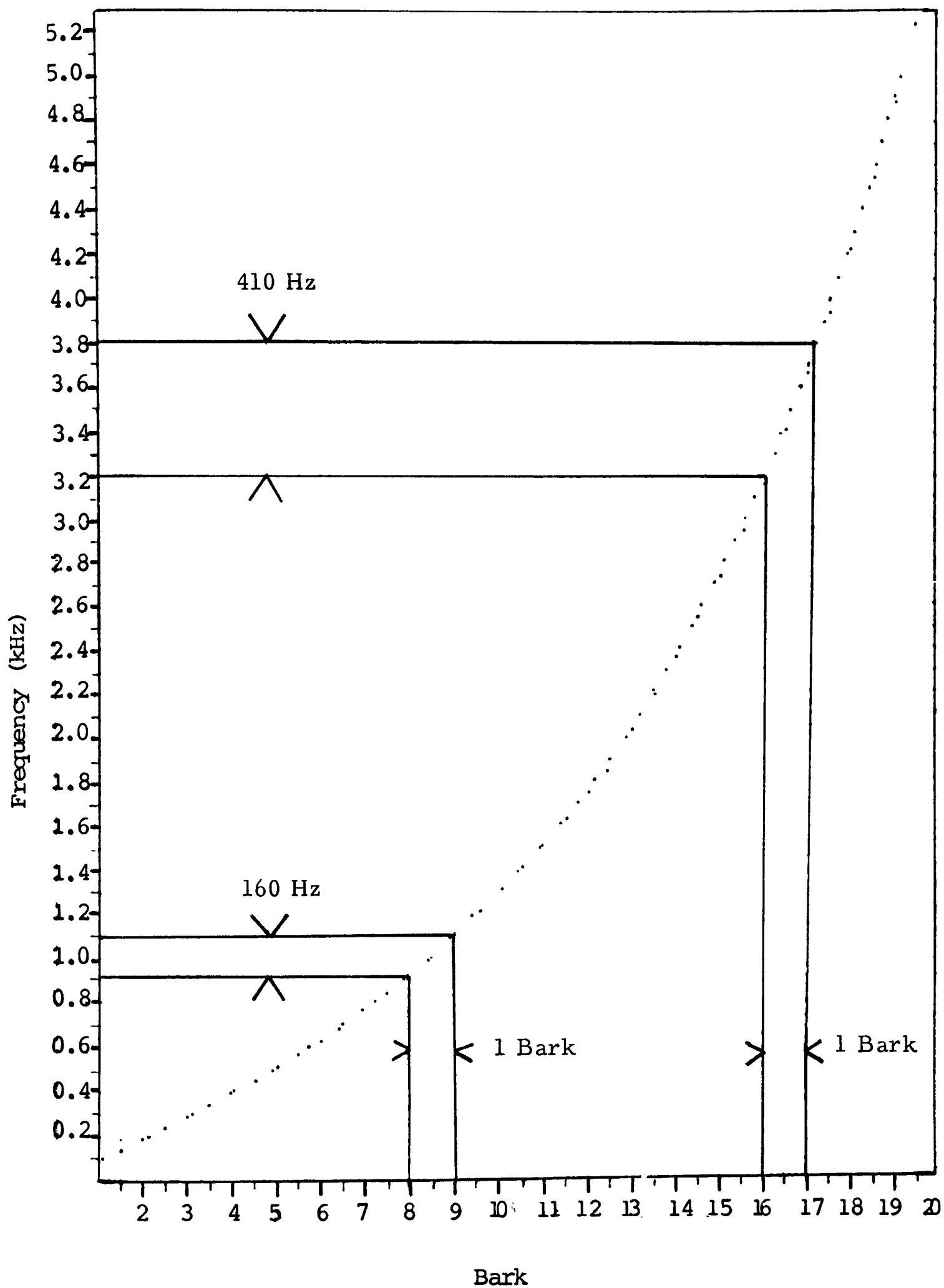


Figure 5.6 Calibration of the critical bandwidth in Bark, showing how it varies with frequency in Hertz.

The basic assumption in opting for a linear Bark scale in a normalization procedure is, then, that a certain pattern of excitation along the basilar membrane is recognized as a given feature regardless of position along the membrane. The significance of this auditory approach is underlined by Traunmüller (1981: 1465):

Only the features perceptible to the listener can play a distinctive role in the process of speech communication - articulatory, or even proprioceptive features cannot.

With these sources of support, the Bark scale seemed indeed well justified as the scale to use in pursuit of the answer to auditory speaker normalization.

5.7.3. The use of a 1 Bark displacement in auditory space as the key to female/male normalization

Using the Hertz to Bark conversion formulated by Zwicker and Terhardt (1980), it was found, in a preliminary study, that the scale factor for normalization of a vowel spoken by a female to that spoken by a male was, on average, 0.994 Bark, or, to all intents and purposes, 1 Bark. This figure receives good confirmation from the conversion of Fant's (1975) formant frequencies values for male and female speakers from Hertz to Bark. Recall also, that the adoption of Bark-scaling, importantly, irons out the non-uniformity which Fant found in his study (see Section 5.5. and Figure 5.3, above), with every formant appearing to be very close to one Bark. One Bark is also mentioned by Traunmüller (1981) to be an appropriate amount by which to bring male

and female values into alignment. Traunmüller's study demonstrates, via four experiments, that the Bark scale is well justified in approximating the frequency-to-place transformation which is performed in the cochlea.

Further justification for expecting a 1 Bark displacement derives from the typical difference between male and female voice pitches. Voice pitch is the most obvious indicator of speaker sex: since a component of the voice fundamental frequency is normally apparent in the spectrum, it may be relevant that a very typical average adult female/male F0 difference would be about 1 Bark, or about 100 Hz at conversational pitches.

Thus if a given vowel spoken by a female is reduced by 1 Bark (or, vice versa, a male vowel increased by 1 Bark), there should be correspondence to the same vowel spoken by a male. The essence, then, of the auditory normalization technique is that a simple displacement of a given auditory spectrum along the Bark scale would be a good approximation to what is needed for male/female normalization.

Notice that what has been outlined above constitutes, as stated, the main, essential ingredients of an auditory theory of speaker normalization. A further extension of such a theory is beyond the scope of this thesis, where the primary objective must be to subject the reasonably uncontentious core of auditory thinking to extensive, practical testing, to a very specific socio-phonetic end.

The reader may wish to be alerted, however, to several minor ways in which the normalization technique described and used here remains to be improved. One concerns the well-known interfering effect upon formant frequencies which is caused by a varying voice fundamental. In certain low-frequency regions of the spectrum, at certain voice pitches, this will mean that a simple linear Bark shifting must be modified somewhat (see Bladon, 1982, for some details). Other minor differences, which may prove to be sex-related and which are not modelled by my technique, include differences of spectral tilt and of certain characteristics of the voice source. Research results are still somewhat inconsistent on these points.

5.8. Incorporation of an auditory theory of normalization into a hypothesis concerning female/male phonetic differences

It has been argued that the use of an auditory normalization algorithm enables much of the incommensurability between female and male vowels to be eliminated. Once applied to the data, this technique should operate rather like a filter, separating out the non-biological differences between male and female vowels. We may then venture further, and hypothesize that any outstanding lack of parity can only be due to constraints outside the physiologically-based differences, that is, a **learned factor related to the sex of the speaker**. We may then ask the further experimental question: will this learned factor be the same

across different speech communities (accents/languages)?

The vowels of children are outside the scope of this present study. In successive work, though, both children and informants of varying ages (and more dialects) could be incorporated: children may possess the key but for the moment, it is women and men who can lead us to the door.

5.9. Conclusion

After evaluating various techniques previously offered as a solution to the normalization issue, dissatisfaction was expressed on several counts. Hence, an innovative auditory normalization theory was introduced, with a basis in psycho-acoustic reality rather than mutable physiology. The theory is validated by some preliminary trials on reported cross-language data, and by the normal difference between female and male voice pitches.

Considerable progress has been made towards testing the feasibility of the first of the hypotheses stated at the outset of this chapter. We now need to apply the findings which resulted in the support of that hypothesis to a testing of the second and third hypotheses. That is to say, by making use of the auditory theory of normalization, we will be able to determine whether there are indeed sex-specific acoustic differences which transcend the physiologically-predicated, and may vary from speech society to society. We test the theory first on two accents of British English, and then on a selection of different

languages. For the results of these empirical tests, we move to Chapter 6.

Chapter 6

Experimental results and discussion of two accents of British English

The last chapter laid the theoretical groundwork for testing various hypotheses. The central objective of this chapter is to investigate the hypothesis, suggested by pilot work (Henton, 1982; Bladon, Henton and Pickering, 1984) that speech differences between males and females (as exemplified by the quality of vowel sounds) may be greater in some accents or speech communities than in others. Indications were that two accents of British English, namely Received Pronunciation (RP) and Modified Northern, would indeed differ in this way.

If so, it would follow that some of those men and women are speaking more like (or, more unlike) each other than would be theoretically expected on physiological or auditory grounds. A learned, socially determined factor could therefore be implicated.

Firstly, we will explain why the two particular accentual-groups of speakers of British English were selected for testing the hypotheses.

6.1. Reasons for choosing RP and MN as the two accents for experimental investigation

The arguments for selecting speakers of Received Pronunciation are:

- (a) there is a plentiful supply of such speakers in Oxford
- (b) there have been suggestions that there is less inter-sex 'distance' between female and male speakers of this dialect (see Elyan, Smith, Giles and Bourhis (1978) - a

- study which will receive more attention in due course)
- (c) middle-class RP speakers are probably least 'affected' by education in a general sense, i.e. they are nearer to the perceived norm to start with.

Thus, if RP could be shown to reveal significant sex-specific differences, then there would be good reason to suppose that other accents/dialects would be likely to do so too.

The other accent, Modified Northern (of Leeds), was chosen for four reasons:

- (a) pilot work had indicated that there were sufficient numbers of speakers who fitted the criteria for MN already in Oxford, and that more speakers were available in Leeds at the time
- (b) it was possible to exactly replicate the techniques used in Oxford for subject-recruitment and recording conditions, in Leeds
- (c) the accent would appear to provide a sufficient contrast to the Southern-based phonology of RP to see if sex-differences were present to the same or differing extents in two accents
- (d) MN might reveal differential amounts of 'modification' by female and male speakers, and thus provide more experimental testing for arguments that women approach norms more readily than men (c.f. Chapter 3).

There was, however, a slight disadvantage in selecting these

two accents for investigation. RP is not a definable geographically-linked accent (see Gimson, 1984), and it is therefore impossible to keep this variable constant. The RP informants all, however, resided in the same area for at least 2 years. The nature of RP thus excludes the adoption of the commendable sociolinguistic 'network' technique described by Milroy (1980), because there is no **delimited community** with which RP can be linked. The same limitation also applies to MN because, as an accent, it is (or has been) by definition removed from its geographical origin.

6.2. 'Lab. speech': a brief defence

The unnaturalness in communicative terms of so-called laboratory speech has already been acknowledged (Bladon, Henton and Pickering, 1984: 63). Nevertheless, given the constraints of this thesis topic, it would have been over-ambitious to try to remedy all previous ailments of phonetic corpora simultaneously. So, in order to make comparisons with other studies possible, it was necessary to record speakers under the usual phonetic idealized and 'unnatural' conditions, as were catalogued in Chapter 4.

The prime data of interest were vowels produced as **citation tokens**, that is to say, tokens of the vowels pronounced in the minimally-interfering environment of /hVd/, where V is a monophthong in British English. Again, the reason for this selection was based on comparability with other phonetic enquiries. Recording /hVd/ tokens in a word list was

further founded on both Labov's (1972) and Trudgill's (1972) findings that this was the most formal speech style adopted. If speakers overcompensate, in a sex-specific manner, in their production of vowel sounds this phenomenon would most likely be revealed in a 'formal' performance. The environment used for the vowels was also based on the ease-of-analysis reasons given in Chapter 4, Section 12.3.

Apart from the evidence to be gained from these isolated, single word tokens, though, it was also interesting to explore whether the sex-differences predicted would be observable across a second type of speech context. For this reason, and because such data were readily available in the corpus recorded (see Appendix B, Exercise 1), **context tokens** of the vowels (i.e. occurring within a sentence context) were investigated.

The way in which the citation tokens were analysed for both accents is described in Section 6.3. The analysis of the context tokens is reported in Section 6.4. There then follow sections devoted to the **results** for (a) the citation forms (Sections 6.5 to 6.11), and (b) the sentence context forms (Section 6.12). Some preliminary findings of note concerning the fundamental frequency of the vowels are also presented, in Section 6.15. The main discussion of these British English results appears in Sections 6.13 - 6.16.

6.3. Analysis of recordings

One objective of my research, as mentioned above, was to

quantify possible vowel differences due to spoken context. Vowel tokens were therefore analysed both as citation forms and when appearing as unstressed in a sentence context. I commenced with the former, which amounted to 880 tokens for analysis (2 accents x 40 speakers x 11 vowels).

6.3.1. Narrow-band spectral sections

Narrow-band, frequency-by-amplitude sections were obtained using a Brüel and Kjaer Narrow Band Spectrum Analyzer Type 2031, selecting a frequency range of 10-5000 Hz. This was used in combination with an X-Y chart recorder (Brüel and Kjaer Type 2308) to obtain chart print-outs of each spectral analysis made.

The Full Scale Level was set at 100 dB or 110 dB and the Full Scale Frequency at 5000 Hz for all vowels. The Spectrum Range was 80 dB.

The spectral section of each vowel was taken, over an 80 ms interval which appeared to be the centre of the vowel, by looking for a steady state pattern in the time domain. This analysis employed a trigger which reacts by taking for analysis one record (i.e. 80 ms) after triggering is set. Triggering is calculated in units of length equal to one record, thus if the trigger is set at 1.4, a record beginning at $80 \times 1.4 = 112$ ms into the vowel will be analysed. By adjusting both the triggering level (so that triggering would not occur on the weaker [h]), and the

number of records taken after triggering, it was thus possible to arrive at a highly accurate representation of the steady state of a given vowel, in both time and frequency domains.

The frequency domain output, or spectral section, was plotted on the chart recorder. Figure 6.1 shows an example of an oscillographic record for the vowel [ɜ:] spoken by a female.

Frequency readings, obtained by moving the cursor on the NBSA screen, are accurate to within 12.5 Hz. By contrast, when measuring conventional spectrograms, the manual procedure introduces a large element of variability into the measurement, and it is in any case impossible to be more accurate than to within 25 Hz. Together with the general objections to the limitations of the spectrograph as an analysis tool for women's voices, raised in Chapter 4, Section 11, the temporal changes in vowels were not relevant for this study. Thus the other main facility of sound spectrographs (the dynamic information) was redundant for these experimental purposes. The overriding reasons for beginning by using an NBSA to measure vowel spectra in isolated tokens lie, then, in its superior frequency resolution, its greater accuracy and clarity, and its convenience and speed of use.

From plots obtained from the chart recorder, vowel formants were identified from the highest distinguishable harmonic

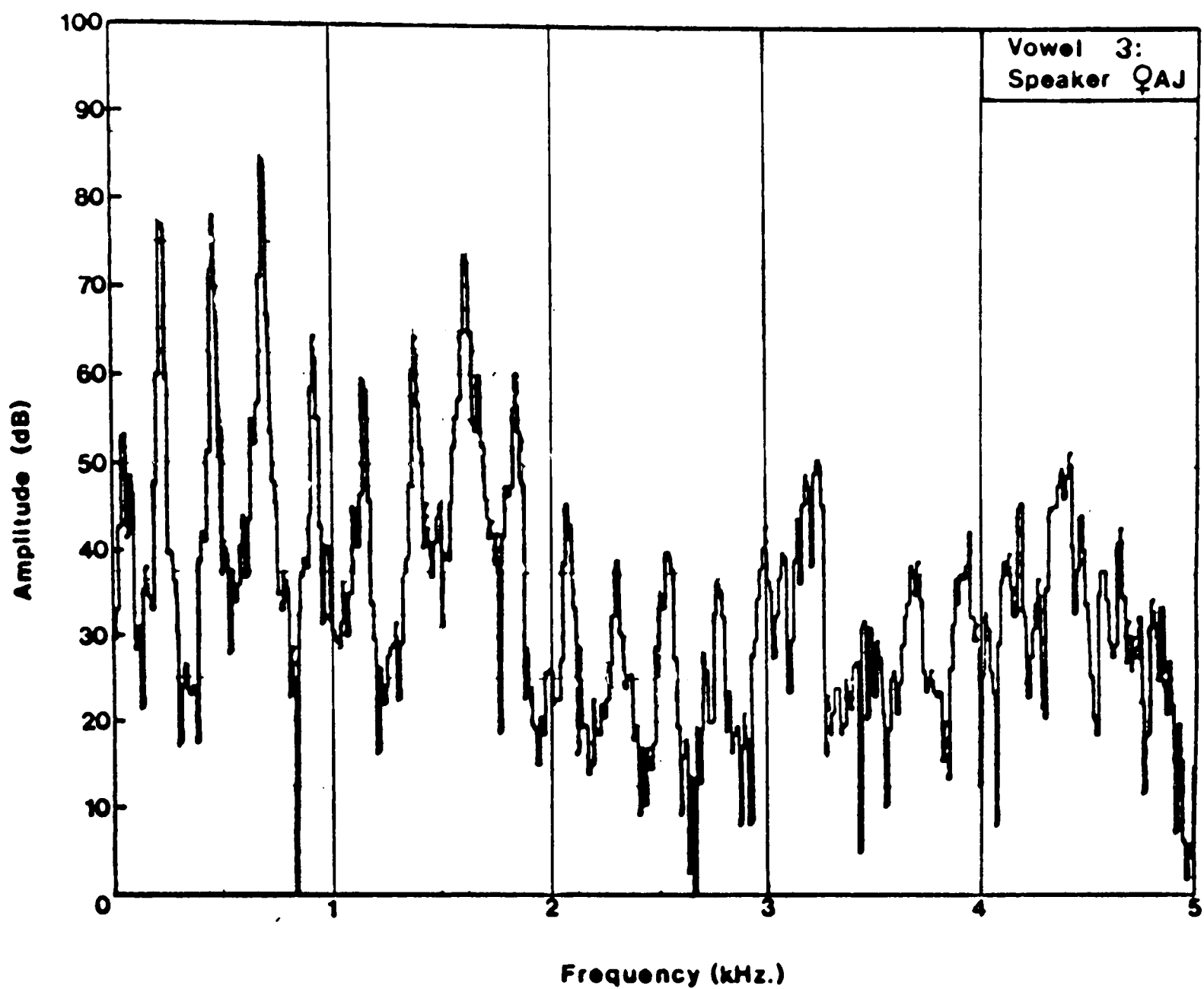


Figure 6.1
 Narrow band spectrum analyzer representation of an /3:/
 vowel, spoken by a female.
 Full scale level: 100dB; F.S. frequency: 5000Hz
 Speaker: AJ (female). Token 3/1. No: 1.0
 Accent: RP

peak, or (in a small number of cases) from spectrograms where peaks were difficult to determine. Readings were taken in Hertz for the formants F1 to F4 for all the vowels. The fundamental frequency (F0) was calculated by locating the tenth harmonic, or the strongest harmonic nearest to the tenth if that were not clearly distinguishable, and dividing by that number. This method provides a more accurate representation of F0 (one-tenth the error) than merely taking the frequency of the lowest harmonic.

6.3.2. Spectrographic analysis

The spectrograph was used as a secondary tool of analysis because, in the early stages of this work, no other instrument for analysis was available. For those vowels which did not exhibit clear formants from narrow band analysis, then, spectrograms were made. The spectrograms were produced from the same token on the tape recording as those used for the narrow band analysis, but now on a Kay Sona-Graph 7029A spectrograph with a frequency range of 5-16000 Hz. For male speakers the frequency range was 80-8000 Hz.

Readings of problematic formants were measured by locating the most intense point of energy in the formant 'bar' and measuring its frequency from the calibrating scale on the spectrogram. To reduce manual measuring inaccuracies arising from the variable position of the baseline (an intrinsic problem with this model of spectrograph), a computer program

was implemented to convert to frequency the measurements in mm of the formant itself, and of the distance from the baseline to the centres of the 1000 Hz and 7000 Hz calibrations.

6.3.3. Analysis by N.E.D. Computer

Even so, after two analytic 'passes' using two different analysis tools, some vowels remained particularly intractable with regard to the measurement of the lowest formants. The final (more recently available) stage of analysis for these vowels, then, was to determine their formants on the New England Digital 16-bit computer, using the Signal File Manager speech analysis program.

The N.E.D. digitizes speech at rates up to 50 kHz and produces spectral sections using an FFT routine. It provides easier verification of the point of analysis on the time waveform. Using a 50 kHz sampling rate, an FFT of 4000 points, and a window length of 80 ms (for the citation forms, at least), the analysis conditions of the NBSA was replicated, achieving the same frequency resolution of 12.5 Hz. An example of a print-out obtained from the N.E.D., via an Epson MX-80F/T III dot-matrix printer, is shown in Figure 6.2. Readings obtained by moving the cursor on the screen were transferred to data charts and later to storage in computer files.

The vowels which proved most troublesome in terms of analysis were the close front and close back vowels (viz.

Spectral Display

Extracted data

File: TDHEAD
Time: 0.141 580 Sec.
Window: 0.080 000 Sec.
Hanning

Frequency	5017.0890 (7.0314)		
Norm.	db	Density	Magnitude
1.0	-73.10	4.8994 E-8	2.2134 E-4
1.0000 E+0	-73.10	4.8994 E-8	2.2134 E-4

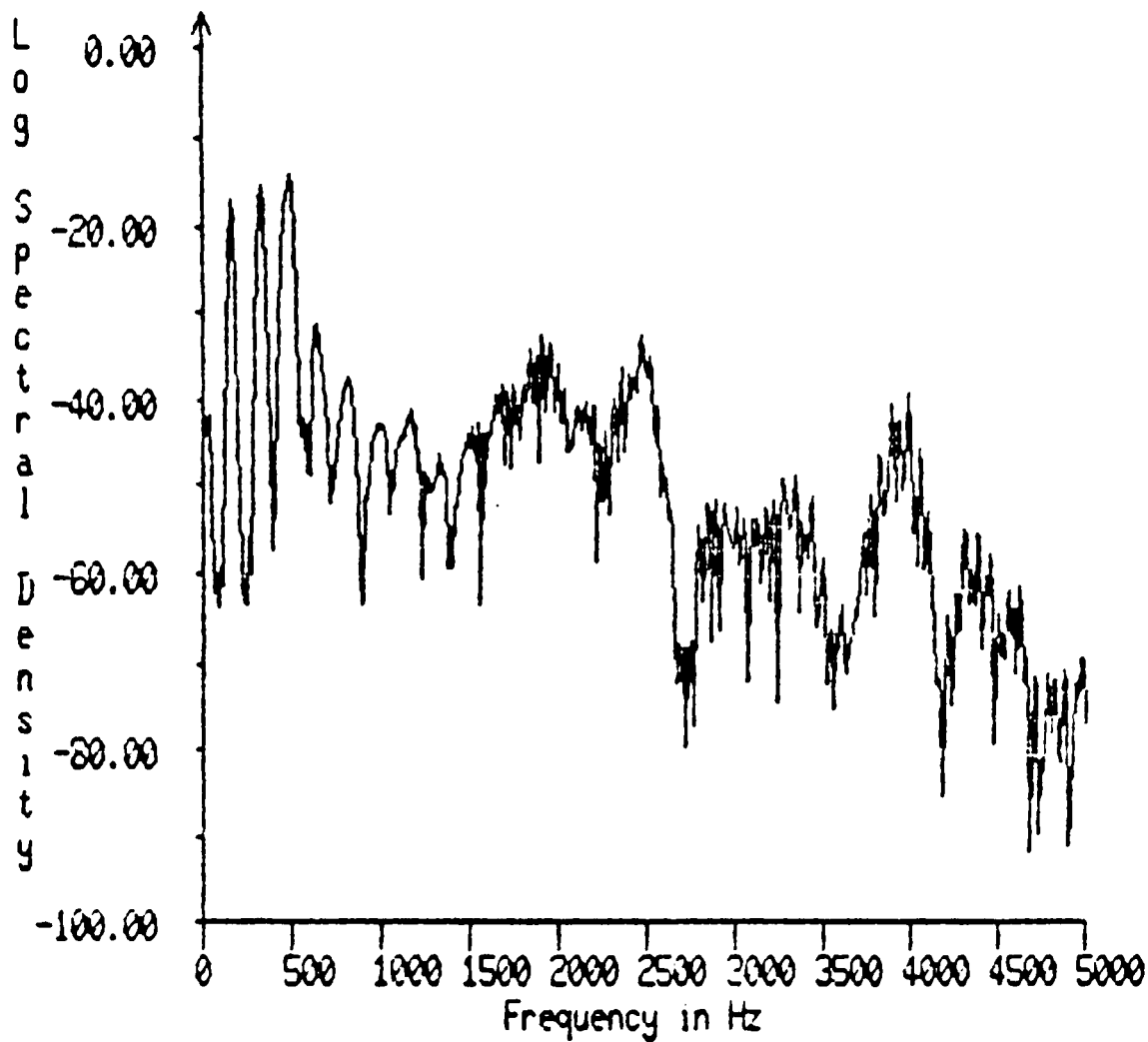


Figure 6.2 Spectrum produced by the NED computer (using the Signal File Manager speech analysis system) of a male [e] vowel.

Range: 0-5000 Hz; Window length: 0.08 sec.
Speaker: TD. Token 3/1.
Accent: RP

/i:, ɪ , u:, ʊ /), together with a few tokens of various other vowels. Vowels spoken by the female speakers of both dialects were particularly resistant. For the close vowels the best way to resolve this problem seemed to be to give priority to ensuring a systematicity of measurement and so to proceed algorithmically. In brief, and commencing with the vowel /ɪ/, this comprised the following:

- (i) it was verified that the spectra obtained from the NBSA were exactly accurate, as shown on the N.E.D.
- (ii) the close vowels have two (sometimes three) very strong harmonics in the low frequency part of the spectrum, i.e. where one would expect to locate F1 (see Figure 6.3, Zone A), suggesting that the auditorily-reconstructed envelope would have a peak somewhere between them: see Figure 6.3, Point B.
- (iii) rather than estimate that peak, in some ad hoc fashion, it was noted that on a large sample, the strongest harmonic was very near to being equally distributed between the F0 component (H1) and the second harmonic (H2). Therefore, it could be presumed that a reasonable average value would result from measuring the actual physical spectrum peak, as represented by the strongest harmonic.
- (iv) the logic of step (iii) is not so persuasive, however, because (a) a huge statistical spread results; (b) the method is quite inappropriate for /i:/; (c) the first harmonic in vowels spoken by female subjects is frequently unusually high in amplitude due to breathy

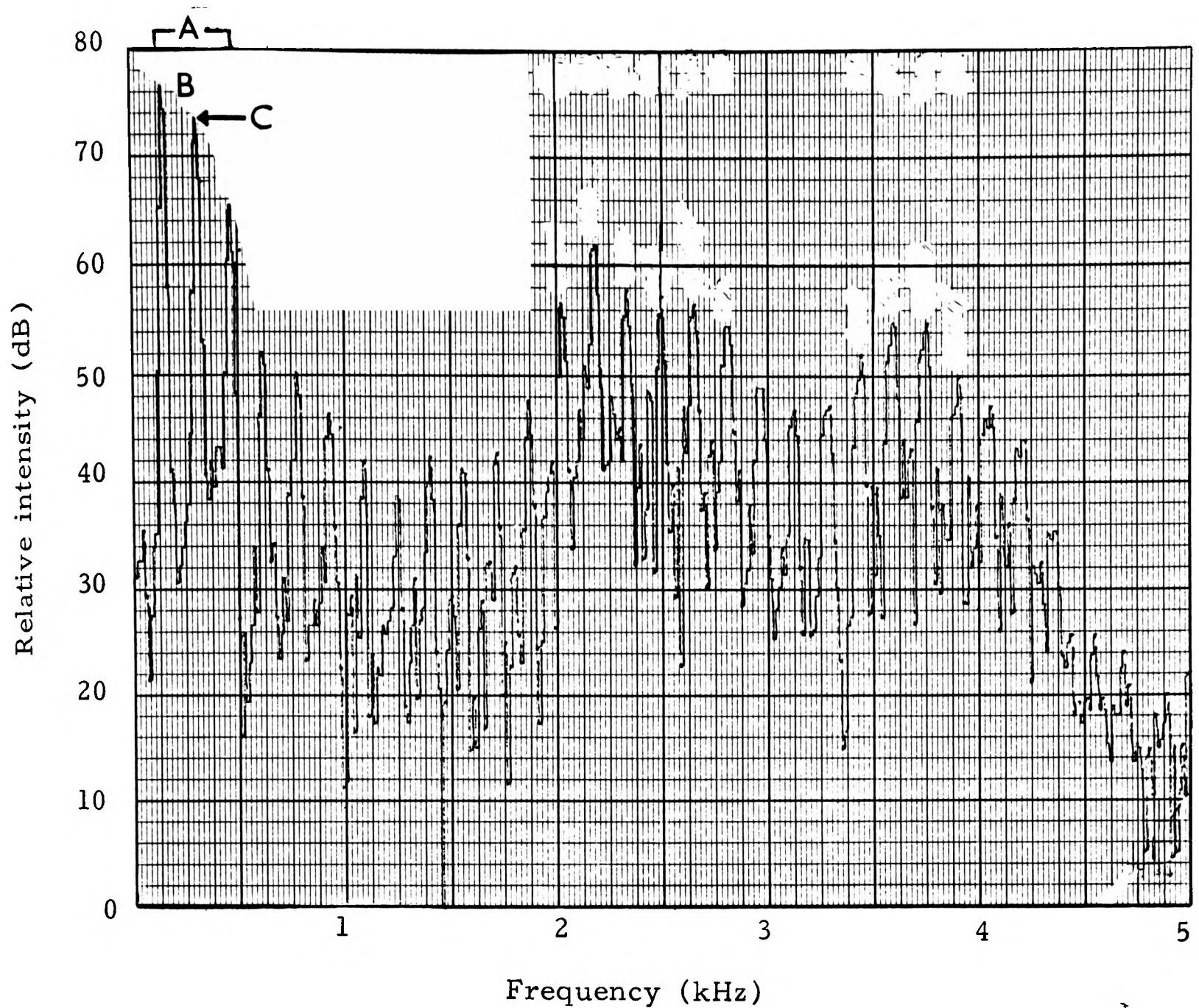


Figure 6.3 Representation obtained from NED computer of an [i] vowel, illustrating difficulties of formant measurements. N.b. the number of harmonics in the low frequency region. For explanation of A, B and C see text.

Full scale level: 100 dB;

F.S. frequency: 5000 Hz.

Speaker: CB (RP male).

voice quality (see Henton and Bladon, 1985); (d) previous reported values for F1 of /ɪ/ and /i:/ indicate a systematically higher frequency F1 for /ɪ/. This would not have been replicated had the 'actual physical peak' method of measurement been employed, because too many values would have been obtained from F0 peaks, thus artificially lowering the expected frequency of F1 for /ɪ/.

- (v) so the decision was taken to systematically measure F1 of the four high vowels from the highest harmonic which was not F0: see Figure 6.3, Point C. This solution, while certainly the most reasonable way out of a universally-acknowledged problem, was unfortunately still not completely free of measurement artefacts (as Sections 6.6, 6.7 and 6.9 note further).

6.4. Analysis of non-nuclear, sentence context tokens

The second type of data in which there was analytical interest were vowels not spoken with nuclear stress and excerpted from running speech (Appendix A, Exercise 1). These were the so-called **context tokens**.

To use the Narrow Band Spectrum Analyser for the purpose of analysing such sentence context tokens would have been inappropriate and probably impossible. It would have necessitated some laborious pre-editing of the signal so as to excise the relevant 80 ms stretches of speech. More importantly, in fluent speech the vowel could not be assumed

to be stationary (as the NBSA demands) for 80 ms.

However, for this particular research purpose, a much more accurate analytical tool had become available. This was the previously-mentioned N.E.D. system. Using the procedure described in Section 6.3.3, except for a redefined window length of 40 ms, tokens produced in the sentence context were analysed.

The results of the analysis of the 880 citation forms and the 320 sentence context forms are presented in Sections 6.5 to 6.12.

6.5. General data presentation: ellipses versus points, or individuals versus means

This discussion will concentrate on the sex-specific differences which emerge from a consideration of the first two formants (F1 and F2) of the eleven monophthongal vowels (see Chapter 5, Section 4 for rationale).

It was decided that the presentation of results should be the same as that used for conventional articulatory-based vowel plots: they thus resemble those of other studies (c.f. Figure 5.2). That is to say, individual tokens were plotted, with values in Bark, with F1 on the y-axis and F2 along the x-axis: see Figures 6.4 to 6.9.

In addition, a table is presented of the standard deviation values which were used to draw the ellipses; this is Table 6.I (which appears following Figure 6.9). In order to

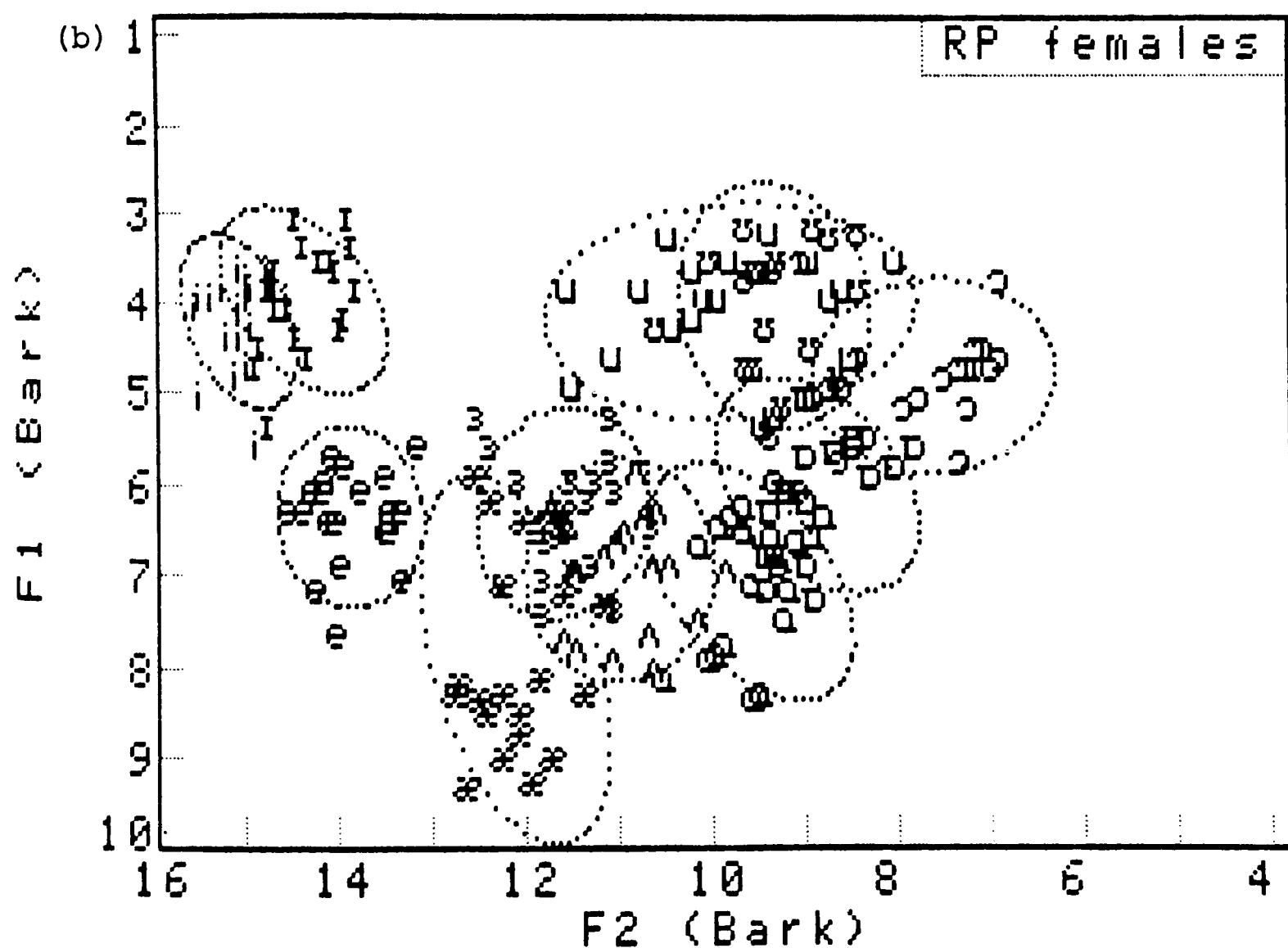
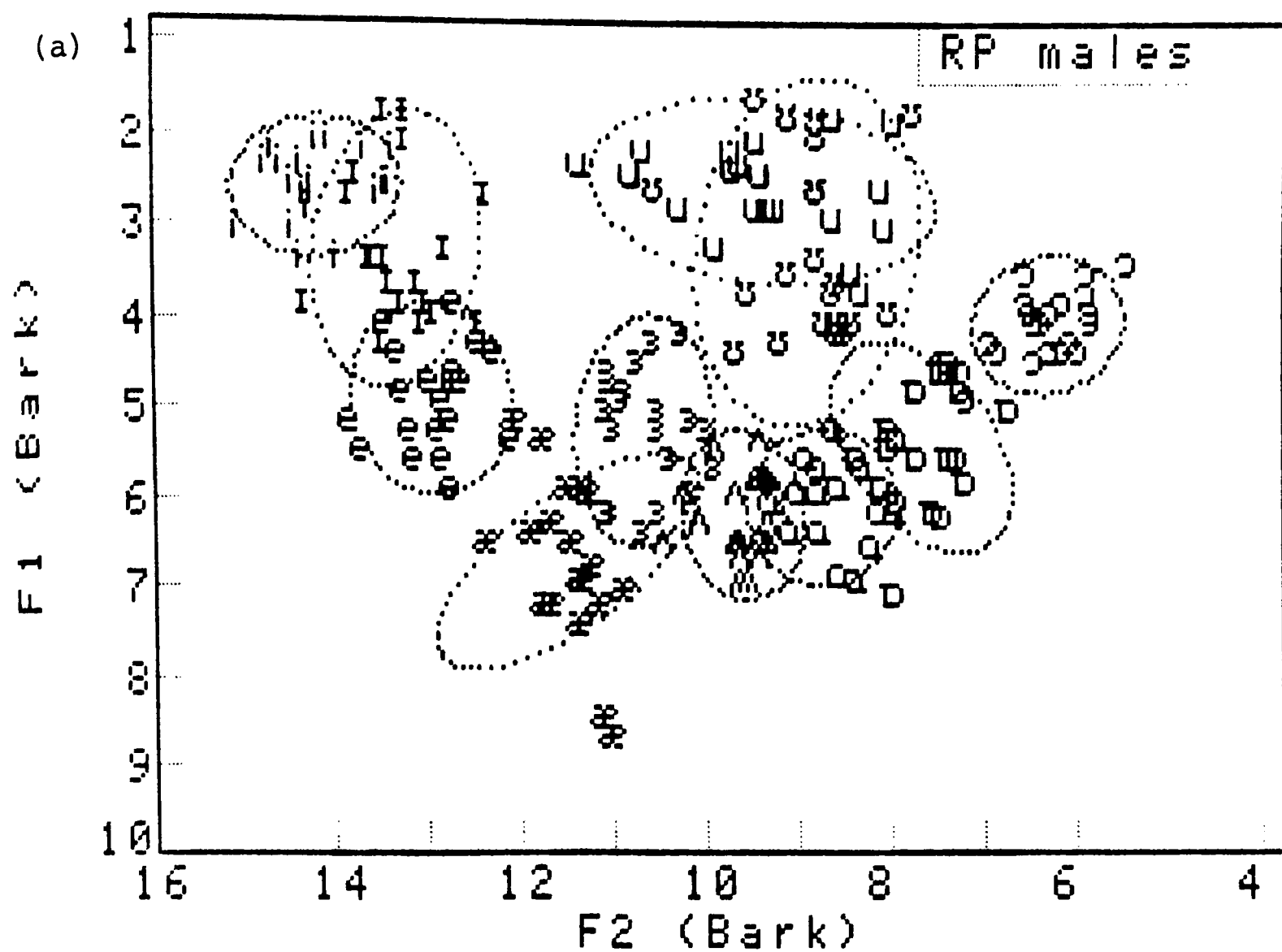


Figure 6.4 Individual (unnormalized) values (Bark) for eleven vowels spoken by (a) twenty male RP speakers, and (b) twenty female RP speakers. Ellipses drawn to 2 SDs.

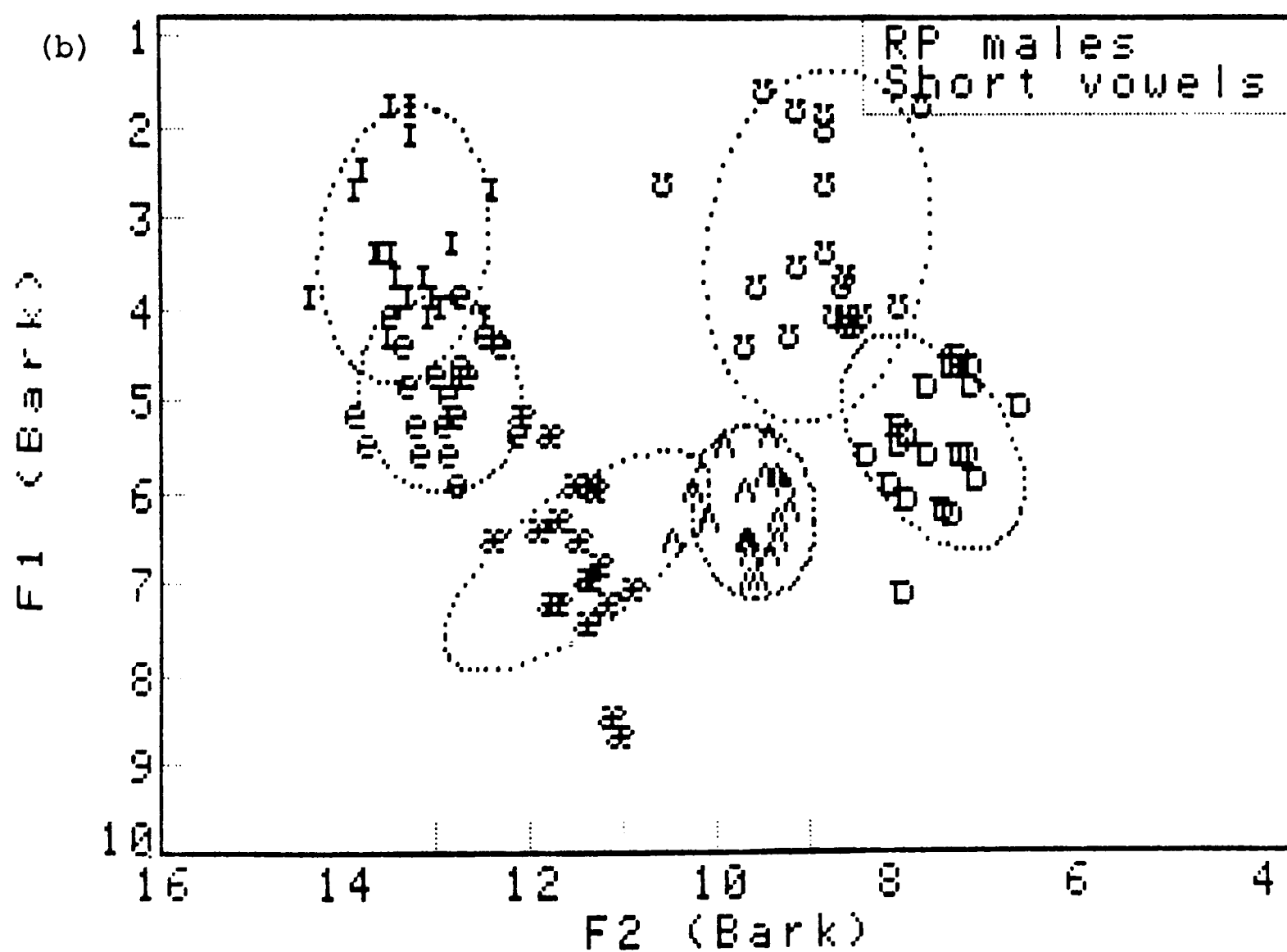
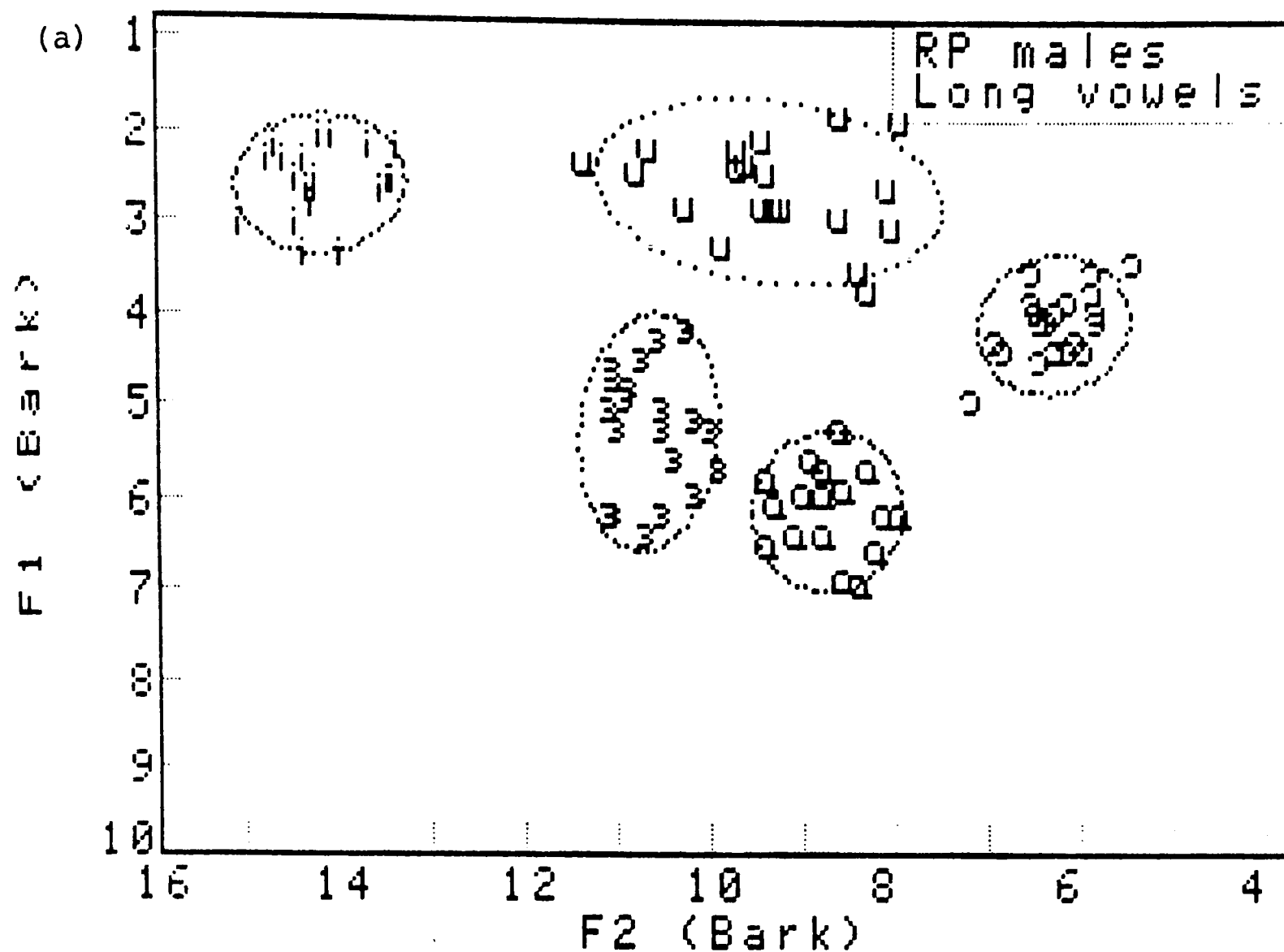


Figure 6.6 RP male speakers' (a) long vowels, and (b) short vowels, plotted in Bark, and with ellipses drawn to 2 SDs.

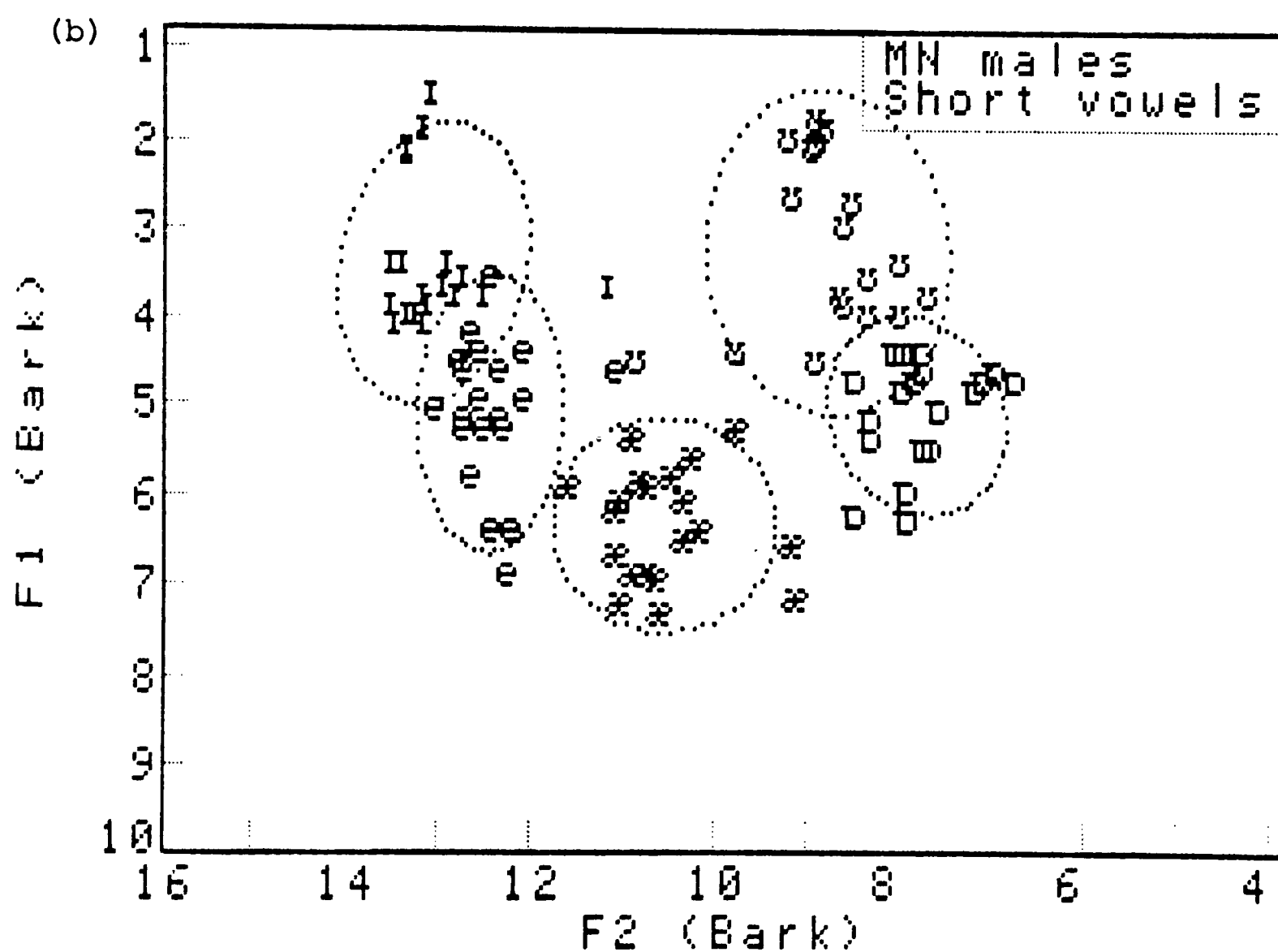
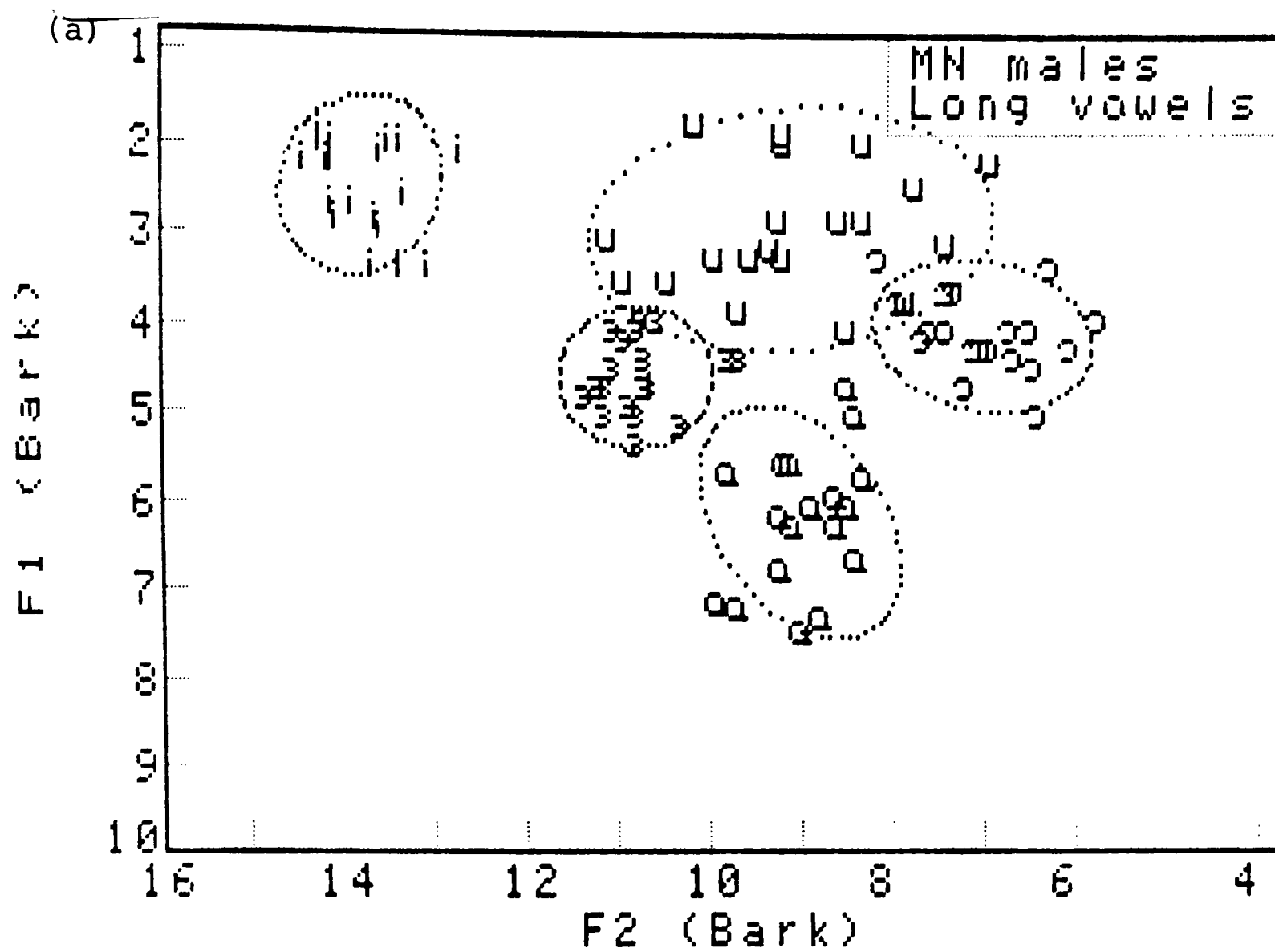


Figure 6.8 MN male speakers' (a) long vowels, and (b) short vowels, plotted in Bark, and with ellipses drawn to 2 SDs.

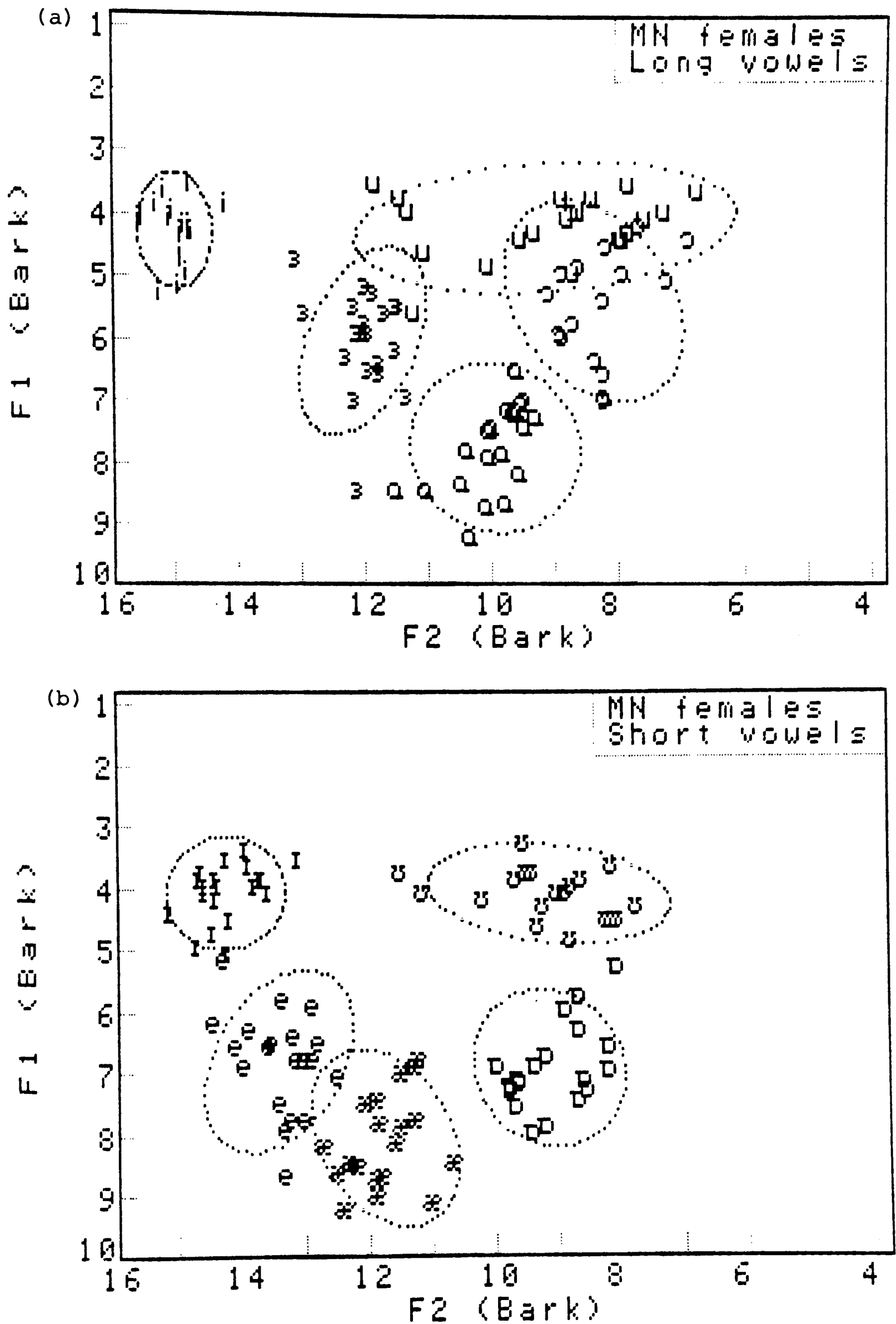


Figure 6.9 MN female speakers' (a) long vowels, and (b) short vowels, plotted in Bark, and with ellipses drawn to 2 SDs.

Table 6.I. Standard deviation values (Bark) for F1 and F2 of eleven vowels spoken by twenty males and twenty females of RP and MN.

		i:	ɪ	e	æ	ɑ:	ɒ	ɔ:	ʊ	u:	ɜ:	ʌ
RP male	F1	.38	.76	.53	.86	.44	.65	.38	.97	.51	.65	.47
	F2	.46	.45	.44	.36	.4	.4	.42	.6	.94	.37	.33
fem	F1	.51	.58	.5	1.04	.74	.68	.54	.69	.61	.59	.61
	F2	.28	.36	.39	.44	.43	.45	.66	.51	1.00	.46	.51
MN male	F1	.51	.81	.79	.6	.72	.57	.42	.92	.68	.4	.81
	F2	.43	.5	.4	.61	.46	.47	.61	.66	1.11	.42	.66
fem	F1	.46	.46	.8	.76	.7	.66	.88	.39	.52	.8	1.16
	F2	.29	.47	.5	.52	.66	.59	.62	.97	1.51	.43	1.02

Table 6.II. Variability of vowels by accent and sex, as expressed by the coefficient of variation of Bark values of F2 (20 speakers per cell).

		i:	ɪ	e	æ	ɑ:	ɒ	ɔ:	ʊ	u:	ɜ:	ʌ	Mean
RP	m	3.24	3.37	3.39	3.13	4.57	5.21	6.66	6.73	10.01	3.46	3.43	4.84
	f	1.86	2.5	2.81	3.62	4.47	5.07	8.66	5.47	10.2	3.96	4.63	4.84
MN	m	3.11	3.83	3.21	5.78	5.11	6.02	8.7	7.52	12.18	3.89	6.88	6.02
	f	1.93	3.3	3.72	4.41	6.63	6.43	7.39	10.61	16.5	3.57	9.63	6.74

discuss **variability** among the individual vowels, however, it is further necessary to express the standard deviations as a percentage of their means: i.e. as a coefficient of variation. This is tabulated as Table 6.II.

Figures 6.4 and 6.5 illustrate individual (unnormalized) values for the eleven vowels spoken by (a) twenty RP male and twenty RP female speakers and (b) twenty MN male and twenty MN female speakers. A cursory look at Figures 6.4. and 6.5 demonstrates three important facts:

- 6.5.1. There appears to be a large amount of overlap between adjacent vowels, in both accents and for both sexes.
- 6.5.2. It is difficult, using this form of presentation, to see the wood for the trees.
- 6.5.3. In either accent, there is a lack of similarity in the overall patterns for the two sexes. In particular, the female vowels are all situated markedly further away from the origin of the graph, i.e. further towards the bottom left-hand corner.

Explanation, and remedy, for the first and second points is easy to provide. The apparent gross overlapping (6.5.1) is partly caused by the distinctiveness of **duration** (length) in British English vowels. Thus while [i:] and [ɪ] or [ʌ] and [ɑ:] may seem to share some phonetic 'space', they will, in fact, be kept separate by their differing durations, as well

as by their quality differences. Further causes for overlapping are (a) the whole quality of a vowel is not captured by its F1-F2 frequency (upper formants are also influential), and (b) an artefact of plotting twenty speakers' vowels on one graph. Hence, the RP male [ɪ] ellipse may encompass some [e] vowels, but those [e] vowels with a low F1 are in fact spoken by different speakers than those speakers who produce [ɪ] with a comparatively high F1 for [ɪ]. It follows, naturally, that no one individual will have [ɪ] and [e] in the same area (since they are not distinguished by length), for otherwise the contrast would be threatened.

To arrive at a practical solution to visual 'friendliness' (6.5.2) in the graphs is simple. Figures 6.6 to 6.9 proceed with the long and short vowels for female and male speakers of RP and MN on separate graphs. It is on these graphs that the following discussions of the particularities of the vowels is focussed.

Before turning to a detailed consideration of the vowels per se, some general remarks about the appropriateness of presenting these particular data as individual tokens, bounded by ellipses should be made. The ellipses in Figures 6.4 to 6.10 are drawn to two standard deviation radii, as used by Labov et al. (1972) and further described by Broad and Wakita (1975). To begin with, the **size** of the sets of **ellipses** drawn to two standard deviations should be mentioned. With a larger population for each sex or accent,

it would be feasible to exclude those values obtained which were obviously 'outliers', e.g. the RP male tokens of [æ], with values of approximately 8.5 and 8.7 and 11 and 11.2 Bark for F1 and F2 respectively; or the MN female token of [e] with values of 8.7 and 11.2 Bark for F1 and F2 (see Figures 6.3 and 6.4). With the still relatively modest populations however, it appeared unjustified to conclude that any data point was an atypical occurrence and they were hence all included.

Secondly, the presentational problems inherent in the plotting of values for individual tokens were not actually detrimental to the research hypotheses. I was interested, it will be recalled, to discover whether there are consistent inter-sex differences between accents/languages and not between individual male and female speakers of those accents/languages. Therefore, a comparison of male-female average differences was what was ultimately required rather than an examination of the individuals' vowel ellipses.

The first issue to be discussed here, then, is the contribution of information shown by the elliptical plots, which is pertinent to the general theory here.

6.6. The RP male ellipses (Figures 6.4 and 6.6)

When examined from the perspective of their individual size and that relative to each other, the RP male ellipses display a gratifying homogeneity of location and distribution. In particular, the loops are especially

compact for the vowels [i:, e, a:, ɒ, ɔ:, ɜ:] and [ʌ].

The **overlap** of [i:] and [ɪ] can be attributed to two factors. Firstly, the two vowels are becoming more alike in terms of quality because [i:] is becoming more open and liable to diphthongization (Wells, 1982). Similarly, the height and degree of centralization of [ɪ] and [e] may vary. As Wells (1982: 291) says:

Relatively close and peripheral qualities are associated particularly, though not exclusively, with old-fashioned RP; relatively open and central qualities are common with younger speakers.

Since Wells never defines what he means by 'younger' and 'older' speakers, it is difficult to place the speakers of my own data on a comparability scale with his. However, it would seem that in this corpus, the speakers (who are not really young) do exhibit the behaviour remarked on, and consequently lessen the qualitative difference between [i:] and [ɪ], and to some extent, [e], too.

A third reason for the lack of distinction between the [i:] and [ɪ] loops (and this is most noticeable in the females' vowels in both accents - see Figures 6.7 and 6.9) derives from the acoustic measurement problems described in Section 6.3.3. This measurement artefact is also responsible for some of the apparent overlap between the close back vowels, [u:] and [ʊ].

The close vowels are generally kept apart nevertheless because of the distinctiveness of length in addition to that of quality in RP English vowels, as already mentioned.

Concerning the apparently considerable coalescence of [u:] and [ʊ], though, there is an added fact to take into account. [u:] and [ʊ] in British English are in the process of unrounding and/or centralizing (see Wells, 1962 and Henton, 1983: 365), and are thus rather variable vowels. The beginning of this trend was observed in 1962 by Wells (p. 25):

One or two occurrences of /ʊ/, too, were even more centralized than one would expect, approaching the position of /ə/.

He proceeds to comment generally that some speakers appear to maintain conservative, back vowels (of Cardinal Vowel 8 and lax CV 8 qualities), while many are in the vanguard and produce vowels which resemble [ʌ] and [ə]. In 1985, as is borne out by Figures 6.4 and 6.6, what was a minority articulatory effect in 1962 has become much more generalized. There is a good and simple phonological reason for the fronting of these two vowels, as Gimson (1980) describes:

The absence in English of any opposition between /u:/ and a vowel of the front, close, rounded type /y/, is an important reason for the relaxation and fronting of this phoneme from a true back position.

Phonological 'space' is plentiful here, whereas in a more open position, the close back pair would encounter /ɔ:/, which is becoming more close (Wells, 1982: 293; Henton, 1983).

In addition Wells (1982: 294) has this to say:

In mainstream RP ([u:]) is often somewhat centralized,

even to the extent that there is no perceptible difference between the allophone used in the environment /j_/ and the phonemic norm.

In view of the experimental evidence presented here, it seems that the description of the "phonemic norm" may need to change.

The crowding of the central and back vowels is also predictable because of the overpopulation of that space in English (compared with the relative phonological paucity of front vowels). The overlap between [æ] and [ʌ] has caught Wells' attention (1982: 291-292):

...the newly current [a] is perceptually very similar to the fronted realizations of /ʌ/ which have been around in RP for rather longer...It may even be the case for some speakers that /æ/ and /ʌ/ are merged, variably at least.

With reference to the size of the [æ] loop, Broad and Wakita (1975: 24, my emphasis) provide some interesting observations:

It ... seems that vowel-to-vowel differences in variability are not preserved from speaker to speaker. Instead (these) limited results suggest an interesting hypothesis, namely, that there is some universal lower limit on the standard deviations for formant frequencies which for any vowel will be approached by some, but not all, speakers.

The present results can be interpreted in terms of this hypothesis to mean that **certain of the vowels studied**, (especially [ɪ], [ʊ], [y] and [ə] approach this limit on repeatability, while the more variable vowels (such as [æ] and schwa) do not.

Their findings, then, support those presented here. Furthermore, there is direct corroboration for the variability of this vowel [æ], again from Wells (1982: 289; 292), where he connects the lowering and centring of /æ/

with that of /r/ and /e/ mentioned above. This general movement is one which will carry RP further away from both American and southern-hemisphere accents of English (but, incidentally, nearer to other British accents).

6.7. The RP female ellipses (Figures 6.4 and 6.7)

The female loops appear on the whole to be larger than those of the males and to overlap more in the central and back areas. The crowding may be partly explained by the acoustic correlates of a shorter female vocal tract length (with a resultant raising of F2 in central and back vowels) and partly by the tendency of back vowels to centralize. In particular our data give good support to the centralizing of [u:] in RP (n.b. the female token with values of 5.03 Bk (534 Hz) for F1 and 11.53 Bk (1575 Hz) for F2).

Again, the large overlap between [u:] and [ʊ] and [i:] and [ɪ] may be attributed to the importance of length differentiation, as well as to measurement difficulties for these vowels, which were particularly persistent for female vowels. The algorithm employed for measuring [i:] and [ɪ] does not seem to have been entirely successful for the female speakers, as [ɪ] is represented as having a lower F1, on average, than [i:], see Figures 6.4 and 6.7.

The size of the [æ] loop has already attracted attention in the discussion of the RP males' ellipses above. Meanwhile, the overpopulation of the female vowels' central open area is much more striking than in the male representation. A

popular phonetic rumour, that females are 'allergic' to extremes of place of articulation is illustrated perhaps. However, this phenomenon is not unique to RP female speakers, as Figures 6.5 and 6.9 (for female MN speakers) illustrate. The rumour-explanation will also be shown to be just a rumour, and refutable by this British English and empirical evidence from five other languages, in a discussion in Chapter 7.

The female [ʌ] encompasses a larger area because of two speakers' allophones, one of which is very open, and the other more raised and back than usual. With these individual variations included in the data, then, a certain degree of expansion of the ellipse is predictable (see Table 6.I for SDs).

In general, the greater variability of the females' vowels may be attributed to two possible causes. Firstly, women may indeed be more unstable, variable and chameleon-like (c.f. Chapter 3, Section 3.2.1) in their phonetic behaviour than are males. Secondly, the spectra of females' vowels are populated by fewer harmonics because of the comparatively higher voice fundamental frequency. Harmonic peaks were used to measure formants in this experimental procedure, and thus a measurement inaccuracy would have greater consequence in terms of formant-plotting for female vowels than for males'.

6.8. The MN male ellipses (Figures 6.5 and 6.8)

Compared in gross terms with the ellipses for the RP males (Figure 6.6), these for the MN males show both greater individual size and greater overlapping between vowels. This picture is entirely predictable, however, when we pause to remember that these speakers have a modified accent: they speak neither mainstream Leeds (or "middle northern" as Wells, 1982, calls it) nor mainstream RP. Some speakers will have modified more than others, who remain more resistant to pressure to change their accent, according to accommodation theory (see Chapter 3.2.1.). Consequently, the standard deviations of the MN vowels are relatively high (compared with the largely stable vowels of RP), causing the ellipses in turn to be enlarged: see Table 6.I.

The overlap between the males' [i:] and [ɪ] vowels in the two accents is remarkably similar. This is indication, perhaps, of the constancy of the perceptual qualities of these close front vowels, and of their homogeneity across accents. (The extraordinary [ɪ] outlier, with Bark values of 3.7 and 11.2 for F1 and F2 is a variant produced by a speaker who also has a very low F2 for [i:], and so appears to maintain a consistent open oral posture for close front vowels.)

MN males' [e], on the other hand, is both more variable and more open than the comparable vowel in RP. This can be assumed to arise because, originally, speakers from Leeds

have a more open [ɛ] vowel in environments where RP has [e] (Wells, 1982: 364).

Despite the occurrence of [a] in the vowel system of Leeds (Wells, *ibid.*), it seems that MN males have not been content to use their original vowel in the modified version of their accent. RP [æ] may be becoming more like [a] - see Section 6.6 - but this variety is rejected by MN males. Instead, they elect to use a more close, centralized [ɘ], which then has very little phonetic space before it encounters [ɑ:]. The explanation lies again in a durational contrast rather than one of quality. According to Wells (*ibid.*), the Leeds accent has no [ɑ:] vowel as such, and contrasts trap and start with [a] and [ɑ:] respectively. Wells states (1982: 360):

The vowel of PALM and START varies from a front [a:] to a back [ɑ:]. The front variety may be identical in quality with the /a/ of TRAP, differing from it only in length, as [pak] pack vs. [pa:k] park; or it may be somewhat less front. This type predominates in the middle north, including the broad urban accents of Leeds and Manchester.

Now, it appears that these modified Leeds speakers have begun to separate the quality of their open vowel somewhat, because the vowels have separable ellipses. However, the speakers have not changed the two vowels' qualities sufficiently to stop them overlapping, and the adoption of the [ɘ] appears at first sight like an over-compensation.

[ɒ] and [ɔ:] overlap to a larger extent than in RP. Once more, however, they may be separated perceptually but this

time by two means. Firstly, they may have varying degrees of lip-rounding. [ʊ] is not necessarily very rounded in northern speech (Wells, 1982: 356), and [ɔ:] appears to be more so. Wells also claims (1982: 360) that [ɔ:] "is often fully open in quality, [ɒ:] ", and we find lingering evidence of that quality retention in our MN male data. This, then, is another reason for the overlap with [ʊ].

[u:] in middle northern is very much as in RP (Wells, 1982: 359). Its large spread replicates the situation in RP, too, where centralized varieties co-exist with others which preserve full backing. Some stretching of the loop may be accountable by the impediments to measurement accuracy which the close vowels in general presented.

The short back vowel [ʊ] has a complicated interaction in Modified Northern with the [ʌ] vowel. This may be observed from the two occurrences of the [ʊ] vowel which lie in the open central area of the vowel quadrilateral (Figure 6.8), and also from the grossness of the ellipse itself, representing large standard deviations for F1 and F2 (Table 6.I). Once more, though, some extension of the [ʊ] ellipse may be due to the measuring problem found in the close vowels.

Full discussion of this interplay between [ʊ], [ə] and [ʌ] is afforded a separate section, namely Section 6.10. In order not to interfere with the presentation of the other short vowels, [ʌ] has been omitted from the plots for the MN

male and female short vowels (Figures 6.8 and 6.9), and is given its own two plots later (Figure 6.10).

Lastly, [ɜ:] has found itself a very compact area: more so, in fact, than in RP. However, given the encroachment on the central area by [ə] and [ɑ:], this distinctiveness is necessary if [ɜ:] is to be preserved. As a result, this vowel is somewhat closer than in RP, and indeed, some occurrences were of an [ə:]-like quality.

6.9. The MN females' ellipses (Figures 6.5 and 6.9)

The expansion of the females' vowel ellipses in comparison with those of the males is generally apparent here, as it was in RP. Again, it is possible to consider that women are more variable in their vowel production than men. This idea will be pursued further in the later parts of this chapter.

There is an interesting phenomenon concerning the close front vowels, which becomes manifest after examining all four plots of the male and female RP and MN [i:] and [ɪ] vowels. While the overlap of [i:] and [ɪ] is roughly the same for all four accent-by-sex groups, the females' short close front vowels in both accents are much more compact. Indeed, they contradict the general tendency exhibited by the other vowels for the female ellipses to be larger. It would be interesting, no doubt, to infer that RP and MN females' [ɪ] vowels are actually **less variable** in their realizations, but this would be a false assumption. The compactness of the [ɪ] loops, especially in the Fl

direction, is most likely to be an artefact of the measurement algorithm employed to derive the first formant of these vowels. Because the algorithm made use of harmonic-peak counting, there was less opportunity for measuring-diversity to occur, due to the fewer number of harmonics in the female vowel spectra. Hence, having decided to measure the first formant from the highest harmonic which was not F0 (see Section 6.3.3), that chosen harmonic was almost uniformly H2 for the female vowels. For the males, however, the chosen harmonic value spread over H2 to H4. This accounts, then, for the greater diversity in the males' realizations of [ɪ] in both RP and MN. In other respects, the MN females' close front vowels are again very similar to the RP females'.

[e] spoken by MN females is even more variable in both formants (see Table 6.II) than the vowel produced by MN males. As can be seen in Figure 6.9, realizations of [e] range from a very close [e̞] to an open [e̝], and the size of the ellipse is duly influenced by the large standard deviation in the F1 dimension (see Table 6.I).

The situation obtaining between /æ/ and /ɑ:/ (or, more accurately, [ɶ] and [ɑ:]) is much the same as for the male speakers of MN, and occurs for the same reasons as outlined in Section 6.8, above. [ɑ:] (or [ɑ:]) and [ɒ] overlap more densely than do the MN males' corresponding vowels. This overcrowding of the back area of the vowel space has already

been mentioned (Section 6.7), in connection with the RP females' vowels, and seems to arise from an over-population of back vowels and a lack of differentiable space. The back and root of the tongue are less flexible than the tongue tip and front, and there is a smaller area at the back of the oral cavity in which it can move. Thus, the open, half-open and mid-back vowels (viz. [ɑ:, ɔ, ɔ:] often collide in the space they occupy for various speakers.

It is noticeable again that the MN females' [ɔ:] is more variable than the males' of the same accent (see Figures 6.8 and 6.9, and Table 6.II). There is cause for speculation, it would seem, that female speakers find difficulty in producing a fully-back mid vowel, of the type which is well-typified in Figure 6.5 as produced by RP males. This articulatory difficulty may occur because of females' smaller oral cavities and their even smaller pharynx cavities, as observed by Fant (1975). Thus, in order to produce a good, contrastive [ɔ:] vowel, females would perhaps have to use larynx-lowering as well as their maximum tongue-backing to produce a successful vowel.

The MN females' [u:] vowel is certainly one of the most variable of all four accent-by-sex groups' vowels (see Figure 6.9 and Table 6.II). This variability is especially strong in the F2 direction, viz, the degree of retraction (or rounding) of the vowels. The large [u:] ellipse is the result of a distinctive behaviour which was auditorily easily-detectible from listening to several MN females. The

[u:] vowel they produced was an over-retracted, very back and very rounded vowel, with occasional apparent larynx-lowering, too. This posture yielded some excessively low F2 values (coinciding with, or even below the males' F2s), and, across the females as a whole, a very wide range of variation in F2. The implications of this unusual **overshoot** behaviour are taken further in the discussion of the results, in Section 6.14.

In connexion with [ʊ], while it is true that MN males' [ʊ] is the more variable in the F1 dimension (see Table 6.I), for MN females [ʊ] is more variable in the other, F2 dimension. Immediate explanation for this differential variability in the two sexes' realizations of this vowel is not readily apparent. However, it may be the case that the MN females have detected the centralizing tendency of the close back vowels in RP, and are more rapid in adapting to that movement in [ʊ] at least than are the males. We must note, of course, that MN females' [u:] and [ʊ] appear to be minimally divided by quality and must rely to a greater extent on the feature of length for their distinguishability. The by-now familiar refrain must be added, nevertheless, that this representation of the close back vowels may be due to the difficulties in measuring these vowels, particularly as produced by female speakers.

MN females' [ɜ:] is evidently more variable, again, than the males'. Indeed, Figure 6.9 shows that one speaker has a

vowel which is of [ɛ:] -like quality, as occurs in Birmingham and Stoke-on-Trent (Wells, 1982: 361). On the other hand, another speaker produces a very open [a] -like variant, which could be in danger of being mis-perceived as [ɶ] in the general MN phonological system, thus eliminating the contrast between heard and hard. The middle-northern [ɜ:] is supposed to be "often much as in RP" (Wells, 1982: 360), but this is not confirmed here. This difference, coupled with the wide variation (see Table 6.II) in the females' when compared with the neatness of the MN males' [ɜ:], can be taken to indicate women's chameleon behaviour once more.

As was the case for the MN males, the females' [ʌ] vowel has been excluded from the lower panel of Figure 6.9. It too will be discussed in the next section.

6.10. The MN /ʌ/ vowel (Figure 6.10)

There are several reasons why the MN [ʌ] vowel deserves special comment. The first is descriptively-motivated. The most overriding reason for reporting on /ʌ/ at some length is its value as a prime indicator of the MN accent per se. The background to the importance of /ʌ/ is reviewed first here, before we disclose the vowel's further worth as containing impressive testimony for (a) accentual variation and (b) differential behaviour by the sexes.

A further reason for isolating the discussion of /ʌ/ arose from a methodological standpoint. After analysing the data, it was found to be nonsensical to discuss a 'mean' /ʌ/

vowel: the reason for this requires some explanation and will become apparent in the ensuing paragraphs here, where we look at the individuals' vowel ellipses for /ʌ/ (see Figure 6.10).

6.10.1. Background

In descriptive terms, one of the most important characteristics which separates northern local accents from southern ones is (Wells, 1982: 351 ff.):

...the absence of the FOOT-STRUT Split, i.e. the lack of a phonemic opposition between the vowels of FOOT and STRUT.

The native accent of Leeds falls well within the isogloss-boundary which divides the northern from the southern accents, and, as such, constitutes a "typical northern accent" (Wells, *ibid*: 350). Native speakers from Leeds have no opposition between southern /ʊ/ and /ʌ/, using /ʊ/ in all environments. Hence, northern English has five short vowel phonemes, whereas southern English has six, the additional one being /ʌ/.

The situation is, however, a little more complex because there is a difference in perceived sociolinguistic prestige between the two phonemic systems (c.f. also Knowles, 1978, on the prestige of /ʌ/ in Scouse). The five-short-vowel system is more local and **overtly less prestigious** than the six-short-vowel system, which is a national and **overtly more prestigious** system (Wells, *ibid*: 351). This prestige rating is also linked to geographical latitude (Wells, *ibid*: 352):

It appears that the further north one goes, the higher up the social scale is the crossover between a five-term system and a six-term system located.

So, two basic factors are seen by Wells to influence the /ʊ/-/ʌ/ interplay:

1. Speakers' position on the social 'scale'
2. How far north speakers live.

We hope to show here that a further factor also has to be taken into account when attempting to predict speakers' use of /ʊ/ or /ʌ/.

It will be recalled, though, that our speakers are not 'pure' northern native speakers, but have become modified through living away from Leeds for a considerable time. What phonological behaviour can be expected from them in the choice between /ʊ/ and /ʌ/? Wells (1982: 352) has documented a modified form of northern accents, too. His description merits quoting at some length:

In accents...intermediate between the broad (with [ʊ] in STRUT) and RP (with [ʌ]), there are commonly two phenomena characteristic just of such intermediate stages. One is the use of qualities for the STRUT vowel which are distinct from the [ʊ] of FOOT but nevertheless perceptually different from any realization of RP /ʌ/; the other is the hypercorrect avoidance of [ʊ] in FOOT words...

Such intermediate qualities for the STRUT vowel include a vocoid somewhat opener than [ʊ], namely a mid back [ʊ], the unrounded equivalent, [ɯ]: and a half-open vocoid, unrounded or slightly rounded, similar to cardinal [ʌ] (and therefore somewhat different from RP /ʌ/, which is usually central rather than back). They also include a mid or half-close [ə], central and unrounded.

The last-mentioned possibility, stressed [ə] in STRUT, seems to be particularly characteristic of northern near-RP, with pronunciations such as cup [kəp], brother ['brədə].

The varying approximations to RP /ʌ/ by our MN speakers are what we wish to concentrate on here, reflecting as they do the intermediate stage so clearly. Wells' second characteristic, namely hypercorrection, will be addressed in Section 6.10.4..

Wells (ibid: 353) concludes his section on the STRUT words by presenting the following table (his Table 187), which summarizes the "typical possibilities for STRUT and FOOT in the north of England":

	STRUT	FOOT	
Broad	[ʊ]	[ʊ]	One phoneme, [ʊ]
Intermediate	{ [ə ~ ʌ]	[ə ~ ʌ]	One phoneme; realization modified
	{ [ə ~ ʌ]	[ʊ]	Two phonemes; incidence may be erratic
RP	[ʌ]	[ʊ]	Two phonemes; /ʌ/ vs. /ʊ/

After consideration of our speakers' results, it will seem necessary to alter and extend Wells' table.

6.10.2. The MN speakers' /ʌ/ data: hypotheses and results

On the basis of Wells' comments above, we hypothesize the following for our MN data:

1. The MN speakers would **renounce** the five-term system because they were fairly high up the social 'scale'.
2. They would be more likely to **adopt** the six-term-system because they had **decreased** the influence of latitude by moving **south**.
3. There would be a **great deal** of **inter-speaker variability** in the realizations of the RP /ʌ/ target-

phoneme.

4. Female MN speakers would show **greater variation** in their /ʌ/ vocoids than the MN males, because of the greater influence of the need to linguistically conform (accommodation theory) on them.

Figure 6.10 provides an immediate answer to our hypotheses. There is obvious evidence that some speakers have adopted a six-term system, because some MN tokens fall into a very similar area as that occupied by the RP /ʌ/ ellipses for males and females (Hypotheses 1 and 2). All the 'transitional' vocoid stages offered by Wells are also present. Figure 6.10 serves furthermore as striking illustration of a first order of just how variable are MN speakers' attempts at RP /ʌ/. The very large ellipses for both MN males and females indicate that the individuals' realizations are extremely diverse (c.f. Hypothesis 3). Indeed, the MN females' vowel ellipse has an articulatory spread ranging from retracted open to half-close back vowels (Hypothesis 4).

While Figure 6.10 provides a good visual contrast between RP and MN, and offers holistic answers to our hypotheses, it is quantifiably more informative if we display coefficient of variation values for the MN /ʌ/ vowels, and compare them with the values obtained for the RP vowel.

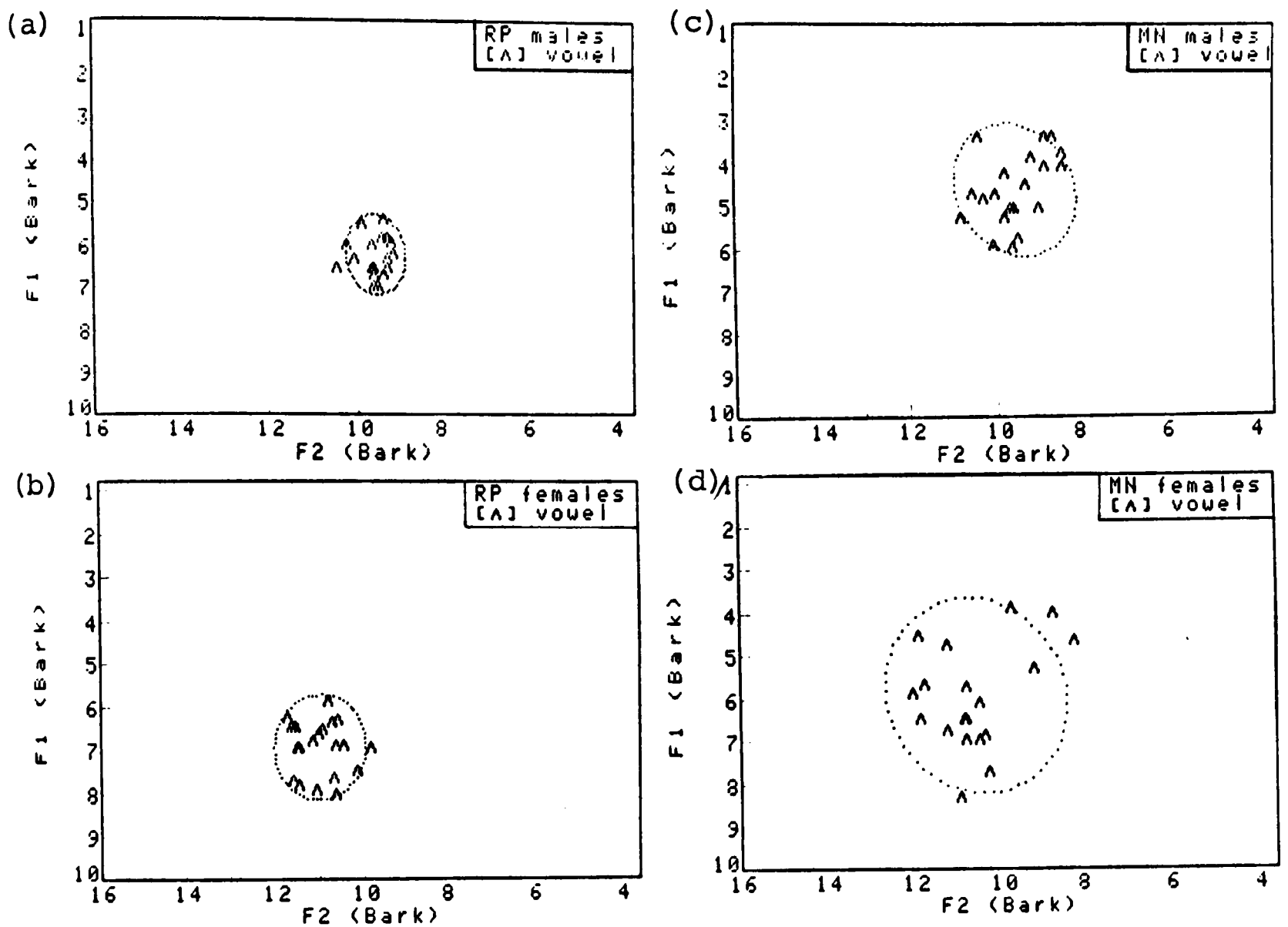


Figure 6.10 The [ʌ] vowel, as realized by (a) RP male speakers; (b) RP female speakers; (c) MN male speakers and (d) MN female speakers. Plots are in Bark, with ellipses drawn to 2 SDs. Notice the variability in the vowels.

Table 6.III Coefficient of variation values of Bark values
of F1 and F2 in the /ʌ/ vowel

		F1	F2
MN	Male	17.53	6.88
	Female	19.59	9.63
RP	Male	7.57	3.43
	Female	8.79	4.63

We may see from Table 6.III that it is particularly in the F1 dimension, which correlates with the degree of openness, that the MN speakers vary so widely. The findings presented in Table 6.III led to the methodological decision mentioned at the outset of this section. It would be firstly counter-productive to include the ellipses of this 'vowel' (or, more accurately, a series of disparate allophones) on plots with other short vowels, since it would conceal more than it would reveal. Secondly, to supply mean values for MN /ʌ/ would be a meaningless exercise.

Figure 6.10 and Table 6.III, then, give support particularly for the third and fourth of our hypotheses. The very much larger ellipses for MN /ʌ/ indicate great inter-speaker variability. Table 6.II attributes that variability more precisely to the female MN speakers, with much greater values shown for the females than the males in MN.

There is a further indication identifiable in Figure 6.10, which relates to the fourth hypothesis. The figure shows

that male MN speakers keep a vowel quality near to their original [u] more frequently than MN females do. Correspondingly, female MN speakers are more susceptible to shifts in their pronunciation towards the target /ʌ/ than are the MN males. Figure 6.10 shows a more frequent achievement of an open central vowel by MN females than by MN males.

These inferences can be taken a stage further. For a more satisfactory account of the MN speakers' attempted realizations of /ʌ/, it was found to be worthwhile to divide the realizations on an auditory basis into three distinct socio-phonetic groups. Table 6.IV illustrates the distribution of these realizations of the citation forms among the males and the females.

Table 6.IV. Distribution of realizations of /ʌ/ by forty MN speakers in citation forms.

Realization	Males	Females
[ʊ]	8	5
[ə]	8	6
[ʌ]	4	9

On the basis of this admittedly small group of speakers, Table 6.IV shows convincingly that the northern accent 'marker' of the original vowel (from the five-term system) is retained more frequently by the males than by the females. This seems to offer a more detailed demonstration of the tendency towards a stronger, more conscious retention of original accentual features by males than by females: of the MN females greater susceptibility to accommodation

theory. Thus the fourth hypothesis again receives support.

6.10.3. Conclusions about MN /ʌ/

The four hypotheses posed about /ʌ/ in the MN accent have been supported by the results from the data. Before concluding this section, we should return to the question of whether Wells' two factors presented earlier is an accurate portrait of the realizational, allophonic behaviour of our MN speakers. The brief answer is 'No'. In particular, we are able to state from our empirical results that speaker-sex also matters. Taking into account the picture presented in our Table 6.IV, we propose an expanded tabulation of typical possibilities for the pronunciation of /ʌ/ and /ʊ/ in speakers from the north of England, into that shown in Table 6.V. The added factors are underlined for emphasis.

Table 6.V. Possibilities for the pronunciation of STRUT and FOOT in speakers from the north of England.

	STRUT	FOOT	
Broad	[ʊ]	[ʊ]	One phoneme, /ʊ/
<u>Males</u>	[ʊ~ə~ʌ]	[ʊ~ə]	<u>Two phonemes:</u> realizations variable, with some hypercorrection
Intermediate			
<u>Females</u>	[ʊ~ə~ʌ]	[ʊ]	Two phonemes; incidence may be erratic
RP	[ʌ]	[ʊ]	Two phonemes, /ʌ/ vs. /ʊ/

In summary for the MN /ʌ/ vowel, then, we can state that the unrounded, central open vowel which is a characteristic of

southern accents is achieved infrequently by modified northern speakers on the whole. Those who do achieve the desired vowel quality are mostly the female speakers. This may be viewed as further support for the theory that women accommodate more rapidly to a perceived prestige norm. Males, furthermore, may consciously retain a distinct regional accentual feature as an indicator of group solidarity, or as a marker of a reluctance to forsake their socio-geographical origins (c.f. Trudgill, 1972). We conclude that Wells' depiction of "northern near-RP", while acknowledging the influence of SES and latitude, fails to take account of a sex-specific difference in the use of intermediate pronunciations of the /ʌ/ vowel, and of greater **systematic** phonetic variation than between just [ə] and [ʌ] in the intermediate stage of the STRUT word pronunciation.

The variables we wish to propose as operating on the pronunciation of /ʌ/ in a Modified Northern accent are therefore:

1. Socio-economic status
2. Geographical location
3. Sex of speaker.

6.10.4. /ʌ/~U/ as an indicator of hypercorrection

In Chapter 3, Section 2, it was mentioned that Labov (1972) had pointed to **women hypercorrecting more than men**. This factor was not an initial research hypothesis here, and so no experiments were designed specifically to test this claim

phonetically in the British English work. However, some peripheral (and certainly inconclusive) evidence indicates that MN females do indeed hypercorrect more than MN males.

The occurrences of hypercorrection appeared in the realization by the MN speakers of the /u/ vowel in hood. In three cases, this vowel (which of course occurs phonemically in both Northern English and RP) was realized as /ʌ/ or /ə/. Two of the hypercorrections were by women, and one by a male.

This sample is admittedly small, and so our inferences cannot be sweeping. Suffice it to say that hypercorrection did occur (even in the most closely-monitored speech style), and in its very limited way, did so more frequently in females than in males.

6.11. Experimental results: mean values

Space and research goals preclude any further discussion of the individuals' vowels per se. It is hoped, however, that the last five sections have provided some useful insights into the precise phonetic nature of the contrasts between RP and MN, and of the sexes' differential contrasts in particular. We now turn to a presentation of the results as means: a presentation which will reveal sex-specific differences as they emerge from the application of the auditory normalization algorithm. As mean values are also those which are available for the languages other than British English (which are treated in Chapter 7), it was in

any case more consistent to use mean values for ease of cross-language comparability and inference, throughout.

This section describes the British English results, in order, for:

1. the citation forms
2. the sentence context forms

Some preliminary data has also been assembled for

3. fundamental frequency (henceforth F0, taken to be the physical correlate of what is perceived as pitch).

This will be discussed at the end of this chapter.

Following the arguments in Section 6.5 here, mean values of formant frequencies (from 20 speakers in each case), in Bark, are presented for the eleven monophthongal vowels of British English in Tables 6.VI and 6.VII. Table 6.VI displays the results for RP, and Table 6.VII those for MN. Variability in the data was calculated, see Table 6.II, and is discussed in due course (see Section 6.14).

Table 6.VI. Mean values (Bark) for RP vowels (citation forms)

Vowel		Male	Female	Difference (f-m)
i:	F1	2.59	4.19	1.60
	F2	14.21	15.09	0.88
ɪ	F1	3.27	3.94	0.67
	F2	13.35	14.40	1.05
e	F1	4.93	6.35	1.42
	F2	12.97	13.90	0.93
æ	F1	6.75	7.91	1.16
	F2	11.52	12.14	0.62
ɑ:	F1	6.13	7.06	0.93
	F2	8.75	9.61	0.86
ɒ	F1	5.45	6.0	0.55
	F2	7.68	8.88	1.20
ɔ:	F1	4.09	4.79	0.70
	F2	6.31	7.62	1.31
ʊ	F1	3.28	4.00	0.72
	F2	8.91	9.33	0.42
u:	F1	2.62	4.07	1.45
	F2	9.39	9.80	0.41
ɜ:	F1	5.28	6.30	1.02
	F2	10.68	11.62	0.94
ʌ	F1	6.21	6.94	0.73
	F2	9.62	11.01	1.39

Table 6.VII. Mean values (Bark) for MN vowels (citation forms)

Vowel		Male	Female	Difference (f-m)
i:	F1	2.46	4.27	1.81
	F2	13.81	15.00	1.19
I	F1	3.40	4.05	0.65
	F2	13.06	14.26	1.20
e	F1	5.08	6.79	1.71
	F2	12.45	13.44	0.99
æ	F1	6.33	8.04	1.71
	F2	10.56	11.78	1.22
ɑ:	F1	6.24	7.76	1.52
	F2	9.01	9.97	0.96
ɒ	F1	5.09	6.92	1.83
	F2	7.81	9.17	1.36
ɔ:	F1	4.12	5.38	1.26
	F2	7.01	8.39	1.38
ʊ	F1	3.26	4.08	0.82
	F2	8.78	9.14	0.36
u:	F1	2.92	4.19	1.27
	F2	9.11	9.15	0.04
ɜ:	F1	4.60	6.03	1.43
	F2	10.80	12.05	1.25
ʌ	F1	4.62	5.92	1.30
	F2	9.59	10.59	1.00

It will be recalled (Chapter 5, Section 5.7.3) that a primary interest in this research is whether 1 Bark is an adequate global adjustment to bring male vowel formants into auditory alignment with females'.

The male-female difference columns in Tables 6.VI and 6.VII are averaged and summarized in Table 6.VIII. On the basis of

this generalized picture, a trend can be seen. Combining the values for both formants, RP requires 0.95 Bark normalization and MN needs 1.2 Bark. In other words, for RP a male/female normalizing adjustment by 1 Bark appears on the average to be about right, but for MN, 1 Bark is distinctly too little. There is a cross-accent discrepancy of some 0.25 Bark on average.

Table 6.VIII. Overall amounts of male/female normalization required (Bark).			

Accent	Formant	Grand Mean (from Tables 6.VI and 6.VII)	Overall Normalization (F1+F2)/2
RP	F1	0.995	0.95
	F2	0.91	
MN	F1	1.402	1.20
	F2	0.995	

The discrepancy can be illustrated most effectively in Figure 6.11, a two-dimensional visualization of auditory space for the two accents. After normalization by 1 Bark, the mean values for RP males and females essentially coincide; by contrast, however, the MN males and females remain significantly far apart.

Prima facie, then, this suggests some confirmation that the two accents of British English are indeed different in their sex-specific behaviours, as was originally hypothesized, at least with regard to the citation forms.

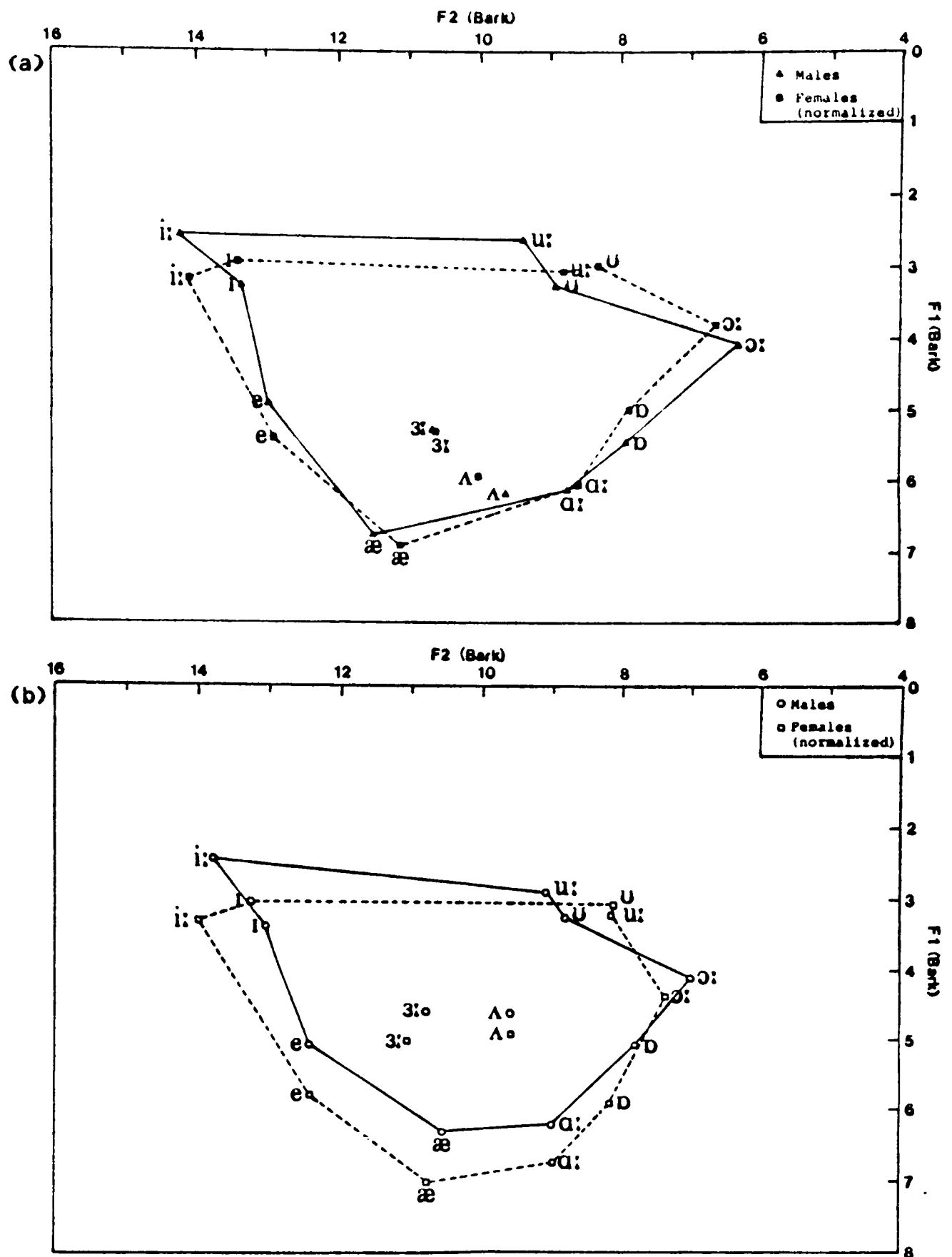


Figure 6.11 Mean formant values (Bark) for citation forms for (a) RP males and normalized females; (b) MN males and normalized females. Connecting the peripheral vowels with a line (solid = males; dotted = females) emphasizes the boundaries of the quadrilaterals in phonetic 'space'. Note how a 1-Bark normalization 'under-normalizes' the MN females.

It was important to establish whether the prima facie conclusions which have been drawn would be supported statistically. Leaving aside certain comments on individual vowels (for which see Sections 6.6 - 6.10), statistical tests were conducted to determine the following:

- (i) was there a significant difference in the RP vowels between males and females **before normalization** (i.e. in the 'raw' data) in (a) F1 and (b) F2?
- (ii) was there a significant difference in MN between males and females **before normalization** in (a) F1 and (b) F2?
- (iii) was there a significant difference remaining in RP between males and females **after normalization** (i.e. with 1 Bark added/subtracted) for both formants?
- (iv) was there likewise a significant difference remaining in MN between males and females **after normalization**, for both formants?

In order to answer questions (i) and (ii), two two-way ANOVAs were carried out. They verified that, as would be expected, there was a highly significant effect of sex (male versus female values) on the raw data prior to normalization. Very large values of the ANOVA statistic F were recorded, in the range 79.95 to 281.95.

The results of the ANOVAS conducted to answer questions (iii) and (iv) are presented in Table 6.IX.

Table 6.IX. Summary of F values of two-way ANOVAs conducted on citation forms, with female values normalized by deducting 1 Bark.

		Dependent variable		
		F0	F1	F2
RP	Sex	8.73**	0.0	2.04
	Vowel	11.29**	248.43**	914.63**
MN	Sex	0.04	12.29**	0.0
	Vowel	15.37**	176.6**	413.15**

** significant at the 0.01 level

This table also includes, for convenience, results for F0: they will be discussed in Sections 6.15 and 6.16.

The results in Table 6.IX for the formants F1 and F2, meanwhile, show first that the choice of vowel has a very large effect on formant frequencies (see the row labelled 'Vowel'). This is a self-evident result which need not be lingered over. The non-trivial demonstration of Table 6.IX is that, after normalizing for sex by 1 Bark, no significant difference remains between the male and female formant data for RP; whereas for MN this cannot be said. A one-Bark normalization is satisfactory for RP, but MN (at least in respect of F1) needs a significantly different amount. It therefore seems justifiable to interpret these findings as indicating broad support for the second of the original experimental hypotheses of sex-specific difference. (c.f. Chapter 1, Section 11). Namely, that within British

English, differing amounts of normalization will be required for different accents.

Given the positive indications of the statistical analysis, it becomes relevant to ask a further question of these findings:

(v) is the difference between the overall normalization requirements for the two accents sufficient to be perceptible? The two accents apparently differ in their sex-specific behaviour by some 0.25 Bark overall (Table 6.VIII). Is this order of discrepancy likely to be detectable by listeners?

There seems little doubt that it is. The question of what is a just-noticeable difference in formant frequency for a speech sound is a rather complex matter, depending on vowel duration among other things, but a conservative estimate of the threshold would be at about 0.15 - 0.2 Bark (Flanagan, 1955; Nord and Sventelius, 1979). It seems safe to suggest therefore that human listeners can attend to, and make use of, the 0.25 Bark discrepancy which was found, if they wish.

6.12. Results for the sentence-context forms

A comparison of values for vowels from citation forms with those from sentence-context forms should reveal whether there are significant differences in formant values for nuclear-stressed and less stressed tokens of the same phoneme. In order to reach some immediate conclusions within

the time scale of this thesis, a decision was taken to concentrate on a subset of vowels from the sentence context, which would be compared with the same vowels in the citation forms. These selected vowels were /i:/ and /a:/ and /ʌ/.

These vowels were chosen for slightly separate reasons. /i:/ and /a:/ because they sample 'polar' regions of vowel space and should therefore give an first indication of whether the space is subject to contraction; /ɜ:/ as a 'touchstone' because it reflects a neutral position of the vocal tract; lastly, /ʌ/ because it had proved to be such a variable vowel in its individual realizations in the citation forms spoken by the MN speakers (see Section 6.10). Secondly, there was a preference for 'long' vowels, as affording greater measurability accuracy when produced in a projected shorter form in the sentence contexts. Furthermore, apart perhaps from /i:/, these vowels were reasonably free from measuring ambiguities. The re-analysis of /i:/ in a different context would, however, provide a useful means of verifying the appropriateness of the algorithm adopted for the measurement of F1, as described in Section 6.3.3.

The results are presented in Table 6.X, together with the statistical significance of these differences, as revealed by t-tests.

Table 6.X. Mean values (Bark) for citation forms and sentence context forms, both accents, together with results of statistical significance. (d.f.=19; two-tailed t-test).

		Male				Female			
		Citn.	S.C.	Diff.	t	Citn.	S.C.	Diff.	t
RP									
i:	F1	2.59	2.39	0.2	1.642	4.19	4.18	0.01	0.013
	F2	14.21	14.09	0.12	1.771	15.09	15.03	0.06	0.939
ɑ:	F1	6.13	6.27	0.14	1.443	7.06	6.83	0.23	1.178
	F2	8.75	8.99	0.24	1.922	9.61	10.03	0.42	2.578*
ɜ:	F1	5.28	5.17	0.11	0.866	6.3	6.44	0.14	1.230
	F2	10.68	10.95	0.27	3.461**	11.62	12.00	0.38	4.067**
ʌ	F1	6.21	6.06	0.15	0.994	6.94	7.39	0.45	2.635*
	F2	9.62	10.12	0.50	3.777**	11.01	11.70	0.69	4.231**
MN									
i:	F1	2.46	2.65	0.19	1.320	4.27	4.29	0.02	0.207
	F2	13.81	13.75	0.06	1.195	15.00	15.00	0.00	0.376
ɑ:	F1	6.24	6.00	0.24	1.610	7.76	7.38	0.34	1.494
	F2	9.01	9.23	0.22	2.428*	9.96	10.15	0.19	1.001
ɜ:	F1	4.60	4.57	0.03	0.417	6.03	5.70	0.33	1.596
	F2	10.80	10.98	0.18	2.606*	12.05	12.31	0.26	2.016
ʌ	F1	4.62	4.36	0.26		5.92	5.74	0.18	
	F2	9.59	9.99	0.40		10.59	11.81	1.22	

* Significant at 0.05 level
 ** Significant at 0.01 level

As can be seen, there are very few significant differences in this table. It is striking that so few vowels are pronounced at all differently in the two types of linguistic context. This finding receives further amplification in the next section.

6.13. Comparative discussion of citation and sentence-context forms

In reference to the overall picture emerging from Table 6.X, the striking lack of variation between the formants for citation and sentence-context forms should be remarked upon. Fewer than a third of the formant differences are significant, indicating that, on the whole these four vowels do not change their quality much with context. What is more, such small changes as there are (illustrated in Figure 6.12, with paired data points) are not clearly 'centralizing' or 'contrast-reducing' in the sentence contexts.

These results appear to be at variance with several experimental studies of so-called 'vowel reduction' (e.g. Lindblom, 1983) and of 'acoustic system contrast' (e.g. Koopmans-van Beinum, 1980). Unstressed vowels in languages (other than British English) have been shown in acoustic terms to 'reduce', 'centralize' or 'show less contrast'. Indeed, Lindblom (1983: 228) has implicated a particular speech production strategy for this:

As the duration of the vowel decreases, there will be less and less time to complete the approach to the vowel target. The vowel gesture will be more and more reduced. It will exhibit undershoot.

Koopmans-van Beinum's (1980) findings narrowed down this phenomenon of reduced acoustic contrast to certain speech styles. According to her, vowels become more and more like each other if speech is 'natural' and 'unmonitored'.

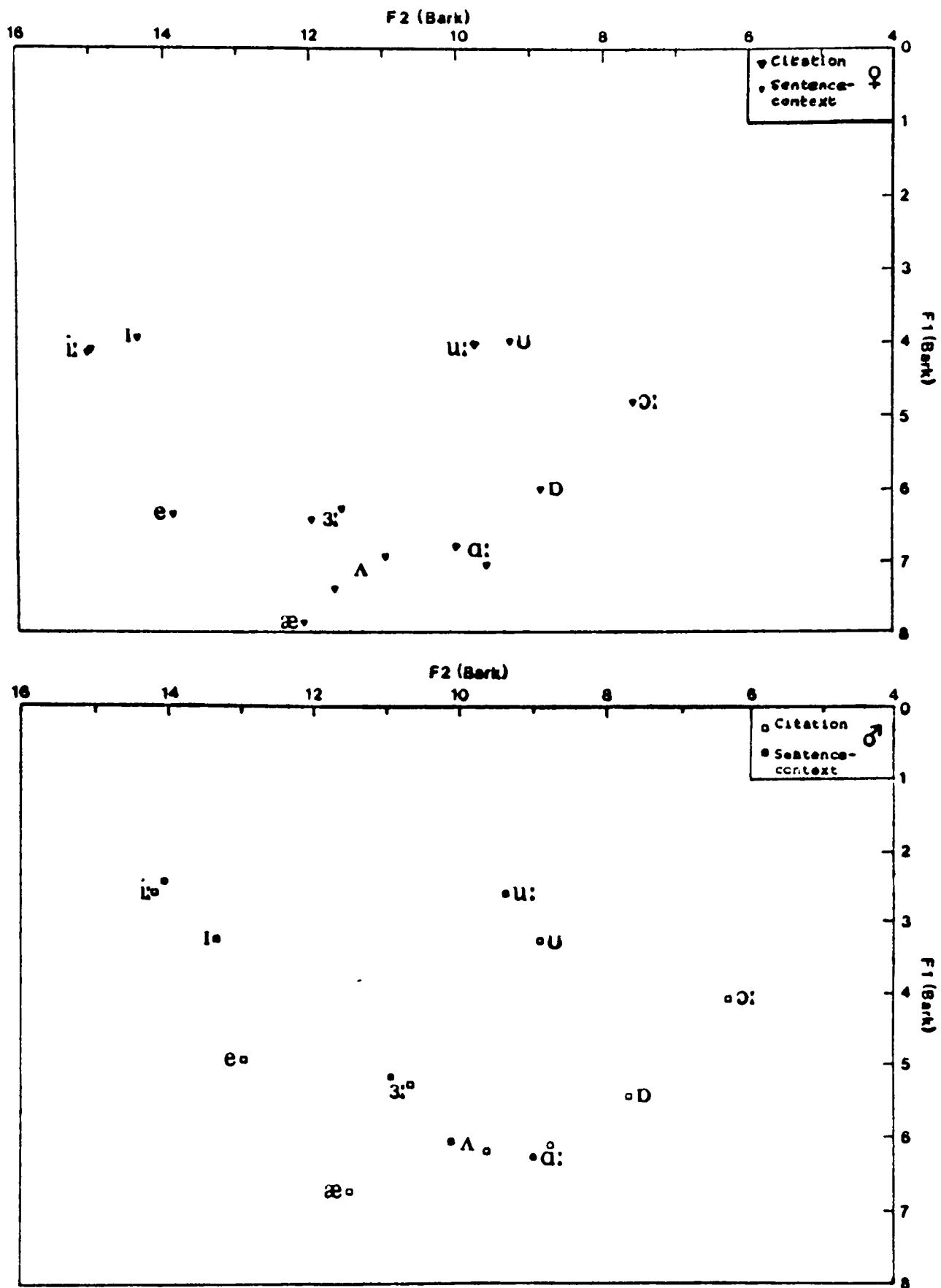


Figure 6.12 Two illustrations of how little sentence-context forms differ from citation forms. Above, the RP females, and below, the RP males. N.b. Sentence-context values are shown only for the vowels /i:/, /ɑ:/, /ɜ:/, /ʌ/.

These sentence-context vowels were indeed reduced in terms of temporal duration, often lasting as little as 40 ms., but negligible evidence appears in Table 6.X (or Figure 6.12) to support the overall tenor of Lindblom's or Koopmans-van Beinum's arguments.

What might explain this somewhat surprising result? A first point of substance is that not all authorities have replicated the vowel reduction results cited above. For example, the results on (American) English by Kuehn and Moll (1976) and by Gay (1978), among others, do not by any means find unequivocal evidence that short duration leads to vowel target undershoot. Rather, the articulators can simply speed up.

Another suggestion would be that it may not be safe to extrapolate from other languages and from specific speech-style contexts, to the two types of environment (viz. single-word citation and word-in-sentence) used in this study. It may be that a much more informal speech style still would have to be examined, or a longer stretch of running speech, in order to uncover evidence of vowel quality reduction in English.

A final, and linguistically more interesting, hypothesis could be based on the way in which extensive vowel-to-schwa weakenings are incorporated **phonologically** in British English, to a much greater extent than in other Germanic languages (on which Lindblom and Koopmans-van Beinum base

their case). This phonological fact might suggest that, in unstressed syllables in English, vowels other than schwa would be unlikely to centralize substantially, in order to preserve perceptual dispersion.

Faced with these possible explanations of the findings, the limitation of this study is that it does not enable one to distinguish a preference among the explanations. This is an inevitable consequence of the time constraints of the thesis. To understand contextual effects on vowel quality more thoroughly would constitute a separate research exercise, using a much greater range of speech styles and linguistic contexts.

6.14. Variability within/among vowels

There are numerous issues of sociolinguistic interest which can be illuminated by a consideration of the variability of this British English data. These include the questions of whether female speech is more variable than male speech, and whether there are differences of variability in certain accents or in specific vowels.

Extensive calculations were therefore made of coefficients of variation (i.e. the standard deviation expressed as a percentage of the mean): refer back to Table 6.II. However, the use to which these calculations could be put was, in part, handicapped by an artefact of the acoustic analysis technique used. Let us briefly note the technical details. The artefact arose out of the decision (Section 6.3) to

measure as formants strictly and only the physical peak components in the spectrum, rather than to attempt to reconstruct a theoretical spectrum envelope in cases where two adjacent peaks were both strong. This decision was justified on grounds of (a) replicability of measurement, and (b) the fact that the speech signal as presented to the human ear contains the physical peaks as they were measured. The acoustic coincidence of a vocal tract resonance straddling two adjacent harmonics therefore introduced into the data a large component of variability in any low-frequency formant measurement. To be on the safe side, therefore, only those variability measures which are well removed from this artefact are consequently analysed here, thus limiting calculations in the main to measures of F2.

Accordingly, Table 6.II provides values of the coefficient of variation of F2 of all the citation vowels. The general conclusions which emerge from this table are the following:

- (i) there is considerably greater variability in MN than in RP. This is not surprising: an accent characterized as 'modified' (viz. MN) can be predicted ab initio to be more variable than one (RP) in which there is, relatively speaking, stability of users' speech over time.
- (ii) it has been widely reported (see, among others, Labov, 1972; Romaine and Reid, 1976; Milroy, 1980 - for a full review of these studies, see now Smith, 1985) that

women are more unstable in their use of linguistic forms than men are, due to their supposed insecurity and/or awareness of need to accommodate to an interlocutor or group with whom they wish to identify (c.f. discussion in Chapter 3). From the average figures of Table VIII, final column, some support for this generalization can be drawn. The mean variability in females is strikingly higher than in males within the MN accent; the RP data are not conclusive on this point. Thus, in connection to our hypothesis in Section 6.1 (d), that MN might reveal differential amounts of modification by female and male speakers, we may offer broad support. Whether the MN females may be said to approach norms more is not conclusive, due to the intrinsic variability of this data.

(iii) Why should the greater variability in females be true of MN but not of RP? A comparable situation has been noted by Trudgill (1972) in Norwich English and by Koopmans-van Beinum (1973) in Dutch. These authors comment that it is especially true of a non-standard or non-prestige accent, that the features which characterise that accent are more firmly retained by male speakers than by females. This certainly conforms to the pattern exhibited by the MN data. The pattern can be highlighted by the variability seen in /ʌ/ in Table 6.III. The greater female variability is consistent with the expectation that the females would

attempt more intensively to approximate to RP.

6.15. F0 (fundamental frequency) of the citation forms

In Section 6.3 it was mentioned that, together with assembling data for the exploration of my central hypotheses, there was the added benefit from the opportunity to assemble some initial findings about F0 (i.e. about speakers' voice pitch) in isolated words. The way in which F0 was measured in the vowels of the citation forms is described in Section 6.3.

Expectations concerning the sex-specific behaviour of F0 would be analogous to those concerning other aspects (such as formant frequencies) of the spectrum. Since the F0 component is normally strongly present in the stationary spectrum of a vowel, and given the theory (see Chapter 5, Section 5.7.3) of speaker normalization by means of displacing a vowel's whole shape in auditory space, it is reasonable to assume that the F0 component should be displaced analogously to the formants. It was therefore feasible to ask of the data-base whether the male/female F0 differences are well normalized by an average 1 Bark shift, or not.

Since these findings for F0 are only preliminary (and are an addition to the research proposals as strictly defined) it would be premature to place great reliance on their finer details. However, the overall mean differences in F0 between the sexes are displayed in Table 6.XI.

----- Table 6.XI. Overall mean F0 of the citation forms (Bark) -----			
	Male	Female (unnormalized)	Difference (f-m)
RP	1.184	1.978	0.794
MN	1.106	2.095	0.989

The column headed "Difference" in Table 6.XI can be taken as expressing the amount of female/male normalization which is ideally required for F0. In these terms, then, we can see that a somewhat similar sort of picture emerges here as did from Table 6.VIII: the two accents behave differently. RP male and female F0's are only about 0.8 Bark apart, whereas the corresponding figure for MN is about 1 Bark. As before, RP appears to require substantially less normalization than MN does.

When two-way ANOVAs were conducted to explore the significance of these F0 results (refer back to Table 6.IX), the results were firmly corroborative. Further confirmation was supplied for the emerging belief that RP male and female speakers speak more like each other than do the MN speakers. This point is taken up again in Section 6.16.

6.16. Implications of the F0 findings

The results have shown that in RP, there is a significant difference remaining in the fundamental frequency (F0) between the two sexes after normalization by 1 Bark. Henton (1984) has examined the possible conclusions to be drawn

from a subset of this data. Now, however, it is possible to go somewhat further in my inferences, particularly with regard to the sociolinguistic interpretation of the results.

For several years, experimental social psychologists have been investigating whether regional accents can be reliably judged as having traits which are more masculine or feminine. The usual comparison is between RP and another regional accent (including Welsh English, Scottish English and Lancashire English). A detailed account of these studies is given by Smith (1985:86-91). While the results of these studies has not been entirely indisputable, and may indeed be open to repeated artefactual influences, they do point towards RP regularly appearing to be **"the voice of perceived androgyny"** (Elyan, Smith, Giles and Bourhis, 1978). That is to say, RP women and men are frequently perceived to be more alike in their speech than are speakers of other accents.

Now, the results of this study do not quantify perceptual reactions, but rather describe productional behaviour in the two sexes in two accents. I appear to be in alignment with the social psychology studies, though, with the finding that, on the whole, RP speakers do appear to be speaking more like each other than the MN speakers. Some speculation as to the cause of this difference between the two accents has been given by Henton (1984) and is still a matter for social psychologists to continue to explore; we may however, consider the further examination of this preliminary finding to be a subject of immediate phonetic research interest.

Possible avenues of such research are indicated more fully in Chapter 8.

6.17. Possible pitfalls in the above analysis: the nature of the "modification" in MN speakers.

Although, as was argued above, the research hypotheses were essentially supported, it can be seen in retrospect that the linguistic behaviour of the Modified Northerners introduced an inherent obstacle to this work.

Notwithstanding the extreme care taken over subject recruitment (c.f. the cross-group commensurability of socio-economic status, stature, age etc.) and recording procedures, considerable variability was found in the MN data (Table 6.II). One must caution against drawing over-positive sociophonetic inferences in this case.

For example, it cannot be established with certainty from this study that, when modifying, all speakers modify in a uniform direction (viz. towards RP). Steps were taken to ensure that subjects had lived in the South of England for at least three years (see Chapter 4), but they need not necessarily have accommodated to RP as the prestige accent in that time: other local accents might have influenced the direction of modification.

Secondly, and similarly, there was a difficulty in gauging the overall degree to which modifiers had modified their accent. Comparatively crude measures such as phoneticians' auditory impressions and the '/Λ/ scale' have been used. /Λ/

provides an easily accessible one-phoneme indicator of modification, but hardly a scale of modification of the whole vowel system. Influences which may be operating on the degree of modification of a speaker's accent may include (a) whether they have a partner who is/is not a Northerner; (b) whether they work closely with other Northerners; (c) whether they consciously seek to maintain their 'Northern roots'; (d) whether they return regularly to their place of origin and (e) whether they are particularly self-aware in their speech habits. This list could be continued, but it serves primarily to provide an impression of the magnitude of the variables which may be influential.

Thirdly, there may have been less than perfect comparability between subjects recruited in Oxford and in Leeds. It may be the case that the speakers who had returned to Leeds after having lived in the South may have been accommodating 'back' to their original accent in order to once more re-integrate into their original society. Thus their modifications would have been less strong than those observed in the MN speakers who had remained in the South, and been recorded in Oxford.

All the above conjectures about the source or sources of variability (for there may be interaction between some of them) lead one to conclude that 'Modified Northern' is not the most accessible or homogeneous of accents. This is, of course, an inevitable consequence of studying a British accent, the speakers of which are socially mobile.

Suggestions regarding the improvements envisaged in this respect are outlined in Chapter 8.

6.18. Conclusions

This chapter has covered a lot of ground. It has expanded on the selectional background to the speakers, and attempted to explain why 'lab. speech' could not be abandoned just yet. Precise analytical procedures were described, including some justification for the decisions taken among the technological choices available.

Expansive accounts have been given of the differences between RP and MN individual vowels' realizations, and more importantly, of the differences between the sexes' realizations of the vowels in the two accents. Several aspects of sex-specific diversity in the pronunciation of individual vowels have been indicated, the most consistent being that women are more variable in their pronunciation than are men. One vowel, namely /ʌ/, was shown to be a good indicator of both degree of modification in northerners' speech, and of sex-related difference in that modification.

The latter sections of this chapter were devoted to results for mean values for the vowels in two separate speech contexts. The results for the citation forms indicate support for the hypothesis that differences exist between accents, which cannot be accounted for in an auditory theory of normalization by 1 Bark. There appears to be a 'learned overlay' such that male-female differences are more extreme

in one accent than the other. This same pattern of accent difference carries over to vowels uttered in different spoken contexts, by females and males. Vowels were investigated both in words spoken in citation form, and in words embedded in sentences. Remarkably little change occurred due to these two contexts. While this finding does pose some questions for those who have found context-specific reduction of vowels in other languages, it is encouraging, by the sheer systematicity of the results across contexts, to infer from this finding at least prima facie evidence for consistency in the methodology and measurement procedures used.

The fundamental frequency of the citation form vowels was also examined to a limited extent, and the results for F0, too, supported the hypothesis that the two accents, RP and MN, would exhibit different degrees of sex-specific behaviour.

From all sections, then, we find underpinning for the notion that **sex-specific differences exist in accents of British English.**

An important further demonstration was that the the observed mean discrepancy of 0.25 Bark was large enough that human listeners would be able to attend to, and make use of, the sex-specific difference between the accents.

And so it has been shown, in accordance with one of the central hypotheses of this chapter, that RP speakers speak

their vowels in such a way that males are more like females than is the case in MN. The discrepancy between accents is significant; and it is large enough for listeners to attend to and discriminate. Furthermore, MN males and females are more unlike each other (i.e. differ by more than 1 Bark) than their physiology would predict. The conclusion is to implicate for these differences a learned, sex-linked, socially motivated factor. It is not possible on the purely physical-phonetic evidence to determine what exactly is the source of this factor. It is worth noting, however, that a factor which would be consistent with my results would be the often noted tendency for the sexes to differ in the extent to which they approximate to the prestige forms of language. The range of prestige-induced variation is likely, virtually by definition, to be greater in the MN ("modified") accent.

Finally, it was the intention to investigate this claim in respect of vowel quality, and this was indeed done. In addition, I have assembled other data, in order to obtain preliminary indications of whether fundamental frequency (F0) shows this same, accent-specific sex difference. It was found that it does. Quantitatively, the difference here is 0.2 Bark (Table 6.XI), equivalent to about 20 Hz. This represents very definitely a detectible difference of pitch, for which the reported thresholds are an order of magnitude smaller (Moore, 1982).

These then are the main conclusions concerning the two

accents of British English. To situate them in a wider context of sociolinguistic research, my view is that a new kind of contribution has been made to existing knowledge about sex-based differences in language. Systematic male/female differences were, until recently, not suspected in the kind of data (on overall vowel quality) which this chapter has examined; and the tools to discover them were certainly not available. It does not seem immodest to suggest that with this research I have pointed the way to a new paradigm of experimental, socio-phonetic studies.

The encouraging and positive verdict which is conveyed by the above conclusions should not, however, be read as totally unqualified. Limitations exist in numerous areas, including my measurement methods, the intrinsic heterogeneity of MN speech, and the scope of the enquiry as enforced by the thesis timetable, all of which one would have ideally liked to avoid. In the course of the next chapter, I examine more closely the main limitations of my study. In so doing, though, some benefit accrues: various further observations emerge which have constructive implications.

Further discussion of some implications of these encouraging results as related to females' intelligibility will appear in Chapter 8. This thesis, however, is concerned with a wider perspective than the inferences to be made from accents of British English alone. In Chapter 7, therefore, I explore how well the auditory theory of normalization by a 1 Bark

displacement can be applied to data from several languages,
other than British English.

Chapter 7

Experimental results from cross-language data

Introduction

In the previous chapter, the auditory normalization theory of displacement by one Bark was tested on two accents of British English. Now we apply the theory to data from other languages, namely General American English, Swedish, Dutch and French. For Dutch, there are data for two dialects: Standard Dutch and that of Utrecht. Details of the sources for each language/dialect will be provided as the sections proceed. These various languages were chosen for investigation for the following reasons:

1. They have a 'representative' number of speakers recorded.
2. They contain vowel data for females and males.
3. Sufficient acoustic detail (i.e. formant frequency values) is provided.
4. They have similar vowel systems, in terms of inventory and size. A similar vowel space is therefore exploited, which facilitates cross-language comparison.

As will be appreciated after reading Chapter 4, they all have their drawbacks, too. These languages/dialects remain the optimal choice, however, when we consider that (a) most other studies exclude data for females; (b) few studies record satisfactory numbers of speakers of both sexes; (c) exact acoustic values are not always published.

There are two hypotheses we are seeking to test on these data:

1. A female-male difference will remain after normalization by one Bark
2. That difference will be heterogenous across languages, i.e. in some societies women and men will speak more like or more unlike each other acoustically than their different vocal tract physiologies would predict.

These hypotheses are pursued through five sets of acoustic inter-sex data, starting with General American (GA), and then following the order of languages/dialects given above.

7.1. The General American English data

These data are supplied by Peterson and Barney (1952). Despite the severe reservations expressed about the homogeneity of these data (see Chapter 4, Section 4.10), no other study has become available in the last thirty years which provides acoustic measurements for large numbers of speakers of GA. To recapitulate, the Peterson and Barney data was assembled from seventy-six speakers: 33 men, 28 women and 15 children. We are not concerned with normalizing for the speech of children here. The mean formant values, converted to Bark (following Zwicker and Terhardt, 1980) for the ten monophthongal vowels of GA English, spoken by these women and men are presented in Table 7.I.

Length marks have been omitted from Table 7.I because length is minimally distinctive in GA. The [ɜ] vowel, while corresponding phonologically to the British English /ɜ:/, is

Table 7.I Mean values (Bark) for General American English vowels

Vowel		Male	Female	Difference (f-m)
i	F1	2.64	3.02	0.38
	F2	13.97	15.18	1.21
I	F1	3.76	4.12	0.36
	F2	13.08	14.46	1.38
e	F1	5.00	5.67	0.67
	F2	12.56	14.08	1.52
æ	F1	6.07	7.58	1.51
	F2	12.12	13.27	1.15
ɑ	F1	6.62	7.51	0.89
	F2	9.07	9.82	0.75
ɔ	F1	5.34	5.50	0.16
	F2	7.43	7.99	0.56
ʊ	F1	4.21	4.48	0.27
	F2	8.64	9.48	0.84
u	F1	2.92	3.57	0.65
	F2	7.65	8.19	0.54
ʌ	F1	5.91	6.85	0.94
	F2	9.65	10.74	1.09
ɜ	F1	4.65	4.74	0.09
	F2	10.49	11.80	1.31

rhotacized in GA. The relative positions of the GA female and male vowels to each other **before normalization** are shown in Figure 7.1a.

Following the technique used in Chapter 6, Section 11, the male-female difference column in Table 7.I is averaged in Table 7.II. This generalized table enables us to show that by combining the averaged values for both formants, it is

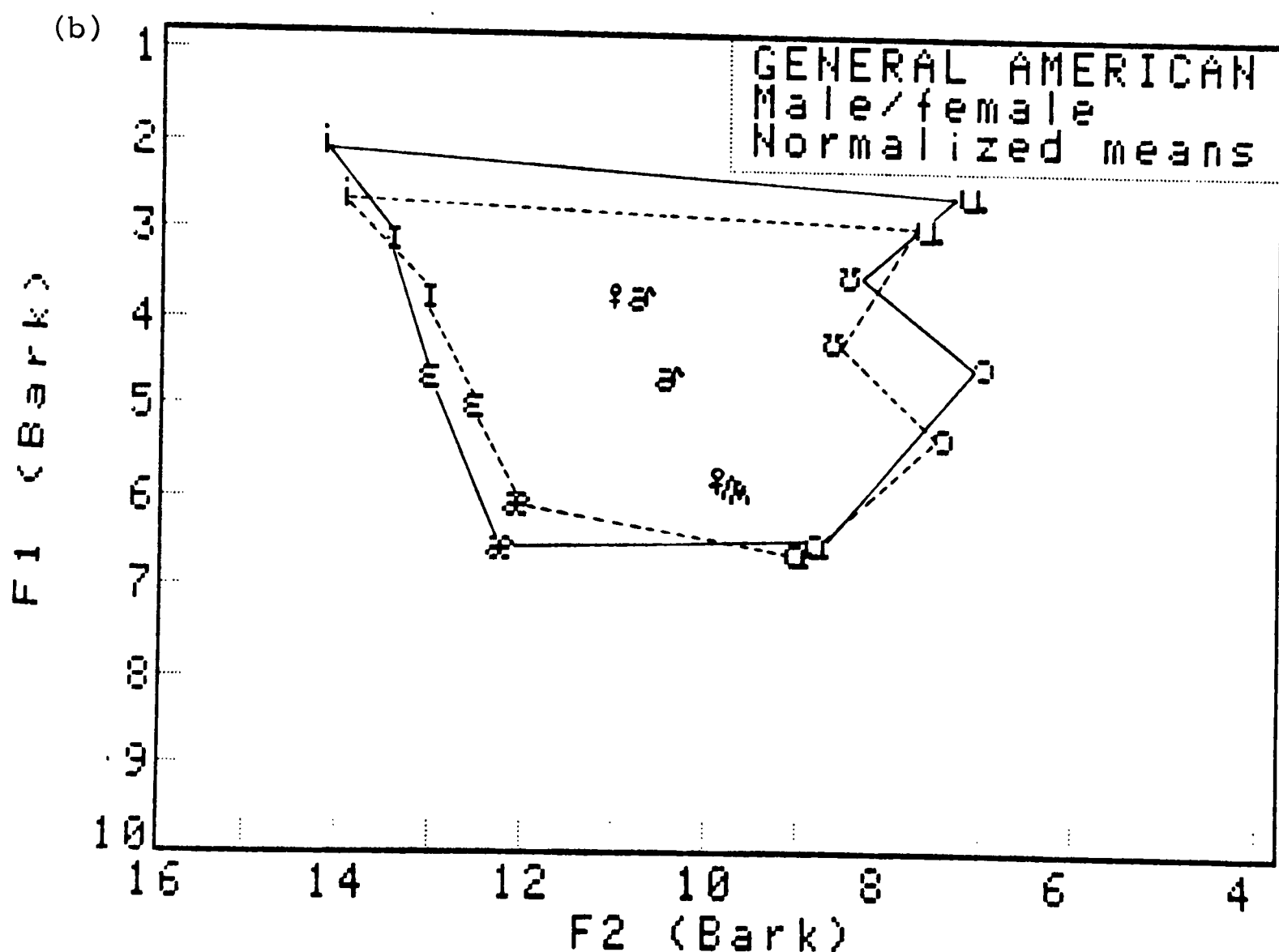
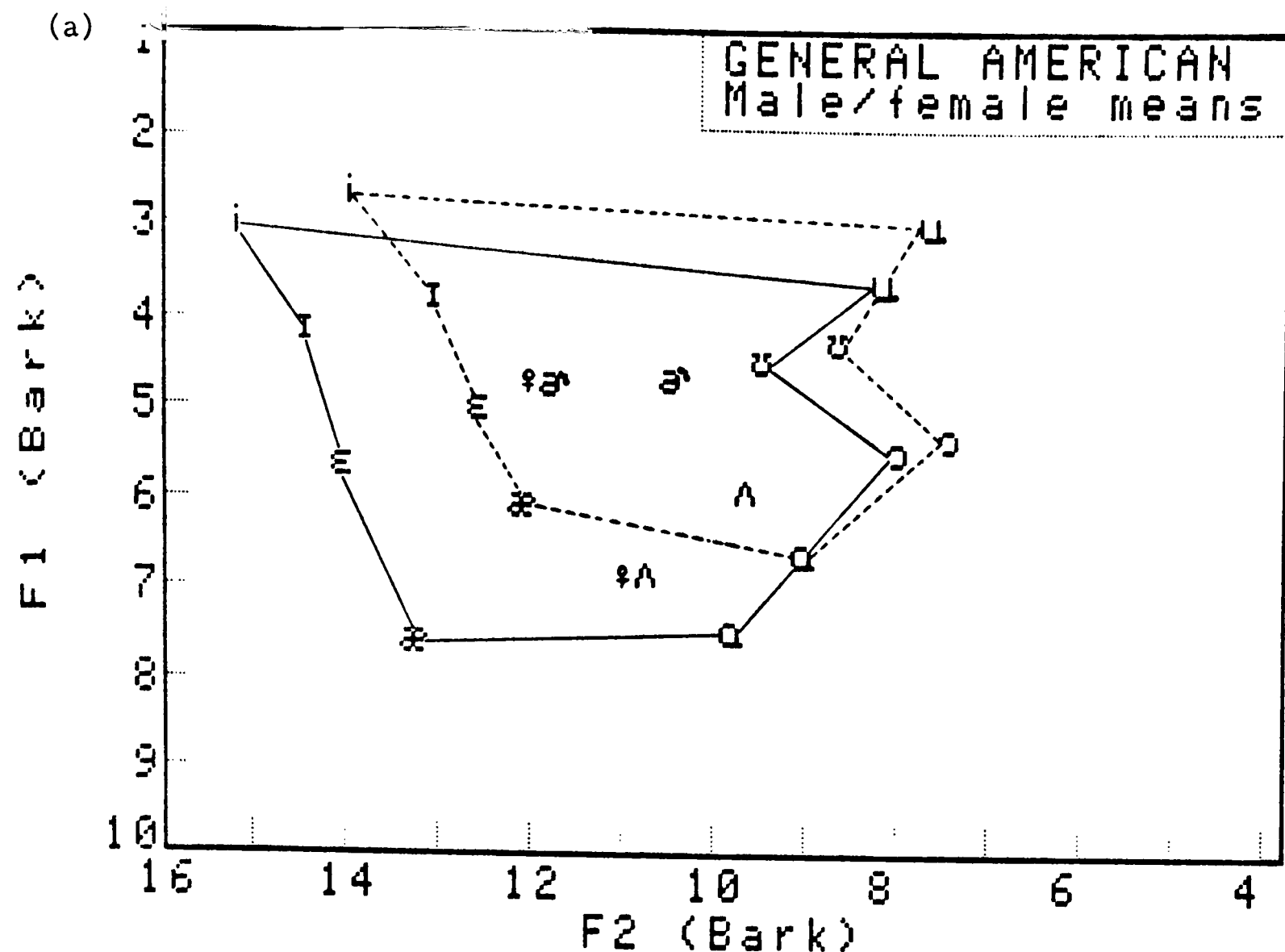


Figure 7.1 (a) Mean formant values (Bark) for citation forms of vowels in General American English (values taken from Peterson and Barney, 1952).
(b) Mean formant values, with female values normalized (Bark) for citation forms of vowels in General American English.

possible to say that GA requires 0.81 Bark normalization. Therefore, for GA a normalizing adjustment by 1 Bark overall is rather too much.

Table 7.II. Overall amount of normalization required (Bark) for GA		

Formant	Grand Mean (from Table 7.I)	Overall Normalization (F1+F2)/2
F1	0.592	0.81
F2	1.035	

Further, graphic illustration of this over-normalization by 1 Bark is given in Figure 7.1b, which plots the mean GA male values and the mean normalized female GA values. It is thus possible to see that the movement in phonetic space by the females' vowel system (with the exception of [æ]) is too great: it tends to overshoot the target of the male system.

In the two panels of Figure 7.1, it is possible to observe both particular and general points relating to the GA vowels. The linking of the female and male peripheral vowels with lines aids these observations. It appears in GA, then, that the only vowels which are normalized successfully for both formants are the open vowels [ɑ] and [ʌ]. The remaining eight vowels are all over-normalized by the 1-Bark shift. This is particularly noticeable in the F1 direction, which correlates with articulatory openness. This would imply that females' vowels need, on the whole, comparatively less normalizing in the open direction than

they do in the front-back (F2) direction. Figure 7.1a shows, for example, that female [ɹ] and [ɔ] have very similar F1 values to the males' **before normalization**.

A second general inference from Figure 7.1 is that female vowels are decidedly **more peripheral** than the males' vowels. In graphical terms, the vowel space delineated by the solid line is larger than that of the dotted line, both before and after normalization. We therefore deduce that GA females exploit on the whole a more peripheral vowel space (i.e. in both F1 and F2 dimensions) than do males. This finding, and how it relates to comments about females 'open-mouthedness', will be taken up again in Section 7.5. We shall also be looking for further signs of such behaviour in the other languages/dialects discussed in this chapter.

The support shown by this GA data for the hypotheses stated at the beginning of this chapter is:

- 7.1.1. An optimal female-male normalization is, on average, 0.81 Bark. Therefore, a **negative female-male difference** remains after normalization by 1 Bark.
- 7.1.2. In GA, females appear to speak their vowels **more like** males than their vocal tract physiologies would predict.

This procedure, of presenting and discussing the mean values in a table, then another table of the overall amount of normalization required, together with graphs of the

male/female mean vowels and the effect of normalizing by 1 Bark, will be replicated for each of the languages/dialects in turn. We will also conclude each section by presenting brief 'answers' to the initial hypotheses in the Introduction to this chapter.

7.2. The Swedish data

Data for acoustic measurements of Swedish vowels spoken by females and males has been published by Fant (1959, and, more accessibly, 1966, 1973 and 1975). Reservations about the sufficiency of Fant's methodology have already been expressed elsewhere (see Chapter 4, Section 10). Recordings were made of seven males and seven females, producing sustained vowels. Although Fant states that the subjects were from "a fairly homogeneous dialectal background" (1973: 32), he later admits (1975: 5) that, "...in the Swedish study...dialectal differences between the female and the male groups" may have been influential (we shall return to a specific example of this later). Thus the homogeneity of Fant's speakers is not to be taken for granted. The values Fant published are complete for the long vowels of Swedish (including the two pre-/r/ allophones), but unfortunately incomplete for the short vowels: he only provides mean formant frequencies for four of the eleven short vowels (including the pre-/r/ allophones). It was therefore decided to focus on the long vowels only. This situation is not ideal, but again, this is the data available for Swedish which best fulfills the requirements listed in the

Introduction to this chapter.

Table 7.III presents the mean formant values, converted to Bark for the eleven monophthongal long vowels of Swedish. The ordering of the vowels has been adjusted from Fant's list, to conform better with other similar tables.

Table 7.III Mean values (Bark) for Swedish long vowels

Vowel		Male	Female	Difference (f-m)
i	F1	2.50	2.71	0.21
	F2	13.32	14.56	1.24
e	F1	3.24	3.53	0.29
	F2	13.27	14.61	1.34
ɛ	F1	4.19	5.13	0.94
	F2	12.40	13.54	1.14
æ	F1	5.64	7.03	1.39
	F2	11.42	12.49	1.07
ɑ	F1	5.44	6.25	0.81
	F2	8.12	8.98	0.86
o	F1	3.87	4.15	0.28
	F2	6.45	7.25	0.80
u	F1	2.99	3.30	0.31
	F2	6.62	6.31	-0.31
y	F1	2.51	2.64	0.13
	F2	12.87	14.46	1.59
ʊ	F1	2.76	2.92	0.16
	F2	11.77	12.81	1.04
ø	F1	3.51	13.59	0.08
	F2	12.00	13.11	1.11
œ	F1	4.95	5.30	0.35
	F2	9.15	10.19	1.04

There are two striking facts which emerge from a

consideration of the Swedish data in Table 7.III. Firstly, the great majority of the vowels need much less than 1 Bark's normalization in the F1 dimension: this will become clearer in the summary table, Table 7.IV. Secondly, Fant seems to have encountered a similar sort of pronunciation behaviour by females in Swedish as was observed in our Modified Northern data. The difference between male and female pronunciations of Swedish [u:] appears as a **negative** value for F2. That is to say, females are producing a more retracted or more rounded vowel than males, on the average (c.f. Chapter 6, Section 9). Fant does not comment on this peculiarity, although he does address the exceptionally **large amount** of male-female difference in F2 of [y:]. This incommensurability he attributes to possible dialectal differences between the two sexes' groups (1975: 4-5). A clearer depiction of the Swedish vowels' relations in terms of sex-related articulations is presented in Figure 7.2a.

The Swedish male-female difference column in Table 7.III is averaged in Table 7.IV. This time, the table shows us that a combination of the averaged values for both formants indicates a 0.72 Bark normalization for Swedish, across all long vowels. Graphic account of the large amount of over-normalization by 1-Bark is supplied in Figure 7.2b, of the mean male vowels plotted with the **normalized** mean female vowels.

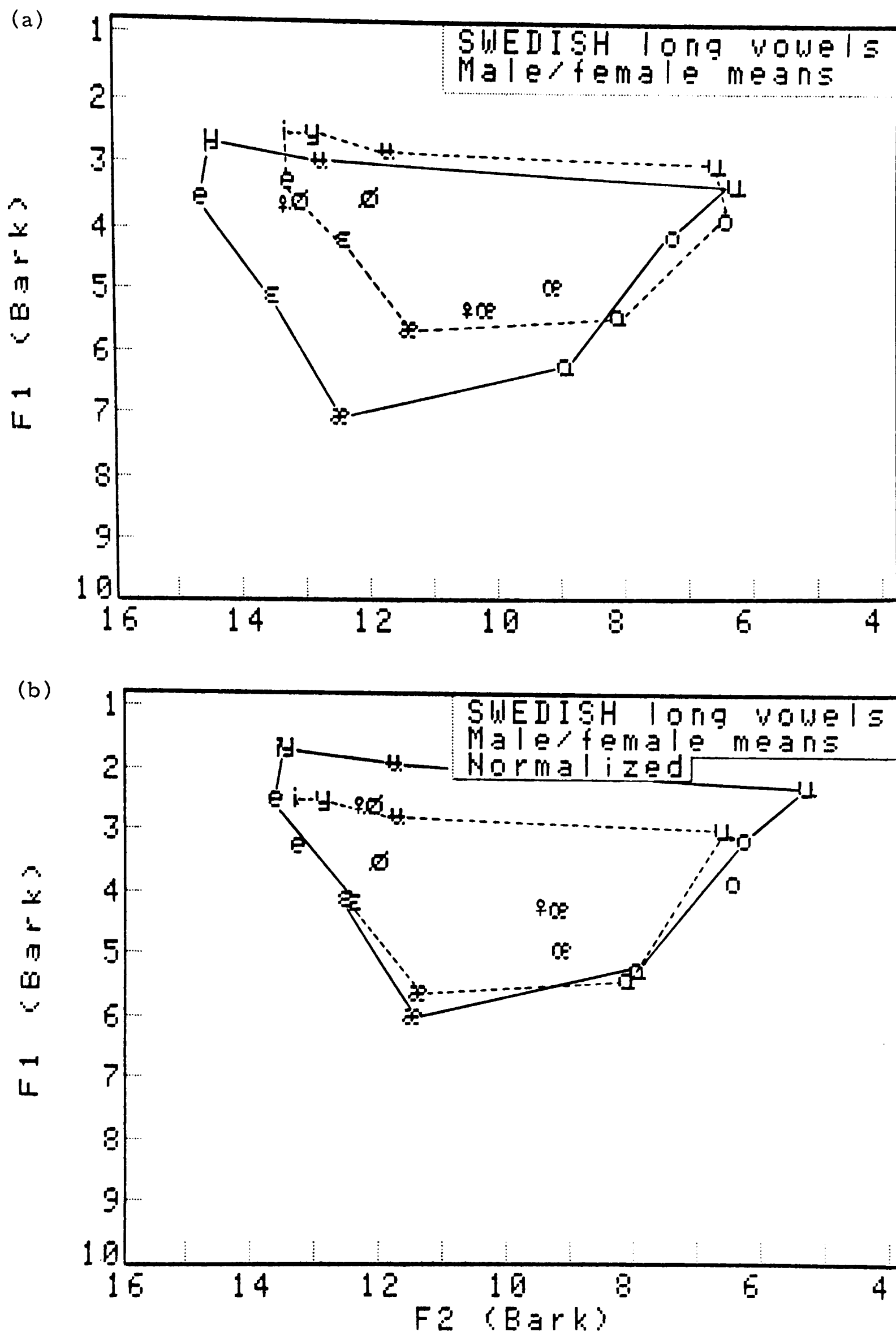


Figure 7.2 (a) Mean formant values (Bark) for citation forms of Swedish long vowels.
 (b) Mean formant values, with female values normalized (Bark) for citation forms of Swedish long vowels.

Table 7.IV. Overall amount of normalization required (Bark)
 for Swedish long vowels

Formant	Grand Mean (from Table 7.III)	Overall Normalization (F1+F2)/2
F1	0.45	
F2	0.993	0.72

Figure 7.2 further indicates vowel-specific behaviour in Swedish. Firstly, we may observe in the upper panel the similarity of the females' and males' [u:], as we remarked on above. Secondly, it is interesting to see how both sexes' close front vowels [i:] and [y:] appear to be hardly distinct in their F1-F2 pattern. Thus, the [i:] and [y:] vowels appear virtually 'on top' of each other, at least in the F1-F2 representation here. The two vowels will, of course, be distinguished clearly by their F3 and F4 values, which are lowered in the case of [y:], as would be expected for a rounded vowel. But rounding should also lower F2, and so the question arises why [i:] and [y:] are virtually coincident in F2. A possible answer is that Swedish [i:] is unusual in its articulation, being hyper-front, and having a maximum point of constriction in the post-alveolar region. This articulation, perhaps surprisingly, creates a formant pattern with lowered F2 and a perceptual effect of centralization.

A further feature of note in these Swedish plots is that for the vowels [ɤ:, ø:, œ:], there is very little difference

between female and male values in the F1 domain. While the similarity may be accounted for by a measuring difficulty again in [ʊ:], which has a low F1 (and is therefore prone to the same problem as encountered with [u:]), this reasoning does not account for the inter-sex similarities of [ø:] and [æ:]. The latter two vowels have higher F1 values and so should not present measuring problems. The general indication from these vowels, when viewed together with the close and half-close vowels [i:, e:, o:, u:], is that the close and half-close vowels all need less normalization in the F1 dimension than do the open and half-open vowels [ɛ:, æ:, ɑ:]. The latter three vowels, as a consequence, respond very well, and align neatly, after a normalizing shift of 1 Bark in both F1 and F2 directions.

Once more we may notice a greater overall peripherality in the females' vowel space. This result echoes the situation found in GA, and commented on in the previous section. It remains to be seen whether this growing picture of females use of greater peripherality in vowel space is supported by our other three languages/dialects.

With regard to the hypotheses posed, we may reply that in the Swedish data support is found of the following nature:

- 7.2.1. A residue of 0.28 Bark remains if female-male normalization is accorded 1 Bark: that is to say, an optimal average normalization for Swedish is 0.72 Bark. Therefore, a **negative** female-male

difference remains in Swedish after normalization by 1 Bark.

7.2.2. In Swedish, females appear to speak more like males than their vocal tract physiologies would predict.

7.3. The Dutch data

Koopmans-van Beinum (1973) collected data for two accents of Dutch: Standard and Utrecht. Her results for Standard Dutch will be described first.

7.3.1. Standard Dutch

Speakers of Standard Dutch were selected on the basis of their "non-regionality" (Koopmans-van Beinum, 1973: 250). This would seem to equate well with the way in which RP is often defined, i.e. that it has no obvious regional affiliation (c.f. Chapter 4, Section 4.12.1). The speakers of Standard Dutch were ten females and ten males. Their exact ages are not published, nor are any further biographical details. The twelve monophthongs of Dutch were elicited from the naming of pictures. While this was a commendable attempt to "prevent the influence which a written text might exert" (ibid: 251), it led nevertheless to the vowels occurring in different consonantal contexts. The investigator did take care, nevertheless, to minimize the influence of consonantal transitions by measuring formants from the steady-state portions of the vowels. She gives more support to this methodology by stating (ibid:

256):

The results that have been found are ... always linked to key-words, that is to say, to certain consonant surroundings of the vowel. This is not a serious disadvantage for formant measurements, as the influence of the bordering consonants on the rather constant middle section of the vowel is small in words pronounced in isolation.

She adds that it is probably the duration of the vowels which is influenced more by the consonantal context. This temporal aspect of vowels is not under investigation in this thesis, though.

Table 7.V catalogues the mean formant values for Standard Dutch-speaking males and females, converted to Bark. Once again, the sequence in which the vowels appear has been altered from Koopman-van Beinum's original tables, so that comparability with others in this thesis is easily achieved.

The original phonetic symbols used by Koopmans-van Beinum are somewhat idiosyncratic, and have therefore been converted to more international symbols for Table 7.V and Figure 7.3. In describing her methodology, the author provides a gloss to the phonetic notation used in her report. This would seem unnecessary at first sight because she uses square brackets (indicating phonetic, rather than phonemic quality). However, the symbols selected for the description of Dutch do not appear to represent conventional phonetic qualities. To equate the Dutch [u] with the vowel in the English word book is mistaken; [ɔ] in Dutch is in fact a short vowel whose quality resembles the English vowel in hoard, rather than (as she writes) hot. Meanwhile, other

Table 7.V. Mean values (Bark) for Standard Dutch vowels

Vowel		Male	Female	Difference (f-m)
i	F1	2.98	3.63	0.65
	F2	14.50	15.06	0.56
ɪ	F1	3.61	4.28	0.67
	F2	13.71	14.74	1.03
e	F1	3.88	4.52	0.64
	F2	13.77	14.58	0.81
ɛ	F1	5.74	6.80	1.06
	F2	12.70	13.81	1.11
a	F1	6.68	7.54	0.86
	F2	10.78	10.92	0.14
ɑ	F1	5.91	6.83	0.92
	F2	10.20	10.12	-0.08
ɔ	F1	5.07	5.63	0.56
	F2	8.04	8.48	0.44
o	F1	4.42	4.78	0.36
	F2	7.85	8.03	0.18
u	F1	3.24	3.88	0.64
	F2	7.24	7.51	0.27
y	F1	3.00	3.58	0.58
	F2	12.49	13.31	0.82
œ	F1	3.90	4.70	0.80
	F2	11.84	12.82	0.98
ə	F1	4.17	4.63	0.46
	F2	11.62	12.38	0.76

vowels in the Dutch system have near-cardinal qualities and are approximated to French example-words.

The confusion caused by the use of non-standard symbols is added to by the statement (ibid: 256) that:

...the vowels have been indicated with phonetic

symbols, but this is meant to signify only vowels as pronounced in these keywords.

It appears, then that we need to be cautious before accepting these symbols as representative of the Dutch vowel system, since they actually describe some specific allophones.

From a consideration of Table 7.IV and Figure 7.3, it seems once more that significantly less than 1 Bark is required to bring male and females into auditory alignment in Standard Dutch, and that the F1 dimension again needs less normalizing than F2 (c.f. GA and Swedish). This phenomenon is particularly noticeable in Standard Dutch in the open vowels, [a] and [ɑ], with the latter vowel needing a negative amount of normalizing female-male. This means, in an articulatory interpretation, that Standard Dutch females are once more producing vowels which are proportionately more open than the males' vowels in this area. We will return to the question of women being more 'open-mouthed' than men in the following paragraphs and in the discussion of all these results, in Section 7.5.

Table 7.VI. Overall amount of normalization required (Bark) for Standard Dutch

Formant	Grand Mean (from Table 7.V)	Overall Normalization (F1+F2)/2
F1	0.683	0.63
F2	0.585	

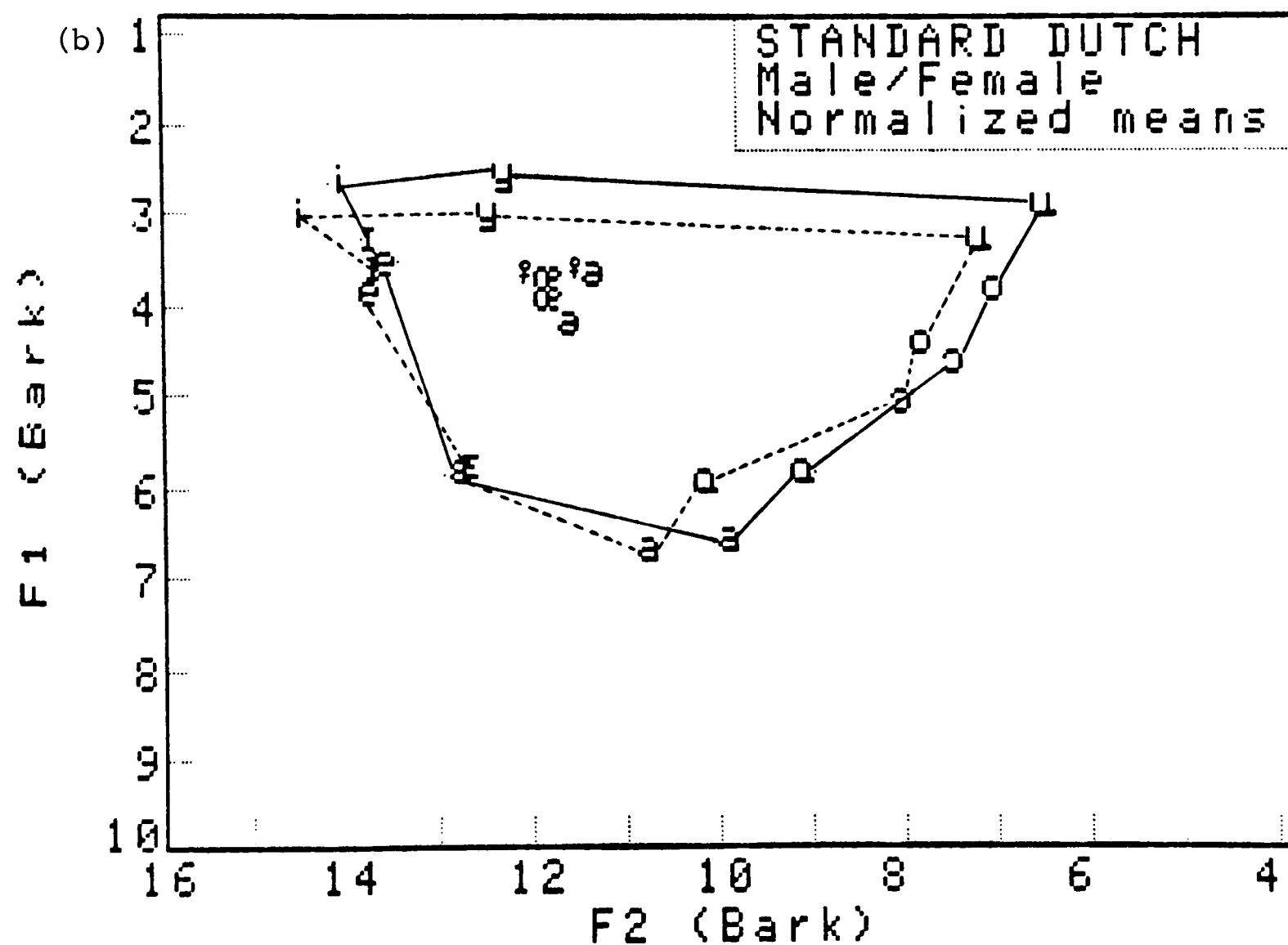
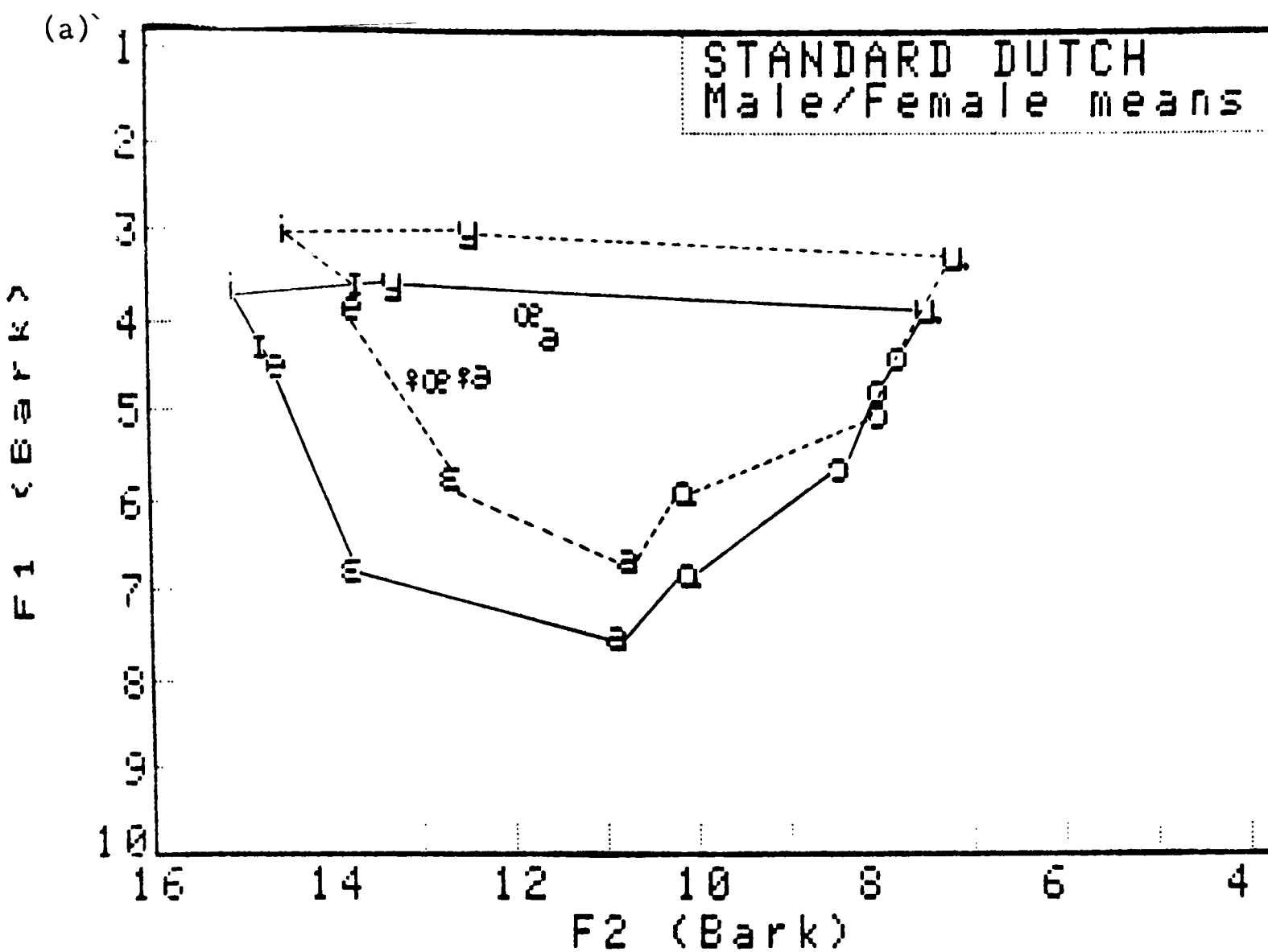


Figure 7.3 (a) Mean formant values (Bark) for citation forms of Standard Dutch.
(b) Mean formant values, with female values **normalized** (Bark) for citation forms of Standard Dutch.

Figure 7.3 shows plainly that 1 Bark is too much by which to normalize for these data: indeed, only four of the eleven vowels (viz. [ɪ, ɛ, ɑ, œ]) begin to approach that amount for both formants. Figure 7.3a also shows how similar female and male vowels are to each other (at least in an F1-F2 plane) in Standard Dutch before normalization. Table 7.VI underlines that deduction, and reveals that a 0.63 Bark normalization is needed, on average, for Standard Dutch.

Another feature of interest in Figure 7.3 is how similar [ɪ] and [e] are in Standard Dutch: their distinction must rest almost solely in their different durations. Again in Figure 7.3, we find evidence of the close back rounded vowel showing little sex-specificity in its formants, and we might once more suspect measurement problems with the females' vowels.

Figure 7.3b illustrates how a 1-Bark normalization causes overshoot for the females' vowel space. The emerging theory that females regularly employ more extreme articulations for their vowels receives some support from this figure, although perhaps not as strongly as from the similar figures for GA and Swedish. In Standard Dutch, it is the females' back vowels [ɑ, ɔ, o, u], together with the close front vowels [i] and [y], which are more peripheral than the males' after normalization. The position of the females' [ə] also reflects this movement. Thus the 'open-mouthed' posture of females seen in GA and Swedish receives some further underpinning in this Standard Dutch data.

We may summarize support for our hypotheses from the results for Standard Dutch as:

7.3.1.1. An optimal female-male normalization for Standard Dutch is 0.63 Bark, which leaves a residue of 0.37 Bark if a 1 Bark normalization is applied. Therefore a **negative** female-male difference remains in Standard Dutch after such a normalization.

7.3.1.2. In Standard Dutch, females appear to speak **much more like** males than their vocal tract physiologies would predict.

7.3.2. Utrecht Dutch

Koopman-van Beinum's (1973) other set of twenty speakers (10 males and 10 females), were garnered in two villages in the province of Utrecht: they had all been born and brought up in Maarssen or Breukelen. Unfortunately, their ages ranged from 25 to 65 years. As was indicated in Chapter 4, Section 4, this range is too great, and is likely to introduce both stylistic and physiologically-linked pitch variations.

The speakers of the Utrecht dialect recorded the same material, with the same instructions, as were given to the Standard Dutch speakers. It appears that no attempt was made to maintain replication of recording equipment, however, since the Utrecht recordings were made on a Nagra portable tape-recorder, with a Sennheiser microphone, while those made in Amsterdam were on an Ampex tape-recorder, with

Table 7.VII. Mean values (Bark) for Utrecht Dutch vowels

Vowel		Male	Female	Difference (f-m)
i	F1	3.26	3.79	0.53
	F2	13.93	14.86	0.93
ɪ	F1	3.88	4.30	0.42
	F2	13.71	14.61	0.90
e	F1	4.67	5.17	0.50
	F2	13.35	14.28	0.93
ɛ	F1	5.67	6.34	0.67
	F2	12.89	13.76	0.87
a	F1	6.31	6.56	0.25
	F2	9.69	10.26	0.57
ɑ	F1	6.25	6.83	0.58
	F2	10.98	11.03	0.05
ɔ	F1	5.40	6.02	0.62
	F2	8.37	9.29	0.92
o	F1	4.71	5.56	0.85
	F2	8.36	8.70	0.34
u	F1	3.75	4.02	0.27
	F2	7.28	7.64	0.36
y	F1	3.47	3.79	0.32
	F2	12.68	12.84	0.16
œ	F1	5.18	5.27	0.09
	F2	11.99	12.99	1.00
ə	F1	4.46	4.71	0.25
	F2	12.03	13.03	1.00

a Philips microphone. This lack of similarity could also give rise to variability in the recordings per se. The author of this Dutch study considers variability in the data in some detail, but does not recognize these possible original sources.

The mean formant values, converted to Bark, for twelve monophthongs produced by twenty speakers of Utrecht Dutch are shown above in Table 7.VII.

As has become customary, the male-female difference column is averaged in Table 7.VIII. The combination of the averaged values for both formants shows that, for Utrecht Dutch, a normalization by 1 Bark is very inappropriate. Again, this result is displayed in Figure 7.4a, where the female vowel space exceeds its 'target' of the males' space by a large amount. By an amount in fact, that is almost as great as it was before normalization (c.f. Figure 7.4b).

----- Table 7.VIII. Overall amount of normalization required (Bark) for Utrecht Dutch -----		
Formant	Grand Mean (from Table 7.VII)	Overall Normalization (F1+F2)/2
F1	0.446	0.56
F2	0.669	

Figure 7.4 reveals some unusual facts about the vowels of Utrecht Dutch. They are:

1. As we have almost come to expect, the position of [u] is very similar for both sexes before normalization. It may be possible to infer that Koopmans-van Beinum has also not evaded the measurement trap encountered frequently in this area.
2. In Utrecht Dutch, the expected location of [a] is

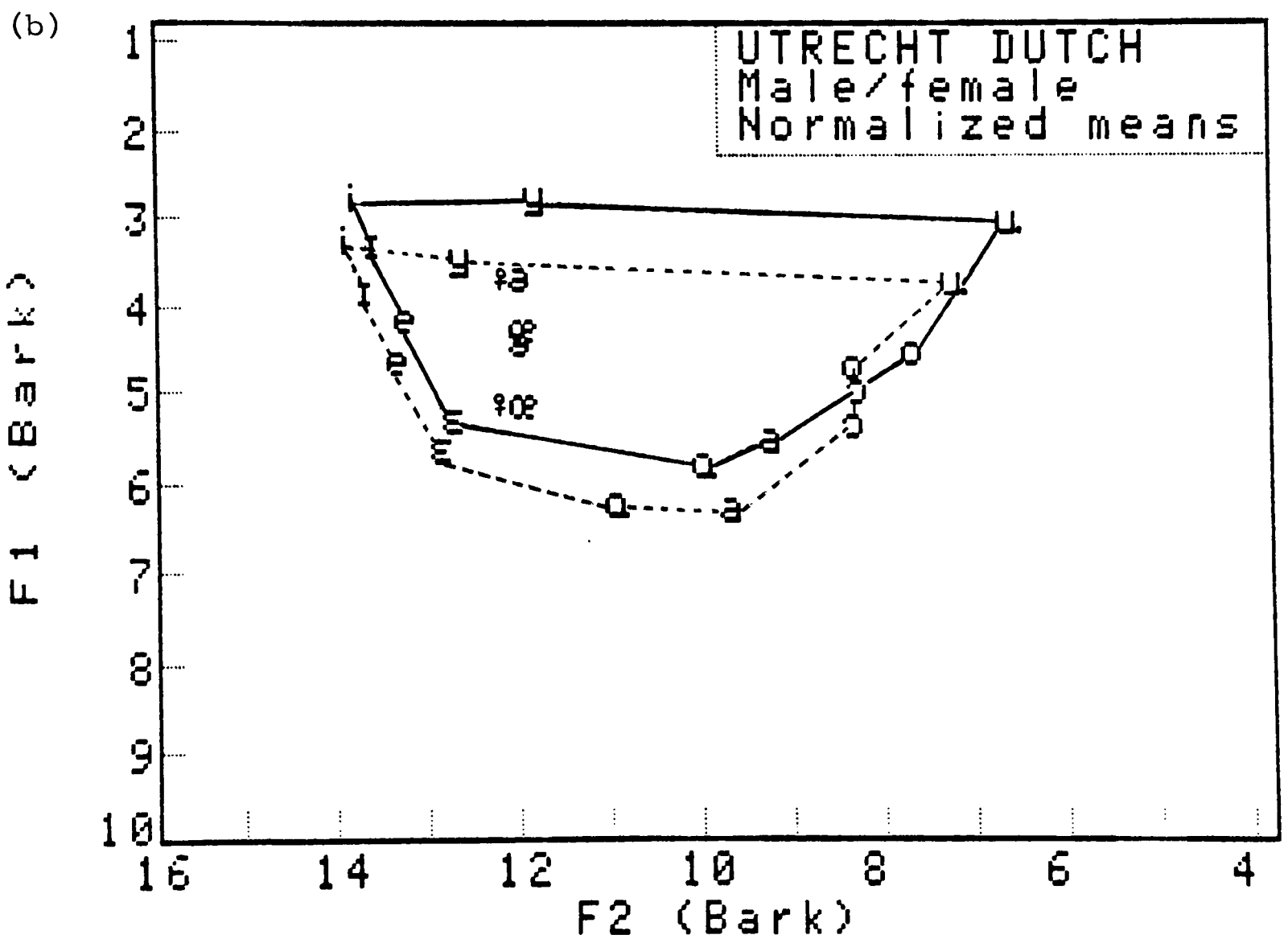
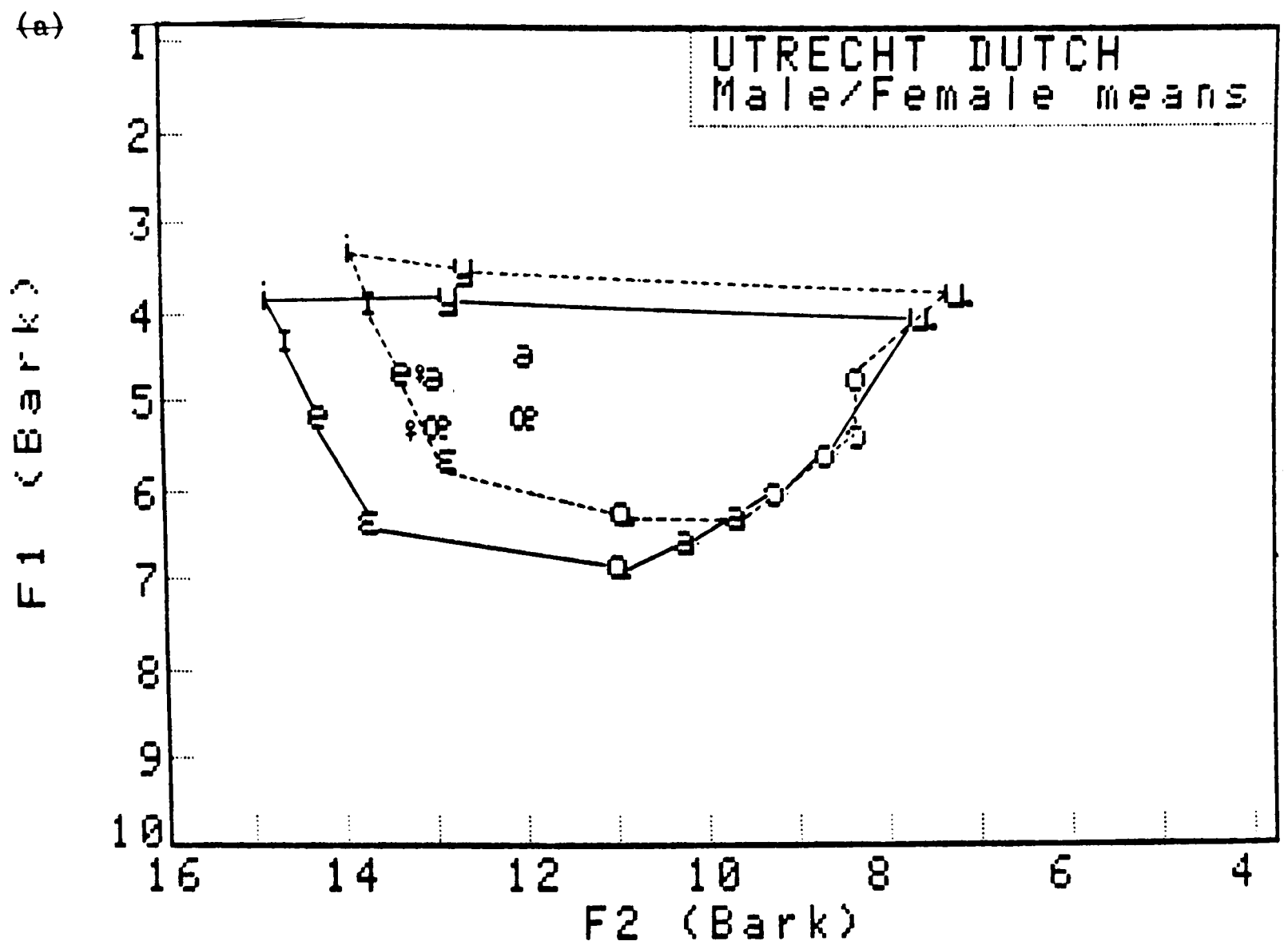


Figure 7.4 (a) Mean formant values (Bark) for citation forms of Utrecht Dutch.
 (b) Mean formant values, with female values **normalized** (Bark) for citation forms of Utrecht Dutch.

transposed with that of [ɑ]. The [ɑ] thus resembles more closely [ɛ]. This is not so for Standard Dutch. Koopmans-van Beinum (ibid: 257) considers the possibility of this effect being the result of 'palatalizing', but cannot provide any conclusions because "...no investigation was made ...as to the place of articulation and articulatory movements."

3. In Utrecht Dutch, the females' [a] and [ɑ] are more similar to each other in quality than are the males. They are separated, according to the original author, by comparative duration.
4. For Utrecht speakers, F1 of [e] and [œ] are considerably higher than for the Standard Dutch speakers. This is caused by the marked diphthongization in the Utrecht pronunciation of these vowels. They thus presented a measurement problem, because the Utrecht [e] and [œ] were not very constant in their quality, and measurements were taken from only one middle point in the vowel. The apparent difference in these vowels between the two dialects may be due, then, only to a methodological artefact. Indeed, the data would probably be improved if the investigator had been rigorous enough to exclude these diphthongal vowels in a study of the monophthongs.
5. Durational differences account for the larger amount of overlap (greater proximity of the mean values) between the Utrecht female vowel pairs [o] and [ɔ], [a] and [ɑ], and [œ] and [ə].

6. The female vowel space in Utrecht Dutch is not markedly more peripheral than the males'.

We have listed a few specific points relating to the Utrecht Dutch vowels and their sex-linked attributes. With reference to the general hypotheses concerning male and female phonetic traits, Utrecht Dutch reveals that:

7.3.2.1. An optimal female-male normalization for Utrecht Dutch is, on average, 0.56 Bark. A large residue of 0.44 Bark occurs if we apply 1 Bark. Therefore, a **very substantial negative** difference remains in Utrecht Dutch after normalization by 1 Bark.

7.3.2.2. In Utrecht Dutch, females appear to speak **much more like** males than their vocal tract physiologies would predict.

7.3.3. Comparison of Standard with Utrecht Dutch

Since the main objective of Koopmans-van Beinum's research was to provide acoustic demonstration of the dialectal differences between Standard and Utrecht Dutch, she devotes considerable space (ibid: 256-257) to comparing the mean formant frequencies for the two. The relevant remarks she makes will be summarized here. A few additional comments of my own will then be included.

1. The vowel "triangles" are very similar overall for the two dialects of Dutch. This is "not to be wondered at" because both groups articulate in roughly the same way.

2. The Standard Dutch "triangles" are larger than those for Utrecht. This indicates that the Standard Dutch speakers make larger contrasts between the various vowels.
3. Diphthongization of a few vowels causes greater variability and, by inference, greater size in the Utrecht Dutch vowel ellipses (drawn to two standard deviations).
4. Various vowels (e.g. [o] and [ɔ], and [œ] and [ə] in Standard Dutch) show an amount of overlap in their ellipses. This is cancelled by differences in duration. A particularly striking example is the overlap between [ɪ] and [e] in Standard Dutch, which appear very similar to each other in quality. This similarity is not replicated in Utrecht Dutch.

Other comments by Koopmans-van Beinum refer to durational differences between the vowels in the two dialects. These do not concern us directly here, except that it is of passing sex-specific interest to note that (ibid: 258) the average duration of the Dutch vowels (both dialects) is greater for women than for men. The investigator does not comment on how remarkably similar the formant values are for the two sexes in both accents of Dutch.

Koopmans-van Beinum's discussion focusses on the features of a dialect compared with the 'standard pronunciation'. Her attention to sex-related patterns is thus only tangential. Therefore we add a few observations based on our own

interest to the comparative list, concerning the separate behaviour of the sexes.

5. The male and female back vowels appear more similar to each other **before normalization** in both dialects than do the front vowels. This is especially apparent in the F2 direction.
6. Although the Standard Dutch female vowels remain on the whole more peripheral than the males' vowels, the Utrecht females' vowels are not so. The overall space occupied by the two sexes' vowels in Utrecht Dutch is **remarkably similar**.

Possible anthropometric causes for this apparent indiscriminability of female and male Dutch vowels (apart, that is, from their length) are mooted in Henton, 1983. The connected implication, viz. the emerging belief that Dutch speakers (and most especially Utrecht females and males) speak in an exceptionally similar manner, are further discussed in that study, where it is revealed that Dutch females and males differ the least according to stature.

7.4. The French data

The acoustic data which has been published for French up to now (Carton, 1976; Landercy and Renard, 1977; Mettas, 1979) was considered unsatisfactory for several reasons. The reasons have been expressed in detail in Chapter 4, Section 10, but may be helpfully summarized here:

1. The data for males (presumably - see below; Carton, 1976; Landercy and Renard, 1977) and for females (Mettas, 1979) were assembled from different

populations, using different experimental methods at different times and locations, and by different investigators.

2. The Carton and Landercy and Renard sets of data are minimal in their detail. The authors do not state even what the sex of their informants is, although it seems safe to apply the experimental default and assume they were male. The authors do not enumerate subjects, nor describe their dialect.
3. The Landercy and Renard data are rounded too grossly (to the nearest 50 Hz).
4. There is no reassurance that the Landercy and Renard data are not from speakers of Belgian French.

These reasons made it clear that the above data could not be used for our purposes. Fortunately, however, more reliable acoustic data for Parisian French has been assembled recently by Lonchamp (forthcoming). The data and figures will be published in a CNET/ENSET monograph to appear in 1986, but Lonchamp has generously allowed the prior use of these data for the testing of our normalization hypotheses.

Lonchamp's data are expressed as **median** values, although he also states that **mean** values are very similar. The corpus contains the ten Parisian French oral vowels, but in slightly differing consonantal contexts. A word list containing the carrier words possibly used by Lonchamp appears as Table 7.IX.

Table 7.IX. Elicitation material for French vowels
(isolated words)

Word	Vowel elicited
pire	i
Pépé	e
père	ɛ
par	ɑ
porc/port	ɔ
peau	o
pou/poux	u
pu	y
peut	ø
peur	œ

Lonchamp (private correspondence) points out that the word list does not provide a balanced sampling of French vowels because the investigators had to use either a /pV/ or a /pVR/ frame. The /pVR/ frame was needed for four of the vowels (viz. [i,ɛ,ɑ, œ]) to lengthen them in order to provide enough sampling points for the investigators' chosen method of formant measurement. The effect of /R/ seems to be to shift F1 upwards for vowels with a low F1 and F2, and downwards for vowels with a high F2. This slight variability induced by consonantal context will have affected the formant measurements for the four vowels mentioned, but the data are nevertheless more reliable than those previously described.

Lonchamp's data were recorded by ten males and nine females, with each token repeated twice by each subject. Table 7.X contains the median values for the ten Parisian French oral vowels, converted to Bark.

Table 7.X. Median values (Bark) for Parisian French oral vowels

Vowel		Male	Female	Difference (f-m)
i	F1	3.00	2.98	-0.02
	F2	13.31	14.40	1.09
e	F1	3.53	4.00	0.47
	F2	12.98	14.13	1.15
ɛ	F1	5.00	6.07	1.07
	F2	12.11	13.36	1.25
ɑ	F1	6.26	7.06	0.80
	F2	10.17	11.22	1.05
ɔ	F1	5.01	5.87	0.86
	F2	8.50	9.59	1.09
o	F1	3.69	4.40	0.71
	F2	7.09	7.54	0.45
u	F1	3.06	3.03	-0.03
	F2	6.88	7.17	0.29
y	F1	2.92	2.97	0.05
	F2	12.23	13.25	1.02
ø	F1	3.68	4.47	0.79
	F2	10.82	11.66	0.84
œ	F1	4.89	5.97	1.08
	F2	10.70	12.00	1.30

There is an important feature to note in Table 7.X. Examining the column of female-male differences, we see that for the close vowels [i, u, y] there is a **negative** or **very small difference** between the females' and the males' vowels in the F1 domain. Now, this is a phenomenon we have encountered before. It will be recalled from the analysis of the British English data (Chapter 6, Section 6.3.3), that particular difficulty was found in measuring the first

formant of the females' close vowels [i:, ɪ, u:, ʊ]. To resolve obvious acoustic misrepresentations (that is, of the females' vowels appearing to have lower F1 values than the males' vowels because of the measurement difficulty), an algorithm was employed for the measurement of F1 of the close vowels in the British English data.

In this Parisian French data there seems to be evidence of a possible similar measurement problem for F1: see Figure 7.5a. F1 for close vowels is low in frequency and very susceptible to being masked by the fundamental component of the spectrum. This likelihood is strengthened in females' close vowels because (a) the F0 is high, and (b) they may have a breathy voice quality which will increase the amplitude of the F0 and thus make it the strongest component in that part of the spectrum. To see F1 in females' close vowels is thus made doubly difficult. From the disparities between the females' and males' values for F1 for the French vowels [i, u, y], it seems reasonable to infer that Lonchamp has also met this problem. We are presented with results which countermand the general expectations of male-female differences in French, as illustrated by the other vowels.

Naturally, the inclusion of these 'suspect' F1 values for the close vowels will affect the overall average amount of normalization calculated for the French oral vowels in toto. Our solution to this is to expand the usual format of the summary table which shows the amount of normalization required for each language/dialect. Table 7.XI therefore

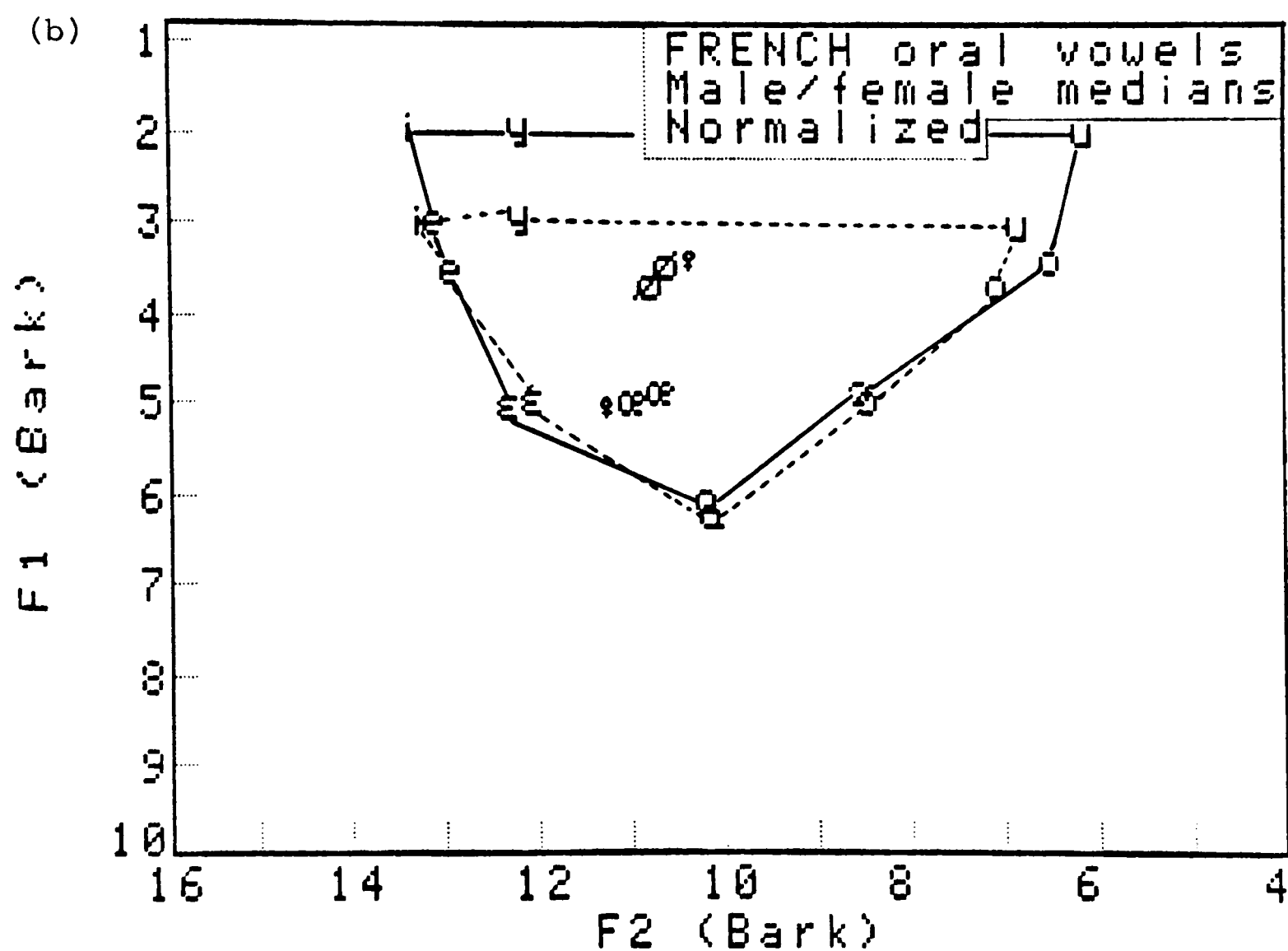
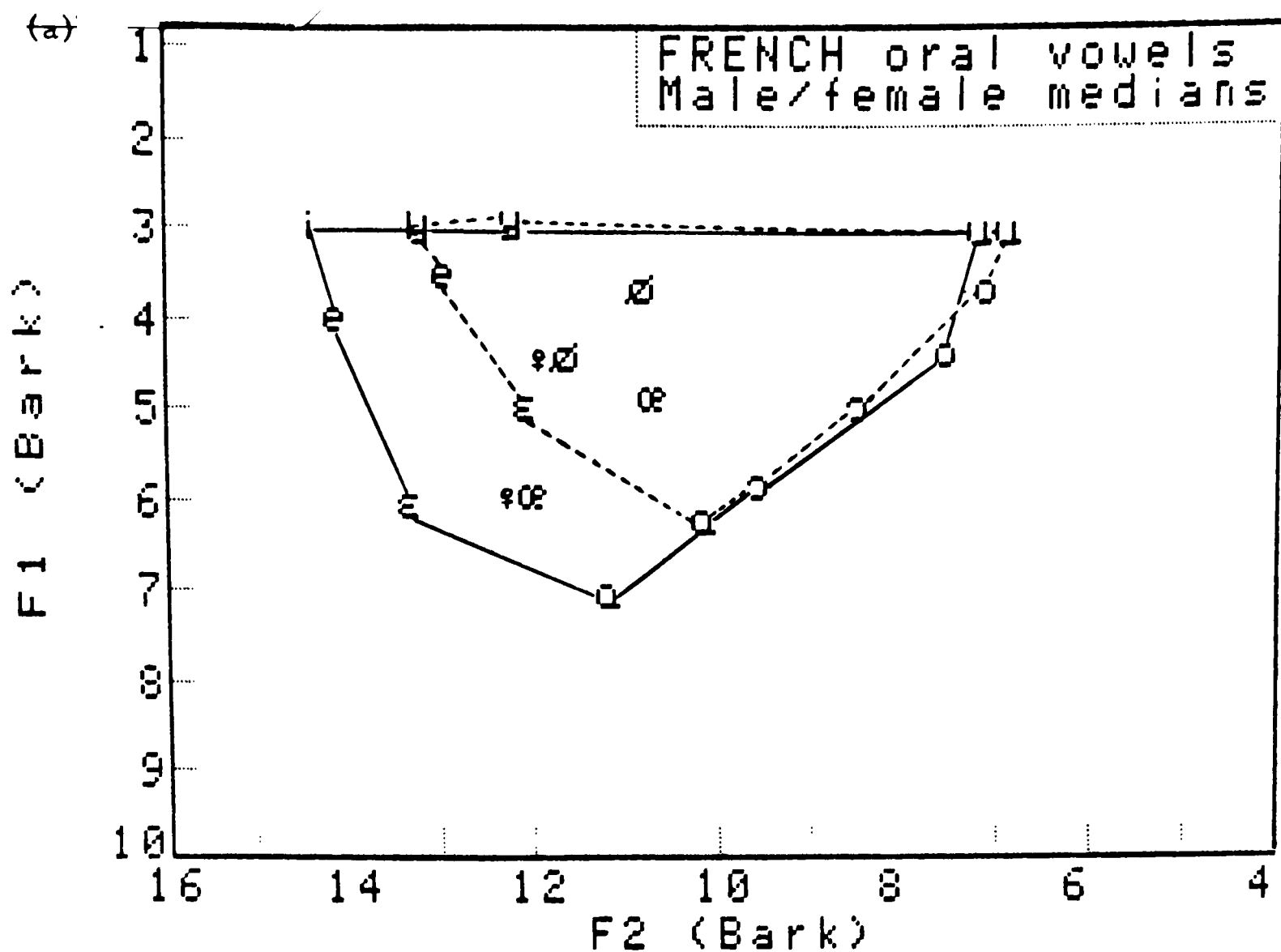


Figure 7.5 (a) Mean formant values (Bark) for citation forms of French.
(b) Mean formant values, with female values **normalized** (Bark) for citation forms of French.

presents an overall normalization amount based on the median values for all ten French vowels, and then shows an amount which has been calculated after omitting the F1 values of the close vowels. The latter result is considered to reflect the normalization required for French females to males probably more accurately. Figure 7.5b then provides further representation of the effect of normalizing the French data by 1 Bark.

Table 7.XI. Overall amount of normalization required (Bark) for Parisian French oral vowels

Formant	Grand Mean (from Table 7.VII)	Overall Normalization (F1+F2)/2
F1	0.578	0.77
F2	0.953	

Overall amount of normalization required (Bark) for Parisian French oral vowels, excluding F1 values for close vowels

Formant	Grand Mean (from Table 7.VII)	Overall Normalization (F1+F2)/2
F1	0.826	0.89
F2	0.953	

7.4.1. Observations from Figure 7.5

The particular characteristics notable in the the vowel diagrams for French include:

1. The unusual linear connection between the close vowels. F1 hardly varies in [i], [y] and [u] in the

females' vowels, forming a pleasing quasi-cardinal alignment.

2. French females' [o] is comparatively less close than the males'.
3. There is negligible difference between the females' and males' [u] vowels, for the reasons discussed above.
4. French females' [ø] is marginally more open than the males' vowel, and the females' [œ] slightly more close. These differences are, however, trivial.
5. The majority of the vowels are relatively well normalized by a 1 Bark shift. The apparent inappropriateness for the close vowels may stem not from their non-uniform behaviour, but from the measurement problems in the original data.
6. Excluding the close vowels, there is little evidence for the females' vowels to be more peripheral than the males'. The only vowel for which females' articulation appears more extreme than the males' is [o].

Thus, from the French results, the support for our hypotheses is:

- 7.4.2. A **small negative** female-male difference remains after normalization by 1 Bark. This difference (excluding the problematic close vowels) is 0.89 Bark.
- 7.4.3. In French, females' speech appears to be **slightly more like** males' than their vocal tract

physiologies would predict.

7.5. Implications of Figure 7.6

The general picture which has emerged is that most of the tested languages/dialects need less than 1 Bark to align female with male vowels. This conclusion is summarized by Figure 7.6, which also includes the results from the British English data. The figure illustrates how the amounts of inter-sex normalization required varies across not only dialects, but across languages too. Greater discussion of the ramifications of Figure 7.6 will be found in Chapter 8.

7.6. Female speakers as more 'open-mouthed' than males?

In the course of discussions in the last two chapters, indications of a certain kind of sex-specific articulation have appeared, namely that females in some speech communities maintain a more peripheral articulatory posture than do males. This tendency was first reported by Labov (1972: 304) in connexion with his sociolinguistic work on the East Coast of the United States:

The sexual differentiation of speakers is... not a product of physical factors alone...but rather an expressive posture which is socially more appropriate for one sex or the other. On Martha's Vineyard, men are more 'close-mouthed' than women, and use more contracted areas of phonological space;...women in New York City and Philadelphia use wider ranges of phonological space than do men.

Labov's comment, that it should not be expected that the linguistic "expressive posture" adopted by women and men should be the same from one social culture to another, may

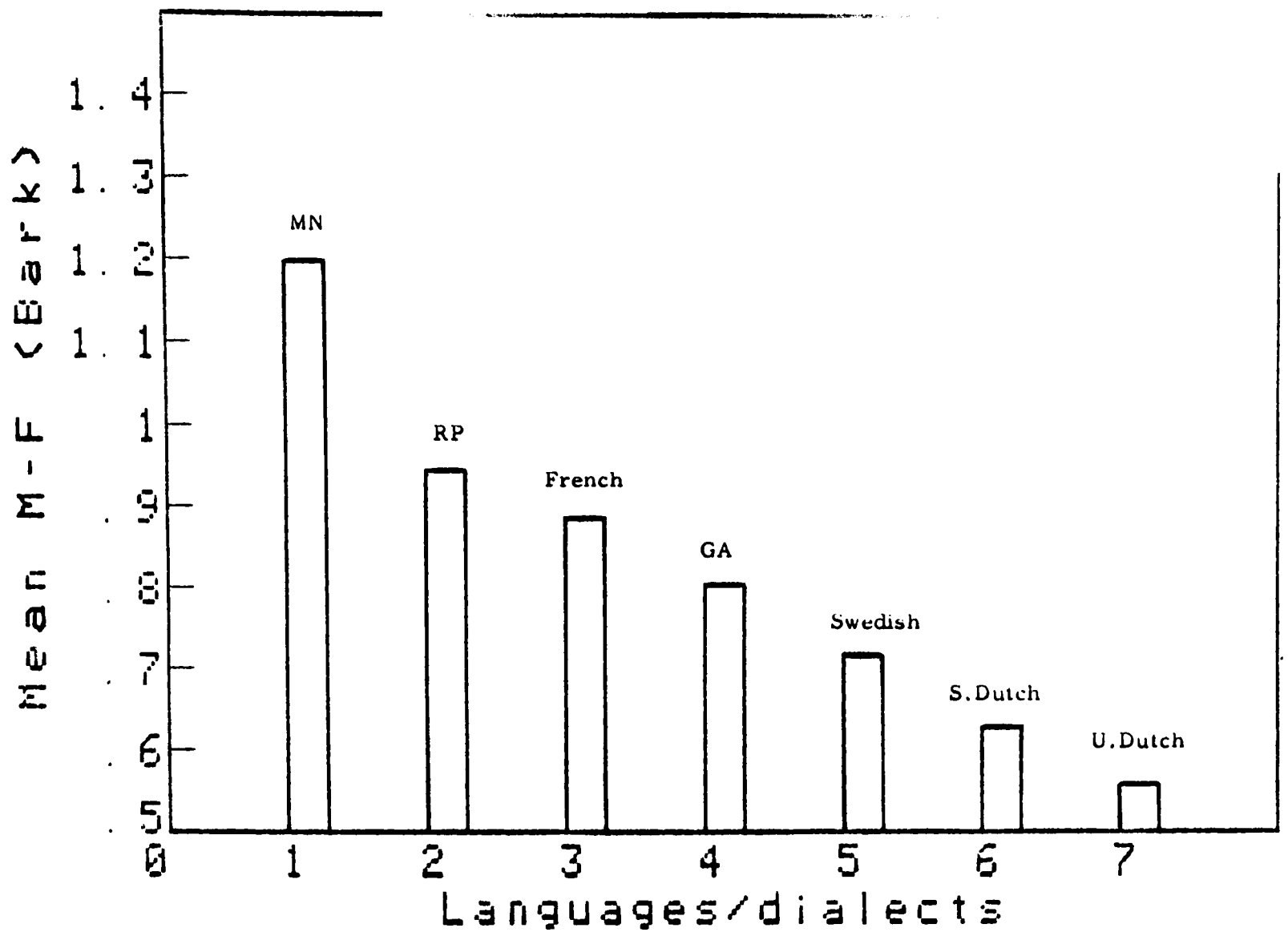


Figure 7.6 Optimum male/female vowel normalization (Bark) for seven languages/dialects. The data for RP and MN are my own; the remaining sources are described in detail in Chapter 7.

be seen as axiomatic by now. Whether "wider phonological space" may be interpreted as meaning the same thing as more open-mouthed is not altogether clear. However, at the least, we would expect from this wording that the overall (both open-close and front-back dimensions) vowel space exploited by women would be larger.

Labov's finding received further support from Goldstein (1980: 232), who explains her vocal tract modelling results by proposing that:

... it would appear that, wherever possible, women tend to use articulations which produce a somewhat wider vowel space than the ones used by men. This wider vowel space does not appear to be dictated by the constraints of anatomy.

So, the two investigators both seem to be indicating a learned, sex-specific articulation attributable to socially-conditioned speech habits.

Since Goldstein's description seems to the more easily interpretable in precise articulatory terms, we shall start here. Assuming that 'wider' and 'more peripheral' are equatable (as seems plausible), is there support for this interpretation in our cross-language data?

Generally, the answer appears in the affirmative. Reviewing the figures in this chapter of mean male and female normalized vowels as represented in the F1-F2 domain, we may observe that for Modified Northern British English, General American, Swedish, Standard Dutch and French, the females' vowel quadrilaterals remain **more peripheral** than the males'

after normalization. Utrecht Dutch is unique in that the females and males' vowel quadrilaterals are so little differentiated **before normalization**, and the apparent peripherality of the close vowels is a reflection rather of over-normalization than originally different articulatory postures. Frequently, then, females articulate with a more peripheral vowel space than do males. This seems to be due to a combination of phonetic factors: females' overshoot of [u:]; a tendency for females to have an opener [æ], and an apparent tendency to have closer close vowels.

We may thus conclude that we find widespread support for Goldstein's general assertion in the languages reported in Chapter 7.

The second question to ask is whether Labov's specific term 'open-mouthedness' is supported by our data? It would seem reasonable to interpret 'open-mouthedness' as equivalent to an opener jaw or lowering of tongue height, which would result in an increase in F1 frequency. From a further review of the relevant figures, it is clear that the females' peripherality is not especially concentrated in the F1 direction. This is true of GA, Swedish, and Standard Dutch. However, important exceptions to this are the British accents RP and MN.

Let us look at these British English exceptions in more detail. We refer to Figure 6.11, of the British English RP and MN mean male and female normalized vowels. In RP, we

may observe that after normalization by one Bark, the vowels [i:, e, æ, ʊ] and, to a limited extent, [ɔ:] appear to remain 'more peripheral' than the males. That is to say, the females' normalized vowel quadrilateral extends beyond the males' but only in the F1 dimension for certain vowels. It would therefore be more appropriate to describe this articulatory habit specifically as 'open-mouthedness' than generally as 'wider'. This RP figure may not be said to provide overwhelming evidence for our point. It must be recalled, however, that RP was the accent which responded best to a one-Bark normalization, and therefore the least amount of socially-motivated sex-distinctive "expression" is to be expected here.

The Modified Northern data, by contrast, emerged as that which required the largest amount of female-male normalization across our selected languages/accents. In Figure 6.11, therefore, there is much stronger indication for speech habits which are not accounted for by anatomy alone.

A closer inspection of the data in Tables 6.VI and 6.VII, and of the summary Table 6.VIII, reveals that, within the global discrepancy between the two British English accents of 0.25 Bark, much the larger contribution is made by the F1 values rather than the F2 values. With the aid of Figure 6.11, this same tendency can be detected: MN males and females differ from each other, in Bark space, more in the F1 dimension. Differences in a traditional 'openness'

direction can be inferred most clearly from the MN data in Figure 6.11. To a first approximation, an F1 frequency increase correlates with an increase in the cross-sectional size of the main oral constriction of the vocal tract (Fant, 1960). This implies also an increase in perceived 'openness' of the vowel (Traunmüller, 1981). So it would be possible to conclude that MN males and females do indeed differ from each other more in what might crudely be termed 'open-mouthedness', than RP speakers do.

With respect to Goldstein's (ibid.) findings, then, only a part of her statements are supported by the British English data. It would seem an improvement to describe the female speakers in these two accents as being more open-mouthed than maintaining a more peripheral vowel space.

By examining our own cross-language data we have thus been able to tease apart Goldstein's terms 'wider' and 'more peripheral' from Labov's 'open-mouthedness'. We have shown that there is evidence of a sex-specific expansion in one articulatory direction, but not necessarily in the other according to accent/language. We may thus conclude that Labov's term 'open-mouthed' as applied to females is somewhat **accent-specific**, and while it is true for the female speakers of Martha's Vineyard, Philadelphia, New York, Modified Northern and, to a lesser extent, RP, it is not appropriate for females speakers of GA, Swedish, Standard Dutch or French. The female speakers of the latter four languages would be more accurately described as

speaking in a more peripheral manner than do the males.

The ramifications of some female speakers speaking more peripherally are interesting when placed in the context of arguments about the relative intelligibility of the two sexes. We do not wish to grapple with this well-contested issue at length: there is, nevertheless, an interesting inference which occurs to Goldstein (1980: 235):

...women are trying to compensate for the undersampling of the vowel spectrum and possible loss of intelligibility caused by their higher fundamental frequency.

There is immediate counter-evidence to this supposition, which Goldstein also provides. She cites Chen (1980), who found that speakers tend to use **higher than usual fundamental frequency** when trying to increase their intelligibility.

The second turning for Goldstein is to connect womens' "wider vowel triangle with their tendency to speak more clearly or carefully" (c.f. Labov's (1972) and Trudgill's (1974) results), and thus avoiding phonological changes which occur in casual speech. There is, however, much conflicting evidence about whether women emphasize acoustic cues, and the results of the experiments here show (a) insignificant differences between speech styles by **both sexes** in British English and (b) only more **openness** for some languages, not an overall increase (i.e. greater "width") in phonetic space.

The insubstantiability of Goldstein's remarks apart, they

nevertheless indicate useful avenues of further research into the question of whether one sex is intrinsically more intelligible.

7.7. Conclusion

In this chapter, the appropriateness of a female-male normalization by 1 Bark has been tested on data from five further languages/dialects. The two hypotheses posed at the outset of the chapter have been thoroughly borne out. Across languages, the disparity between normalized females' and males' speech varies from comparatively little (say, French) to quite substantial (say, Utrecht Dutch), see Figure 7.6. We find in fact that **females and males tend to speak more like each other on the whole than their vocal tract physiologies would predict.**

En route to these conclusions, we have observed that other studies suffer from similar measurement problems as afflicted our own. Unfortunately, these other studies did not necessarily take steps to resolve the difficulties, leading in some vowel-specific cases to improbable female-male normalization results.

Against the background of our argument for a uniform normalization in auditory space, a qualification needs to be made. There is a tendency (in GA, Swedish and, perhaps, Utrecht Dutch) towards quite a large discrepancy in the normalization required for F1 and F2. In these languages,

F1 needs less normalizing than F2, which generally approaches a one Bark value. Thus for certain languages/dialects, the concept of a uniform auditory normalization remains valid, whereas for others (those mentioned here) it needs to be tempered somewhat. This finding was shown to be connected to the question of 'open-mouthedness', which was found to be only truly appropriate for the British English data. In other languages, it is more exact to describe females as maintaining a more peripheral vowel space.

Chapter 8 now advances the discussion of socio-phonetic and linguistic inferences from our experimental results, by incorporating a consideration of non-vocoid segments, and possible sex-differentiated voice qualities.

Chapter 8

Expanding the examination of phonetic sex-specific differences

In accordance with the research objectives, we have so far displayed extensive evidence for phonetic sex-specific differences existing (to differing degrees) in several accents/languages. This chapter seeks to take these observations further by incorporating:

1. A constructive critique of some the findings of Chapters 6 and 7, indicating how they may be developed more widely.
2. An examination of phonetic segments other than vowels for indications of sex-appropriate behaviour.
3. Some proposals for investigating suprasegmental sex-specific differences.
4. An example of differential voice quality usage by the sexes.

8.1. Future work implied by the results of Chapters 6 and 7

In Chapter 6 it was indicated that the new methodology used in this study has very considerable potential for exploitation of other socio-phonetic comparisons across speakers and across accents. There are many fruitful routes for future research. A few are outlined below.

8.1.1. Other accents as objects of comparison

Some discussion about the intrinsic problems of investigating the Modified Northern accent appeared in Chapter 6, Section 17. A possible way of circumventing this obstacle (viz. too great an internal variability) would be

to compare RP with a more stable and homogeneous accentual group than MN: e.g. unmodified Liverpool or Manchester. Again, it would be necessary to ensure that the accent chosen had a recognizable acrolect, in order for it to be commensurable with RP.

8.1.2. A closely-controlled foreign language as an object of comparison

An alternative further step would be to investigate a foreign language for phonetic sex-differences, according to the methodology described in Chapter 4. The same rigorous selectional criteria would have to be applied to the collection of that data, a pre-requisite which effectively means collecting the foreign language database de novo, because little, if any, attention is given to the insurance of sociolinguistic homogeneity in existing studies.

From the preliminary indications gained in Chapter 7, French or Dutch would offer interesting possibilities for reinforcing the finding that male/female vowel differences are socially conditioned, but differently in those languages from English. An examination of an ethnic minority language of Britain, such as Punjabi or Hindi, would also generate worthwhile sociolinguistic hypotheses within our paradigm.

8.1.3. The influence of researcher-sex on subjects' speech

The interaction of the sex of the interviewer with the interviewee (referred to in Chapter 2, Section 2.2.5, and

Chapter 4, Section 9) is still, as Smith (1985: 82) concludes, a variable of "unknown quality and quantity". To duplicate my experiment, using a researcher of the opposite sex (viz. male instead of female) may indeed reveal interesting and possibly significant interactional effects (c.f. Markel, Prebor and Brandt, 1972).

8.1.4. Follow-up routes to the provisional findings concerning F0 differences

The preliminary results for F0 in the British English citation forms lead to several possible developments:

(a) Intra-vowel variability with regard to F0

Previous work has indicated that some vowels do have intrinsically higher/lower F0s. Is this consistent across sexes and across accents?

(b) Evidence from long-term averages of F0

Citation forms can only be regarded as a very superficial indication of possible pitch differences within and across sexes/accents/languages. In order to quantify pitch differences reliably, recordings of speakers should be of at least of 40 secs. duration (Nolan, 1983: 123). A comparison of long-term F0 across accents and sexes is desirable, especially in the context of the current requirements of the speech technology industry. The proposed contribution could be to assemble the closely-controlled data from which to obtain these comparisons and to verify whether there are pitch differences which extend beyond the physiological determinants and are a product of sociological role

expectations.

(c) Dynamic sex-based features of F0

Further advantage should be taken of the data assembled from Exercise 3 of the British English recordings. A comparison of pitch variation in rising and falling tunes would add the much-needed empirical support to the body of work which has already speculated that female speakers exploit more dynamic pitch contours within a given pitch range (see, for example, Terrango, 1966). Furthermore, it has been suggested, not uncontroversially, that women employ a greater pitch range with greater variability than do males (Herbst, 1969; Takefuta et al., 1971; McConnell-Ginet, 1978). Such differences may also be specific to accent. However, this is an area of research where assertions are rife, but empirical demonstration is markedly lacking. A position has now been reached where these enquiries may be advanced.

More suggestions relating to the investigation of long-term average F0, and dynamic sex-based features of F0 are taken up in Section 8.3.

8.1.5. Contextual variation in speech

The limitations of the work on vowel quality changes in different linguistic contexts were apparent from Chapter 6, Section 13. Too often in the past (and this study is not immune from this criticism) experimental research on language has concentrated on a somewhat artificial style of 'laboratory speech', and indeed on words in citation or in a

uniform (and often contrived) context. Actual language use is of course much less homogeneous. Spontaneous versus rehearsed speech, fluent versus hesitant, rapid versus slow, and formal versus colloquial, these are just some of the dimensions of variation upon which basic, investigative work is sorely needed.

8.1.6. Other spectral attributes

To examine the **spectral tilt** of female and male vowels would possibly yield further evidence of sex-differentiation. The prediction would be that the angle of tilt of the spectrum, as it increases in frequency, would be somewhat steeper for females than for males. There are indications from a study by Monsen and Engebretson (1977) that the decrease is approximately -12dB per octave in male vowels, but -15dB in females' vowels.

Inter-sex differences of formant **bandwidth** may also be posited, with females' bandwidths being wider than males' (Fant, 1979). It is not known yet whether this difference is perceptible.

These then, are a few envisaged developments of the work on vowels and F0. But what if we extend the horizon further from vowels alone, and start to explore languages for other sex-linked segmental behaviour? This expansion is adumbrated in the next section.

8.2. Further segmental examples of possible sex-linked differences

The comments (some rather speculative) contained in this section are founded on Table 8.I, which has been constructed from discoveries in phonetic and phonological literature dating from the beginning of the century. It does not claim to be exhaustive, but rather serves as an indicator: a basis for hypothesizing worthwhile (or, conversely, sterile) avenues of future work. The table follows the IPA convention in its listing of the manners of articulation for consonants.

A cursory glance at Table 8.I gives us to believe that certain classes of sound have been, or are more likely to be, exploited as phonological sex-markers than others. We have already shown earlier that **vowels** have been the segments most thoroughly investigated, both by sociolinguists (see Table 3.II), and in phonetic studies concerned with male-female normalization (see Chapter 5, Sections 5.3 to 5.5). These types of study are thus omitted from Table 8.I.

Table 8.I. Segmental phonetic/phonological differences,
 according to sex of speaker

Segment	Language	Source
Plosive	Cham	Blood (1962)
	Chuckchee	Bogoraz (1922)
	Eskimo	Boas (1911)
	Gogo-Yimidjir	de Zwaan (1969)
	Gros Ventre	Flannery (1946)
	Scot. English	Romaine & Reid (1976)
	Yana	Sapir (1929)
	Yukhaghir	Trudgill (1974)
Nasal	Bengali	Chatterji (1921)
	Yana	Sapir (1929)
Lateral (incl. lat.fricts.)	Belfast English	Owens (1977)
	Canadian English	Sankoff & Cedergren (1971)
	Chuckchee	Bouda (1953)
	Kolymskij Russian	Bogoraz (1901)
	Koryak	Stebnickij (1934)
	Russian & Ukrainian dials.	Serech (1952)
	Yana	Sapir (1929)
Trill	Chuckchee	Bogoraz (1922)
	Danish	Jespersen (1922)
	French	Jespersen (1922)
	Panamanian Spanish	Cedergren (1970)
Flap/tap (post-vocalic /r/)	Amer. English	Levine & Crockett (1966)
		Anshen (1969)
		Cook (1969)
		Wolfram (1969)
		Labov (1972)
	Scot. English	Macaulay (1978)
		Romaine (1978)
Fricative	Amer. English	Schwartz and Rine (1968)
		Anshen (1969)
		Wolfram (1969)
		Labov (1972)
		Fasold (1972)
	Belfast English	Milroy (1980)
	Chuckchee	Bogoraz (1922)
	Castilian Spanish	Lorenzo (1966)
	Panamanian Spanish	Cedergren (1970)
	Yana	Sapir (1929)
	Yao	Jespersen (1922)

Table 8.I continued		

Approximants & semi-vowels	Cham ?S. Brit. English Norwich Brit. Eng. Japanese Yana	Blood (1962) Edwards (1903) rep. by Jespersen Trudgill (1974) Edwards (1903) rep. by Jespersen Sapir (1929)
Ejective Implosive Click		
Consonant clusters	Amer. English	Wolfram (1969)

While we have indicated that male-female differences in vowels can be hypothesized to exist in every language, there are several important reasons why we cannot make that assumption straightforwardly for the consonants. These reasons include:

8.2.1. The relative paucity of investigations into consonants, as compared with those for vowels. The most probable reason that there are no entries under the categories Ejective, Implosive and Click is their comparatively rare occurrence in the phonological inventory: sex-differences may exist, but the segments themselves have not been sufficiently documented for such occurrences to be noted.

Of those apparent differences appearing in Table 8.I, furthermore, some few may be attributed to ethnological peculiarity rather than variation by sex per se. Jespersen (1922) describes Yao, for example, where women used to

insert a large wooden disk within the upper lip, which made it impossible for them to pronounce [f]; as it is the women who teach the children to speak, the sound [f] disappeared from the language while the custom prevailed!

Other differences may be little more than casual observation. The remarks about Southern British English and Japanese by Edwards (1903: 79) and reported by Jespersen (1922: 243), for example, fall into this category. They say nothing more than that "in Southern England the sound /m/ is scarcely ever pronounced except in girls' schools", and that Japanese women use atashi (as opposed to male watashi) to express the first person. Jespersen interprets this as "an example of the liberties which (the) women may take"!

8.2.2. The relative linguistic functions of vowels and consonants across languages indicates that vowels exhibit the most individual speaker characteristics. Thus speaker-sex (along with tone, affective state etc.) is more likely to be shown by vowels. Paralinguistic information is also most frequently conveyed by vowel-like sounds, e.g. exclamations, expressions of pain, surprise, anger and so forth. Consonants, on the other hand, are much more language-specific, and are used for the conveyance of linguistic information.

8.2.3. Certain consonants, e.g. front fricatives and many plosives, including non-pulmonic ones, are unlikely to have vocal tract resonance properties which would show acoustic

spectral differences of a sex-linked kind. The sex-specific behaviour of fricatives has been examined acoustically to a limited extent by Schwartz and Rine (1968); Ingemann (1968) and, more thoroughly, by Bladon (1985). Their results indicate that different fricatives seem to carry differing amounts of sex-specific information.

Recent work by Weeninck (1985) suggests that male-female differences in the production of plosives bursts are not to be expected either (except, perhaps, in the case of /k/). Burst spectra templates are too 'gross' to allow for much male-female differentiability. Furthermore, plosives vary quite considerably according to neighbouring vowels: they are thus too variable in their realizations to be a robust cue to speaker sex.

8.2.4. Other consonants are unlikely to show a sex-specific difference because there is already a great deal of **individual speaker variation** in their production. This argument applies to nasals, where individuals' nasal structures and degree of constriction may vary greatly.

8.2.5. Finally, the nature of consonantal sex-specific differences in Table 8.I is not at all regularly patterned. Females' realizations may differ from males' according to place of articulation, or manner of articulation, or direction of the airstream, or even voicing, or any combination of all four parameters. Thus we find, for instance, among the laterals great heterogeneity in the

sexes' productional behaviour across languages. In Belfast English, there is a complex interaction in the use of 'clear' (palatalized) [l̥] and 'dark' (velarized) [ɫ] between the sexes, which also interacts with age and speech style. French Canadian women pronounce /l/ in pronouns and articles more frequently than do men: thus /l/ alternates with ø (Sankoff and Cedergren, 1971). In Chuckchee (Bouda, 1953) and Koryak (Stebnickij, 1934), females realize liquids as sibilant affricates. Meanwhile, Bogoraz (1901) reports female speakers of Kolymskij Russian (spoken in North-Eastern Siberia) as supplanting all /l/s and palatalized /r/ by /j/. In other scattered Russian and Ukranian dialects, females lose the velarization of /l/ (Šerech, 1952). Lastly, Sapir (1929) records that female speakers of Yana assimilate following open vowels, if they are preceded by a liquid. Sapir's transcriptional technique makes it difficult, however, to determine whether the quality of the /l/ itself is different for females. For further demonstration of the heterogeneity of articulations within IPA class, the reader is referred to the sources given in the table.

From these five points, then, we may conclude that future investigations of consonants as indicators of sex-differentiated pronunciation will need to be language-specific. Such work may, with a few exceptions (such as sibilants) be negative in its findings and rather unrewarding because of the acoustic structure of the

consonant classes in question.

8.3. Suprasegmental sex-specific differences: some proposals

Six years ago, Smith (1979: 122) indicated the richness of rewards to be gained from examining the suprasegmental domain for sex-linked behaviour:

Their study promises to find enthusiastic support from those interested in sex and language, for many argue that non-segmental features are the most recognizable and informative clues to speaker sex within a given language.

Surprisingly little research has been initiated, however, for various reasons which we will expand on here.

The most obvious area of suprasegmental diversity between the sexes is that of pitch, or speaking fundamental frequency (SFF). Topics which arise rather specifically out of our own enquiries have received some attention in Section 8.1.4, above, but there are many broader possible routes of exploration. The kinds of empirical question which may be asked are:

1. Do speakers of different languages adopt a sex-determined SFF which is significantly higher/lower than their vocal anatomy would predict? Is this difference, furthermore, of differing amounts across languages?
2. Is a certain long term SFF associated with a sex, as interacting with socio-economic status?
3. Do speakers of either sex alter their SFF by differing amounts according to register, or speech style?

4. Do males and females use significantly different SFF when speaking to children, and is the sex of the child also an important variable?
5. Do pre-adolescent children exhibit sex-linked SFF behaviour? If so, how early may it be detected?

As was shown in Chapter 1, Section 7, preliminary answers to these sorts of question are starting to appear, e.g. de Pinto and Hollien, 1982 on question 1; Lieberman, 1967, on question 4; Weinberg and Bennett, 1971; Meditch, 1975 and Bennett and Weinberg, 1979, on question 5. Concerning questions 4 and 5 in particular, researchers find that childrens' sex is reliably identifiable **without** the presence of F0, implying that "vocal tract resonance properties of pre-pubertal speakers provide primary cues about sexual identity (Bennett and Weinberg, 1979: 188). Hence, the **development** of sex-related **laryngeal behaviour** is beginning to be charted. Although the answers to our questions tend to be affirmative ones, these endeavours should be made more coherent, so that we may begin to form a unified view of the social sex-linked nature of SFF.

Further suprasegmental areas of research into sex-specific behaviour include vowel length; intensity (loudness); speech rhythm and stress (both word and sentence) and speech fluency/hesitancy. These areas do not however seem to have attracted much experimental investigation, as Crystal (1975: 85) observed:

It is probable that there are important non-segmental

differences between the speech habits of men and women in most languages, though very little data has been analysed from this point of view.

Crystal cites anecdotal observation of male-female suprasegmental differences such as the phrase 'stop clucking like an old hen' and the glissando effects, complex-tone usage, breathiness and moving to falsetto, all involved in the production of so-called 'simpering' voice of effeminacy in English.

An early, and probably not entirely reliable, comment about comparative vowel length by Jones is reported by Jespersen (1922). Jones apparently claimed that whereas RP males tended to produce /sɔ:ft/ and /gə'l/, women pronounced these words as /sɒft/ and /gæəl/. Searching for quantifiable correlates of stereotyped judgements about speakers' voice quality, Aronovitch (1976) found that his parameters of 'Intensity average; Intensity variance; Rate; Fundamental frequency average; Fundamental frequency variance and Sound-silence ratio' were employed differentially by raters for the two sexes. His goal however, was to explore primarily whether there are any reliable connections between voice quality and stereotyped judgements, and not to examine diverse suprasegmental behaviour of the sexes. Unfortunately, his findings have yet to tantalize other researchers into further work along these lines.

Intensity per se has been examined to a limited extent. Markel, Prebor and Brandt (1972) revealed that males spoke

with greater average intensity in interpersonal communication than did females, although both sexes address a member of the **opposite** sex with greater intensity than a same-sex member. A study by von Raffler-Engel and Buckner (1976) showed that women raise their intensity more than men when compensating for external noise. Thorne, Kramarae and Henley (1983) provide brief summaries in their annotated bibliography of the few studies which have been conducted into sex-linked verbal fluency in adults. Smith's main point still stands, however, when he remarks that there are "only two paralinguistic features (viz. loudness and speech fluency, my brackets) for which there is even a modicum of data" (1979: 125).

In relation to word and sentence stress, there is the usual array of subjective remarks. Jespersen (1922: 243), for example, claims that "exaggeration of stress" is a conspicuous characteristic of female speech; and his intuitive successor, Lakoff (1975: 56) states that "Women speak in italics". As far as we are aware, though, no specific **empirical** sex-linked investigation of stress is known as yet.

Sex-based intonation tunes have attracted attention, but too often in an unsystematic way, with little use of experimental measurement and too great a reliance on subjective judgement (see Key, 1972; Brend, 1975, and McConnell-Ginet, 1983). Better results are provided by Pellowe and Jones (1978), who showed that women used more

rising tones than men in Tyneside speech. Women also exploited a greater variety of intonational tunes. Results assembled by Elyan (1978) for female and male students in Bristol lend more support to this observation. Edelsky (1979) reports that women used more complex intonation patterns when interacting with a female interviewer, thus indicating a listener-orientated form of intonational variation. More extensive exploration of these tendencies, across accents, is still required, though. In general, intonational studies have made use of impressionistic description, but any cross-language study would, however, be difficult because of the widely varying descriptonal conventions used for the intonational repertoire of a given language.

Lastly, and in relation to suprasegmental variation, we may pick up a category which was introduced in Chapter 2, namely Pronunciation Dependent on Sex of Person Spoken To (see Table 2.II). There we indicated that such differences may be implicit in every social, linguistic interaction, i.e. that the speaker is aware of the sex of the listener in the majority of cases, and accommodates accordingly to that sex. The examples we are able to give under this heading largely appear in the suprasegmental rather than the segmental domain. Apart from the four original ideas already incorporated in Table 2.II, we may mention an explicit case in Yana (Sapir, 1929). In this language, male-male interactions are conducted with both speaking "fully and

deliberately", whereas when women are involved, "a clipped style of utterance" is used. Interrogation is also indicated differently by the two sexes in Yana: women lengthen final vowels, but men add a segmental suffix, /-n/. Whether this truly counts as a suprasegmental disparity, however, depends on the descriptive argument about whether length is a nonsegmental feature. Finally, **tone** is used in Chichimeca (Driver and Driver, 1963) to separate males and females in the same family with identical names, and in Koasati (Haas, 1944) to distinguish between the sexes.

There are obviously many challenges still facing investigators, but we hope that these few suggestions may provoke empirical enquiries soon.

8.4. An example of differential voice quality usage by the sexes

Various patchy references to females and males having habitually different voice qualities can be found in the literature: they are usually tangential to another discussion and anecdotal. Two such examples may be given as illustrative. Concerning Swedish, Elert (1968: 57) remarks that ingressive voice is used especially by female speakers to utter the word ja. Bolinger (1980: 100) claims that in Arabic and Spanish, men manipulate their larynxes to glottalize their speech and exaggerate the 'macho' effect. What he is probably referring to is the phonetically more precise statement by Ferguson (1964: 274), that in Arabic **velarization** indicates masculinity (and its avoidance, the

opposite).

Batstone and Tuomi (1981:111) have recently been rather more specific. They state:

Much of what is perceived in a verbal message is influenced by the metalinguistic or paralinguistic aspects of language which include ... voice quality differences such as breathiness, harshness or hoarseness.

They investigated the perceptual characteristics of female voice from this premise, and we will return to their results in due course. But we may see immediately that if such claims are to be accepted about sex-differentiated voice quality, then they need to be substantiated by experimental investigation into their physical production. The results could then be valuable to many avenues of research, including clinical phonetics, speech therapy and the fields of speech synthesis and speech recognition. Just such empirical work, inspired by the enquiries of this thesis, was undertaken for the quality of **breathiness** in British English. Here we provide a summarized version of the findings, which are described in full in Henton and Bladon (1985b).

8.4.1. Breathiness in British females and males

From auditory impressions formed while analysing the large database described in Chapter 6, we believed that female speakers' vowels were more breathy than males'. This part of my study was initiated, then, by three motives:

1. To establish whether female speakers are physically more breathy than males in vowel production
2. To show that breathiness may be reliably measured acoustically
3. To highlight the normative/linguistic/clinical implications if female speakers are habitually more breathy than males.

The two accents, Received Pronunciation and Modified Northern were thus analysed using acoustic parameters. The acoustic measurement of breathiness chosen for this study was the same as that described by Bickley (1982: see Henton and Bladon, 1985b, for full justification), namely the difference in amplitude between the first and second harmonics of a vowel. This measure is shown for the four vowels of RP in Table 8.II.

Table 8.II Average difference in amplitude (dB) between the first and second harmonics in male and female speakers of Received Pronunciation.

Vowel:	/æ/	/ɑ/	/ʌ/	/ɒ/
Females	8.4	6.4	6.2	3.3
RP Males	0.98	0.77	0.16	0.39
	7.42**	5.63**	6.04**	2.91**

Difference in amplitude across all vowels (mean of means) = 5.5dB

** Significant according to a t-test, p<0.01

The differences by sex are all highly significant. The least dramatic result was for /ɒ/, but this is perhaps to be

expected given that this vowel is no longer really open in RP (see Henton, 1983, Fig. 2).

The results for RP therefore confirmed our impression that women's vowels are physically more breathy than men's. But would this confirmation extend to the second accent in the corpus, MN?

MN was investigated using only three open vowels because the open vowel /ʌ/ is not a native vowel in this accent (see Chapter 6.10). Table 8.III shows the results for MN.

Table 8.III Average difference in amplitude (dB) between the first and second harmonics in male and female speakers of Modified Northern.

Vowel:		/æ/	/ɑ/	/ɒ/
MN	Females	10.26	7.59	6.31
	Males	2.41	1.55	2.48
		7.85**	6.04**	3.83**

Difference in amplitude across all vowels (mean of means) = 5.91dB

** Significant according to a t-test, p<0.01

These results showed the same significant trend as for RP. Moreover the mean difference between the sexes was clearly very similar in the two accents.

It was thus concluded that there is evidence of breathiness in female speakers of two accents of British English; that it is consistent, and that it occurs to a much greater extent than in male vowels. Furthermore, the difference was such as could certainly be perceptible; for, in languages

which exploit a linguistic contrast of breathiness (versus modal voice), such magnitudes of difference do separate the two types of vowel (see Bickley, 1982: Table 2).

Now, breathiness is likely to impair the perceptibility of speech and may render it more monotonous. The apparent paradox of females using an inefficient mode of phonation bears some discussion. Women are, it seems consciously 'queering their own pitch'. We propose (Henton and Bladon, 1985b) that women do so because, at its simplest, they wish to sound 'sexy'.

The linguistic association of breathy voice with sexiness is well attested (Crystal, 1975: 85; Daniloﬀ et al., 1980: 175; Laver, 1980: 135). In an experiment by Tuomi and Fisher (1979), when male and female speakers were asked to produce a 'sexy' voice, they all lowered their SFF, decreased their speech rate and the **females** frequently injected breathiness into their voices. (Other strategies for sounding 'sexy' may also be available, of course. Austin (1965) asserts - from introspection? - that "high, oral and giggling sounds are appropriate for females in courtship, whereas males produce low and nasal sounds." In addition, Batstone and Tuomi (1981) were surprised to find that the stereotypes of connection of "lowness and breathiness with sexiness" were not found in their later study of Canadian speakers: they suggested that, because their speakers were young, the association may be an age-specific one.) The weight of evidence does however suggest

to us that breathiness is a regular indicator of 'sexy' or of courtship behaviour.

We argue, then, that by **appearing to sound aroused**, women may be regarded with greater approbation by male interlocutors. If this device is effective, it may develop into a longer-term vocal setting. At the same time, however, the use of a pseudo-sexy voice over a prolonged period could give rise to vocal fold abuse and abnormalities. Cooper (1983) has shown breathiness and lowered pitch to be linked to vocal pathologies. Consequently, the implications of our findings for linguistics and for language pathology are quite serious. The kind of question which arises for the future is:

- (a) would British female speakers attempting to learn a language where modal and breathy voice contrast (e.g. Gujarati, Nepali, Igbo or Mazatec) habitually produce only breathy vowels?
- (b) how much breathiness in female speakers is tolerated in languages such as the above, where breathy segments are distinctive?
- (c) how do British speech therapists decide what is 'normal' and what is 'pathological' with regard to breathiness in females, when the perceptual norm established uses male speech as the baseline?

Our findings have some applications which deserve to be utilized without delay: it appears necessary for speech therapy purposes to define two separate breathiness norms

for British English speakers, according to their sex. Also, it would seem important to incorporate breathiness into current efforts towards the satisfactory synthesis of (British English) female speech.

8.5. Summary

The extension of sex-related enquiries into pronunciation differences beyond vowels has revealed that among **segmental** sources, **vocoids** are likely to yield the most rewards. For reasons which span acoustics, linguistic function and intrinsic variability, the search for generalised sex-based differences in **contoids** will in our judgement probably be arid. On the other hand, the research conducted for this thesis on vowels could profitably be pursued further with a wider range of databases, with interactional variables, and with greater exploration of speaker pitch. Indeed the whole gamut of suprasegmental differences offers possibilities for further expansion. Our own initial analysis of the comparative use of breathiness in British English provides firm indications that the sexes may habitually use different voice qualities for socio-phonetic purposes. The consequences of such behaviour are important both for phonetic methodology and for its theoretical framework; their implications need to be brought to the attention of speech pathologists, therapists and the speech technology industry now.

Chapter 9

Applications, the validity of investigation and conclusions

At this point it should be said that I hold as a personal tenet the idea that the linguist must be a conscious Janus: regardful of the broad scope of 'cultural' influences while also envisaging the applications of her/his findings. A few applications for the sex-specific phonetic differences we have illuminated were mentioned in the course of discussion of the experimental results. Here, we will briefly add some more suggestions.

9.1. Applications in language-learning

Findings such as these can be vital for second or foreign-language methodologies. The probability that women and men speak differently should be incorporated into course-book writing and classroom behaviour (see Fortune and Fortune, 1984). Limited personal experience in teaching immigrant women revealed that their lexical and intonational requirements were quite different and separate from those of men. The women needed to cope with social situations and daily crises with a vocabulary and access to paralinguistic aids often of low priority to men. Catering for these needs is naturally more effective and efficient if linguistic disparities identified by linguists are exploited in the learning situation.

Psychological resistance to language learning can have many causes (c.f. Rivers, 1964), but one which may be influential is the sex of the teacher. Students react, often subconsciously, to being taught and made to reproduce

language which is atypical of them as individuals. Thus it happens that the reason why a group of Arabic-speaking males, say, may seem especially recalcitrant in mastering stress patterns or emphatic intonation tunes is not due to the intrinsic difficulty of detecting the relative and appropriate pitch changes, but rather that these suprasegmental features are associated in Arabic with women's speech. The teacher is then seen as both erroneous in assessing what is necessary to their requirements and of dubious sexual determination. Further blocks of difficult consonant clusters or contorted syntax are irrelevant: the students have already 'switched off' because they would rather be silent than sound like women.

Similarly if a given phonological variant is always associated with female speakers, then there is little reward in attempting to inculcate males with it. An awareness of sex-specific alternants is therefore vital to the teacher of any foreign language if their pedagogy is to be profitable.

9.2. Considerations for language-planning

With more directly political ramifications, sex of the speaker has been shown to be not insignificant in studies of bilingualism.

Firstly, we must state that often, discussions of the growth of pidgin and creole languages (e.g. by Reed, 1943, or Salisbury, 1962) offer no interpretation of the findings about women: either the data were considered unimportant or

quite simply no investigation was made in the first instance. This situation is, of course, familiar and has been described in Chapters 3 and 4. 'Chorus' statements start to reappear (c.f. Chapter 3, Section 3.5). Nichols (1978) utters the refrain this time:

...women are eliminated from many studies because of classificatory problems.

Greater complexity arises, however, when the comparative behaviour of women and men is noted. In studies of bilingualism and code-switching, two clearly opposite types of behaviour have been attested. The first is the **maintenance** (conservative) position by women; i.e. women retained the first language longer than men when code/language-mixing occurred (see Jespersen, 1922; Diebold, 1961; Lieberman, 1965) because they did not leave the home. The converse appears, though, when we find women to be **adopters** (innovators) (see Oftedal, 1973; Hartford, 1976; Solé, 1976; Feinberg, 1977; Trudgill and Tzavaras, 1977; Gal, 1978; Valdés-Falles, 1978; Patella and Kuvlesky, 1979).

The main explanation offered for this interesting bifurcation is that in societies where a language/dialect was needed for **occupational mobility**, it is the men who first encounter and first adopt the new language/dialect. Women remain domestic and neither have access to nor much reason to learn a second language. However, when women **do** come to code-switch, or live in a bilingual society, **they** are the ones who 'mobilize out' of their first language/dialect most rapidly. This is especially prevalent among younger

women. The reason given by Solé (1976: 37), is that females are:

...far more exogamous than men (and are) seeking to upgrade socioeconomic and personal status.

This type of explanation also accounts for why Gal's (1978) 'Peasant men can't get wives'.

The conflicting situations, and the explanations offered for them, serve as reminders of the argument which was presented in Chapter 3, and further underline the need to expand the current conservative versus innovative binary division. While instances of assimilation to a second language/dialect reveal interesting differential sex-based movements, so do occurrences of so-called dissimilation. When minority groups consciously reject the national language, and revert to an ethnic language, is it the females or the males who lead the phonological changes? Such investigation remains to be made, but is essential if language planning is to be successful.

A relevant conclusion which can be drawn from investigations into bilingualism is that women in 'traditional' and stable societies lag behind in the adoption of new forms (cf. Papua New Guinea; W. Africa), whereas those in mobile societies are in advance of men. In such discussions, we are reminded again of the image of 'women in flux', vacillating between conservatism and innovation.

In transitional societies (as studied by Nichols, 1978) women exhibit **both** conservative and innovative traits.

Linguists at work in such societies should focus on this apparent paradox (see Chapter 3, Section 2), perhaps adopt the concept of the chameleon, and enable teachers to create an adequate identification model for pupils to adhere to. The success of language planning must be dependent on such considerations, for otherwise the time-sanctioned exclusion of (the female) half of the population will be perpetuated.

9.3. Applied medical aids

The uses of my research are not all either educational, political or commercial. In providing aids for the sick, aged, and handicapped, phonetic and phonological differences must be incorporated. As was shown in Chapter 8, speech pathologists have to consider the sex of the patient if they are to offer preventative or therapeutic treatment for patients suffering from either organic or psychogenic disorders.

Connections which have been established between various types of psychological disorders (ranging from schizophrenia to manic depression) are currently open to criticism because of their lack of terminological clarity. However, it does appear tenable that correlations between mental state and that of the larynx can be established. Depressives, for example, employ monopitch (i.e. lack of variation in fundamental frequency), exhibit poor spectral harmonics when analyzed acoustically, and have poor articulation in general (Ostwald, 1964: 25). To bridge the descriptive gap between

psychiatry and phonetics/phonology, a precise inventory of what are acceptable or unacceptable departures from the 'well-balanced' norm would be invaluable. As was suggested in Chapters 3 (Section 3.3) and 8 (Section 8.4.1), the establishment of a norm should be made with great sensitivity to both the sex of the speaker, and the society in which that speaker lives. It is disturbing to conjecture that patients may be receiving treatment for disorders, which have linguistic manifestations, simply because an understanding of sex-preferential forms is lacking.

Hearing aid technology is least likely to be advanced by an incorporation of sex-variants, as the main concern is with perception rather than production. Yet it does not seem unreasonable to imagine that in the development of aids to hearing placed in telephones (i.e. where the sex of the speaker is not immediately determinable from face-to-face contact), researchers need to allow for the effect of widely differing frequencies, as produced by women, children and men. Similarly, if any synergistic action is relevant in hearing-aid construction, then account must be taken of the speakers' physical and learned vocal tract behaviour.

Breakthroughs are being made rapidly in helping the handicapped to speak. Sufferers from cerebral palsy need no longer be denied the freedom of speech and excluded from the linguistic body politic. The primary need was for victims to be given a **voice**, but recently steps have been taken towards giving the handicapped an **appropriate voice**, in the

form of 'DECtalk', as developed by Klatt. Nevertheless, it is indicative that most other synthesized speech is still being developed in the adult male mode. This reflects of course the bias of the available production data, the inadequacies of the hardware and the fact that "women continue to be one of the mysteries of the universe" (Shuy, 1970: 856), whether they be physically handicapped or not.

9.4. Usefulness in advertising

Though perhaps less vital, the study of sex variables may be of use to the field of advertising. The most frequent arguments linking language and advertising relate to sexist images and statements and are theoretically dealt with by the Advertising Standards Authority in Britain. However, it would doubtless be highly advantageous to advertising executives to be informed of the characteristics of specific sex-determined languages. If they have a product which they wish to promote with 'voice overs' especially with women in mind, then to use **their** language would be most effective; the same would apply to men. This point is already attested by some of the stereotyping present in commercials now, and it seems sinister to bolster the agents' bias with linguistic proof. It is nevertheless a side-effect which must be acknowledged.

9.5. Automatic speech recognition

Speech synthesis and speech recognition are still in their

comparative infancies. However the latter is being used commercially for such tasks as voice-actuated wheelchairs; security and access control, and banking and credit-card transactions. It is obvious that the sex of the speaker must be one of the very first factors to be analyzed. At a purely acoustic level, this still presents problems (c.f. discussion in Chapter 4, Section 4.11) because clear-cut formant frequency parameters would be presumably programmed. As was demonstrated in Chapter 6 and elsewhere, it is extremely naive to suppose that an automatic speech recognition device can cater for male/female speaker differences by some simple scaling of the acoustic signal. This will be a research issue for some time to come, but, as we have advocated, an auditorily based approach to speaker sex differences ought to offer some practical promise.

Another major unsolved issue is the question of tolerable deviation, which must be firstly defined and then incorporated into the recognition per se. Those systems currently operating (e.g. ARPA SUR: see Lea, 1980), which use syntactic, semantic and pragmatic (task-orientated) constraints, yield an 'intelligent' response to restricted speech acts. Their efficiency would be surely greatly improved if they could be programmed to 'recognize' sex-linked constraints too.

One application of speech recognition of a limited type is **keyword spotting**. It is reported that in 1977 a speech recognition system correctly identified the word 'Kissinger'

said by nine newsreaders over many hours with 95% reliability (Moshier et al., 1977:3-4; cited by Lea, 1980: 73). In order to achieve this, the possible phonetic and phonological different realizations by men and women must be encoded. This description did not reveal whether the newsreaders were all of one sex (as one suspects was in fact the case), but the task was carried out in idealized hi-fi conditions. In 'real' situations conditions would be less than ideal with, for example, a high degree of 'channel noise' probable. Hence such initial naive enthusiasm should perhaps be dampened by the unknowns provided by 'life'.

We may see from the above then, that despite the anecdotal and intuitive flavour of many of her remarks, Lakoff (1975: 43) concluded appropriately that:

Linguistic imbalances are worthy of study because they bring into sharper focus real-world imbalances and inequities. They are clues that some external situation needs changing, rather than items that one should seek to change directly.

We would like to emphasize that linguistic imbalances include **phonetic imbalances**. The remark subsumes several points. Firstly, there is a subtle answer to the Sapir-Whorfian debate, with the acceptance of the belief that reality is the stronger determinant than language. Secondly, the reams of argument which have been produced concerning Miss, Mrs or Ms, pronominal generality, chairpersonship and so on are so much hot steam obscuring the social bedrock. Thirdly, and most importantly for our argument throughout this thesis, linguistic information contains **clues to some**

external situation. That external situation, to which so many indications have been given, is that women and men behave differently in their speech.

The specific research concerning sex-appropriate behaviour conducted for this thesis has been into the phonetic and phonological aspects of speech. The conclusions are now summarized.

9.6.-9.14. Conclusions

9.6. Critical caution should be exercised in accepting the findings of early (pre-Labovian) studies of speaker sex as influential on phonological variation. The apparent evidence for such variation could too often be attributed to investigator bias, phonetic imprecision, misinterpretation and the lack of a quantitative approach. Such studies are nevertheless of some value to current research, for they serve to indicate a long history of observation and interest in the linguistic manifestation of speaker sex, the widespread nature of sex-linked variation, and pronunciation traits which might bear further and more rigorous experimental examination.

9.7. Labovian methodology highlighted speaker sex as a variable more distinctly, but largely en route to the main interest in variation as a source for linguistic change. The criticisms of the Labovian model, in Chapter 3, should thus be placed in the context of the understanding that the study of speaker sex per se was not central to such

research, which had rather different aims from those of this thesis. Nevertheless, those aims were capable of becoming somewhat obscured by the inclusion of speaker sex as a factor, but without sufficient explanation of its role in linguistic change. Therefore, we find serious reasons for the unreliability of the interpretation of the role of speaker sex given by Labov, and those who have worked within the Labovian paradigm. These include:

9.7.1. The fact that, using contemporary sociolinguistic analyses, women cannot be assigned a reliable socio-economic status. Thus conclusions concerning, for example, the role of lower middle class women as prime agents in the process of linguistic change, are undermined. This a priori definitional obstacle accounts for the main flaw in Labov's (and others') attribution of the behaviour of women.

9.7.2. The concept of a dichotomy consisting of conservatism or innovation in language is too simplistic. A more productive description is multifaceted, allowing speakers to **accommodate** to what they perceive to be the appropriate style of the interactional moment. Thus shifts may be in either a 'conservative' or an 'innovative' direction, according to the motives of the speaker. There is nevertheless an apparent tendency for **bilingual** women to consistently adopt

innovative forms.

- 9.7.3. The speech of females is assessed as **passive**. A reconsideration of what female speakers are seeking to achieve in conversation would transform this somewhat negative label, and show females as being more constructive and more capable of advancing conversational co-operation. The analysis of the speech of women may also be affected more particularly by the Observer's Paradox, because of their greater speech-consciousness, and their reluctance to have unusual/non-'standard' forms of speech recorded.
- 9.7.4. The notion of a unilinear norm cannot be supported. Norms vary in different speech societies, and fluctuate according to age, SES, geographic location, level of employment and so forth. It is likely that females and males from the same speech community differ in their evaluation of norms: thus it cannot be stated unambiguously that females uniformly move towards prestigious forms.
- 9.7.5. Sampling techniques were not always ideal in Labovian models. An element of bias was possible.
- 9.7.6. An unmodified version of the Labovian model is not always suitable for application in societies other than New York. Its use in its 'pure' form can give rise to inappropriate conclusions.
- 9.7.7. Even with improved databases and methodological

remains a barrier to the assessment of phonetic and phonological variation in casual speech. As Labov (1964: 167) remarked more than twenty years ago, we still face "the familiar problem of whether the light is on or off when the refrigerator door is closed".

9.8. Chapter 4 showed that the majority of investigations into speaker-sex differences at the **phonetic** level have important methodological deficits. Sampling of speakers is frequently too haphazard, unbalanced, and, almost invariably, too small in numbers to be representative. In comparing the phonetic practices with an ideal set of criteria proposed for database collection, it was found that most previous studies were unsound. The design of the criteria for ensuring maximal homogeneity of experimental data which was accomplished in this thesis may thus be regarded as a considerable contribution to the progress of phonetic research.

The database assembled for two accents of British English is, to the best of my knowledge, the most closely-honed available for any language. It is hoped that the parameters described here and practiced in my research will be adopted widely now in phonetic and phonological research in general.

9.9. The issues of speaker normalization were recounted, and several previous attempts particularized. What emerged from this discussion was the insufficiency of articulatorily-

this discussion was the insufficiency of articulatorily-based approaches to normalization, and the likelihood that an auditorily-based normalization procedure would be a better solution, tending towards a more uniform method. Its validity lies in the fact that it attempts to represent how the brain analyses vowel tokens produced by speakers of different sexes.

9.10. In applying this auditory normalization algorithm to our very-carefully controlled database, we discovered not only evidence of current trends in the vowels of two accents of British English, and a useful mini socio-phonetic scale in the /ʌ/ vowel, but also (more importantly for the main research interest) support for our hypotheses concerning the types of differential phonetic behaviour by males and females. Firstly, it is possible to characterize the amount by which speakers differ in speech communities. Secondly, that difference was shown to vary across speech communities in one language. Thirdly, there was some evidence that females are more variable in their pronunciation than men. This evidence may, however, have been open to artefact, and so possible refinements to our research methodology were then discussed.

9.11. Further important findings from the British English results concerned citation and context forms, and the fundamental frequency (F0) of vowels. The preliminary indications are that differing speech contexts affect vowels

much less than might have been previously posited. It was also mooted that there is sex-specific behaviour in the control of F0, but our results for this factor were, of course, very limited. Both of these aspects would provide, no doubt, rewarding further research.

9.12. The extension of the auditorily-based normalization procedure to data from five further languages/accents provided greater underpinning of the original research hypothesis that different speech communities would require differing amounts of male-female normalization. In several societies, females and males appear to speak more like each other than their vocal anatomy would lead us to expect. We were thus able to implicate a socially-conditioned element in speech production.

Whether it is acceptable to depict females as speaking more 'open-mouthedly' was also explored. The conclusion was that this term cannot be applied universally, and that such an articulatory posture is accent-specific.

9.13. Finally, the possibilities of sex-differentiated pronunciation of phonetic segments other than vowels were examined. By combining indications from previous studies with acoustic knowledge, we were able to decide that such future research would need to be language-specific. The investigation of certain classes of sound, furthermore, would probably yield only negative results.

A sex-specific approach to the classification of voice

quality, on the other hand, would be greatly beneficial in practical applications. The study of breathiness was illustrative of the need to acknowledge the sex of the speaker when diagnosing a pathological or non-pathological voice. Such findings should also be noticed by those engaged in speech synthesis.

9.14. All these kinds of variation mentioned above are ones which the human listener's brain is able to compensate for, and 'understand through'. Yet, research is very far from even a preliminary account of this ability. To take a couple of examples: our understanding of the social judgements English listeners make, in an 'on-line' fashion when listening to a speaker, or equally, our understanding of the 'on-line' task of an automatic speech recogniser, is rudimentary indeed. To describe, model and understand either of these processes depends crucially on finding experimental ways to factor out the variation due to speaker sex and linguistic or stylistic context. Undeniably, there are large research challenges and grandiose goals in this area. In my estimation, they will occupy language research for some years yet. However, the methodology presented in this thesis may provide one point of entry to this challenge.

The constant argument of this thesis has been that sex of the speaker, combined with the social expectations made of the role of that sex, must be acknowledged as an important,

sometimes the paramount, variable in phonetic and phonological research. This connexion gave birth to the concept of **socio-phonetics**. The enumeration of some applications of this paradigm is further testimony to the kind of investigations undertaken specifically here, and also of the urgency of the need to press on with this type of research.

APPENDIX A

Questionnaire given to subjects prior to recording

INFORMANTS' SPECIFICATIONS

DATE:			
NAME:			
SEX:		AGE:	ACCENT: RP/MN
HEIGHT:		WEIGHT: (approx.)	HEALTH:
EDUCATION:	(i) Secondary		
	(ii) Higher		
OCCUPATION:			
TIME IN PRESENT OCCUPATION:			
PARENTS' OCCUPATIONS:	(i) Mother		
	(ii) Father		
WHERE DID YOU GROW UP?			
WHERE HAVE YOU LIVED IN THE LAST 3 YEARS? (Brief Report)			
DO NOT WRITE BELOW THIS LINE			
Place of Recording			
Tape-recorder			
Tape			
Microphone			
<u>COMMENTS</u>			

Appendix B

Recorded material

Exercise 1

1. James wondered who'd borrowed his secateurs.
2. Roy Hudd's wife is appearing in his show.
3. Eric had had a heart attack.
4. Alice hadn't heard of "The Muppet Show".
5. Custer wanted to head them off at the pass.
6. Brian's empty hod lay next to the wheelbarrow.
7. Mary remembered she hid it in the attic.
8. Henry gave no heed to her warning.
9. Diana had discovered an enormous hoard of silver.
10. Janet found it hard to imagine.
11. Maggie wore a white hood to the fancy dress party.

Exercise 2

head	heard
heed	hood
hod	who'd
Hudd	hard
heard	Hudd
hood	hoard
had	hod
hard	had
hid	hid
who'd	head
hoard	heed

Exercise 3

1. Did you say who'd or heed?
2. Did you say heed or hid?
3. Did you say hid or head?
4. Did you say head or had?
5. Did you say had or hard?
6. Did you say hard or Hudd?
7. Did you say Hudd or hod?
8. Did you say hod or heard?
9. Did you say heard or hoard?
10. Did you say hoard or hood?
11. Did you say hood or who'd?

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<u>Page and line</u>	<u>Error</u>	<u>Correction</u>
(Ext.Abs.)2: 8	non-parametric	parametric
14: 7	haufig Überdauert	häufig überdauert
22: 3-4		Join to first paragraph
34: 16	quantative	quantitative
41: 12	(insert)	be
49: 21	occurences	occurrences
86: 9	Ferrguson	Ferguson
105: 26	are	is
111: last	1974 (1st.inst.)	1972
161: 22	hochdeutsch	Hochdeutsch
: 26	varities	varieties
163: 23	apect	aspect
170: 16	misogeny	misogyny
: 17	Jesperson	Jespersen
256: Table 6.V	(invert labels) Males Females (add curly bracket)	Females Males
272: 6	VIII	6.II
279: 21	the the	the
286 ff.Figs.7.1-7.5.....	inclusive should be labelled:	Males - dotted lines Females - solid lines
319: 14	differententiated	differentiated
357: 1	Line of text missing (insert)	"scrupulousness, the Observer's Paradox still"
358: 1	Repetition of line	Delete Line 1
364: 4-5	Underlining omitted	Underline book title
367: 33	sociolnguistic	sociolinguistic
384: 2	Öman	Öhman