

**VARIATION AND (SOCIO)LINGUISTIC THEORY:  
A CASE STUDY OF TYNESIDE ENGLISH**

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## Abstract

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This thesis is concerned with the relationship between (a) patterns of sociolinguistic variation and (b) issues in theoretical linguistics. The patterns of sociolinguistic variation are derived from data collected from twenty speakers of Tyneside English. The recordings of the speakers were made broadly following a social network model, divided to sample the speech community along parameters of age and gender. The issues in theoretical linguistics concern the semantics and (morpho)syntax of modal verbs in English, and the phonological behaviour of the oral stops in specific linguistic environments. The thesis aims to show how a holistic approach to variation in the speech community, informed by knowledge of both sociolinguistic and formal linguistic theory, can best account for the data.

The introduction expands on the aims of this thesis, and provides a more detailed synopsis of the materials in each chapter than is given in this abstract. Chapter 1 briefly summarises certain aspects of the historical evolution of the Tyneside English (TE) accent, along with some analysis of TE syntactic and morphological patterns, to set the main discussion of the variables in the following chapters within a wider context. Chapter 2 provides a discussion of the semantics and (morpho)syntax of the modal verbs in standard English, with some commentary on relevant aspects of the historical evolution of the modals, which draws on theoretical aspects of both the Principles-and-Parameters and Minimalist frameworks. Chapter 3 examines patterns of glottalisation and glottalling in English, with specific reference to previous studies of TE, as well as to relevant work in current phonological theory, particularly Lexical and Metrical Phonology, along with a selective investigation into the historical evolution of these phenomena in TE (using material from the *Survey of English Dialects*) and other varieties of British English. Chapter 4 considers the issue of gender-based variation and its implications



for linguistic maintenance and change. Chapter 5 presents a detailed discussion of the methodology used for the collection of data for this thesis, as well as an analysis of the data itself, and how these data correlate with the various social groups. Chapter 6 provides a synthesis of the sociolinguistic materials considered in chapter 4 and part of chapter 5 with the theoretical materials considered in chapters 2 and 3 and the remaining part of chapter 5. The thesis ends with a concluding chapter which considers the broader issues of social variation and linguistic theory in light of the findings reported in this thesis.

To Pat Trousdale, and the memory of Fred Trousdale

## Declaration

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I declare that this thesis has been composed by me and that it is my own work

# Contents

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Abstract	i
Dedication	iii
Declaration	iv
Acknowledgements	viii
<b>Introduction</b>	<b>1</b>
<b>1 Characteristics of Tyneside English</b>	<b>10</b>
1.1 Consonants	12
1.1.1 Variable realisations of the voiceless stops	12
1.1.2 The (h) variable and the /hw/ cluster	13
1.1.3 The lateral approximant /l/	13
1.2 Vowels	14
1.2.1 The FOOT/STRUT split	15
1.2.2 The BATH set	15
1.2.3 The NURSE/NORTH merger	16
1.2.4 The long low front vowel	17
1.2.5 The FACE and GOAT sets	19
1.2.6 The MOUTH and PRICE sets	21
1.2.7 The GOOSE set	22
1.2.8 Unstressed vowels	23
1.3 Some (non-phonological) grammatical features	24
1.3.1 General non-standard features	25
1.3.2 Non-southern non-standard features	29
1.4 Summary	37
<b>2 Some features of present day Standard English modal auxiliaries</b>	<b>38</b>
2.1 Mood and modality	38
2.2 The central modals	43
2.2.1 Some morphosyntactic features associated with the central modals	44
2.3 Semantic classification of the central modals	51
2.3.1 Some working definitions	52
2.3.1.1 Modality and proposition	52
2.3.1.2 Types of modality	54
2.3.2 The semantics of the central modals	57
2.3.2.1 <i>Can</i>	58
2.3.2.2 <i>Could</i>	60

2.3.2.3	<i>May</i>	61
2.3.2.4	<i>Might</i>	63
2.3.2.5	<i>Shall</i>	63
2.3.2.6	<i>Should</i>	64
2.3.2.7	<i>Will</i>	67
2.3.2.8	<i>Would</i>	68
2.3.2.9	<i>Must</i>	69
2.4	Syntactic classification of the modals	71
2.4.1	Pollock (1989)	71
2.4.2	Ouhalla (1991)	86
2.4.3	The diachronic development of the modals, and $\theta$ -role assignment	93
2.5	Summary	100
<b>3</b>	<b>Glottalling and glottalisation, with particular reference to TE</b>	102
3.1	Environments in which G applies	104
3.1.1	Received Pronunciation (RP)	104
3.1.2	Tyneside English (TE)	113
3.2	G and phonological theories	124
3.2.1	Metrical and Autosegmental Phonology	125
3.2.2	Lexical Phonology	128
3.3	Traditional dialect material	132
3.3.1	<i>Survey of English Dialects</i> data	133
3.3.2	O'Connor (1947)	141
3.4	Diachronic development of G in RP	143
3.5	Summary	148
<b>4</b>	<b>The gender debate</b>	150
4.1	Gender-based variation in speech: a discussion of the principles	152
4.1.1	Gender and social class	161
4.1.2	Gender and social category affiliation	164
4.2	The 'traditional' approach	168
4.2.1	Norwich	168
4.2.2	Belfast	173
4.3	Challenging the traditional approach	180
4.3.1	Gender and linguistic variation	180
4.3.2	Glottalling and glottalisation in Tyneside English	183
4.3.3	The formation and development of New Zealand English	188
4.3.4	The Fenlands	193
4.3.5	Milton Keynes	198
4.4	Summary	200
<b>5</b>	<b>The Newcastle survey: method and data</b>	202
5.1	Selecting speakers, circumstances and linguistic variables	202
5.1.1	Social characteristics of the networks	208
5.1.2	Constraining the circumstances of the recordings	214

5.1.3	Selecting the linguistic variables	216
5.2	Collecting the texts	222
5.2.1	Elicitation tests	223
5.3	Identifying the linguistic variables and their variants in the text; processing the figures	226
5.3.1	Grammatical variation	226
5.3.1.1	<i>Can</i>	227
5.3.1.2	<i>Could</i>	231
5.3.1.3	<i>May</i>	234
5.3.1.4	<i>Might</i>	235
5.3.1.5	<i>Shall</i>	236
5.3.1.6	<i>Should</i>	237
5.3.1.7	<i>Will</i>	239
5.3.1.8	<i>Would</i>	241
5.3.1.9	<i>Must</i>	242
5.3.1.10	<i>'ll</i> and <i>'d</i>	249
5.3.2	Summary of grammatical variation	256
5.3.3	Phonological and phonetic variation	260
5.3.3.1	(t) __ # C (e.g. <i>hit Peter</i> )	262
5.3.3.2	(k) __ # C (e.g. <i>thick soup</i> )	263
5.3.3.3	(t) __ # L or S (e.g. <i>hot roll</i> )	264
5.3.3.4	(t) __ # V (e.g. <i>get Alan</i> )	265
5.3.3.5	(k) __ # V (e.g. <i>sick animal</i> )	268
5.3.3.6	(t) __ Pause	269
5.3.3.7	(k) __ Pause	273
5.3.3.8	(t) __ true C (e.g. <i>football</i> or <i>boots</i> )	274
5.3.3.9	(k) __ true C (e.g. <i>Parkside, mix</i> or <i>takes</i> )	276
5.3.3.10	(t) __ N (e.g. <i>Britain</i> )	277
5.3.3.11	(k) __ N (e.g. <i>bacon</i> )	278
5.3.3.12	(t) __ V (e.g. <i>lighter</i> )	279
5.3.3.13	(k) __ V (e.g. <i>jacket</i> )	280
5.3.4	Syllable initial glottalling and glottal reinforcement	281
5.4.4	Summary of phonological variation	284
5.5	General summary	285
<b>6</b>	<b>Synthesis</b>	287
6.1	The modals	287
6.2	The oral stops	304
6.3	Further sociolinguistic issues	315
6.4	Summary	318
	<b>Conclusions</b>	320
	<b>References</b>	326

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## Introduction

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This thesis has two broad aims. The first goal is an attempt to consider the relationship between the sociolinguistic enterprise on the one hand, and aspects of theoretical linguistics on the other. The terminology is unfortunate, but commonplace - ascribing the epithet *theoretical* to one branch of linguistics may be taken to imply that other branches do not have any coherent theories. Indeed, such a deficit is one of the criticisms voiced by Rickford (1988), as quoted in Hudson (1996: 257), against the sociolinguists of the 1980s:

[Sociolinguists have] a tendency to be satisfied with observation and description, and [are] insufficiently imbued with the thirst for theoretical explanation and prediction which drives science onward.

I doubt whether many sociolinguists of today would accept this accusation. They may defend themselves by suggesting that, in many ways, a number of theories have been advanced, certainly since the inception of Labov's quantitative paradigm, covering a whole host of social phenomena: class, network, gender, ethnicity and age are often the primary social variables which have been used in an attempt to explain and predict patterns of socially-sensitive language use, as many sociolinguistic textbooks will show.

Undoubtedly, however, there has been (and continues to be) a clear-cut division between the 'theorists' on the one hand, and the 'sociolinguists' on the other. To illustrate this point, I quote extensively below from one of the leading authorities in each field. Firstly, from the 'theorist' camp, the series of comments in (1) below are made in Chomsky (1979)<sup>1</sup>.

- (1) a. NC there are some who claim at times that there are certain theories concerning the study of language in society. Perhaps so, but I have not as yet *seen* such

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<sup>1</sup> In what follows, MR refers to Mitsou Ronat, a French linguist whose conversations with Chomsky form the basis of Chomsky (1979). NC refers to Chomsky himself.



- theories, or any specific account of the principles involved. (Chomsky 1979: 54)
- b. NC he [Labov in Labov (1972) GT] is doing something very useful on the level of educational practice, in attempting to combat the prejudices of the society at large - and that is very good. But on the linguistic level, this matter is evident and banal ... It is evident that the language of the ghettos is of the same order as that of the suburbs ... what disturbs me are the theoretical pretensions. We have here good descriptive linguistics, but it takes no sophistication in linguistics to establish the socially relevant conclusion. (Chomsky 1979: 55)
- c. NC Sociolinguistics is, I suppose, a discipline that seeks to apply principles of sociology to the study of language; but I suspect that it can draw little from sociology, and I wonder whether it is likely to contribute much to it.
- MR In general one links a social class to a set of linguistic features in a manner that is almost bi-unique.
- NC You can also collect butterflies and make many observations. If you like butterflies, that's fine; but such work must not be confounded with research, which is concerned to discover explanatory principles of some depth and fails if it does not do so. (Chomsky 1979: 57)

Such a view was reinforced a decade later by Smith (1989: 180), as quoted in Hudson (1997a: 74) as follows:

- (2) Any social parameter whatsoever may be the locus of some linguistic difference. Unfortunately nothing of interest to linguistic theory follows from this, so quantifying the difference is irrelevant to linguistics even though it may be of interest to the sociologist if it gives him or her a recognition criterion for some socially relevant variable.

Secondly, from the 'sociolinguists' camp, below is a comment from Chambers (1995):

- (3) it is certainly not true - and Chomsky may agree with this in view of the way categorical linguistics has developed since 1965 when he first stated the idealization - that

variation theory must incorporate or in any other way take account of the specific postulates of categorical grammar. That it to say, variation theory need not incorporate notions like - to cite just a few - the affix shift transformation (Chomsky 1957: 39-42), the Katz-Postal principle (Chomsky 1965: 132), the specified-subject condition (Chomsky 1973), the root clause filter (Chomsky and Lasnik 1977: 486), or the antecedent trace chain (Chomsky 1988: 116-7). These postulates gather dust with dozens of others in the generativist scrapyard that is surely one of the most bizarre and tragicomic residues of any intellectual tradition.

(Chambers (1995: 29-30))

The battle lines seem to be clearly drawn; yet not all linguists have taken such diametrically opposed views on structural and social linguistics. Hudson (1986, 1997a, 1997b) takes an approach which attempts to offer the best of both worlds, by incorporating sociolinguistic data into a specific (cognitive, ‘prototype’) model of language structure. His concept of language structure is radically different to what he calls the “classical theories” (Hudson (1997a: 74)) of Lexical Phonology and Principles-and-Parameters, both of which have been invoked to explain patterns of variation in various speech communities, as Hudson notes: for instance, Guy (1994) examines alternation between the presence and absence of the alveolar oral stops word-finally where they follow another consonant (as in *guest/guessed* and *bold/bowled*) within a framework of Lexical Phonology, while Kroch (1994) considers the development of *do*-periphrasis (and the concomitant changes involving ‘verb raising’) in early Modern English. Hudson (1997a) discusses a number of issues which he considers problematic for the theories involved (while accepting the validity of the (sociolinguistic) data), as he also does briefly elsewhere: Hudson (1996: 254-5) illustrates the “serious weaknesses” of the Lexical Phonology model proposed by Guy, and argues, in relation to the Principles-and-Parameters model, that it “would certainly be interesting if Chomsky’s strictly asocial theory of language structure turned out to be suitable for explaining variable data, but at present this seems unlikely”. But while there may be no consensus on which theoretical model of

language structure is best suited to incorporating and explaining variable data, Hudson, Guy and Kroch do at least seem to agree that there is a real need to synthesise the findings of sociolinguistic investigations with aspects of theoretical linguistics.

This thesis is an attempt to add to the debate discussed above. In what follows, I hope to illustrate how an examination of variable data can relate to sociolinguistics and to theoretical linguistics, and therefore, how sociolinguistics and theoretical linguistics must relate to one another. As noted above, while variability - whether that is determined by linguistic context or speaker variables, or (most likely) both - has been recognised as central in both sociolinguistics and in theoretical linguistics, only a few researchers seem to have attempted to bridge the gap, to argue that theoretical linguistics and sociolinguistics can inform one another, thereby producing a very exciting concept of language structure, which might satisfy both the theoreticians and the variationists. This clearly involves a somewhat radical rethink of some concepts on *both* sides of the divide, but I hope this thesis is a small step in the right direction, showing how vital it is that sociolinguists are aware of issues and ideas provided by theoretical linguists, and how variable data, with its extralinguistic correlates, can inform the development of linguistic theory. The theoretical models adopted in this thesis are of the more 'classical' kind, in terms of Hudson's definitions. However, these models are not adopted wholesale; at certain points in the thesis (particularly in the final chapter (chapter 6), where the synthesis of the sociolinguistic and structural analyses takes place), aspects of the models are questioned, and proposals made for a modification which might better account for the variable data. The syntactic model I use is based crucially on Pollock's (1989) 'split-InfI' hypothesis, with some reference to later developments in works which adopt the broad framework of Chomsky (1995); the phonological model I use attempts to consider aspects of Lexical Phonology (particularly, the division of the phonological

component into two subcomponents, those of lexical and postlexical phonology) alongside aspects of Metrical Phonology (particularly with regard to syllabification). The syntactic and phonological models are discussed in detail in chapters 2 and 3 respectively, as I describe below.

This first aim of the thesis also partly incorporates the second. In order to consider the importance of sociolinguistic data for linguistic theory, it is obviously important to collect that data. This thesis, then, is also an investigation into sociolinguistic variation amongst a group of speakers who reside in the South Gosforth area of Newcastle upon Tyne, in the north-east of England, and the sociolinguistic emphasis is particularly on the importance of gender as an extralinguistic variable conditioning the use of certain linguistic forms.

These are the two broad aims of the thesis; below, I give a chapter-by-chapter summary of the main arguments.

The first chapter provides a summary of certain features of Tyneside English (TE), the variety of English spoken in the north-east of England, in and around the city of Newcastle upon Tyne. This is provided in order to set the discussion of the specific linguistic variables under investigation in the remainder of the thesis within a wider context, so that a reader unfamiliar with this particular variety of English might be made aware of the ways in which TE as a whole is different from the standard English dialect, and the Received Pronunciation (RP) accent. The chapter deals with the historical evolution of some of the distinctive phonological characteristics of the accent, and with grammatical features that are (a) shared with other non-southern non-standard dialects or (b) shared with a 'general' non-standard variety of English, i.e. those forms which can be found over a wide geographical area, in Britain and beyond. Reference is made to historical and theoretical issues where appropriate. In terms of the historical evolution of the accent, much of chapter 1 is concerned with how the vernacular variants arose, and attempts to chart some developments from



Old English (OE) onwards; but consideration is also given to more recent historical - and on-going - changes which seem to be arising through dialect contact, and possibly increased standardisation. In terms of both accent and dialect, then, emphasis will be placed on seeing patterns of variation in TE as gradient phenomena; rather than simply assuming a vernacular-standard dichotomy, the discussion will highlight how a continuum exists between most local variants (the vernacular ones) and least local variants (the standard ones).

The second chapter introduces the grammatical variables which are to be analysed in the speech of the informants used for this thesis, namely the modal verbs. After a discussion of the concepts of mood and modality, the argument moves on to attempt to classify the central modals of present-day Standard English on morphosyntactic grounds. Following the establishment of this 'core', I look first at the semantic characteristics of these forms, considering mainly the types of modality expressed in the standard variety, and then at their syntactic features. This involves a somewhat lengthy discussion of various structural issues (after Pollock (1989) and Roberts (1993)) with reference to earlier stages of English, varieties of present-day English other than the standard, and French.

The third chapter introduces the phonetic and phonological variables which are to be analysed in the speech of the informants used for this thesis, namely the oral stops, and specifically at patterns of glottalling and glottalisation, and other weakening phenomena. It considers patterns of variation with the oral stops across different accents of English, and how such variability needs to be addressed by the theories which propose analyses of these phenomena. The evidence is primarily taken from existing analyses of RP and TE, with the aim of testing claims about TE against the data collected from the informants. In addition, some reference is made to the historical evolution of glottalling and glottalisation in British English generally, and data is analysed from the *Survey of English Dialects (SED)* (Orton and

Halliday 1962-3) to provide some evidence of recent patterns of variability in (p), (t) and (k) in those two localities investigated by researchers for the *SED* which were closest to the area of Newcastle in which the informants for this thesis reside.

In chapter 4, I discuss a variety of issues associated with the social variable of gender. I discuss the details of the methodology used for the collection of data in chapter 5; but it is important to point out here that the speakers used in this project were stratified by neither social class nor social network: all twenty speakers were part of one general network, and the crucial stratifications were age and gender; the primary focus of my investigation was to consider the way in which variants were distributed by gender in two distinct age groups. This is why the concept of gender is given such a detailed treatment in this chapter. In recent years, there have been a number of studies which have attempted to investigate the role of the sexes in the propagation of linguistic change. This chapter reviews a variety of such studies, some of which uphold the view that women use fewer non-standard forms than do men of a similar social standing in the same stylistic circumstances, and some of which challenge this traditional approach. The chapter also considers how gender interacts with other social categories.

As mentioned in the previous paragraph, chapter 5 provides a detailed description and analysis of the data and data collection carried out for the survey. It gives a range of information concerning the twenty informants whose speech is analysed, and describes the socio-economic characteristics of the area of Newcastle in which the informants live, based on information gathered from data in a draft of a ward poverty profile, which used material derived from the last census (in 1991). In addition to information about the informants themselves, the chapter also provides a report of the way in which the texts were collected, including a critique of this aspect of the methodology, highlighting advantages and disadvantages<sup>2</sup> of the approach

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<sup>2</sup> It should perhaps be pointed out from the outset that the entire methodology - from deciding on the type of sample of the population required, through arranging the recordings, collecting the texts,

which I employed. After a discussion concerning the problems associated with the identification of linguistic variables and variants in any given sociolinguistic text, the results of the investigation are provided, with statistical analysis where appropriate. The findings concerning modal verb usage are presented first, and then the findings concerning variation in the use of the oral stops. Some analysis of individual patterns is given here, but the analysis of patterns as a whole - and their implications for (socio)linguistic theory - are discussed in the final chapter.

This final chapter, then, addresses the main aim of the thesis, detailed at the beginning of this introduction. It attempts to show that socially sensitive variation can be important for our understanding of the structure of language, and can - and indeed should - be accommodated even within what seems to be a strictly asocial theory of linguistic structure. The chapter addresses the principle of the resetting of parameters during the course of syntactic change. It suggests that the resetting of parameters may not always be simply a binary matter, and that there are other issues and stages in any given syntactic change, as Roberts (1993) illustrates. Indeed, the concept of variation - whether that be determined by linguistic context or extralinguistic variables or both - is argued to be a clear indication of these stages in a change. In terms of phonological theory, there is a detailed discussion of lexical and postlexical rules associated with various processes affecting the oral stops, particularly with regard to their syllabification, and how this relates to the sociolinguistic distribution of the variants. Throughout this discussion, reference is specifically made to the distribution of the linguistic variants - of the modals and of the oral stops - to the social variable of gender, and any patterns which can be linked to changes in apparent time<sup>3</sup>.

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transcribing the data, processing the results (including the statistical analysis) to analysing the data and producing this final thesis - was carried out by me alone, so that any and all of the problems associated with the methodology presented here and in chapter 5 are mine and mine only.

<sup>3</sup> An apparent time study is defined by Chambers (1995: 193) as one in which “different age groups are observed simultaneously and the observations are extrapolated as temporal”. As Chambers goes on to point out, a key assumption with apparent time studies is that “the linguistic usage of a certain age group will remain essentially the same for that group as they grow older” (Chambers (1995: 194)).

The thesis ends with a concluding chapter, which attempts a summary of the main findings, and some proposals for future research, with a larger corpus.



# 1 Characteristics of Tyneside English

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In this introductory chapter, I attempt a brief synopsis of (a) differences between Received Pronunciation (RP) and Tyneside English (TE); (b) the historical evolution of the TE accent<sup>1</sup>; and (c) some features of TE grammar which are different from that of standard English.

In terms of accent variation, there has recently been some very detailed work carried out on the phonology of present-day TE, some of which deals with patterns of consonantal variation (Docherty and Foulkes 1999; Foulkes 1997; Docherty *et al.* 1997; Milroy *et al.* 1994), and some of which investigates vocalic variation (Watt 1998, Watt and Milroy 1999), and the reader is referred to these works for a more detailed discussion of the on-going changes. My aim in this chapter is to couch this present-day phonological variation within its historical context where possible. The distinctions made between certain varieties in what follows (such as 'northern' vs. 'southern') are gross distinctions, which do not take into account any fine regional or social stratification; but this is not the purpose of this chapter. The phonology section of this chapter is intended to provide an indication of the broad differences between (localised) forms of specific varieties, and to illustrate *the way in which the most localised variants evolved*, to compare these with less localised forms (the least localised variety being RP). However, some reference is made to the data provided in Watt and Milroy (1999) which give details on some of the variants which, while growing in currency in TE, are not associated with the traditional vernacular, but are rather the result of dialect levelling through contact with other varieties. Aspects of the syntax and morphology are also

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<sup>1</sup> I am very grateful to Derek Britton for advice and guidance on the historical evolution of TE and other northern English varieties; a very full account of the history of north-eastern varieties in the early Modern English period is provided by Hampel (1998).

considered with regard to their ‘degree of localness’; and how such local forms are to be analysed within a minimalist framework is also addressed.

In the following discussion, I make use of Wells’ (1982: 119 *et passim*) term *lexical sets*. He states (1982: 119-20):

The use of one vowel or another in particular words (lexical items) can be illustrated by tabulating their occurrence in the set of keywords, each of which ... stands for a large number of words which behave in the same way in respect of the incidence of vowels in different accents.

These key words are frequently mentioned in the following discussion, and appear in the text in small capital letters, thus KIT. I begin the discussion by considering consonantal features, and then move on to the vowels. §1.3 deals with grammatical constructions, and attempts to consider how such variation between standard English and TE can be accounted for within a theoretical approach to English syntactic structure, as well as providing some information on features in TE shared with other non-standard varieties.

In what follows, I discuss a number of features which rely heavily on the work of traditional dialectology, an approach to variation which often makes use of terms such as *dialect boundary* and *isogloss*, terms which Hudson (1996: 38f) considers to be abstractions from reality. Hudson’s argument is that such isoglosses (from Greek *iso* = same + *gloss* = tongue, lines on traditional dialectology maps which mark “the boundaries between two regions which differ with respect to some linguistic feature (for instance, a lexical item, or the pronunciation of a particular word)” (Chambers and Trudgill 1998: 89)) cannot delimit varieties of a language because “varieties do not exist. All that exists are people and items, and people may be more or less similar to one another in the items that they have in their language” (Hudson 1996: 39). Even if we retain the notion of variety, it is never possible to suggest that any given dialect boundary

will be clear; there is bound to be a degree of fuzziness at any transition zone between two varieties. While recognising these alternative views on the notions of ‘variety’ and ‘boundary’, I present the following features of (particularly vernacular aspects of) TE in a more traditional manner, in an attempt to reconstruct those items used by some TE speakers which are least similar to those used by other speakers of English; I recognise that the distinctions may not be ‘real’, but they provide a useful starting-point for the reconstruction of aspects of the evolution of TE.

## §1.1 *Consonants*

### §1.1.1 *Variable realisations of the voiceless stops /p, t, k*<sup>2</sup>

There are a number of different realisations of the oral stops in TE. This is the phonological variable under investigation in the present study, and the processes are discussed in more detail in the remainder of the thesis. Glottalisation or glottal reinforcement - hereafter GR - describes the type of realisation of the voiceless stops in syllable-final position in RP. The release of the bilabial, alveolar or velar stop is in many varieties of English accompanied by glottal stricture, and this closure of the glottis often precedes the articulation of [p t k] (cf. Giegerich 1992: 220-1), so that we have in RP *pick that up* as [p<sup>h</sup>tʔk ðaʔt ʌʔp]. As far as present day TE is concerned, the realisation of the voiceless stop phonemes in syllable final position (and sometimes syllable initial position before a weak vowel (Wells 1982: 374)) is highly variable: often they are realised as a glottal stop [ʔ] as in *butter* [bʊʔɑ], and in such cases the oral stop is not reinforced but replaced. This process is often referred to as glottalling, glottal replacement, or glottal substitution, and will be referred to hereafter as GS. In cases of

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<sup>2</sup> This discussion of the voiceless stops is deliberately brief, as these variables are discussed in much more detail elsewhere in the thesis.

GR in TE, Wells (1982: 374) suggests that the glottal masks the oral plosive burst, so *butter* [bʊtʔɑ], while O'Connor (1947) has [ʔp ʔt ʔk] where the p, t, k symbols here represent a very weak [b̥], [d̥] and [g̥]. In addition to GR and GS, which can affect all three of the voiceless stops, the alveolar stop /t/ can be realised as a tap [ɾ] or an approximant [ɹ]. All of these features of GR, GS, and the tap and approximant variants of /t/ in TE are discussed in much greater detail in chapters 3, 4, 5 and 6 of this thesis.

#### §1.1.2 *The (h) variable and the /hw/ cluster*

Wells (1982: 374) argues that, unlike most other urban accents of English, TE has no H-Dropping, thus *happy* [hapʔi]. This contrasts with nearby urban accents such as that of Wearside where H-Dropping is common. Despite the potential dialect contact with H-Dropping speakers, there is no indication of H-Dropping in the speech of the informants for this thesis, a result which concurs with the findings of Watt and Milroy (1999: 30). Hughes and Trudgill (1987: 71) are not quite so categorical as Wells, suggesting that /h/ is *generally* present. The /hw/ cluster is generally not a feature of present day TE, so that *which* and *witch* are homophonous: [wɪtʃ].

#### §1.1.3 *The lateral approximant /l/*

In RP the lateral /l/ has two allophones, a 'clear' one [l] and a 'dark' one [ɫ]. There is alveolar contact in both cases but in the latter the back of the tongue is raised toward the velum or retracted toward the uvula (Giegerich 1992: 211). [l] appears in syllable onsets, [ɫ] elsewhere: *lilt* [lɪɫt], *lump* [lʌmp], *pull* [pʊɫ]. In TE however, there is no allophony, since /l/ is clear in all positions: [lɪlt], [lʌmp], [pʊl].

§1.2 *Vowels*

The table below is a combined adaptation of Wells (1982: xviii) and Watt and Milroy (1999: 27), giving the realisations of the vowels in the (Wellsian) lexical sets in both RP and TE. The aim of the following discussion is to explain the historical evolution of the differences noted.

**Table 1**  
Keywords and their vowels in RP and TE (modified, after Wells 1982 and Watt and Milroy 1999)

KEYWORD	RP	TE	KEYWORD	RP	TE	KEYWORD	RP	TE
KIT	ɪ	ɪ	NURSE	ɜ:	ɜ:~(ø:~ɔ:)	CHOICE	ɔɪ	ɔɪ
DRESS	ɛ	ɛ	FLEECE	i:	i:~(ei~ii)	MOUTH	aʊ	aʊ~(eʌ~u:)
TRAP	ɑ	ɑ~ɑ:	FACE	eɪ	e:~(ɛ:~iə~eɪ)	NEAR	ɪə	iə~iə
LOT	ɒ	ɒ	PALM	ɑ:	ɑ:~ɒ:	SQUARE	ɛə	ɛ:~e:
STRUT	ʌ	ʊ(~ə)	THOUGHT	ɔ:	ɔ:~(ɑ:)	START	ɑ:	ɑ~ɒ
FOOT	ʊ	ʊ	GOAT	oʊ	o:~(ø:~ʊə)	NORTH	ɔ:	ɔ:~(ɑ:)
BATH	ɑ:	ɑ	GOOSE	u:	u:~øʊ	FORCE	ɔ:	ɔ:~(ɑ:)
CLOTH	ɒ	ɒ	PRICE	aɪ	aɪ~eɪ	CURE	ʊə~ɔ:	ʊə~ʊə

The variants in brackets are less common than those unbracketed. Many variants of the vowels in TE have marked sociolinguistic correlates; these issues are discussed by Watt and Milroy (1999): my purpose here is to illustrate the evolution of the local forms.

### §1.2.1 *The FOOT/STRUT split*

Inputs to TE /ʊ/ are Middle English (ME) /u/ (as in e.g. *put*) and ME /o:/ (as in e.g. *good*). The former laxed to /ʊ/ in the course of ME; in a set of words which contained the latter, the vowel shows pre-Great Vowel Shift (GVS) shortening, laxing and rounding to /ʊ/ - see further §1.2.7 below. TE differs from RP in that in RP, some of the reflexes of ME /u/ and /o:/ which had developed to /ʊ/ in early Modern English (eModE) underwent a further lowering to /ʌ/ (as in e.g. *bud*, *flood*), and this further lowering did not take place in TE. This failure to lower is not characteristic of just TE; this is one of the major shibboleths of northern English English as a whole. Beal (1999: 134) argues that the ‘unsplit’ /ʊ/ was a marker of northern English by the middle of the eighteenth century. Recent developments in TE have included the shift to [ə] in the STRUT set particularly, possibly as a fudged<sup>3</sup> form where speakers have targeted the RP [ʌ] variant: Beal (1999: 135) suggests that such forms are common in MC Newcastle English, particularly in Jesmond, a very affluent area of the city, which Beal likens to Morningside in Edinburgh and Kelvinside in Glasgow. The accents of all three areas represent a modified standard in the respective cities, where ‘modified standard’ means a locally prestigious variety, characterised by variants which are often targeted by lower middle and upper working class speakers in more formal styles.

### §1.2.2 *The BATH set*

Another northern English English shibboleth is the development of eModE /a/ before the voiceless fricatives. In the late seventeenth century, the vowel began to lengthen in a number of environments, including not only the voiceless fricatives, but

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<sup>3</sup> A fudged variant is one which shares a degree of phonetic similarity between two competing variants in any given phonological change: thus [ɻ] is often found as a fudged variant of the STRUT/FOOT vowels, being central and unrounded like [ʌ] but closer in height to [ʊ]. This could also hold for [ə].

before /r/ (*car, cart*) and before nasal + voiceless obstruent clusters (*dance, plant*). This lengthened variant subsequently developed to /a/ in the late modern period. The progress of this vowel in these environments is variable in TE: retraction and lengthening has certainly taken place before historic rhyme /r/, which Beal (1985) refers to as ‘burr retraction’; but in the other environments, as elsewhere in the north, the vowel has remained front. ME /a/ before [ŋg] clusters remained in the far north as an unrounded front vowel, being backed and rounded to [ɔ] in (proto-)RP. In current TE forms such as [laŋ] *long* and [raŋ] *wrong* are restricted to the broadest variety, and seem to be recessive, although as characteristic features of the vernacular, they may be being eroded only slowly. See further discussion of the long low front vowel in §1.2.4 below.

### §1.2.3 *The NURSE/NORTH merger*

In eModE, proto-RP /e i u/ neutralised before /r/, resulting in [ɜ:r] with pre-/r/ lengthening and [ɜ:] with /r/ loss c. 1800. In broad TE, the lexical set NURSE (the set of words which contained ME /er ir ur/) seems ultimately to have merged with the NORTH set in [ɔ:ɪ]<sup>4</sup>, where backing from [ɜ:] to [ɔ:] is due to the following uvular. Loss of this uvular [ɪ] - retention of which is very rare in the urban area surrounding Newcastle, though it is still a feature of the surrounding rural area of Northumberland - results in present day TE /ɔ:/: *bird* [bɔ:d], *Percy* [p<sup>h</sup>ɔ:si], *nurse* [nɔ:s]. This evidence would therefore suggest a merger of ME /ir ur er/ and ME /or/ in this accent. Wells (1982: 375) notes that in some less broad Tyneside speech, words of the lexical set NURSE have /ɜ/ or a rounded and fronted variant /ø/ and that hypercorrections like *short* \*[[ø:t] do not occur. This, combined with other empirical work carried out in the Tyneside area, has

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<sup>4</sup> EModE evidence for this in Newcastle is provided by examples such as *short* ‘shirt’ (*Municipal Accounts* (1593), 32).



led Watt (1998) and Watt and Milroy (1999) to argue that the merger did not take place in every sociolect in the TE-speaking community.

#### §1.2.4 *The long low front vowel*

The phoneme /a:/ in present-day TE develops both from the monophthongisation of northern ME /au/ (from Old English (OE) /ɑ:/ + /y/ (e.g. *knawan* ‘to know’)) and from the monophthongisation of general ME /au/ which itself developed from:

- (i) OE /ɑ/ + /y/ (e.g. *dragan* ‘to draw’)
- (ii) Anglo-Norman (AN) /au/ (e.g. *sauce*)
- (iii) Breaking of ME /a/ before liquids (e.g. *all*)

These yield the present-day TE forms [dra:] *draw* and [sa:s] *sauce*: variants of this type were regularly heard in the speech of the informants<sup>5</sup> for this thesis, thus [a:l] *all* (Robbie) and [na:] *know* (Tom). In non-Northern ME the reflex of OE /ɑ:/ + /y/ was /ou/, that is the first element of the diphthong was rounded and raised rather than fronted. /ou/ subsequently monophthongised to /ɔ:/ and was raised during the GVS to /o:/, which in RP was subject to high-mid diphthongisation c.1800 to [ou], hence the present day RP form [nou] *know*. Outwith the north-east, ME /au/ from the other inputs listed in (i) to (iii) above either monophthongised to /ɔ:/ during the GVS (hence present day RP [drɔ:] *draw*) or monophthongised to /ɑ:/ and then backed to [ɑ:] when it occurred before a nasal or a voiceless fricative (hence present day RP [ɑ:nt] *aunt*, [hɑ:f] *half*). A further feature of the Northern dialect in ME is that /a/ often failed to undergo homorganic lengthening or was shortened back to /a/ in early ME. In words such as *cold*, *hold* and *would*, subsequent /l/ vocalisation (which also occurred in many Southern dialects in the context

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<sup>5</sup> For more detail on the informants for this thesis, see chapter 5.



/a + l + C/ for example in *palm*, RP [pɑ:m]) and compensatory lengthening of the vowel yields the present day TE forms [ka:d] [ha:d] [wa:d].

A question exists as to whether we should classify /a:/ as a separate phoneme or as an allophone of /a/ in present day TE (Wells 1982: 375). In the broadest variety of TE, [a:] occurs before a final voiced consonant or final voiced consonant cluster, as in [ba:d] *bad* and [la:nd] *land*, as well as in the words which derive ultimately from ME /au/ as discussed above. In less broad varieties of TE, however, there is not a minimal pair *tack* - *talk* /tak/ - /ta:k/. In other words, [a] and [a:] are in complementary distribution for such speakers, as in (1) below:

- (1) *The low front vowel in modified TE*  
 /a/ → [a:] / \_ C [+ voice] (C [+ voice])  
 [a] elsewhere

For modified TE there is no need to posit a separate phoneme /a:/ since [a:] is merely an allophone of /a/. In broad TE, however, there is reason to argue that /a:/ is a separate phoneme, since there are minimal pairs of the type *tack* - *talk* /tak/ - /ta:k/ and *whack* - *walk* /wak/ - /wa:k/. This is then an instance of phonemic variation between broad TE and the reference accent RP; that is, there is a difference between the two accents in terms of their respective phonemic inventories. In terms of low vowels RP has both /a/ (as in *Dan*) and /ɑ/ (as in *darn*); in broad TE, there is /a/ (as in *Dan*), /a:/ (as in *dawn*) and /ɑ/ (as in *darn*). This has interesting repercussions for classification of the RP and TE vowel systems in terms of the features [± Tense] and [± Long]. We can posit a redundancy rule [+ Tense] → [+ Long] for both RP and broad TE. That is, in both of these accents, all vowels which have the qualitative feature [+ Tense] - vowels which are produced “with a deliberate, accurate, maximally distinct gesture that involves

considerable muscular effort” (Giegerich 1992: 98) - also have the quantitative feature [+ Long]. However, given the discussion of /a:/ in TE, it does not follow that we have the redundancy rule [+ Long] → [+ Tense]. To put this another way, while all tense vowels are long in both accents, not all long vowels in TE are tense. The long low vowel does appear to be recessive in TE though, and only characteristic of the broadest variety: Watt and Milroy (1999: 27) describe the long vowel [a:] in *lad* and *band* as “rather uncommon in contemporary Newcastle English”<sup>6</sup>.

### §1.2.5 *The FACE and GOAT sets*

The high-mid vowel phonemes /e/ and /o/ have a range of realisations in present-day TE (Wells 1982: 375, Watt and Milroy 1999). Words of the lexical set *FACE* have [e(:)] or [eə ~ ɪə] and *GOAT* [o(:) ~ e(:)] or [ʊə ~ əə], and for some TE speakers [ə:], according to Wells: Watt and Milroy’s analysis is given in Table 1 above. Examining the development of the vowels in the *FACE* set first, TE [eə] derives from ME /a:/ (long as a result of Middle English Open Syllable Lengthening (MEOSL)) raised to /e:/ with subsequent diphthongisation in eModE to [eə]. In the north, this set would then have fallen in with words which had OE /a:/ such as *stan* ‘stone’, and this contrasts with the development of /a:/ in the south, as is shown in (2) below:

(2) *Development of OE /a:/*

(a) Northern: /a:/ → /a:/ → /ɛ:/ → /e:/ (→ [eə ~ ɪə])

(b) Southern: /a:/ → /ɔ:/ → /o:/ (→ [oʊ])

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<sup>6</sup> On the various eighteenth and nineteenth century developments affecting the distribution of this vowel, see Beal (1999).

I take 'Northern' in (2) above to correspond here roughly with those traditional dialects to the north of the Humber.

The vowel in words in the GOAT set (that is, those with [əə] and the raised variant [ʊə] in present day TE) derive from ME /ɔ/ which underwent MEOSL to /ɔ:/ which raised during the GVS to /o:/. There is a problem, then, in that in the south, words with OE /ɑ:/ fell in with the GOAT set while in the north, they fell in with the FACE set and that, observing evidence from eModE written records in Newcastle, it seems that the reflex of OE /ɑ:/ was usually written <o>, the graph which was used in southern varieties for the (proto-RP) reflexes of OE /ɑ:/, namely [ɔ:] or [o:]. The influence of the standard written form as well as the emerging prestige accent variety seems to have affected the members of the upper strata of Newcastle society who replaced the local pronunciation of the vowel in words which had OE /ɑ:/, which was [iə] or [eə], with the vowel they had in words which derived from ME /ɔ:/, namely [oə]. For many speakers [oə] and the raised variant [ʊə] were monophthongised to [ə:] which explains the variants in present day TE noted by Wells (1982: 375) and Watt and Milroy (1999: 27 *et passim*). [iə] reflexes of OE /ɑ:/ are now only found in relatively broad varieties of TE, and only in a handful of lexical items, such as *who* [wi:] and *home* [jɛm] (< [hjɛm] < [hɪəm]). Such forms did appear in the data collected for this thesis, however: from Robbie, the form [ni:wɛ:] *nowhere*, and from Kevin, [ni:] *no*. Current patterns of variation suggest that the RP-like forms [eɪ] and [oʊ] are not gaining widespread currency in TE, despite, as Watt and Milroy point out (1999: 42), the gradual progress north of the isogloss dividing the northern monophthongal forms from the southern diphthongal forms. Increasingly

levelled<sup>7</sup> variants are favoured, particularly supra-local forms which are shared with other northern varieties.

Orton (1933: 50) has a discussion of reflexes of OE /ɑ:/ in the speech of residents of Byers Green, a village eight miles south-west of Durham. In this discussion, he cites a variant [iá], where the second element of the diphthong carries the stress; thus he gives *bone* as [bíán]: such variants are very rare in TE, to the extent that they are not recorded as variants in Watt and Milroy (1999), though they were characteristic of a recent Durham survey (Kerswill (1987)).

### §1.2.6 *The MOUTH and PRICE sets*

ME /u:/ did not diphthongise in the North during the GVS, and many speakers of broad TE retain this vowel in words in the MOUTH set: thus, in the speech of one of the younger males recorded for this thesis, the form [əbu:ʔ] *about* (Tom) is attested. There is also a fudged variant [əu], as well as the ‘standard’ [aʊ]. Words in the PRICE set show variation between [aɪ] and [ɛɪ]. Some debate exists as to whether these forms are in free variation or whether they are phonetically conditioned. Wells (1982: 376) suggests that, for some speakers, there is a ‘Scottish-type distribution’ (which I take to be referring to the Scottish Vowel Length Rule (SVLR), though this is not made clear) given that [aɪ] occurs before a voiced fricative and also finally, with [ɛɪ] elsewhere. This is discussed further by Milroy (1995).

In certain words of the PRICE set, there has been no diphthongisation of late ME /i:/ as part of the GVS. This results in forms such as *night* [ni:ʔ] and *alright* [a:lri:ʔ], although the length of the vowel is only variably long, and as Derek Britton (personal

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<sup>7</sup> *Levelling* is defined by Williams and Kerswill (1999: 149) as follows: “a process whereby differences between regional varieties are reduced, features which make varieties distinctive disappear, and new features emerge and are adopted by speakers over a wide geographical area”.

communication) has informed me, the shorter variants seem more regular in north-eastern English English in compound words such as *fortnight* [fɔːʔniʔ]. This ‘failure to diphthongise’ is something of a misnomer: in the late Middle and early Modern period, TE, and also other accents of areas of the far north of England, had retained the palatal fricative in such words, so that whereas *night* in proto-RP was [ni:t] (and the vowel could thus diphthongise to [aɪ] as part of the GVS), the word in eModE TE surfaced as [niçt], and the loss of the fricative did not take place until after the GVS; that is, the input in TE at the time of the GVS was a short vowel anyway, and would not therefore have diphthongised. These developments are summarised in (3) below:

(3) *Historical development of night in RP and TE*

(a) RP:	OE <i>niht</i>	/nixt/
	Loss of fricative	/nit/
	Compensatory lengthening	/ni:t/
	GVS	/naɪt/
(b) TE:	OE <i>niht</i>	/nixt/
	GVS	/nixt/
	Loss of fricative	/nit/
	(Variable) compensatory lengthening	/ni(:)t/

§1.2.7 *The GOOSE set*

Vowels in the *GOOSE* set derive from ME /o:/. In the north, it seems as though such vowels developed either (a) to /ʊ/ before the GVS, thus falling in with ME /u/ words (cf. §1.2.1 above) or (b) to a fronted variant /ø:/. This form can be found in the rural areas surrounding Tyneside in words such as *moon* [mɪən] and *spoon* [spɪən], but such forms are rarely found in TE; the only regularly occurring form which would seem

to indicate this northern fronting is *do* [di:], which derives from [diu] (with word final monophthongisation); such a form appeared in the corpus of data collected for this thesis, specifically as a male feature. Historical evidence suggests one of two possible things: that the influence of proto-RP in early modern Newcastle English was strong enough to check the fronting in the city, or that the process of ‘Northern Fronting’ was never fully lexically diffused in all sociolects in the speech community: such evidence consists of spellings such as: *shoutinge bowes* ‘shooting bows’ (*Wills* (Lewen) 1562), *toules* ‘tools’ (*Wills* (Tedcastle) 1569) and *spownes* ‘spoons’ (*Wills* (Cook) 1569). Such <ou> and <ow> spellings would seem to indicate a high back vowel [u:], thus suggesting a potential ‘standardising’ influence of proto-RP raised (as opposed to vernacular fronted) developments of words in the ME /o:/ set.

#### §1.2.8 *Unstressed vowels*

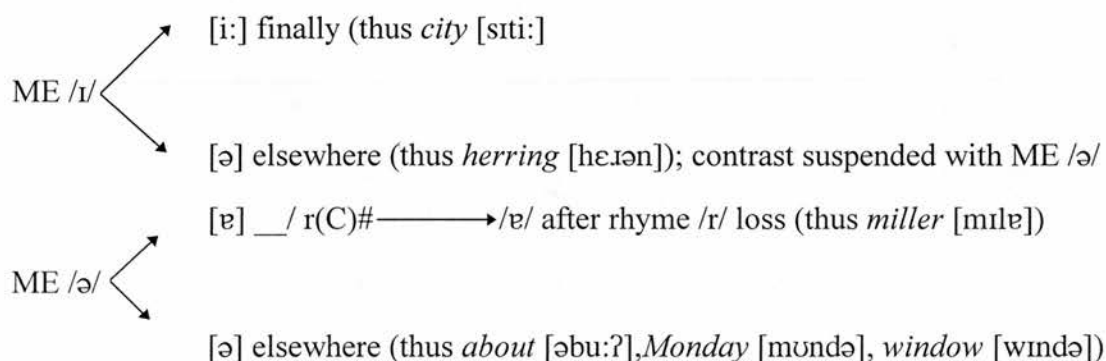
Unstressed /ɪ/ has a number of allophones in the present day TE accent. Word finally, /ɪ/ is realized as [i:], as in *city* [sɪtʔi:], but in non-final position it surfaces as [ə], as in *hobbit* [hɒbət]. The distinction between the reflexes of ME /i/ and /ə/ in unstressed syllables has therefore been neutralised in the present day accent (*herring* and *heron* both as [hɛrən]) in non-final position. The lowering and centralization of ME /i/ in unstressed syllables is suggested by eModE evidence such as *sattan* ‘satin’ (*Wills* (Lewell) 1569) and *Pillgram Streit* ‘Pilgrim Street’ (*Wills* (Lewen) 1569). Wells (1982: 376) states:

Tyneside has /ə/, not the more usual /ɪ/, as the weak vowel in words such as *voices*, *ended*; on the other hand some Geordies have /ɪ/, not /ə/, in words such as *seven*, *almond*, *impression* [-fɪn].

Wells does not mention that the [ɪ] occurs in words which have a following nasal, but this seems to be an important conditioning factor. This may also be an instance of hypercorrection, since words such as *pilgrim*, *hopping* and *kitchen* would have either /ə/ or a syllabic nasal in present-day TE.

In final position in <-er> words such as *butter*, *lover*, an unstressed vowel [ɐ] is found in the present day accent. The quality of this vowel is likely to be due to the influence of the following uvular [ʁ] which was once a feature of the accent, and is suggested by eModE evidence such as *millar* ‘miller’.

We can summarise the history of the unstressed vowel system of TE as follows:



This leads to minimal pairs of the type *city* - *sitter* and *fellow* - *feller* in broad TE.

### §1.3 *Some (non-phonological) grammatical features*

There are a number of ways in which the grammar of TE differs from that of present-day Standard English (PDSE), many of which are amply documented by Beal (1993). Some of these features seem to form part of a general non-standard variety across the British Isles; some are still supra-local but restricted to the north of England and (the central belt of) Scotland; and others are highly localised in that they are restricted to the north-east of England. Rather than simply assume a standard vs. non-



standard dichotomy, then, it would seem preferable to consider such variation as part of a continuum, the standard variety as the ‘least local’ dialect, and vernacular TE as the ‘most local’ (cf. Milroy, Milroy and Hartley (1994) for a similar argument for phonological variation). In this section, I note some of the grammatical characteristics of TE under the following headings: general non-standard (§1.3.1) and non-southern non-standard (§1.3.2). Beal (1993) notes similar features to those given here, and many of the examples are taken from that work; however, I have tried to include some further historical and theoretical issues where possible. The theoretical model adopted here is that set out in Radford (1997). While some problematic features of this model (as applied to the TE data) are discussed, the general principles behind such a theory are not discussed at this stage in the thesis: given the complexity of the issues involved, this might detract from the exposition of TE grammar which is intended in this chapter. However, in parts of the remainder of this thesis (chapters 2, 5 and 6), certain principles of this theory are examined in more detail, specifically, those parts of the theory which concern analysis of auxiliary verbs generally, and the modals in particular. In addition, the following discussion of TE grammar makes no reference to the use of modal verbs in that variety; these forms are analysed separately elsewhere in the thesis, since they are the grammatical variables analysed in the speech of the informants.

### §1.3.1 *General non-standard features*

Like many other non-standard varieties, TE displays syncretism between past tense and past participle forms of certain verbs which are distinct in the standard variety, thus *I ran* and *I have ran* (PDSE *I ran* and *I have run*), *it sunk* and *it has sunk* (PDSE *it sank* and *it has sunk*). Notice then that the form in TE can correspond to either the



PDSE past tense form or the PDSE past participle form. Such forms commonly occurred in the data collected for this thesis.

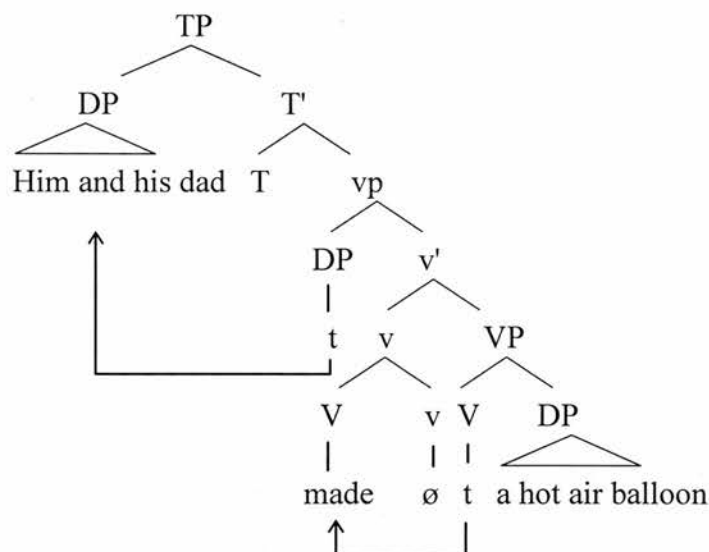
Certain patterns of negation in TE are consistent with those in other non-standard varieties. The use of *never* meaning ‘not on one specific occasion’ (as opposed to the standard ‘not on any occasion’) has been recorded in TE, particularly as an emphatic marker (Beal 1993: 198, and cf. Cheshire 1998); in the corpus of data collected for this thesis, for instance, the use of *never* meaning ‘not on one specific occasion’ is clear in Kevin’s discussion of a supposedly sure-fire winner at the racetrack - *it never came in*. Also as a marker of emphatic negation (in its historical - specifically pre-prescriptivist - uses) is the use of multiple<sup>8</sup> negation, such as *You bring it up or I won’t have none* (McDonald (1980: 13), quoted in Beal (1993: 198)).

In her discussion of aspects of the noun phrase in TE, Beal (1993: 206) notes that in compound (i.e. co-ordinate) subjects “the objective pronoun may be used as the subject in Tyneside English”. This raises interesting issues concerning the concepts of checking and uninterpretable grammatical features (cf. Chomsky 1995, Radford 1997: 170-8). Quoting McDonald (1980: 16), Beal gives the following as an example of TE: *One day him and his dad made a hot air balloon*. Ignoring the adverbial for the purposes of this discussion, we could posit the following derivation for this sentence, shown in (4) below:

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<sup>8</sup> Barber (1997) terms this cumulative negation, which nicely encapsulates the emphatic dimension.

(4)



Our primary interest lies in the position of the subject DP, when it moves to spec-TP position. Following up a conjecture by Henry (1995), we could suggest that the reason the DP *Him and his dad* is assigned objective case (as opposed to the standard English subjective case assignment *He and his dad*) is because it fails to move to spec-AgrSP position, and it fails to move to spec-AgrSP because in such constructions in this variety, AgrSP has weak specifier-features<sup>9</sup>. If the DP cannot move to spec-AgrSP, it must check its objective case in spec-TP as a last resort (cf. Radford 1997: 429). Note that this clause construction is slightly different from that in Belfast English as analysed by Henry: she suggests that in Belfast English, simple DPs (i.e. non-co-ordinated DPs) can appear in subject position with objective case marking (thus *Them is no good*), and the concord between subject and verb may also be non-standard<sup>10</sup>. This might well be taken

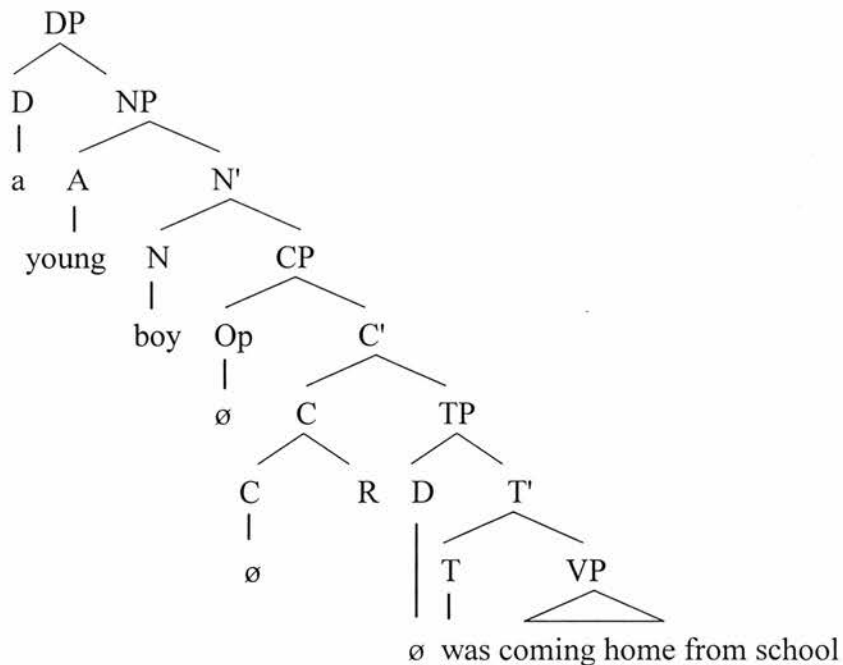
<sup>9</sup> In this theory of language structure, specifier-features are defined as features "which determine the kind of specifier which a given head can have" (Radford 1997: 528). Weak features are those which cannot trigger movement.

<sup>10</sup> This raises some further problems concerning the checking of number agreement features which are not discussed here.

as an indication that the parameters for raising to spec-AgrSP are different in different varieties of English.

Further issues concerning nominal syntax are discussed by Beal (1993: 207) under the topic of relative clauses. TE displays a wide range of non-standard relative features; for instance, there is variation between TE and the standard in terms of which relative pronoun is selected: the relative pronoun *which* can occur with human antecedents (in *the ladies which accompanied him had curly hair*) in TE but not in the standard; and *what* is a relative pronoun in TE but not in the standard (in *The coats what the men wore were very long*). In addition, in TE and other general non-standard varieties (and in earlier stages of English (cf. *My father had a daughter loved a man* Shakespeare, *Twelfth Night* Act 2, scene 4, line 106)), the subject of a relative clause may be phonetically null, so that the structure of *a young boy was coming home from school* in *Leck is a young boy was coming home from school* (McDonald (1980: 20), as quoted in Beal (1993: 208)) is as in (5) below (cf. Radford 1997: 305-7):

(5)



Such an analysis rests on the idea that null-subjects are viable in subordinate clauses in this variety. Radford (1997) argues that certain evidence from early Modern English<sup>11</sup> could be presented to argue that at that stage of its development, English allowed null subjects in certain main clauses, so such null-subjects in relatives are instances of the conservative nature of some aspects of TE grammar, an issue to be discussed further in relation to the modal system of TE, in chapters 5 and 6. Such patterns relate to some features of standard English too, especially where *have* and *be* are the verbs in the superordinate clause, and where the subject of that superordinate clause is indefinite (in instances such as *There are some people don't like him*), but this issue is not investigated further here.

### §1.3.2 *Non-southern non-standard features*

In terms of inflectional morphology, there are a couple of verbs in TE which display a different past tense formation from that of the standard dialect. For instance, they may form their past tense through root modification (change in the root vowel) in the non-standard varieties, but form their past tense through affixation (addition of a bound suffix) in the standard. Put another way, there are instances where verbs are strong in the non-standard dialects and weak in PDSE. An example of this is *treat* (past tense in TE /tret/ (strong) and in PDSE /tritɪd/ (weak)). With *sell* and *tell*, the past tense formation in TE is weak<sup>12</sup> (/selt/, /telt/) but in PDSE is semiweak<sup>13</sup> (/sold/, /told/). An

<sup>11</sup> For example, *hast any more of this* (Shakespeare, *The Tempest*, Act 2, scene 2, line 123).

<sup>12</sup> Notice, however, that the allomorph of the past tense morpheme is not what one might expect: in PDSE when the stem ends in a voiced segment other than /d/, the allomorph is /d/, so we should expect /seld/ etc. here. However, the allomorph selected is that which normally attaches to stems ending in voiceless segments other than /t/, so that voicing assimilation is absent. This absence of voicing assimilation is said to be a characteristic of certain semiweak verbs also (see next footnote).

<sup>13</sup> I take the term semiweak from Guy and Boyd (1998: 196-7) - such verbs are claimed to be "unique in English because they combine morphological characteristics of both the strong class and the weak. In the

instance of this from the recordings made for this thesis is as follows: *he telt his lass that he was afraid of flying* (Tom).

There is still, in TE and other non-southern varieties, aspects of a specific pattern concerning concord which is discernible in northern dialects of ME. Schendl (1994: 148) provides a detailed summary of this pattern which is worth quoting at length:

From at least the 14<sup>th</sup> into the 20<sup>th</sup> century, the choice of the *-(e)s* plural suffix in Scots and northern varieties of British English has been governed by the so-called “personal pronoun rule” (McIntosh 1983: 237) or “Northern Present-Tense Rule” (“NPTR”) (Montgomery 1994) according to which a plural form *-es* is required unless the verb has a personal pronoun subject immediately preceding or following it (McIntosh 1983: 237). This rule can be broken up into two separate syntactic constraints on the use of *-(e)s*: firstly, the “Type of Subject Constraint”, which states that the verb is marked with *-(e)s* “if its subject is anything but an adjacent personal pronoun (Montgomery 1994: 86); secondly, the “Proximity of Subject Constraint”, which “marks with an *-s* any verb having a personal pronoun subject not adjacent to the verb” (Montgomery 1994: 88). These constraints yield the following patterns: *People EATS; they EAT; they EAT and DRINKS*.

McDonald (1980: 12), quoted in Beal (1993: 194), gives evidence from TE of this rule in operation in the present: examples such as *Things has changed* and *I daresay the pitmen maybe swears amongst theirsels* suggest that the {S} suffix is added to verbs whose subjects are 3P forms, and Beal (1993: 194) notes that, “which the exception of the past tense form *was*, this pattern apparently occurs in Tyneside only when the subject is a noun rather than a pronoun”, though she provides no further examples of this.

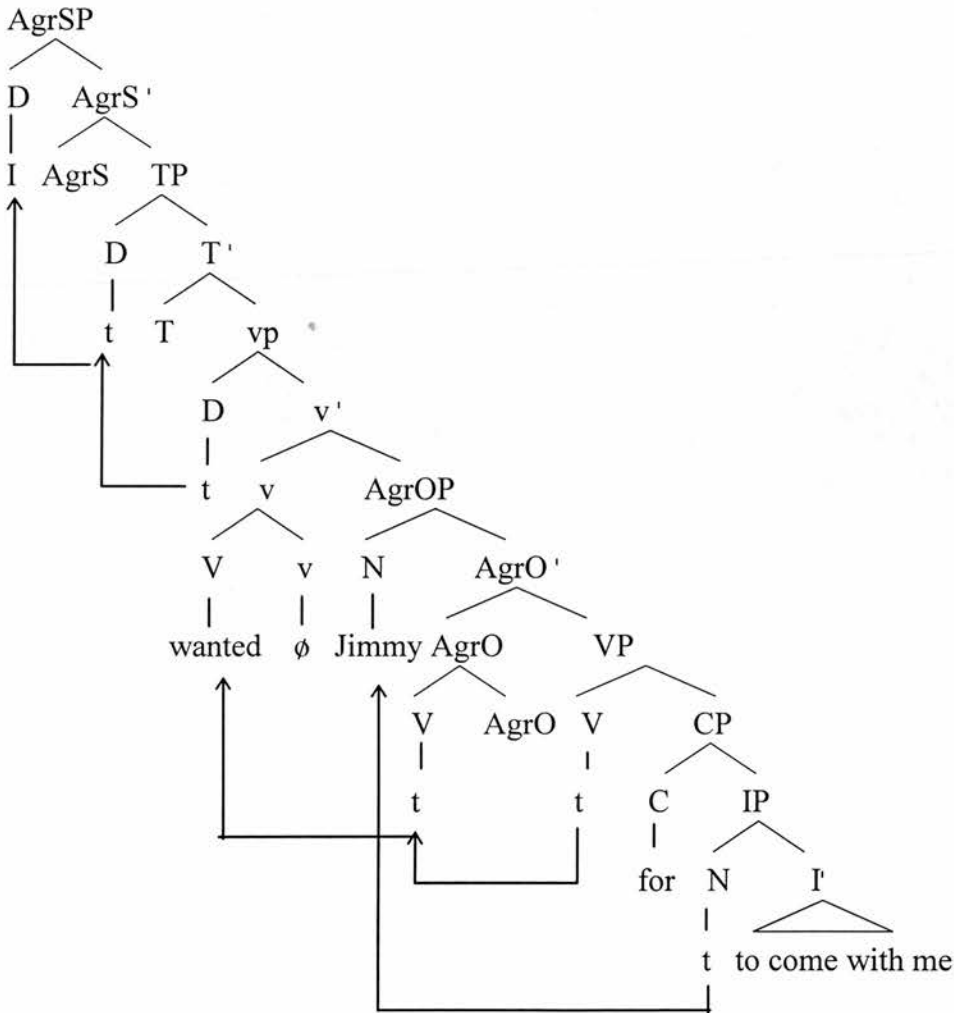
*For to* constructions in TE are described by Beal (1993: 200) as similar to those in Scots and Irish dialects, giving TE examples from McDonald (1985) such as *The*

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past tense, they have a root-vowel change (/slip-slept, luz-lost/) like strong verbs and an apical stop suffix like the weak verbs, a reflex of the *-ed* past tense marker. However, this class cannot be treated as simply an overlap of the other two classes, because some of its members show unique properties: absence of voicing assimilation in the suffix (compare *dreamt* with *creamed*) or regressive assimilation (*lost, left*.)”

*firemen were putting on breathing apparatus for to go into the house.* She notes that such *for to* constructions are particularly used with the sense 'in order to'. However, this is not always the meaning of *for to* constructions, as Harris (1993) and Henry (1995) note. The structure of utterances such as *I want Jimmy for to come with me* in Belfast English (given in Radford 1997: 474-6) is as follows<sup>14</sup> in (6) below:

(6)



<sup>14</sup> This is a slightly fuller derivation than Radford gives, as it includes the spec-vp to spec-AgrSP (via spec-TP) movement which he notes but does not illustrate. The complexities of the derivation are not discussed here as they are not directly relevant to the point being made.

In this construction then, the *for to* clause is complement of the V *want*; in the TE example cited previously, the clause is not complement of the V *put*; rather, it is some sort of result clause. Both types of constructions feature in the TE vernacular.

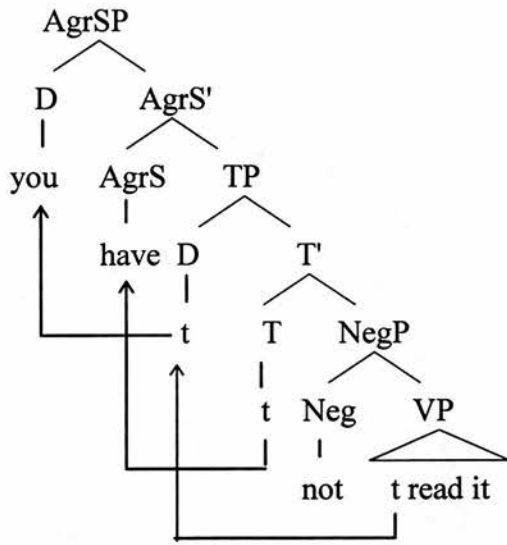
Beal (1993: 199) notes specific patterns of uncontracted negation in TE which are shared with some varieties of Scots. In addition to the uncontracted forms such as *cannot* [kanət] (on which see further §5.3.1.1), Beal notes the behaviour of such forms in negative interrogatives. Consider the following in standard English:

(7) Have you not read it?

(8) Haven't you read it?

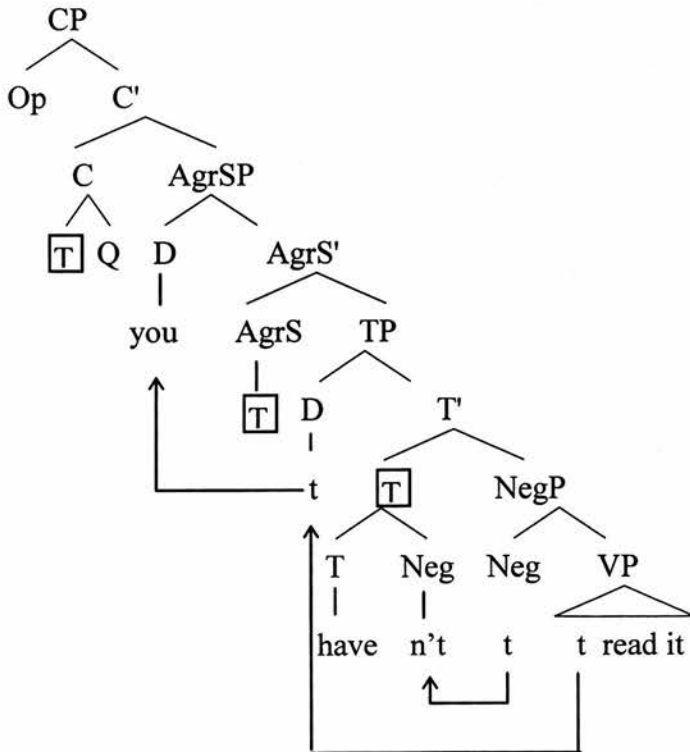
Beal argues that the unmarked form is (8), with (7) reserved for emphatic or highly formal contexts. In TE, however, (7) is the unmarked form, and this variation can be accounted for in the following way. If we assume, following Radford (1997: 231-5), that *not* is not a VP adverb, but rather the (functional) head of NegP, we could analyse the structure of the declarative *you have not read it* as follows (ignoring the internal structure of VP) in (9) below:

(9)



We can posit the following derivation for the standard English unmarked form *Haven't you read it* as that shown in (10):

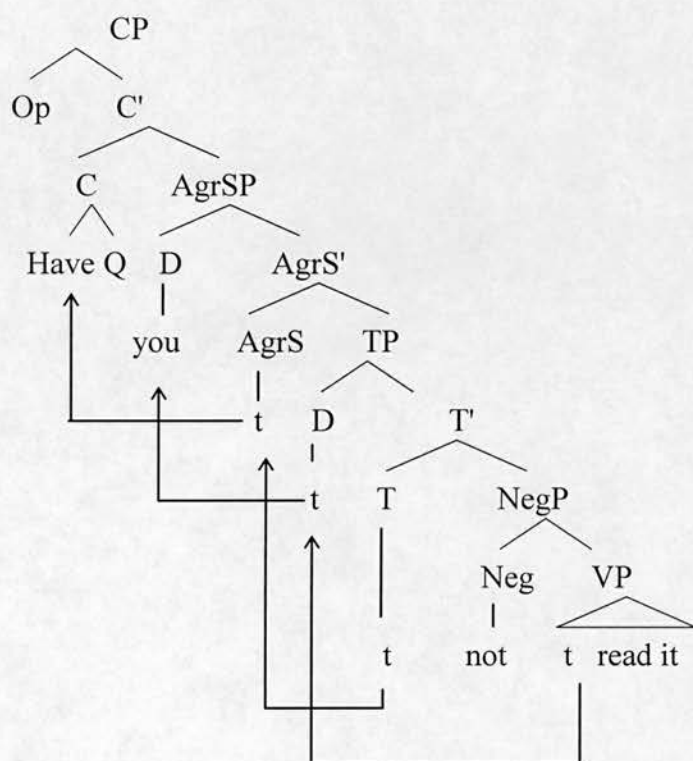
(10)





In the diagram above, I have used the symbol  $\boxed{T}$  to indicate a complex head, generated as head of TP, which undergoes head movement to AgrS and thence to C, where it adjoins to the (strong) question affix Q. The creation of this complex head is motivated by greed<sup>15</sup>: the head of NegP is here a reduced form, a bound clitic, that is, a morphological form which is unable to stand alone and must attach to a host (here, the auxiliary *have* generated under T). Having formed this complex head, the entire constituent undergoes movement, not just one of its constituent parts. By contrast, the TE variant, *Have you not read it?*, is given under (11):

(11)



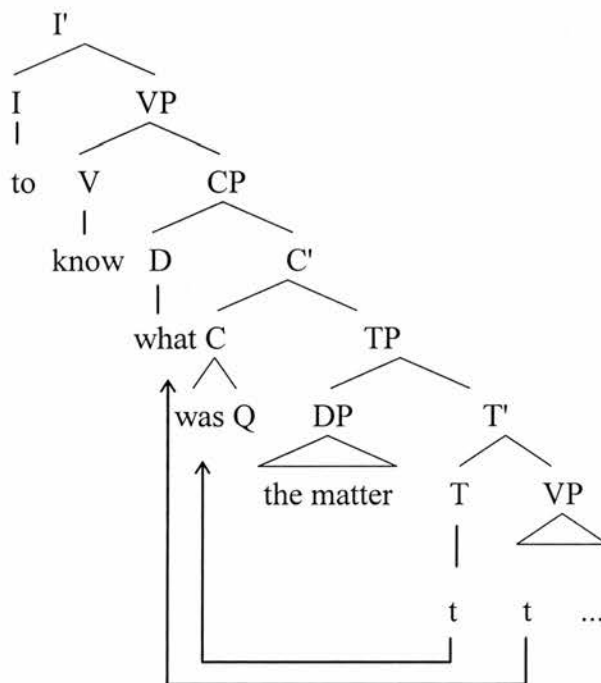
In TE, then, *not* remains as a free form, head of the NegP. There is no need to posit movement of the head to T to form a complex head (i.e. no adjunction), since there is no

<sup>15</sup> "Greed A principle of grammar ... which specifies that constituents move only in order to satisfy their own morphological requirements" (Radford 1997: 510).

greed: the fact that *not* is not phonologically reduced allows it to stay as head of its own phrase, and as a result, the only head movement is that of a simplex head T to C via AgrS.

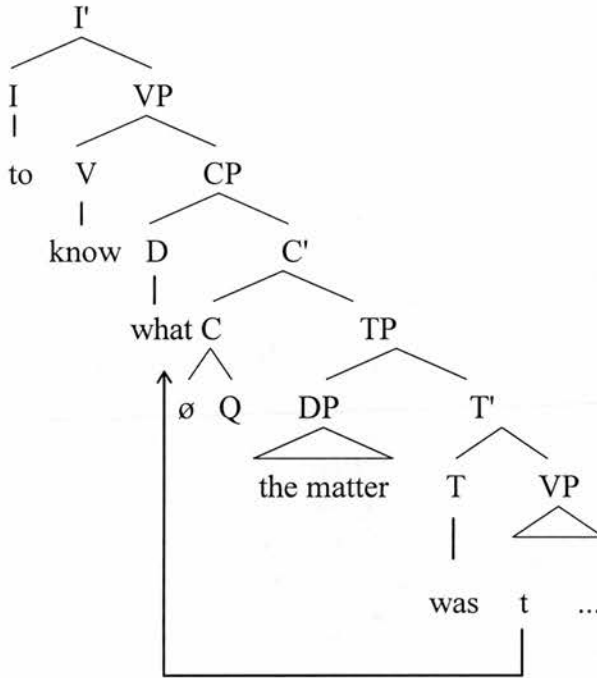
Finally, another feature of TE (shared with Irish English) are embedded questions, of the type *When he discovered I wasn't at school, he wanted to know what was the matter* (Beal 1993: 204). This contrasts with the standard English form *When he discovered I wasn't at school, he wanted to know what the matter was*. The dialects vary in that TE displays T to C movement in embedded questions, while this is blocked in the standard variety. The Radford model (1997: 282-91) seeks to explain such variation as follows; for ease of exposition, the constituent *to know what was the matter/ to know what the matter was* will be analysed. In TE, the structure of *to know what was the matter* is as in (12) below:

(12)



The head C comprises a strong question affix to which *was*<sup>16</sup> adjoins via head movement. Such movement is blocked in standard English, however, resulting in *to know what the matter was*, illustrated in (13):

(13)



The analysis is somewhat unfortunate, however, as Radford (1997: 288) points out. It requires the positing of a null complementiser ( $\emptyset$  in (13) above) which satisfies the morphological properties of the Q affix, making the complex head C well-formed and blocking movement from T to C: the head features of Q are satisfied since Q is affixed to a 'suitable' host. The problem arises from the fact that such an analysis is stipulatory rather than explanatory, as Radford himself admits.

<sup>16</sup> The issue of the precise syntactic status of copular *be* (i.e. whether it should be generated under V or T) is not addressed here, but cf. §2.4.

#### §1.4 *Summary*

This introductory chapter has attempted to present some features of the accent and dialect of TE from descriptive, historical and theoretical perspectives, drawing together some of the issues and data provided in standard reference works on the subject. The evidence suggests that while there are certain features unique to the north-east of England, many aspects of the phonological and grammatical variation presented here can also be found in other non-southern varieties, and many others in general non-standard dialects and accents. Such an approach is broadly in line with analyses of variation which do not consider that varieties of language should be classified as *either* standard *or* non-standard, *either* local *or* non-local, *either* vernacular *or* non-vernacular. Such binary choices fail to reflect the broad diatopic and social spread of such variables; rather, it is preferable to consider variation as a continuum from most local to least local (cf. Milroy, Milroy and Hartley (1994)).

## 2 Some features of present-day Standard English modal auxiliaries

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There is, perhaps, no area of English grammar that is both more important and more difficult than the system of the modals

(Palmer 1990: x)

In this chapter, I present an analysis of features of modal verbs - forms such as *can* and *might* - in standard English. The chapter begins with a discussion of the distinction between mood and modality, before moving on to an attempt to establish a core set of modal verbs of standard English on morphosyntactic grounds: this core, so established, will form a set of variables for analysis in the speech of the Tyneside English (TE) informants. A semantic and syntactic classification of the core is then provided, while the final section considers some aspects of the historical evolution of the modals in the standard variety.

### §2.1 *Mood and modality*

A discussion of features of present-day standard English (hereafter PDSE) modal auxiliaries will inevitably involve a discussion of the terms *mood* and *modality*. Palmer (1986:16) defines modality as “the grammaticalisation of speakers’ (subjective) attitudes and opinions”. The use of the term grammaticalisation here is possibly problematic: the term often refers to diachronic developments within a language whereby certain lexical items/clauses/phrases, in certain contexts, lose their original meanings and come to serve a specific grammatical function independent of that original meaning; it does not seem clear as to whether this is the meaning which Palmer intends. A more likely interpretation is that modality is a means of non-lexical linguistic expression of a speaker’s attitudes and opinions. Indeed, Hopper and Traugott (1993: 1) make clear distinctions between these two separate meanings of the term ‘grammaticalization’: the first concerns the *framework* within which we

can understand and explicate the development and use of grammatical forms within a language; in other words, “an approach to language study” (Hopper and Traugott 1993: xv) - this is the more general of the two meanings of the term, and the one I think is being applied by Palmer in his discussion of modality; the second, more specific interpretation of the term is used to describe the *process* “whereby items become more grammatical through time” (Hopper and Traugott 1993: 2). An example of this is the development of the OE construction *þa hwile þe*, lit. ‘the space of time that’ to *wile*, or ‘while’ in ME as in the following examples (1) and (2):

(1) he hæfde god geþanc  
he had good mind

þa hwile þe he mid handum healdan mihte  
the space of time that he with hands (DAT PL) hold (INF) was able

bord and brad swurd  
shield and broad sword

‘He was in good spirits while he held a shield and a broadsword in his hands’

(2) and ðat lastede þa xix wintre wile Stephne was king  
and that lasted then 19 winters while Stephen was king

‘And that then went on for nineteen years, while Stephen was king’

In this case, a construction involving the content (or lexical) word *hwile* has, since OE, been grammaticalised to a function word, a temporal conjunction, a grammatical item which links parts of a discourse. This sense of grammaticalisation is not as applicable to Palmer’s definition of modality, though it is of great importance in any diachronic study of the modal verbs. It is clear, however, that modality in language, in terms of its function, is related to the expression of a speaker’s attitude. But what form can (or indeed does) this modality take in present day English?

Lass argues that modality “is closely connected but not identical to the traditional grammatical category of mood” (1987: 167). As has been noted elsewhere (Hoye 1997: 37), the distinction between mood and modality has not always been consistently maintained. Palmer (1979: 4) initially proposed that modality is a concept which belongs to the linguistic field of semantics, but later suggested that modality is a grammatical category “similar to aspect, tense, number, gender etc.” (Palmer 1986: 1). As will be seen, this thesis follows the general line of Hoye’s (1997) argument, namely that mood refers to a grammatical (or more specifically morphosyntactic) category, while modality refers to the entire range of modal contrasts, which can be expressed grammatically or morphosyntactically (by, for example, modal verbs), lexically (by modal adverbs such as *possibly*) and even prosodically (where, for instance, expression of a speaker’s doubt is often characterised by a fall-rise intonation pattern). What we are dealing with, then, is a concept which is concerned with the expression of a speaker’s attitude toward the factual content of his utterance (modality) and the variety of ways in which that concept is formulated in language (morphological category of mood, grammatical (sub)category of modal verb, lexical class of adverb etc.).

An initial consideration must be an attempt to define what is meant by mood and how (or indeed if) it is expressed in PDSE. The discussion will be almost exclusively restricted to the nature of mood and modality in English, but cf. Palmer 1986, Lyons 1977 for a more extensive discussion of mood and modality cross-linguistically. Palmer (1986:21) argues that “the term ‘mood’ is traditionally restricted to a category expressed in verbal morphology”. (3) and (4) below indicate the difference between indicative and subjunctive mood, respectively, in Latin:

- (3) Viv- i- t rex  
 live IND 3SPRES king (NOM SING)  
 'The king lives'

- (4) Viv- a- t rex  
 live SUBJ 3SPRES king (NOM SING)  
 'May the king live'

There are remnants of the subjunctive in PDSE, marked on the third person singular form of many verbs by the absence of the inflectional suffix {S}:

- (5) Long live the king!  
 (6) I demand that he finish his thesis on time

and in the case of *be*, the use of *were* in the protasis of *irrealis* constructions, and of *be* in subordinate clauses that function as the complement of certain verbs. Examples (7) and (8) illustrate these features respectively<sup>1</sup>:

- (7) If he were wise, he would drink fewer pints of Boddingtons  
 (8) The coroner ordered that the body be exhumed

As Lass (1987: 167) points out, the subjunctive mood in Latin can express a variety of *modalities* (i.e. the attitudes/opinions of the speaker): optative modality (the expression of wishes) in the case of (4) above and hortatory modality (the expression of exhortations) as in (9) below:

- (9) laud- e- mus te  
 praise SUBJ 1PLPRES YOU (ACC SING)  
 'Let us praise thee'

(5) is much more marginal, more idiomatic, than either (6) or (7) above, which are general though now obsolescent. Can we, then, posit a subjunctive mood in standard English? If we argue that mood is expressed by means of morphological inflection in PDSE, then mood as a grammatical category is not productive or widespread in the standard dialect: notice that (6)-(8) are most likely to appear in very formal discourse. Some uses of the modal verbs, as discussed in §2.3.2 below, can be seen as

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<sup>1</sup> Traces of the subjunctive might be considered even more extensive on the grounds that certain uses of the past tense forms of verbs have modal uses (as in *If I had money, I'd move to Maui*), though this is not discussed further here.



periphrastic subjunctives, however, and the subjunctive mood did exist in earlier stages of the language:

(10)	Đa hæðnan Philistei	behton	hiere	sceattas
	NOM PL MASC	VII 3PPL PAST INDIC	DAT SING FEM	ACC PL MASC
	the heathen Philistines	promised	to-her	monies
	wið ðæm ðe	heo	beswice	Samson
		NOM SING FEM	1 3PSING PRES SUBJ	ACC SING MASC
	on consideration	she	betrays	Samson

'the heathen Philistines promised her money if she would betray Samson'

In summary, if we restrict the definition of mood to the 'the expression of (some types of) modality by means of verbal inflection', it would not be unreasonable to suggest that, with the few exceptions exemplified above, mood is not a grammatical category expressed in PDSE (but cf. Huddleston (1984: 164)).

Modality, as was noted above, can be expressed grammatically in a number of other ways in PDSE. Consider the following sentences:

- (11) He has gone to Newcastle
- (12) He might have gone to Newcastle
- (13) Perhaps he has gone to Newcastle
- (14) It is possible that he has gone to Newcastle

When a speaker uses a construction such as (11), he commits himself to the truth of the proposition he expresses, i.e. he makes a categorical assertion. Lyons (1977: 794f) describes such constructions as 'epistemically non-modal', or factive utterances (on 'epistemic' see §2.3.1.2 below). Non-factive utterances, such as (12)-(14), illustrate a speaker's non-factive attitudes or opinions regarding the proposition. In (12), the modality is expressed by means of a modal verb, *might*; in (13), by means of a modal adverb, *perhaps*; and in (14), by means of a modal adjective, *possible*. This would seem to suggest two things: firstly, that modality can be expressed by

members of various grammatical categories; and secondly, that modality is propositional, not exclusively (or even predominantly) verbal. For the remainder of this part of the chapter, I will concentrate on the various modalities expressed in English in constructions such as (12), that is, those containing modal verbs, since they are to be the syntactic variables in the study as a whole.

## §2.2 *The central modals*

Before examining the different types of modality expressed in PDSE by means of modal verbs, an attempt must be made to clarify the concept of a morphosyntactic category ‘modal’, and specifically the ‘central modals’. Many (synchronic and diachronic) accounts of modal verb usage in English (e.g. Nagle 1989, Lightfoot 1979) make use of terminology such as semi-modals, marginal modals and quasi-modals for items such as *used to*, *have to* and *need*, and such distinctions are generally made on (morpho)syntactic grounds. Below is a list of features often associated with the central members of the modal set, used to distinguish the hard core from other verbs, auxiliaries, marginal modals and semi-modals. It is important to clarify, however, that modality *per se* may be expressed by non-members of the core<sup>2</sup>, and I intend to discuss features of the non-core members as and when they emerge from the data collected from the informants used in this project. That is to say, if there is a specific use of, for example, *have got to* in TE, then comparison will be made to the use of such a form in PDSE in the chapters on TE (i.e. chapters 5 and 6). The next section explores some of the features of the central modal verbs in PDSE, in order to outline a syntactic and semantic framework which is applicable to PDSE and which might function as a control against which the non-standard uses might be compared.

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<sup>2</sup> Indeed it is debatable as to whether all of the central modals (such as the *can* of ability, in *John can speak French*) always express modality.

### §2.2.1 *Some morphosyntactic features associated with 'central' modals*

In this section, I discuss some of the central issues associated with the classification of the modals, with the objective of establishing a set of criteria which can be used to categorise the modals as a distinctive 'group' in English syntax. I realise that some of the terminology is quite loose here; but this is justifiable, given the widespread disagreement in classification of these linguistic items. Each subsection is headed by a general claim; each claim is discussed, and a conclusion based on that claim is drawn; each conclusion leads to the claim in the following subsection. The discussion in this section does not deal with notional criteria used to determine syntactic categories and classes; by notional criteria, I mean (broadly) a method of classification which focuses primarily on what users of language perceive as "conceptual properties" (Anderson 1997: 2) of a given major word class (or subcategory of that class). This notional aspect of classification of modal verbs is the topic under discussion in the next section: detailed discussion of notionalism and its application to the study of modal verbs is left until then.

The following list of features is often used in attempts to distinguish core members of the group of modals from peripheral members and other verbs. Table 1 below lists non-main verbs<sup>3</sup> of English which express modality. By applying each of the features to this list of verbs it is possible to establish a set which could be classed as the core: the others will be categorised as non-core. In other words, only those which fulfil the criteria of all of the following features will be classified as core modals.

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<sup>3</sup> I discuss the syntactic status of modals in some detail in §2.4; but the model adopted in this thesis (which borrows aspects from the Principles-and-Parameters and Minimalist frameworks) considers a main or lexical verb to be a verb which functions as head of VP. In a sentence such as *We might catch the bus*, *catch* is head of the VP *catch the bus*, and is therefore a main verb.

**Table 1**  
Non-main verbs of English which express modality

<i>can</i>	<i>could</i>	<i>may</i>
<i>might</i>	<i>shall</i>	<i>should</i>
<i>will</i>	<i>would</i>	<i>must</i>
<i>ought (to)</i>	<i>be going to</i>	<i>be able to</i>
<i>used to</i>	<i>have (got) to</i>	<i>be to</i>
<i>need</i>		<i>dare</i>

## Feature 1

### The central modals of PDSE lack non-finite forms

The following examples illustrate this feature for *can*:

- (15) (a) \*Old Sam might can sunbathe beside a stream  
 (b) \*Old Sam is canning sunbathe beside a stream  
 (c) \*Old Sam has can/could sunbathe beside a stream  
 (d) \*Canning sunbathe is dangerous for pensioners like Sam  
 (e) \*To can sunbathe is dangerous for pensioners like Sam

Notice that some of the verbs in Table 1 can occur in non-finite form: the various forms of *have to*, for example, could replace those of *can* in (15) above, and the resulting sentences would be grammatical (*Old Sam might have to sunbathe beside a stream* etc.), whereas *ought to* and *will* lack such non-finite forms. There is clear variation (both regionally and socially) in non-finite uses of *used to*. For some speakers, the 'correct' negation of *I used to like curries* is *I usedn't to like curries*; for others, *I didn't used to like curries*. It seems that the latter form (with *do*-periphrasis and non-finite *used to*) is the newer form, but has gained in currency relatively rapidly and is now quite common in PDSE (see further Feature 3 below).

Verbs in Table 1 which display Feature 1:  
*can; could; may; might; shall; should; will; would; must; ought to; be to*

## Feature 2

**The central modals in PDSE do not show inflectional morphological agreement in the third person singular of the present tense.**

That is, they lack the {S} suffix which, when they occur in that particular form, is a distinctive feature of other verbs:

- (16) (a) \*He mays find Case Grammar a shade perplexing  
 (b) He finds Case Grammar a shade perplexing

Note that these features are peculiar to modals alone: for instance, the auxiliary verbs used in PDSE to denote progressive and perfect aspect, and passive voice (i.e. *be* and *have*) have non-finite forms (Feature 1) and show morphological agreement in the third person present tense form (Feature 2), as does auxiliary *do*.

Verbs in Table 1 which display Feature 2:  
*can; could; may; might; shall; should; will; would; must; ought to; used to*

## Feature 3

**The central modals are operators**

That is to say, along with the other PDSE auxiliaries, *have*, *be* and *do*, the central modals display the NICE properties:

- (17) He can reverse round the corner
- |     |  |           |
|-----|--|-----------|
| (a) | He can't reverse round the corner            | Negation  |
| (b) | Can he reverse round the corner?             | Inversion |
| (c) | He can reverse round the corner and so can I | Code      |
| (d) | He <i>can</i> reverse round the corner       | Emphasis  |

Note that these properties are not displayed by main (lexical) verbs (cf. \**He reversesn't round the corner* etc.)

The 'marginal' auxiliaries *need*, *dare*, *used to* and *ought to* are somewhat erratic with regard to Feature 3. Below are the NICE forms for each of the four verbs listed above, with my own view on their grammaticality in PDSE. Grammaticality judgements in the following sentences are purely subjective, and I am aware that what I find acceptable, other PDSE speakers may not, and *vice versa* (as is also the case with the negative forms of *used to* discussed above). This is in itself a salient point: while no PDSE speakers would consider *I might could get it changed* well-formed, there is clearly a varying sense of acceptability regarding the following. The fact that the (non-)grammaticality of such sentences is not clear cut in PDSE might suggest that the verbs in question should not fall into the 'central' modal group.

(i) *Need*

\*He need stay up until you return

(a) He needn't stay up until you return

(b) Need he stay up until you return?

(c) \*He need stay up until you return and so need Max  
(cf. ??He needn't stay up until you return and neither need Max)

(d) \*He *need* stay up until you return  
(cf. He *needn't* stay up until you return)

(ii) *Dare*

\*He dare come in too late

(a) He daren't come in too late

(b) Dare he come in too late?

(c) \*He daren't come in too late and so dare Max  
(cf. ??He daren't come in too late and neither dare Max)

(d) \*He *dare* come in too late  
(cf. He *daren't* come in too late)

(iii) *Ought to*

He ought to lower the price of Boddingtons

- (a) He oughtn't to lower the price of Boddingtons
- (b) ?Ought he to lower the price of Boddingtons?  
(cf. ??Ought he lower the price of Boddingtons)
- (c) ?He ought to lower the price of Boddingtons and so ought Max
- (d) He *ought* to lower the price of Boddingtons

(iv) *Used to*

He used to lower the price of Boddingtons

- (a) ?\*He usedn't to lower the price of Boddingtons
- (b) ?\*Used he to lower the price of Boddingtons?
- (c) \*He used to lower the price of Boddingtons and so used Max
- (d) He *used to* lower the price of Boddingtons

Duffley (1994: 214ff) provides evidence from a variety of corpora (Brown University (American), Lancaster-Oslo/Bergen (British) and Strathy (Canadian)) which suggests that *need* and *dare* display morphosyntactic features which they share with modals in some cases (e.g. *I dare not think*, *Need you make that dreadful noise?*), in that the verbs can:

1. be directly negated by *not*
2. undergo inversion without *do*-support
3. appear in tag-questions
4. appear without the {S} inflection in their third person singular, present tense forms.

However, as Duffley (1994: 215) points out, what “distinguishes *need* and *dare* from the core modals is that they are also used as non-auxiliaries”. He then goes on to list a variety of features associated with ‘full’ verbs, such as negative and interrogative

forms with *do*, and, crucially (though this is not emphasised in Duffley's article), use of *need* and *dare* with the *to* rather than the bare infinitive. These differences might be summed up as follows: *need* and *dare*, when they are followed by a bare infinitive, will occupy the head I position of IP, and when followed by a *to* infinitive, will occupy the head V position of VP. If *need* or *dare* occupies V, and V-to-I movement is blocked in PDSE, then the negative and interrogative forms will require *do* as a last resort<sup>4</sup>.

The crucial aspect of this discussion is that *need* and *dare* are not consistent in their morphosyntactic patterning. Other evidence suggests that *need* and *dare* behave like operators for some speakers only in sentences which, following the terminology of Quirk *et al.* (1985: 138), have 'negative import'. Such sentences do not always contain the negative particle *not* or the verbal clitic *n't*, since there is negative import in the examples in (18) below:

- (18) Noone dare vote Tory in Byker  
I hardly dare vote for Red Ken

This can be tested syntactically by the polarity of the tag question (which would in this case be positive). However, the form of such a tag question is a moot point:

- (19) (a) ?Noone dare vote Tory in Byker, dare they?  
(b) ?Noone dare vote Tory in Byker, do they?

This is perhaps further evidence to support the claim that we should not include *dare* (or *need*, which follows a similar pattern) in the classification of the central modals.

Similarly, the operator status of *used to* is questionable, since for many PDSE speakers, the formation of questions from 'kernel clauses' containing this verb often requires *do*-support (e.g. *Did he really used to be a punk?*). Denison (1993: 323) notes that there was, up until c.18c., a present tense of *use to*, meaning 'be in the habit of'; this present tense form is now obsolete, but can be exemplified by the following, from Pepys' *Diary*:

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<sup>4</sup> For a full explanation of this analysis, see below §2.4.



(20) a fat man, whom by face I know as one that uses to sit in our church

Denison goes on to suggest that *used to* with *do* support is characteristic of informal and non-standard usage, while instances of *used to* displaying the NICE properties appear “rather old-fashioned” (Denison 1993: 323). This irregularity of distribution might allow us to exclude *used to* from the core.

*Ought to* is on the periphery of the central modals from the evidence above. Huddleston (1984: 165) argues that it is questionable whether *ought to* can appear in the apodosis of an unreal conditional construction, but Denison (1992: 239) suggests that this is grammatical in PDSE, though he too notes that *ought* is “on the margins of the modal system”. Consider (21) below:

- (21) (a) If Barry came, we could go to the pub  
 (b) ?If Barry came, we ought to go to the pub

The apodosis of an unreal conditional requires a modal verb in the past tense: hence, of the verbs in Table 1, only *could*, *might*, *would*, and (for some speakers at least) *should* could appear in this position. Further evidence to suggest that *ought* is on the periphery of the modal system is that it requires a *to*-infinitive as complement. *Must* cannot appear in the apodosis of an unreal conditional either, so we might want to consider *must* marginal too, as we have with *ought*. But the diachronic development of *must* (morphosyntactically) is in contrast with the other modals. The form *must* derives from *moste*, this latter being a “remote or subjunctive preterite” (Warner 1993: 174-5) of OE *mot*, meaning ‘can’ or ‘must’ (Denison 1993: 295). OE *mot* was one of the so-called ‘preterite-present’ verbs, whose “present tense is derived from a type of Indo-European perfect formation ... and [whose] conjugation is very like that of the regular strong preterite” (Warner 1993: 140). Since the present tense form *mot* does not survive in PDSE, *must* appears to be a preterite-present twice over: as Denison (1992: 240) argues, *must* “is [now GT] a present-tense form with a few remnant past-tense uses, probably because it has no related past-tense partner”. This contrasts with other OE preterite-presents which evolved into PDSE modals, such as

*magan* (3 sing pres indicative *mæg* > *may*; 3 sing past indicative *meahte* > *might*). Despite the fact that *must* behaves like *ought* with regard to its inability to appear in apodoses of unreal conditionals, *must* nevertheless is still unambiguously an operator, so it displays Feature 3.

Verbs in Table 1 which display Feature 3:  
*can; could; may; might; shall; should; will; would; must; have (got) to; be to; be going to; be able to*

Combining the three features, we have:

Verbs in Table 1 which display all three Features:  
*can; could; may; might; shall; should; will; would; must*

On morphosyntactic grounds, these are then the central modals of PDSE. These forms themselves, however, are by no means uniform morphosyntactically (cf. the discussion of *must* above, and consider the ungrammaticality of *\*He mayn't have visited Leith docks* as compared to *He may not have visited Leith docks*). This illustrates the fact that, on morphosyntactic grounds, the modals are notoriously difficult to classify precisely<sup>5</sup>.

### §2.3 *Semantic classification of the central modals*

If a brief attempt to outline some of the formal morphosyntactic ways in which we can identify the central modals of PDSE seems ambitious, any endeavour to classify the semantics of these verbs is even more formidable. It may be wise to begin with some initial terminology from scholars, looking at some specific examples, and finally (in this section at least) to examine how precise such terminology is.

<sup>5</sup> The situation is complicated even further when tense-time relationships are considered. I do not go into this in great detail, but merely mention here that in the two sentences *I can come if you want* and *I could come if you want*, while *could* is classed as the 'past tense' form of *can*, the time reference is, in both cases, to the future.



### §2.3.1 *Some working definitions*

#### §2.3.1.1 *Modality and proposition*

Consider (22) and (23) below:

(22) He could sign the treaty

(23) He could not sign the treaty

We can note that (23) is ambiguous, since the two meanings of (23) are (a) ‘It is possible for him not to sign the treaty’ and (b) ‘It is not possible for him to sign the treaty’. It is claimed (cf. Palmer 1990: 34) that semantically in (a) it is the **proposition** (*he sign the treaty*), or more specifically, the meaning carried by the lexical verb, which undergoes negation, whereas in (b) it is the **modality** (displayed by *could*) that is negated. Joos (1964: 149,151) prefers the term ‘event’ for whatever is expressed by a lexical verb which follows a non-epistemic (cf. §2.3.1.2) modal (e.g. *He mustn't come in yet*). However, as Palmer (1990: 34) argues, it is simpler to use a general term which can be applied to “all types of modality”, although strictly speaking it is not the modality to which we are applying this term, but rather to the non-modal elements of the predicate. The distinction between modality and proposition is important in this regard. In examples such as (24):

(24) He mustn't sign the treaty

*mustn't* indicates the necessity for something not to be done (i.e. propositional negation), but morphologically it is the modal which is marked for negation. In other words, if we suggest that in (24) above there is a proposition [HE SIGN THE TREATY], and a marker of modality [MUST], we can decode the meaning of (24) as [MUST] [-HE SIGN THE TREATY], where the tilde marks the semantic negation. But in terms of the syntax, the negative clitic (a bound morpheme) does not attach to the non-modal elements of the predicate: it attaches to the modal verb. This complexity in the syntax-to-semantics mapping suggests that we need some definition of proposition; a

useful one is provided by Lyons (1977: 141-2), who defines a proposition as “what is expressed by a declarative sentence when that sentence is uttered to make a statement”, and later in the work introduces a notational system:

with which we can conveniently distinguish modal and non-modal utterances of various kinds. Categorical assertions will be represented symbolically as

$$..p$$

The first full stop stands for the unqualified neustic, and may be read "I-say-so"; the second full stop stands for the unqualified topic, and may be read "it-is-so" ... all three of the components of the utterance may be negated so that

$$\sim ..p$$

will mean "I-don't-say that it is the case that  $p$ " (non-commitment);

$$.\sim.p$$

will mean "I say that it is not the case that  $p$ " (denial); and

$$..\sim p$$

will mean "I say that it is the case that not- $p$ " (context-free assertion of a negative proposition)

(Lyons 1977: 802-3)

I have quoted this at length since it is an important attempt to formalise the difference between modal and non-modal utterances (which may or may not include modal verbs). Note for example it allows us to represent the structure of (25) as *poss poss p*:

(25) Perhaps he can speak French

i.e. "I say possibly that it is possibly the case that  $p$ " where  $p$  is the proposition 'HE SPEAK FRENCH'. I will use this framework henceforth as a means of establishing the difference between **subjective** and **objective** modality, where this is necessary. "I say possibly", related to the unqualified neustic, displays subjective modality; "it is

possibly”, related to the unqualified tropic, displays objective modality. This might also help an analysis of modal vs. propositional negation:

(26) Sue mightn’t go to the party = . poss  $\sim p$

(27) Sue can’t go to the party = .  $\sim$  poss  $p$

(26) means ‘I-say-so it-is-possible that SUE NOT GO TO THE PARTY’, and (27) means ‘I-say-so it-is not-possible that SUE GO TO THE PARTY’. The system is not perfect, however, but I will withhold discussion of its weaknesses, particularly with regard to the distinction between epistemic and deontic/dynamic (or root) modality. For now it is sufficient to say that this method helps to distinguish both between modality and proposition and between subjective and objective modality.

#### §2.3.1.2 *Types of modality*

I introduced above the concepts of **subjective** and **objective** modality. Alongside these terms, it is important to consider the notions of **epistemic** modality and **root** modality, the latter of which is sometimes (e.g. by Palmer 1990) subdivided into **deontic** and **dynamic** modality. This tripartite division has as its roots the modal logicians’ distinction of three modi (or categories of modality), as noted by Klinge (1993: 318):

EPISTEMIC modality, the modality of knowledge and belief, such as ‘it is believed that’ and ‘it is known that’;

DEONTIC modality, the modality of permission and obligation, such as ‘it is permitted to’ and ‘it is obligatory to’;

DYNAMIC modality, the modality of ability and disposition, such as ‘X is able to’ and ‘X is willing to’.

Let us begin with epistemic modality.

Epistemic modality is concerned with judgements made regarding the validity of a proposition, based on the speaker’s knowledge: it is “the modality of propositions, in the strict sense of the term, rather than of actions, states, events, etc.” (Palmer 1990: 50). Bound up in these judgements are the notions of **possibility**

(what one might tentatively, following Palmer (1990: 57), describe as a ‘weak’ epistemic judgement) and **necessity** (a ‘strong’ epistemic judgement). Consider the relationship between *may* and *must* in the following sentences:

(28) He may find it difficult supervising Geordies

(29) He must find it difficult supervising Geordies

Paraphrasing<sup>6</sup> (28), we have ‘It is possible that he finds (or will find) it difficult supervising Geordies’. But for (29), the paraphrase follows a different pattern, since it does not mean ‘It is necessary that he finds it difficult supervising Geordies’. A more accurate paraphrase would be ‘the only possible conclusion is that he finds it difficult supervising Geordies’. Possibility and necessity, then, will need to be considered separately in terms of the relationship between the marking of the modality and the nature of the proposition.

Deontic modality “is concerned with the possibility or necessity of acts in terms of which the speaker gives permission or lays an obligation for the performance of actions at some point in the future” (Hoye 1997:43). This is exemplified in (30) and (31) below:

(30) You can leave now if you want to

(31) You must leave this very second

where (30) is paraphrasable as ‘It is possible for you (or I give you permission) to leave now’ and (31) is paraphrasable as ‘It is necessary for you to leave now’.

Dynamic modality is problematic. It is more controversial (in terms of its status within a formal modal system) than epistemic and deontic modality, and Palmer argues that it “is not, perhaps, strictly modality at all” (Palmer 1990: 7). It is subject- rather than speaker-orientated - this is, crucially, what distinguishes it from epistemic and deontic modality; what is at stake with dynamic modality is not the

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<sup>6</sup> See Klinge (1993) for a convincing argument concerning the problematic nature of paraphrasing such modal utterances.

views or wishes of the speaker, but rather some kind of comment on or assessment of the subject of the utterance. This can be exemplified as follows:

(32) Bob can speak Italian

A paraphrase of (32) might be ‘Bob has the ability to speak Italian’, where it is clear that the speaker’s opinion is not really at issue, other than what passes for fluency for one speaker may not do so for another. These then are the three major types of modality with respect to which we will be discussing the semantics of the modal verbs in this thesis.

A question remains regarding the relationship between subjective and objective modality on the one hand, and epistemic, deontic and dynamic modality on the other. Recall that subjective modality is that which relates to an inference by the speaker, while objective modality is that which relates to something externally verifiable. On these grounds (cf. Palmer (1990: 7) and Lyons (1977: 792)) we can suggest that epistemic modality in language is a type of subjective modality, as is deontic modality. Both are concerned with the *speaker’s* awareness of the truth, likelihood, imposition etc. of the proposition. Dynamic modality does not have this subjectivity, and is therefore more closely aligned with objective modality; but as noted above, there are doubts as to whether or not such forms as ability *can* and future and volitional *will* display any kind of modality at all.

Furthermore, there is the complicating factor of indeterminacy which needs to be addressed. Ambivalence is a marked feature of modality in English, and this ambiguity is compounded by the fact that the same modal verbs can be used to express different types of modality, as can be seen in the following examples:

(33) Fiona may be an electrician

(34) You must have a new suit

In (33), the two possible interpretations are as follows: ‘it is possible that Fiona is an electrician’, where the modality is epistemic, or ‘I give permission for Fiona to be an



electrician', where the modality is deontic. In (34), the two interpretations are: 'from the evidence available to me, I conclude that you have a new suit' (epistemic) and 'it is necessary for you to have a new suit' (deontic). Two major points arise here. Firstly, the ambiguity is much greater in the second of these two examples, in that while epistemic or deontic interpretations of (34) are equally likely, a deontic interpretation of (33) is less likely than an epistemic reading. This may again be associated with the semantics of the individual modal (or more likely the individual main verb) involved (here *may*), though there are examples with *may* where epistemic and deontic readings are equally likely (for instance, *you may have a bicycle*). Secondly, this ambiguity is particularly noticeable in decontextualised examples: it is likely that the intended interpretation will follow from the discourse in which the utterance occurs.

In summary, this section has briefly introduced the terms modality, proposition, subjective, objective, epistemic, deontic, dynamic and indeterminacy, provided some working definitions, and outlined how they are to be understood within the framework of this thesis.

### §2.3.2 *The semantics of the central modals*

The discussion in this section is an attempt to establish which of the various types of modality outlined in §2.3.1 can be expressed by the central modal verbs classified in §2.2. Going through each of the verb forms in turn, I will attempt to establish whether or not they express epistemic, deontic and dynamic modality in PDSE. This will allow us to compare the means of expression of these types of modality in PDSE with the data collected from the informants in chapter 5. The aim in this section is not to provide an exhaustive account of the semantics of modal verbs in PDSE: for such information, see Coates (1983), Palmer (1986, 1990) and



Hoye (1997). Rather, it is an attempt to establish which verbs are relatively more or less common in expressing the variety of modalities already established. As a result, this section provides only a cursory glance at the complexities of the semantics of the modals: such complexities will be discussed when the data from the informants (which provide the evidence for the sociolinguistic correlations at the heart of this thesis) are introduced.

### §2.3.2.1 *Can*

Epistemic uses of *can* are complex in PDSE. In non-assertive<sup>7</sup> contexts, there is evidence to suggest an epistemic function, as in (35) below:

(35) Can it be I'm falling in love?

which has the interpretation 'Is it possible that I'm falling in love?'; but outwith these contexts, the epistemic status of *can* is by no means as clear cut. Perkins (1983: 35) argues that examples like (36) below (which is my own creation, not Perkins', but which is of the type he describes) illustrate an epistemic use of *can* in an 'assertive' context:

(36) Beer can make you put on weight

As Hoye (1997: 85f) argues, it is by no means clear that this has any real epistemic sense. There is a clear difficulty here in terms of interpretation: does (36) question the validity of the proposition BEER MAKES YOU PUT ON WEIGHT, in which case this *is* an example of epistemic modality, or does it have the sense of a statement that there exist in the world circumstances in which beer potentially makes you put on weight, in which case this is an example of dynamic modality? The latter interpretation is favoured. Notice that it is arguable that the modality here is subject<sup>8</sup> not speaker-orientated (a characteristic of dynamic modality), and its meaning is more closely

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<sup>7</sup> i.e. contexts associated with negative or interrogative clauses.

<sup>8</sup> There is, I think, a potential problem in associating dynamic modality solely with subjects, given sentences such as *It can rain for hours*, but this issue is left unresolved here.

linked to the tendency of the proposition to occur, rather than a questioning of its inherent truth value. Even with examples like (37):

(37) you can hardly blame him

where the modality expressed by the modal is interpretable as epistemic, it is also true that the context is non-assertive, since the adverb *hardly* gives the utterance negative import (tested syntactically by the polarity of the tag question, here *can you?*).

Deontic interpretations of *can* are slightly more clear cut than epistemic interpretations. The verb is widely used in PDSE for seeking permission as in the examples in (38):

(38) Lyra can leave after she has finished her work

Can I leave you to it?

and there are dynamic interpretations for *can* too, as in (39):

(39) Bill can speak Portuguese

However, it is clear that there is a substantial degree of indeterminacy regarding interpretations of some of the modalities expressed, as in (40):

(40) Jimmy can play with you now

While the modality here is clearly not epistemic, out of context it is not possible to ascertain whether the meaning is ‘Jimmy is able to play with you now (that his broken arm is mended)’ (dynamic), ‘I give permission for Jimmy to play with you’ or ‘It is possible for Jimmy to play with you now (that he has done his homework)’ (deontic). This of course need only be a problem if we insist on attempting to understand the semantics of modal verbs outwith the discourse in which they are located (see Klinge (1993)); that is, if we insist on a non-pragmatic account. Nonetheless, it is important to recognise that in isolation, such indeterminacy exists, and that certain of the modal verbs are ambiguous in the modality they express: here,

it is clear that in ‘assertive’ contexts, *can* can mark both dynamic and deontic modality.

### §2.3.2.2 *Could*

Epistemic interpretations of *could* in assertive contexts are more likely than those of *can*, as (41) shows:

(41) Melanie *could* be a policewoman by now

ie ‘given my knowledge of affairs, it is possibly the case that MELANIE IS A POLICEWOMAN’, although as shall be shown later, *could* is more tentative than *may* or *might* in the expression of epistemic possibility. Here then is a semantic difference between these two morphologically related<sup>9</sup> forms: *can* does not express epistemic possibility in assertive contexts, unlike *could*.

Non-epistemic interpretations of *could* are closely aligned to those of *can*; indeed, the major difference between the two is often one of tentativeness, with the historically past tense form *could* being more tentative than *can*. Sometimes, however, the tense-time relationship between *can* and *could* is clear. Example (42) below illustrates the use of *can* to express deontic modality:

(42) Passengers *can* smoke on this flight

Replacing *can* with *could* would not change the modality, but would alter the time reference, locating the granting of permission in past time, so that the permission is not necessarily granted anymore:

(43) Passengers *could* smoke on this flight

Dynamic possibility can also be expressed by *can* and *could*, though there may be some debate about the extent to which the following examples express possibility rather than ability:

(44) This sword *can* slice through any armour

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<sup>9</sup> It has to be admitted that this morphological relation is somewhat opaque, however.

(45) This sword could slice through any armour

In terms of the relationship between *can* and *could* here, this is an example of *could* expressing a greater degree of tentativeness than *can*. It has been suggested (Hoye 1997: 78) that root ability be seen as a special case of root possibility, and that the distinction between a dynamic possibility interpretation of (44) ('it is possible for THIS SWORD TO SLICE THROUGH ANY ARMOUR') and a dynamic ability interpretation ('THIS SWORD is able to SLICE THROUGH ANY ARMOUR') is so small to be practically meaningless; as Hoye (1997: 90) argues, the "indeterminacy between dynamic possibility and ability senses makes it all but impossible to distinguish between them and it seems pointless to argue which of the two senses prevails".

#### §2.3.2.3 *May*

There can be little doubt that *may* functions as a marker of epistemic possibility, as example (46) illustrates:

(46) Tom's behaviour may surprise his father

Again there is a possible indeterminacy between epistemic and deontic interpretations of *may*, as in (47):

(47) Your brother may have some sweets

where the epistemic interpretation is 'It is possible that YOUR BROTHER HAS SOME SWEETS' and the deontic interpretation is 'It is possible for YOUR BROTHER TO HAVE SOME SWEETS'. Again it is likely that the intended interpretation would be clearer if the utterance were located in a specific discourse, rather than be taken as an isolated example. Furthermore, with (46), even out of context, a deontic interpretation is highly marginal, as it would have the meaning 'it is permitted for TOM'S BEHAVIOUR TO SURPRISE HIS FATHER', in the sense of his appearance being allowed to surprise him. This may well have something to do with the nature of the subject of the sentence, and the

thematic role<sup>10</sup> it is assigned by the verb: *surprise* in (46) might be said to assign the  $\theta$ -role of INSTRUMENT to the subject DP *Tom's behaviour* and EXPERIENCER to the object DP *his father*, and if the INSTRUMENT DP contains an inanimate or abstract noun, it cannot, when it appears with a modal such as *may*, express deontic possibility (cf. the ambiguous *Tom may surprise Jill*, where a deontic interpretation might suggest the DP *Tom* as AGENT, and an epistemic interpretation might suggest the DP *Tom* as INSTRUMENT, like the DP *Tom's behaviour* in (46): this analysis, however, suggests  $\theta$ -roles may vary dependent not on the argument structure of the main verb, but on the semantics of the modal<sup>11</sup>). However, what is clear is that *may* in PDSE frequently functions as a marker of epistemic modality.

In non-epistemic occurrences, *may* may have a similar meaning to dynamic *can*, as in (48):

(48) Many Hibs supporters may be seen in Easter Road pubs

This raises another issue which has not yet been dealt with, namely the degree of formality of the discourse. In terms of type of modality, there may be little to distinguish between *may* and *can* in (48) and (49) below:

(49) Many Hibs supporters can be seen in Easter Road pubs

but even in PDSE, (48) would be typical of a relatively formal piece of discourse, certainly more formal than (49). This is reflected also in dialect and diachronic usage: McDonald (1981: 27f) suggests that historically, the development of *can* and *may* might be seen in terms of a 'semantic rivalry' which *can* seems to be winning. In OE, *magan* was often used in an 'ability' sense (cf. Traugott 1972: 198-9); in ME, *can* gradually took over this ability sense and by ModE, *may* ceased to occur in these contexts. Similarly, McDonald argues, in ME *may* took on a permission sense, which was adopted by *can* in eModE and by PDSE *may* is losing ground again. Certainly in some regional varieties occurrence of *may* (in whatever sense) is

<sup>10</sup> The issue of thematic roles is discussed in detail in §2.4.

<sup>11</sup> On which, again, see further §2.4 below.

increasingly uncommon; in Trousdale (1994), in over 5 hours of recordings of various groups of Tynesiders, *may* occurred only twice, and other linguists have argued that *may* “is extremely rare in Tyneside” (McDonald and Beal 1987: 49). It has also been suggested (Kytö 1987) that *may* is becoming marginalised in the standard variety too (cf. Palmer 1990: 103-11).

#### §2.3.2.4 *Might*

An epistemic reading of *might* is clear in (50) below:

(50) John might change the lock for me

where *might*, more specifically, marks epistemic possibility. It has been suggested (Palmer 1990: 45, 58) that in such instances *might* is merely a more tentative form of *may*, and McDonald (1981) notes that, in her non-Tyneside corpus of data, ‘epistemic possibility’ uses of *may* and *might* were almost equally divided (43.48% and 55.62% respectively), a view upheld by Coates, who suggests that for standard English speakers *might* is no longer the tentative form of *may*, but merely its semantic equivalent, with the meaning ‘it is possible that’ (Coates 1983:153).

A deontic reading is required in instances such as (51):

(51) You might tell me next time you plan to be out late

Hoye (1997: 99) argues that deontic interpretations of *might* tend to be rather formulaic. It might also be argued that the deontic force in such a reading is weaker than with *should*, for instance: in other words, the tentativeness usually associated with epistemic readings of *might* might equally be applied to its non-epistemic uses.

#### §2.3.2.5 *Shall*

This seems to be a rare form, both in PDSE and certain regional varieties (cf. Hoye 1997: 120, McDonald 1981); Trousdale (1994) had no instances of *shall* in his

(admittedly small) database; and therefore the discussion of the form will be limited here. From examples such as (52), it is clear that *shall* functions epistemically:

(52) I think I shall probably have retired by then

though such an epistemic reading may have more to do with the adverb *probably* than with the modal verb; and from (53) that it functions deontically:

(53) I shall reply to all letters

Furthermore, it is clear from both (52) and (53) that this seems to be a marker of relatively formal discourse. This is made more clear when *shall* in (52) and (53) is replaced with *will*; and the cline of (in)formality is increased even further if *shall* is replaced by the cliticised form *'ll* (which seems to neutralise any (prescriptive) contrast between *shall* and *will*). With its epistemic reading, *shall* relates to some sort of prediction; and with its deontic reading, *shall* implies some sense of purpose or intent.

#### §2.3.2.6 *Should*

While it is clear from the following:

(54) He should have been drunk after last night's party

that *should* can function as a marker of epistemic modality, it is the deontic readings of *should* on which I intend to concentrate, as there are some crucial areas of debate surrounding such readings. Anderson (1991: 22) ascribes to *should* the status of a "minimal periphrasis". This follows from his argument that certain pairs of constructions enter into a paradigmatic semantic relationship. Consider for instance the following pair:

(55) Vera was running for the bus

(56) Vera ran for the bus

The first of these examples is syntactically more complex than the second, in that it involves a two-verb sequence, while the second has only the one (main) verb, and it



is the semantic relationship between the complex and the simple form that Anderson defines as paradigmatic. The nature of the periphrasis is variable, that is, it may involve “complementary or free variation; it may be contrastive” (Anderson 1991: 19). *Do*-periphrasis is, for instance, in complementary distribution with the simple form: in those circumstances in which *do*-periphrasis appears (negation, interrogation and emphatic use, for instance), the simple form is prohibited; conversely, in those circumstances in which the simple form appears (positive declaratives), the periphrastic form is prohibited (in standard English at least; in some non-standard varieties, the periphrastic form in positive declaratives functions as a marker of habitual aspect (cf. Garrett 1998)). By contrast, consider the following:

(57) Bob had lived in Walkerburn for 2 years

(58) Bob lived in Walkerburn for 2 years

In (57), the periphrastic form, there is the entailment that Bob is or at least may still be living in Walkerburn; this meaning is excluded from (58), the simple form: the entailment here is that he is no longer living there. A further complexity is introduced (Anderson 1991: 19) by examples like the following:

(59) Sam sunbathed every Monday

(60) Sam used to sunbathe every Monday

(61) Sam sunbathed last Monday

(62) \*Sam used to sunbathe last Monday

In other words, the periphrastic form *used to* is only in free variation with the simple form in certain semantic interpretations: Anderson (1991: 19) suggests periphrastic forms of this type have a ‘deneutralising’ function, “in forcing one reading”.

This all relates to *should* in the following way. In certain uses, *should* seems to function as a periphrastic subjunctive: by that it is meant that the form enters into a periphrastic semantic relationship with the simple (uninflected) subjunctive, which,



as noted at the beginning of this chapter, is not particularly productive or widespread even in the standard dialect. An example of this is as follows:

(63) I insist that she finish by midnight

(64) I insist that she should finish by midnight

This suggests that one of the meanings of *should* in PDSE is that it is a periphrastic marker of the mandative subjunctive. This interpretation of the semantics of *should* in such instances calls into question the observations made by Palmer and Bira as reported by Hoyer (1997: 109): “Palmer (1990: 122) argues that ‘it is not at all clear that English makes any distinction between SHOULD and OUGHT TO’ and Bira (1979: 206) also remarks that ‘Most native speakers of present day English do not feel any difference between the two modals and use them interchangeably’”. This suggestion seems to be based on examples such as the following:

(65) If Bob thinks we should go, we ought to go

in which the modality expressed in the embedded clause in the protasis is semantically identical with the modality expressed in the apodosis. I am not absolutely convinced that there is semantic equivalency even here, though I am sure other standard English speakers would disagree with me: the modality expressed is deontic, but in the apodosis here, *ought to* might imply a sense of moral obligation, which is lacking in the modality of the protasis. However (65) is (for me) semantically equivalent to (66):

(66) If Bob thinks we ought to go, we should go

i.e. they are in free variation, which would support Palmer and Bira’s argument. This would suggest two things: firstly, that both *should* and *ought to* can represent different types of deontic modality (with or without this sense of moral obligation) and secondly, for those who argue that different types of modality are expressed in the protasis and apodosis of (65) above, that our interpretation of the modal meanings inherent in *should* are further conditioned by the type of clause (for instance,

conditional) in which they appear. Nonetheless, it is hard to argue that *should* is invariably semantically equivalent to *ought to*: (64) is not semantically equivalent to (67) below:

(67) I insist that she ought to finish by midnight

In other words, in instances where *should* functions as a periphrastic subjunctive, the semantic equivalency with *ought to* does not hold.

### §2.3.2.7 *Will*

A central complexity with *will* is the extent to which its use with reference to future events should be subsumed under a general epistemic heading. *Will* (like *shall*) is in its epistemic use clearly associated with prediction, and if such prediction relates to the speaker's assessment of the probability of occurrence of the proposition, then it is reasonable to associate 'future' *will* with epistemic *will*.<sup>12</sup> As Anderson (1971: 105) notes, 'non-complex' (his term for epistemic here) *will* can sometimes be "indifferent" to future versus non-future interpretations, the time reference being marked by adverbials: this might account for instances such as the following, in Palmer (1990: 148):

(68) My babe-in-arms will be fifty-nine on my eighty-ninth birthday

Palmer suggests this is future *will*, although he notes (1990: 163) that, given the frequent indeterminacy between the epistemic *will* and future *will*, little is gained in an attempt to distinguish them. He argues (1990: 57) that a useful paraphrase of epistemic *will* might be 'a reasonable inference is that PROPOSITION', but that in instances such as (69)

(69) John will go to London tomorrow

attempts to distinguish between the two interpretations fail: in other words, in (69), there is simultaneously both reference to future time and a possible paraphrase 'it is a

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<sup>12</sup> See further Palmer (1990: 161-3), Jenkins (1972: 73), Huddleston (1976: 69) and Coates (1983: 177-83) for further discussion on the pros and cons of analysing 'future' *will* as epistemic.

reasonable inference that JOHN GO TO LONDON TOMORROW'. When *will* co-occurs with *be* as a marker of progressive aspect, an epistemic interpretation is often favoured:

(70) John will be going to London tomorrow

Palmer argues that even though the progressive aspect is marked, in instances such as (70) "there is no duration". I do not quite see how this helps the distinction between future *will* and epistemic *will*, however, specifically because I fail to see how there is no duration in (70) or how that, even if there is no duration, the epistemic reading is clearer or more marked than a simple future one. It seems to me more logical to see a wide range of interpretations of epistemic modality, one of which (specifically related to 'prediction' modals like *will*) is reference to future time. Broadening the scope of epistemic interpretations bypasses the need to establish criteria to distinguish forms which many speakers of PDSE will see as semantically identical.

#### §2.3.2.8 *Would*

*Would* functions as a past tense form of *will* and as a hypothetical marker (Coates 1983: 204), with both epistemic and deontic interpretations, as evidenced by (71) and (72) below:

(71) He worried that she would be miserable this Christmas

(72) I asked you not to do this, but you would come and spoil it all

A possible paraphrase for epistemic *would* might be 'it was predictable that PROPOSITION'; while this instance of deontic *would* has a clear sense of some sort of insistence on the part of the subject noted by the speaker. Other possible meanings of *would* in PDSE (on which see further Coates (1983: 206ff)) are willingness and intention.

### §2.3.2.9 *Must*

*Must* is a clear marker of epistemic necessity, as (73) shows:

(73) He must have been a salesman in a past life

This could be paraphrased as ‘the only possible conclusion is PROPOSITION’, that is, a very strong epistemic judgement. Coates (1983: 31ff) argues that epistemic *must* carries the meaning of logical necessity or confident inference, conveying “the speaker’s confidence in the truth of what he is saying, based on a logical process of deduction from facts known to him (which may or may not be specified)” (Coates 1983: 41), and despite Palmer’s (1990: 53) objection to the use of ‘confidence’ since it relates too closely to certainty, this seems to be a useful description of the meaning inherent in the use of *must* epistemically. Coates (1983: 42) also notes that epistemic *must* was rare in her corpus of everyday spoken English, while Hoyer (1997: 101) found that epistemic and non-epistemic occurrences of *must* were in roughly equal proportions.

*Must* is used as a marker of necessity where “in assertion, there is little or no involvement of the speaker” (Palmer 1990: 113) in which case we have an instance of dynamic necessity, paraphrasable as ‘it is necessary for PROPOSITION’. This is illustrated in (74):

(74) We must be at Heathrow by 6 to meet Mark

but as Hoyer (1997: 103) argues, the lack of involvement of the speaker (which is central to the notion of dynamic modality) is questionable here, since it is by no means certain that the authority does not rest with the speaker. This line of argument is questioned by Palmer (1990: 113) with his suggestion that given that it is unusual to lay an obligation on oneself, many instances of *must* occurring with a first person subject will be neutral. This is made clear by (75), where the modality is clearly deontic:

(75) You must be at Heathrow by 6 to meet Mark

and (76), which is a truncated version of an example given by Høye himself (1997: 107)

(76) A plant must receive a good supply of both sunshine and moisture is clearly neither epistemic nor deontic, which provides evidence for dynamic modality as a subcategory of root modality.

Further complications (if any are needed) are noted by Coates (1983: 33) who abstains from splitting ‘root necessity’ instances of *must* into the two subgroups of deontic and dynamic modality on the grounds that instances of supposed deontic modality (such as (75) above) are paraphrasable as an extension of dynamic modality, that is ‘it is necessary for PROPOSITION and I order you to do so’. Developing this further, Coates suggests that there may be a cline (more accurately, a continuum of meaning, with a core and a periphery, between which two extremes there exists “considerable fuzziness” (Coates 1983: 34)) of root interpretations of *must*. It would seem that at one end of the cline lie what Palmer would label ‘deontic’ instances, and at the other, ‘dynamic’ instances. The examples of these extremes are given below as (77) ‘deontic’ and (78) ‘dynamic’, taken from Coates (1983: 34-5):

(77) ‘You must play this ten times over’, Miss Jarrova would say, pointing with relentless fingers to a jumble of crotchets and quavers

(78) Clay pots .. must have some protection from severe weather

It is clear even from this brief summary of the semantics of the modals in PDSE that the range of meanings is vast, and that the discussion presented here is likely to need significant expansion when it comes to dealing with the non-standard data.

## §2.4 *Syntactic classification of the modals*

In this section, I intend to discuss some of the theories concerning the syntactic status of the modals in English. The discussion is to be presented through detailed analysis of a specific article and the relevant sections of a specific book. The analysis draws on a number of aspects of the theory; as a result, much of the following goes into great detail on the premises on which the arguments are founded, which entails a discussion of broader syntactic issues.

### §2.4.1 *Pollock (1989)*

Pollock's analysis of the modals is part of a general discussion on the structure of I(nflection) P(hrase)<sup>13</sup> in U(niversal) G(rammar). I go into this in considerable depth, before I get to the discussion of modals: this is crucial, since the argument concerning the modals is built on his general theory of IP structure. His central argument (stated in Pollock (1989:365)) is that various differences in the behaviour of French and English with regard to negation, question formation, floating quantifiers and quantification can be deduced from principles of UG and a parameter<sup>14</sup> concerning the degree of opacity of Agr(eement) in the specific language. This entails that grammatical features previously argued to be contained within I should be reanalysed: IP should not be considered as one constituent with two<sup>15</sup> features [ $\pm$  Tense] and [ $\pm$  Agreement]; rather, each of these features is the head of its own maximal projection, TP and AgrP respectively, and NegP is similarly a maximal projection headed by Neg (which would mean, though this is not explicitly stated in the article, that Neg is a syntactic (presumably functional, not substantive) category in English). It is also suggested that TP, AgrP and NegP are barriers

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<sup>13</sup> On the notion of I as a syntactic category, see §2.4.2.

<sup>14</sup> A parameter is defined by Radford (1997: 267) as a "dimension of grammatical variation between different languages or different varieties of the same language".

<sup>15</sup> Other analyses suggest that I has four immediate constituents (T(ense), Agr, Neg, and Modal), on which see §2.4.2



(maximal projections which serve to inhibit certain types of movement and government)<sup>16</sup>.

The grounds on which Pollock makes his claim are as follows. He assumes (following the general arguments of UG) that French and English have the same D-structure, represented in part below:

[<sub>IP</sub> NP I (<sub>NEG</sub> not/pas)] [<sub>VP</sub> (Adv) V ...]

He then goes on to consider data such as the following (the examples are my own, but adhere to the main point of Pollock's theory):

- (79) \*He loves not Mary  
 Il n'aime pas Marie  
 \*Il ne pas aime Marie  
 He hasn't any wool

Data such as that in (79) suggest that, within the framework outlined by Pollock, V-to-I movement (hereafter V-to-I) in finite declaratives is (a) lexically restricted in PDSE, in that the majority of lexical verbs cannot move to the head of IP: the only exceptions are *have* and *be*; and (b) obligatory in Modern French. The article considers other additional questions, relating to Affix Movement<sup>17</sup> and blocking, but these will not be considered in detail here.

These questions were considered by examining a range of different V movements. The first is V movement in infinitives, which differ from finite clauses in the feature composition of their I, the former being [- finite], the latter [+ finite]. In Modern French, variation regarding V-to-I exists as is exhibited by the following (curtailed) examples (Pollock 1989: 373, 374):

<sup>16</sup> The term *barrier* will be redefined later in this thesis; what is given here is just a working definition.

<sup>17</sup> In the current version of the model, Affix Movement has undergone a reversal of fortunes and ... well, movement. The earlier idea (explored in the Pollock paper) was that the grammatical features which headed I (such as tense and agreement) were *lowered* to the V in VP in PDSE. Current thinking would suggest that the features of the V percolate *up* to I (in order to be checked against the head features of spec-IP, the subject: see Radford 1997)).

- (80) N'être pas heureux ...  
 BE (INF) NOT HAPPY  
 Ne pas être heureux ...  
 NOT BE (INF) HAPPY  
 \*Ne sembler pas heureux ...  
 APPEAR (INF) NOT HAPPY  
 Ne pas sembler heureux  
 NOT APPEAR (INF) HAPPY

The data in (80) illustrate that V-to-I is optional with *être* (and further analysis of variation in Modern French shows that this holds true for auxiliary and lexical *avoir*), yet results in an ill-formed construction if the V is a lexical verb (such as *sembler*) and the clause is non-finite. This is identical to the behaviour of *be*, *have* and lexical verbs in finite clauses in English. However, modal-like infinitives, such as *pouvoir* and *vouloir*, can undergo V-to-I in French, as in (81):

- (81) Je pensais ne pas pouvoir dormir ici  
 I (1SNom) THOUGHT (1Imperf) NOT CAN (INF) SLEEP (INF) HERE  
 Je pensais ne pouvoir pas dormir ici  
 I (1SNom) THOUGHT (1Imperf) CAN (INF) NOT SLEEP (INF) HERE

Pollock (1989: 375) argues that both of these constructions are acceptable in Modern French, though the latter is more likely to be used in the literary language. Pollock does not suggest any semantic difference between the two: that is, he does not suggest that the first example in (81) displays negation of the modality while the second suggests negation of the proposition. Indeed the argument rests on such constructions being semantically equivalent, if stylistically varied. However, just such a difference is illustrated in (82) below for PDSE:

- (82) The Senate could not convict Clinton

which has two possible readings: 'It is not possible that THE SENATE CONVICT CLINTON' (negation of the modality) and 'It is possible that THE SENATE NOT CONVICT CLINTON' (negation of the proposition). I return to this issue later in this section, when the notion of NegP, a phrase headed by a negative functor, is discussed.



Returning to V movement in infinitives, Pollock (1989: 376ff) shows that, given restricted V-to-I in Modern French infinitival clauses, strings such as Adv(erb) + V + DP and Q(uantifier) + V + DP should surface when they never do so in finite clauses, and this is shown to be the case, as in (83) below:

- (83) À peine parler l'italien après cinq ans d'étude  
 HARDLY SPEAK (INF) ITALIAN AFTER FIVE YEARS OF STUDY  
 \*Pierre à peine parle l'italien  
 \*PETER HARDLY SPEAKS (3SPRES) ITALIAN

However, equally acceptable is the construction in (84):

- (84) Parler à peine l'italien après cinq ans d'étude  
 SPEAK (INF) HARDLY ITALIAN AFTER FIVE YEARS OF STUDY

while (85) is ill-formed:

- (85) \*Ne comprendre pas l'italien après cinq ans d'étude  
 \*UNDERSTAND (INF) NOT ITALIAN AFTER FIVE YEARS OF STUDY

This presents specific problems for the grammatical model presented thus far since this would seem to indicate a type of V movement that is not V-to-I; specifically this is V movement to a position somehow 'lower' than I, yet 'higher' than *pas* and V/VP. Such V movement Pollock labels "short" V movement (1989: 381).

"Short" V movement in English infinitive clauses is lexically restricted, just as it was suggested V-to-I was lexically restricted in English finite clauses, and indeed that restriction is to *have* and *be*, as (86) shows:

- (86) To hardly understand syntax after years of study  
 \*To understand hardly syntax after years of study  
 Bob is said to be seldom late  
 Marge was understood to have never seen a party like it

We are left, then, with the following features of present-day standard English and French, with regard to V movement:

<i>Clause structure</i>	<i>French</i>	<i>English</i>
V-to-I in finite clauses where V is		
(a) lexical	✓	✗
(b) <i>be/être</i> or <i>have/avoir</i>	✓	✓
V-to-I in infinitive clauses where V is		
(a) lexical	✗	✗
(b) <i>be/être</i> or <i>have/avoir</i>	✓	✓
“Short” V movement where V is		
(a) lexical	✓	✗
(b) <i>be/être</i> or <i>have/avoir</i>	✓	✓

so that V-to-I is restricted iff (if and only if) “short” verb movement is restricted. This makes it desirable to see V-to-I not as one head movement, but as a series of two shorter movements, the first being “short” verb movement, and the second being the movement from that intermediate landing site to I.

This in turn raises problems for the model. If V-to-I is actually a series of shorter movements it is necessary to consider two major issues. The first is that the clause structure for French and English presented earlier in this section will require modification. Pollock (1989: 383) suggests the following:

$$(87) \quad [{}_{IP} NP [{}_I V_i] (\text{pas/not}) e_i [{}_{VP} (\text{Adv}) t_i \dots]]$$

and the two verb movements are  $t_i$  to  $e_i$  (“short” verb movement) and  $e_i$  to  $V_i$  (the movement from the intermediary landing site to I). This in itself entails the second issue: to what does the verb move to in “short” verb movement? What is this intermediary landing site?

Basic principles of the model Pollock adopts force us to assume that this intermediary position is a head of some sort, either a lexical category (like V) or a

functional category<sup>18</sup> (like I); given that V movement is head movement, it is subject to the H(ead) M(ovement) C(onstraint) as formulated by Chomsky (1986a: 71):

Movement of a zero-level category  $\beta$  is restricted to the position of a head  $\alpha$  that governs the maximal projection  $\gamma$  of  $\beta$ , where  $\alpha$   $\theta$ -governs or L-marks  $\gamma$

A category which is L-marked is one which is a direct complement of a head, and  $\theta$ -government is a ‘stronger’ version of ordinary government, which can be broadly defined as follows. A category is said to *govern* another category if both m-command each other, where m-command can be defined as follows (a modification of Haegeman 1991: 125):

A m-commands B iff A does not dominate B and every maximal projection that dominates A also dominates B

and a governor must be a head. What distinguishes  $\theta$ -government from ordinary government is that  $\theta$ -government only applies to categories capable of assigning  $\theta$ -roles to their arguments. This asserts that head movement is local in operation, in that heads can only move to head positions in the next highest phrase in the tree; “short” verb movement fulfils the HMC if we assume that *e* in (87) is a head of some sort; this head Pollock labels as Agr, so “short” verb movement is V-to-Agr. In prior work, Agr and T were considered to be grammatical features under I; this analysis argues for a ‘split-Infl’ hypothesis, that T and Agr are not just grammatical features but functional heads in their own right, and therefore able to project into maximal projections, AgrP and TP as is shown in the diagram (88) below, a modified version of that given in Pollock (1989: 384):

(88)  $[_{TP} NP[_{T} T (pas/not)]_{AgrP} Agr [_{VP} (Adv) V]]]$

V-to-I therefore is reanalysed as two shorter verb movements, V-to-Agr and Agr-to-T: these movements both satisfy the HMC.

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<sup>18</sup> On which see §2.4.2 below.

A further refinement of the argument, and one specifically pertinent to the categorial status of modals, relates to  $\theta$ -theory<sup>19</sup>. A central aspect of this theory is that many (lexical) verbs have the capacity to assign  $\theta$ -roles (*thematic* roles) to their arguments; these thematic roles indicate the semantic relationship between, for instance, the subject and the direct object (which are *grammatical* roles played by certain constituents) and the verb. Let us consider the following two sentences as examples:

(89) Bob kicked the ball

(90) Sam enjoyed the concert

In (89) and (90), traditional grammars assert that the two DPs *Bob* and *Sam* are the subjects of their respective verbs, while the DPs *the ball* and *the concert* are the (direct) objects. But it is clear that the roles played by these DPs are distinct: while *Bob* instigates the action denoted by the verb *kick*, it is difficult to argue that this holds true for *Sam* and *enjoy*. It has therefore been argued that these DPs have different thematic roles in the structure of the proposition as a whole. *Bob* in (89) is assigned the  $\theta$ -role AGENT or ACTOR<sup>20</sup> by the verb *kick*<sup>21</sup>, while *Sam* has the  $\theta$ -role EXPERIENCER assigned by the verb *enjoy* in (90). However (and this is the central issue as far as V movement is concerned), Pollock argues that precisely those verbs which show variation in English and French in terms of V movement (namely *avoir/have* and *be/être*) are those which are unique to  $\theta$ -theory: “they arguably fail to assign any  $\theta$ -role to the constituents they are subcategorised for” (Pollock 1989: 385). Now this is debatable: for instance, it is possible to suggest that in (91), the subject DP *Graeme* is assigned the  $\theta$ -role EXPERIENCER:

<sup>19</sup> I merely summarise one aspect of  $\theta$ -theory here. The literature on this topic is vast: for more detail, see Radford (1997: 324ff) and references therein.

<sup>20</sup> The nature, number and nomenclature of  $\theta$ -roles is not fixed: it may be that many would disagree with my analysis here.

<sup>21</sup> Or by the *v*-bar *kick the ball* (see Radford (1997: 325) and Chomsky (1986a)): the debate over which constituent (the verb or the verb and its complements) assigns the  $\theta$ -role to subject DPs is not of concern here.

(91) Graeme had a nightmare about his thesis

and that  $\theta$ -role is assigned by *have* (which functions here as a lexical verb). However, what is interesting is that for many, it would seem only marginally acceptable (for me, ungrammatical) to suggest V movement for the above (cf. \**Graeme hadn't a nightmare about his thesis* vs. *Graeme didn't have a nightmare about his thesis*). More problematic is the behaviour of what Radford (1997: 235ff) describes as *Baa baa black sheep* varieties of English in which the following are grammatical:

(92) Have you any wool?

(93) We haven't any wool

Pollock's analysis here is awkward, in that it seems to be too abstract and convoluted: he suggests (1989: 388) the following "exotic structure" for such constructions as (94), the declarative form of (92):

(94) [<sub>s</sub> You<sub>i</sub> have<sub>j</sub> e<sub>j</sub> [<sub>sc</sub> P e<sub>i</sub> [any wool Loc]]]

where P is a phonetically null preposition and Loc an abstract predicate. P  $\theta$ -marks the subject DP (here *you*), while Loc  $\theta$ -marks the object DP (here *any wool*); P assigns the  $\theta$ -role GOAL to *you*, and (though Pollock doesn't state this) Loc presumably assigns the  $\theta$ -role THEME to *any wool*. This allows Pollock to argue that *have* (and by a similar argument, existential *be*) have regular lexical entries (for *have*, as transitive).

This seems to create more problems than it solves. It would be preferable to try to account for the seemingly aberrant behaviour of *have* and *be* in this regard by suggesting that these verbs are a vestige of the V-to-(Agr-to-)T(-to C) movement which applied to lexical verbs (including existential *be*) at earlier stages of English:

(95) Saw you not his face? (Shakespeare, *Hamlet*)

(96) Art any more than a steward? (Shakespeare, *Twelfth Night*)

These data suggest we need to explain two things: firstly, why most lexical verbs no longer move to Agr (and thence to T and C), and secondly why *have* and *be* are exceptions to this rule. Pollock's argument regarding the nature of Agr is helpful here. He suggests (1989: 385) that English Agr is not morphologically rich: it shows only a small range of inflections to express agreement<sup>22</sup>. Further, a language with Agr that is not morphologically rich cannot permit the transmission of the  $\theta$ -roles of the V; that is, it is opaque to  $\theta$ -role assignment. If Agr is opaque, the  $\theta$ -grid of the V cannot percolate<sup>23</sup> up to Agr; as a result the foot of the train (a trace) has no  $\theta$ -grid to assign, which violates the  $\theta$ -criterion, as stated below (Haegeman 1991: 63):

**Theta criterion**

1. Each argument is assigned one and only one theta role
2. Each theta role is assigned to one and only one argument

But it is clear that at earlier stages of English, Agr was richer morphologically, and could therefore permit the assignment of  $\theta$ -roles; in other words it was transparent to  $\theta$ -role assignment, allowing for grammatical constructions such as (95) and (96) in eModE. What is also clear is that the most morphologically irregular verb forms in PDSE that can function as lexical verbs are *have* and *be*. What I am suggesting, then, is that those varieties of PDSE which still allow *have* and *be* movement to Agr are still resetting the parameter which blocks such movement in other varieties. In other words, while all varieties of PDSE have reset the parameter for verbs which are morphologically 'regular' in terms of their inflectional agreement properties, some varieties are still showing the vestiges of an earlier transparent Agr with relation to *have* and *be*, the least 'regular' forms. This morphological change, between eModE and PDSE, Pollock refers to in his discussion of the modals, and it is that discussion to which I (finally!) turn.

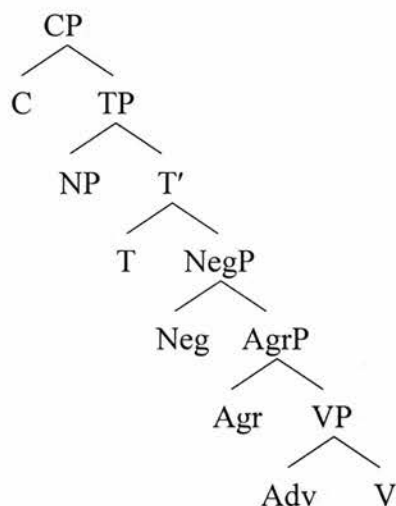
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<sup>22</sup> This contrasts with the morphological richness of French.

<sup>23</sup> i.e. become attached to a head through attraction.

What the argument above has shown is that Pollock (1989) believes V-to-I movement to consist essentially of two separate movements, V-to-Agr and Agr-to-T. Pollock (1989: 397) suggests the following structure, given in (97) for TP in PDSE:

(97)



This is an important analysis, for a couple of reasons, each of which I discuss separately below<sup>24</sup>:

### 1. Modals are base generated under [ $\pm$ Past] T

This introduces a number of debatable points, which are to be discussed in the remainder of this chapter, not just in this section. By arguing for the ‘split Infl hypothesis’, that Infl should actually be reanalysed as consisting of a number of functional heads, Pollock can suggest a reason for the apparent morphological irregularity of PDSE modals as discussed in §2.2.1 above. If the modals are generated under T, they have no need to amalgamate with Agr, and this has repercussions for the morphological forms of the modals. Amalgamation with a

<sup>24</sup> Equally important is a discussion of whether or not modals assign  $\theta$ -roles: this is not discussed here, but in §2.4.2.



head (normally V) is required for the Agr features (such as the {S} ending which attaches to 3SPres verbs) to become overt. But with the modals, since no such amalgamation takes place, no such morphological structure is displayed. This correlates a proposed structural property of the modals with their defining morphological characteristic, which is very neat. It is problematic, however, when one considers the patterns associated with *dare* and *need* discussed above (§2.2.1). Recall that these forms were discounted as being core modals since they do not have all the NICE properties; we can explain negation and inversion factors along the lines described above, so in *he daren't/needn't come* and *Dare/Need he come?*, we might suggest that the modals here are base generated under T, with no amalgamation with Agr. But how are we to explain the following:

(98) \*He dare/need come *and* \*He dares/needs come

(99) \*He dared/needed come

(100) He hardly dare/need show his face round here

(98) causes problems for the model, since it seems that *dare* and *need* are generated neither in T nor in V; (99) causes problems because it suggests that *need* and *dare* are only generated under [-Past] T, never [+Past T], and (100) causes problems because in constructions involving semi-negative adverbs like *hardly* (from which Pollock begins his argument), *dare* and *need* do seem to behave like modal verbs generated in T. In other words, such a model will work for the core, but not for marginal members like these. I do not pursue the matter further here (since I am investigating the behaviour of the core as previously defined) but raise the problems to illustrate that the model presented by Pollock may need modification<sup>25</sup>.

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<sup>25</sup> Pollock (1989) does not list those verbs which he considers to be core modals.



## 2. The existence of NegP

The existence of NegP presupposes that Neg is a head, and therefore some sort of syntactic category. The nature of such categories I discuss later (§2.4.2), but the concept of NegP seems to me to be important both in terms of syntactic structure and in terms of the semantics of negation. Consider the two sentences below:

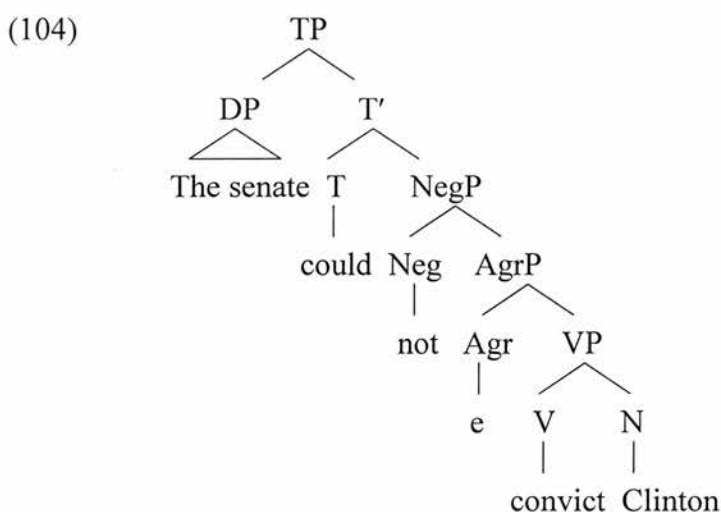
(101) The Senate could not convict Clinton

(102) The Senate couldn't convict Clinton

There are two possible readings of (101): 'It is possible that [THE SENATE NOT CONVICT CLINTON]', where the proposition is negated; and 'It is not possible that [THE SENATE CONVICT CLINTON]', where the modality is negated. However, only the latter interpretation is possible in (102). Recall also in this respect (23) above, given here as (103):

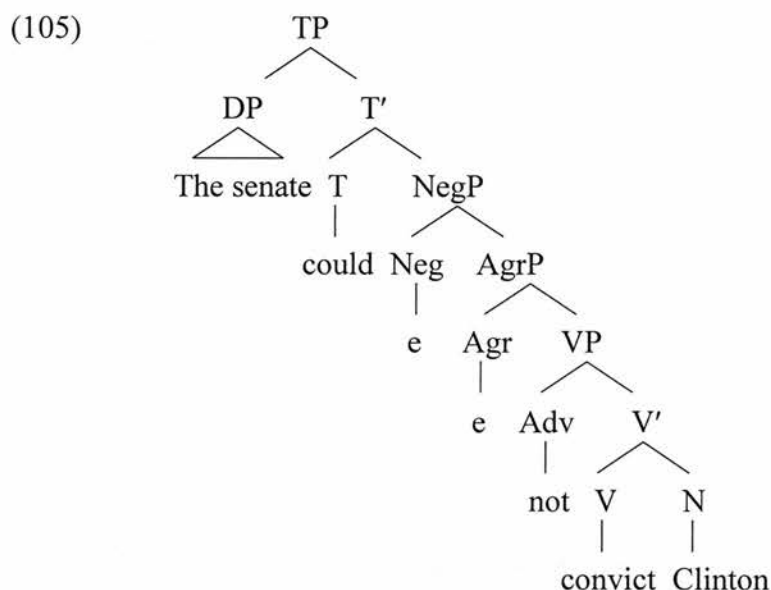
(103) He mustn't sign the treaty

where the proposition is negated semantically, but the bound clitic attaches to the form which displays the modality. How are we to analyse such constructions syntactically, while allowing for the variant interpretations? The structure of (101), following Pollock's hypotheses, is given below as (104):



But this does not show clearly whether the modality or the proposition is negated. The scope of the Neg would seem to cover VP (incidentally, Neg c-commands VP,

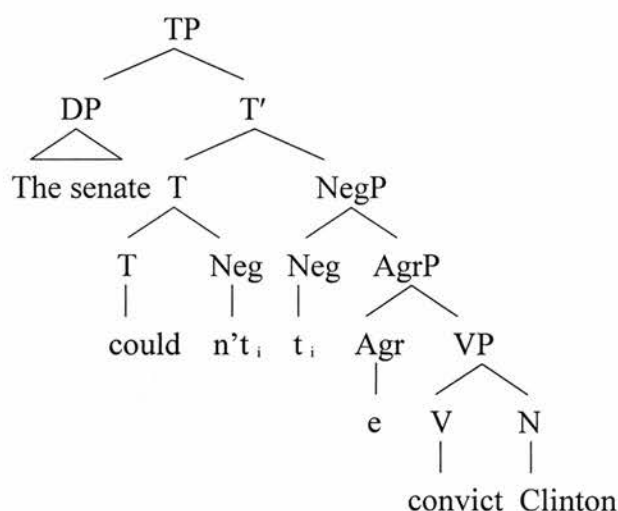
since the mother of Neg, NegP, dominates VP, and Neg and VP are disconnected (that is, Neg  $\neq$  VP, and neither dominates the other); this would suggest negation of the proposition. But a clearer marking of negation of the proposition would surely be as follows ((105)):



but this too is ultimately unsatisfactory, since we would need to suggest that *not* is both a syntactic head of a maximal projection and an adverb, depending on what, exactly, is negated. Furthermore, this distinction would only come into play for sentences containing certain (but by no means all) modals such as that in (101): in *The Senate did not convict Clinton/The Senate will not convict Clinton*, there is no ambiguity, and the analysis given in (104) would be adequate.

Equally problematic is (102). Radford (1997: §6.6) discusses some issues surrounding negation. His solution to the clitic form *n't* is as follows: as a bound morpheme generated under Neg, *n't* must cliticise onto the preceding auxiliary (which suggests a process of adjunction to an already existing head). Aspects of this process of negation were discussed earlier in this thesis in relation to Tyneside English (cf. §1.3.2). His analysis (which I have modified slightly to incorporate the notion of the 'split Infl' hypothesis) of (102) is given overleaf as (106):

(106)



The bound clitic *n't* moves to adjoin to T *could*, forming the complex head T *couldn't*, which c-commands its trace under Neg. But again this is potentially problematic in terms of scope, since the Neg trace  $t_i$  c-commands VP, but our interpretation of the sentence rules out the possibility of propositional negation. A converse problem exists in (103), where the semantics suggest negation of the proposition, but the syntax shows the clitic adjunction to the modal head, thus formally marking the modal component as negated. This evidence would seem to suggest some problems for the model as presented above.

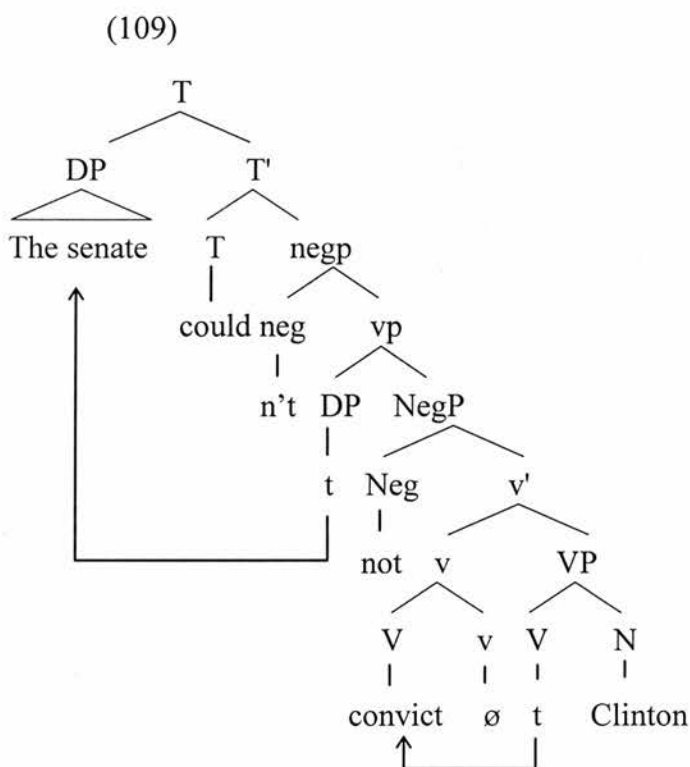
A possible solution to the problem might be as follows. The crucial problem with (101) is the *scope* of the negation: where (101) means 'it is possible [THE SENATE NOT CONVICT CLINTON]', the scope of the negation is VP-internal; in the sense 'it is not possible [THE SENATE CONVICT CLINTON]', the scope of the negation is VP-external. Furthermore, in a construction such as (107), both types of negation occur:

(107) The senate couldn't not convict Clinton

and in (108) (= (102), repeated here for convenience), the cliticized form forces an unambiguous reading, where the negation is VP-external:

(108) The senate couldn't convict Clinton

Let us assume that (at least<sup>26</sup>) two types of ‘negative phrase’ exist within a clause. Firstly there exists an inner NegP core, which marks VP-internal negation. Secondly, there is an outer negp shell, which marks VP-external negation. There are clear parallels here with inner VP cores and outer vp shells as postulated by Chomsky (1995: chapter 4) and Radford (1997: chapter 9), used to characterise (among other things) patterns of  $\theta$ -role assignment<sup>27</sup>. (107), which illustrates both types of negation pattern, would therefore have the structure as given in (107) below. Note that, in fact, the term ‘VP-internal negation’ should really be classified as ‘vp-internal negation’; that is, the inner NegP core is located outside VP but inside vp. I am ignoring issues of AgrPs as they are not relevant to the immediate discussion.



<sup>26</sup> In Cormack and Smith (forthcoming), an analysis of English negation is presented in which three distinct positions for negation/polarity are postulated. Their concept of an ‘Echo’ position does not concern us here, though their notion of Pol[NEG] and Adv[NEG] are in some ways similar to my negp and NegP respectively.

<sup>27</sup> For instance, the  $\theta$ -role THEME is assigned VP-internally (eg to the DP *the ball* in *the ball rolled down the hill*), while the  $\theta$ -role AGENT is assigned vp-internally (eg to the DP *the boy* in *the boy rolled the ball down the hill*).

(109) represents the stage of the derivation before the clitic *n't* in *neg* adjoins to *could* through *greed* (as discussed in §1.3.2 above). Such an analysis would account nicely for tag-question data in TE, in which utterances of the type *You can't come, can't you not?* are well-attested (cf. Beal (1993: 203), where the tag again suggests negation both internal to *vp* and external to *vp*).

Note that in such an analysis, only *neg* (not *Neg*) can be cliticized (*\*The senate could not n't convict Clinton*). But then how are we to account for (103), repeated below as (110):

(110) He mustn't sign the treaty

Here it would seem as if the negation is generated morphosyntactically at *neg*, but semantically (in terms of the scope of the negation) at *Neg* (since the proposition, not the modality is negated here). Again, we could potentially make use of the concept of *greed* here: while the negation is generated under *Neg*, the clitic moves (via head movement to *neg* initially, and thence to *T*) to satisfy its morphological properties as a bound form.

#### §2.4.2 *Ouhalla* (1991)

*Ouhalla's* account is based within the same general framework as that of *Pollock*, but with some important modifications, which will become apparent as this section progresses. There is, traditionally, a division of syntactic categories into two classes, 'open' and 'closed'. Those in the open class, including nouns, verbs and adjectives are labelled as being substantive categories; those in the closed class, including determiners and complementizers, are labelled as being functional categories. His central claim is that variation in language is determined by functional categories, that it is the functional elements of language that are parametrised. The idea that functional aspects of language which often appear as affixes are in fact

syntactic categories in their own right means that NEG, AGR and T are all assigned an autonomous syntactic status.

It is clear that there are specific grammatical differences between the substantive categories and the functional categories; two of those specifically pertinent to the modals are discussed below:

### 1. **Thematic grids** (Ouhalla 1991: 10-13)

Functional categories lack thematic grids: they lack arguments and therefore cannot assign thematic roles; auxiliaries lack arguments (*\*Bob does the ball to Mary, \*Sam can beside a stream*) and therefore do not assign thematic roles; they are therefore aligned in this respect with functional categories. Other verbs, such as *give* and *sunbathe* do have arguments, and therefore assign thematic roles: *give* has three arguments to which it assigns the thematic roles of AGENT, THEME and GOAL; *sunbathe* has one to which it assigns the role of THEME. This suggests that not all verbs are substantives: main (or lexical) verbs belong to the substantive category, auxiliaries to the functional category. This in turn suggests that auxiliaries are likely to be involved in parametric variation.

### 2. **Functional categories as affixes** (Ouhalla 1991: 15)

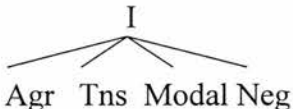
Generally, though not exclusively, functional categories are affixal in nature. This means that they take the form of a bound morpheme which attaches to another (mostly substantive) category. Consider in this regard the marking of past tense on weak verbs in English: *walked*, *jogged* and *sprinted* all consist of a verb plus inflectional affix - which marks past tense - realised in each case as one of three predictable allomorphs [t], [d] and [ɪd]. The first allomorph is affixed to stems ending in a voiceless segment other than [t]; the second to those ending in a voiced segment other than [d]; and the third to those ending in either [t] or [d]. This then

implies that functional categories have m(orphological)-selectional properties<sup>28</sup>: here, T attaches to V, since T is affixal. Ouhalla's argument is that all functional categories - be they bound or free - have m-selectional properties. English auxiliaries are not affixal: they nonetheless have m-selectional properties, one such property being the information that this particular category is non-affixal and will therefore not need to attach to any other form. However, Ouhalla does not state what the other m-selectional properties of auxiliaries are.

Ouhalla's argument, then, is based in the idea that inflectional elements, previously thought to have been generated under I, are to be assigned separate categorial status. This has implications for the number and type of maximal projections possible: along with the substantives V and N (and their respective maximal projections VP and NP), functional categories such as T (or TNS in Ouhalla's notation) NEG and PASS also have the maximal projections TNSP, NEGP and PASSP. The argument for suggesting such a wide range of separate functional categories is as follows. I was previously assumed to be the position under which Modals and the NEG element were generated, along with T and Agr, so that a sentence like (111) below has an I constituent such as that in (112):

(111) Bob might not meet Louise

(112)



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graph TD
  I[I] --- Agr[Agr]
  I --- Tns[Tns]
  I --- Modal[Modal]
  I --- Neg[Neg]
  
```

There are clearly a number of potential problems with this analysis, as noted by Ouhalla (1991: 56). Firstly, the justification for having infinitival *to*, Neg and the modals as inflectional elements in English is by no means clear. In Italian, inflected infinitives are overtly marked (so *cantare* = *canta* + *re*, where *re* is an affix, a bound morpheme, marking the infinitive). But while Radford (1997: 54) argues that

<sup>28</sup>This is not meant to suggest, as Ouhalla notes (1991:16), that substantives lack m-selectional properties. The crucial point of this section is that, although auxiliaries do have such properties, they differ from some other functional categories by being non-affixal.

infinitival *to* in English has much the same function as the infinitive inflection in Italian, it is clear that it has a very different morphological form. This same argument might be applied to Neg and the modals: should we categorise the adverb *not* and the verbal forms *might* and *should* as inflectional elements? Ouhalla's argument here moves away from that of Pollock as discussed above, since the latter assumes that the modals are indeed base-generated under T. A central problem with (112) above is that it suggests I represents a position, not a category or even a class of categories, which, as Ouhalla notes (*ibid.*), works against the general generative principle that heads of syntactic structures project from the lexicon.

Indeed, the head status of I is another controversial area: in (112) above, the head of I is not stipulated; as it is presented in (112), it looks as if I may be multi-headed. Some elements are obligatory in finite clauses, others optional: Agr and Tns are necessary for the assignment of nominative case to the subject NP in spec-IP position, but Neg and the Modals are not necessary for the creation of a well-formed clause. The multi-headedness of I "violates the isomorphic constraint on categories and their projections" (*ibid.*).

None of this, however, proves that Neg, the modals *et al.* should be assigned a categorial status. All it does is show the inadequacy, for English certainly, of the earlier I analysis. In the following paragraph, I discuss Ouhalla's arguments in favour of the thesis that Neg behaves like an independent syntactic category. Similar arguments are put forward for the categorial status of T and Agr, which I do not discuss<sup>29</sup> (though the reader is referred to Ouhalla (1991: 62-9)).

Consider the following:

(113) Stuart spoke German

(114) Stuart did not speak German

(115) \*Stuart spoke not German

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<sup>29</sup> I have not discussed this here for two reasons: the first is that the categorial status of T and Agr has already been investigated in §2.4.1; the second is that the focus of this chapter is on modality.



The data above for PDSE suggests that the appearance of a negative element blocks movement of V to I: Neg blocks the merger of V with Agr and T. The problem is, if we accept that I has the form illustrated in (112), and thus that Neg is base generated under I, there has been no violation of the HMC (as stated in §2.4.1 above) in (115). In other words, the structure of I as given in (112) will generate (115) as well formed when it clearly is not. What seems to be happening is that the Neg element is blocking V-to-I in (115), and as such is likely to be the head of a maximal projection functioning as a barrier to movement: V would need to move to Neg (since heads can only move to the head of the next highest maximal projection), not to I. Ouhalla (1991: 59) also suggests that V-to-I through Neg (i.e. through a process of adjunction, and subsequent extraction of the newly formed complex head) is also banned, since neither Neg or V are affixal: this is less convincing, since Neg has the potential to be affixal in its cliticised form *n't* (which has to be an affix, since it is a bound form), as shown in §2.4.1.

Ouhalla suggests (1991: 69) that there have been two general hypotheses regarding the syntactic status of the modals. The first is that they are base-generated, like T and Agr and Neg, under I: this has been shown to be a problematic account of the structure of I, so that such an analysis will need heavy modification. The second hypothesis is that the modals are some form of verb - defective, but nonetheless verbal - and thus base generated under VP. Ouhalla discounts this analysis on both empirical and theoretical grounds, by considering the following data:

(116) John should not vote Conservative

(117) \*John voted not Conservative

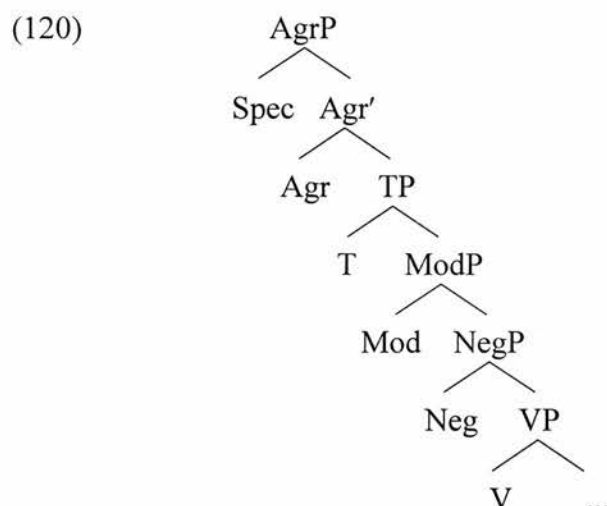
(118) \*John not should vote Conservative

(119) John did not vote Conservative

The data above shows clear problems with the 'main verb hypothesis'. (116) and (118) show that modals must appear before Neg; (117) shows that main verbs cannot

do so - indeed, they must appear after Neg. With this information, we would be justified in questioning the main verb hypothesis. Recall that V-to-I is blocked in PDSE in negative sentences, since such movement violates the HMC. On similar grounds then, it would be fair to argue that modals cannot be generated under V, and move to I. A further potential problem with this hypothesis concerns the claim that the modals do not assign thematic roles nor enter into any kind of thematic relationship with the arguments of the verb (discussed in detail in §2.4.3); the framework in which Ouhalla presents his argument is one in which the “predicate phrase is the domain of theta-marking and theta-receiving elements only” (Ouhalla 1991: 31). This also suggests that the modals cannot originate in VP.

The problem with the analysis of I given in (112) is that it requires some sort of extrinsic ordering to ensure that Modal appears before Neg: if Modal appeared after Neg, the resulting clause would be ungrammatical as shown in (118). However, it has been established that the Neg element is head of its own maximal projection; and similar arguments are put forward to suggest that T and Agr are also independent syntactic heads. This militates in favour of an analysis, different from that provided by Pollock, where the modal, when present, heads its own maximal projection in the following structure (modified slightly from Ouhalla (1991:71), which suggests that a daughter of Agr' is Agr', not Agr, though this may be a typographical error) in (120):



Modals undergo movement to T (M-to-T movement) to support the affixal element generated under T, which allows for the modal to be inflected for T. The problem with this part of Ouhalla's argument (and one which he does not address) is that not all modals are inflected for tense: *must* stands alone as an exception, yet *must* be accounted for in terms of the framework. One possible option might be to suggest that the lexical entry for *must* specifies that it does not inflect for tense, but this may be unsatisfactory, as it seems stipulative rather than explanatory: we would have to invoke this exception to fit the theory, rather than provide a principled account of why this is so. Movement to Agr is blocked for the same reasons outlined in §2.4.1.

This, as Ouhalla (1991: 71) points out, provides an interesting analysis in terms of the diachronic development of the modals. Denison (1993: ch.11) provides copious data suggesting that in earlier stages of the language (particularly in OE), those verbs which developed into our PDSE modals took arguments. I give one such example here (Denison 1993: 306):

- (121) Binnan þrym nihtum cunne                      ic his mihta  
 within three nights know (PRES SUBJ) I his powers  
 'Within three nights may I know his powers'

The verb *cann* here takes the DP *his mihta* as direct object; furthermore, it assigns that DP a thematic role (possibly that of THEME). On this criterion then (that *cunnan* has an argument to which it assigns a  $\theta$ -role), it must be base-generated under VP. The claim that the subsequent failure to assign  $\theta$ -roles, when such modals lost the ability to take arguments, meant that these forms, originally substantives, became functional categories, and thus became increasingly less and less verb-like (cf. Roberts (1985, 1993)) is the topic of the next and final section of this chapter.

#### §2.4.3 *The diachronic development of the modals, and $\theta$ -role assignment*

The issue of whether the PDSE modals assign  $\theta$ -roles is part of a larger question, namely whether the modals should be considered verbs (and hence generated under V) or as functional categories (generated under T). I think that  $\theta$ -role assignment can be considered in two distinct but related ways; firstly, the extent to which modals assign  $\theta$ -roles to their complements, and secondly, the extent to which modals assign  $\theta$ -roles to their subjects. Any such discussion will relate such patterns to diachronic developments, and include other issues such as finiteness. It should be noted that the following discussion is selective, since not every issue concerning the historical evolution of the modals can be covered<sup>30</sup>.

Let us begin the discussion with a consideration of the following four PDSE sentences (the last two provided by Warner (1993: 17)):

- (122) (a) The farmer might kill the duckling  
 (b) The duckling might be killed by the farmer
- (123) (a) Mothers will smack their children ('habitual' *will*)  
 (b) \*Children will be smacked by their mothers

In (122), the passivisation has had no effect on the well-formedness of the sentence. This can be taken as evidence that the epistemic modal *might* is "transparent"

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<sup>30</sup> For more detail on the diachronic development of the modals, see Denison (1993), Lightfoot (1974, 1979, 1991), Roberts (1985, 1993), van Kemenade (1987), Hudson (1997a, 1997b), Warner (1993) and below.

(Anderson 1991: 15), by which is meant that such modals do not “impose an argument structure independent of the lexical verb they accompany” (Anderson 1991: 15). In other words, the fact that, in (122), *the farmer* is assigned the  $\theta$ -role AGENT and *the duckling* the  $\theta$ -role THEME is concerned solely with the properties of the lexical verb *kill*; the properties of *might* are irrelevant as far as  $\theta$ -role assignment is concerned. But how can such an analysis be squared with the data in (123), where, similarly, the subject of the active sentence is assigned the  $\theta$ -role AGENT and the direct object is assigned the  $\theta$ -role THEME?

Notice that the modality expressed by the modal in (122) is different from that in (123). In (122), the modality is epistemic, and in (123) it is root. Epistemic modals never impose selectional restrictions on their subjects; and not all root modals do so either, as (124) shows:

- (124) (a) The farmer must kill the duckling  
 (b) The duckling must be killed by the farmer

Yet (123) shows that some root modals do impose an argument structure independently of the lexical verb they accompany; it has further been suggested (on which see further Warner (1993: 16)) that deontic modals in general assign some sort of role to the deontic source, or to the addressee or entity on whom the obligation is laid or to whom the permission is granted. In (125) and (126), from Roberts (1985: 50), a comparison is made between root modals and agent-oriented adverbs:

- (125) (a) John can (ability) read Arabic  
 (b) John deliberately read the forbidden text
- (126) (a) \*Arabic can (ability) read easily  
 (b) \*The forbidden text deliberately read easily

In (126), Roberts argues for *read* as a ‘middle’ verb, one which is “formed from transitive verbs by a lexical process of deletion of the agent role” (Roberts 1985: 50): though he gives no examples of a well-formed sentence with ‘middle’ *read*, I assume he is referring to constructions such as (127):

(127) This review of *Hamlet* reads well

The reason (125) is grammatical but (126) not concerns the properties of root modals and agent-oriented adverbs: they both require an agent argument in the clause in which they appear, though this is true of control structures in general. However, “this argument is always the argument of some other predicate” (Roberts 1985: 51), and if a proposal was made to allow the root modal to assign a  $\theta$ -role to an argument which has already been assigned a  $\theta$ -role by the lexical verb, such a proposition would be in violation of the  $\theta$ -criterion outlined in §2.4.1 above, namely that each argument can bear one and only one  $\theta$ -role.

In order to avoid this violation, Roberts invokes the concept of *adjunct  $\theta$ -roles* which are not subject to the  $\theta$ -criterion nor to another principle which Roberts invokes, the V-visibility condition (Roberts 1985: 29), which states that a verb assigns a thematic role if and only if it is governed<sup>31</sup>, though this latter statement strikes me as confusing. There surely is no need to claim that modals are exceptions to the V-visibility condition if one argues (as Roberts does) that modals are not generated under V but under T (or I in this earlier analysis). And from a historical perspective, we need not claim that *can* in (128) is an exception to the V-visibility condition either:

(128) Yet can I music too (Lovelace, *Poems* (1649))

because here *can* is a (governed) main verb (taking a direct object complement). In other words, at the stage at which the modals were analysed as verbs, they could assign  $\theta$ -roles just like any other lexical verb; at the stage at which they were analysed as being generated under T, they were no longer verbs (and hence the V-visibility condition could not - by definition - apply to them), and merely assigned adjunct  $\theta$ -roles.

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<sup>31</sup> The definition of government is given in §2.4.1 above.

(128) above clearly relates to the issue of modals historically assigning  $\theta$ -roles to their complements (as opposed to their subjects), and while there are instances in PDSE of adjunct  $\theta$ -role assignment to subjects, no such instances exist in which the modal assigns any sort of  $\theta$ -role to nominal complements, for the simple reason that, being generated under T, modals do not have nominal complements in PDSE. The topic of the diachronic development of the modals is a very large one, and has been widely debated since David Lightfoot's initial analysis (Lightfoot 1974). In the following discussion, I will only consider a narrow part of the debate, focusing primarily on three aspects of syntactic change classified by Roberts (1993): Parameter Resetting (PR); Diachronic Reanalysis (DR) and Steps.

A Step is defined by Roberts (1993: 158) as follows: "The appearance of a new construction, or a significant increase in the frequency of a construction, in a set of texts can be thought of as a step". In the history of the English modals, the drift from root to epistemic modality is classified as a step (Roberts 1993: 310-1). Roberts argues that throughout the history of English, there has been underway a lexico-semantic change which has meant that the modals have been increasingly used as markers of epistemic modality to the exclusion of root interpretations. It will be seen later in this thesis (chapters 5 and 6) that this aspect of syntactic change is on-going in the dialect of the informants from Newcastle. Following Chomsky (1986b: 19f), Roberts makes use of the distinction between I(nternal)-languages and E(xternal)-languages. I-languages are infinite, and considered to be the 'true' object of linguistic inquiry, in that the grammar which underlies I-languages is Universal Grammar. By contrast, E-languages are finite, a set of sentences or a group of corpora analysed by the linguist. Roberts classifies Steps as "the diachronic relation between E-languages" (Roberts 1993: 158). But the concept of Steps can - and indeed I think should - be extended to incorporate not just diachronic relations but also sociolinguistic variations, where the corpora of forms and variants collected by



sociolinguists can be considered as E-languages. Thus Steps could also be considered as the sociolinguistic relation between E-languages, in addition to and in complement with the diachronic perspective. Thus in terms of syntactic change - and indeed language change generally - we need to consider not just diachrony, but synchronic variation in the E-grammars of identifiable innovators and conservatives in a range of social groups. This notion is developed more fully in chapter 6, where the results of the investigation of the speech of the informants are analysed.

Diachronic Reanalysis (DR) occurs when “a given construction has the structure S at period P and structure S'  $\neq$  S at period P'” (Roberts 1993: 158). His DR for the modals (and periphrastic *do*, which I do not deal with here) is as follows in (129) (Roberts 1993: 315):

- (129) a.  $NP_i [T^{\circ} do/M_j T^{-1}] t_j [t_i VP]$   
           b.  $NP [T^{\circ} did/M] VP$   $\Rightarrow$

Certain aspects of the notations used in this DR in (129) require explanation. Firstly, the superscript ‘ $\circ$ ’ is intended to mark a head, thus  $T^{\circ}$  is the head of TP etc. Secondly, the superscript ‘ $-1$ ’ is intended to mark an element within a complex head  $X^{\circ}$  which has specific bound morphemes<sup>32</sup> which trigger incorporation of one head to another (see further Roberts 1993: 43ff). Thus, in the analysis of V-movement in French outlined and discussed in §2.4.1 above, we can suggest that there are specific morphological features residing in T and Agr which motivate incorporation (or movement) of V to T to Agr. Thus  $Agr^{\circ}$ , for instance, is a complex head, with a slot containing the bound morphemes and a slot which can host the moved element.

(129a) raises a number of complex issues, not all of which can be dealt with in sufficient detail here. However, the following points are, I think, fundamental to

<sup>32</sup> Roberts (1993: 244) specifies that “if a formative is of category  $X^{-1}$ , it is a bound morpheme, but ... if a formative is a bound morpheme it is not necessarily an  $X^{-1}$ ”. In English, the past tense suffix *-ed* and the third-person singular present tense suffix *-s* are inserted at  $T^{\circ}$  and  $Agr^{\circ}$  respectively since they are “inflections without a paradigm” (Roberts 1993: 244), by which I assume he means that the present-day English inflectional system is not rich.



Roberts' arguments. Firstly, (129a) suggests that prior to the DR, modals were (a) verbs and (b) verbs which took sentential complements: this explains the 't<sub>j</sub> [t<sub>i</sub> VP]' part of (129a), since t<sub>j</sub> marks the base-position in which the modal is generated, while t<sub>i</sub> marks the base-position of the surface subject; thus, an ME sentence such as (130)

(130) Thy godfadris wyff thow shalt not take

would, in Roberts' analysis, have the following structure, as in (131):

(131) [<sub>V</sub>shal][<sub>S</sub>[<sub>NP</sub> thow][<sub>VP</sub> not take thy godfadris wife]]

The modal then raised to T (and thence to Agr, though this is not at issue here), and the subject raised also; the (thematic) fronting of the complement of *take* to spec-CP is again not at issue. This explains the tense and agreement markings on the modal. Specifically, in ME, the morphological features residing in T<sup>-1</sup> triggered incorporation of the modal into T. Roberts (1985, 1993: 315) suggests that the reason that the modals were later base generated in T (i.e. *why* the DR postulated in (129) took place) is related to the morphological irregularity of the premodals (i.e. the modals of OE and ME) and the loss of the subjunctive inflection: these issues - as well as the discussion of whether or not premodals were raising or control verbs - are left unresolved here, though the reader is referred to Roberts (1993: 310-8) and references therein for details of the debate.

Again, Roberts (1993: 158) explains DRs solely in relation to generational differences: "We can think of DRs as relations between the E-language of one generation (ambiguous trigger experience susceptible of a 'simpler' analysis in the sense defined earlier), and the I-language of a subsequent generation". The concept of simplicity is an important one for Roberts' theory, which he summarises in his notion of the Least Effort Strategy or LES (Roberts: 1993: 156). The LES argues for a minimal number of chains and chain positions (where chain is understood to be any given constituent plus its traces in the course of a derivation) in the acquirer's

representations of sentences of the input to acquisition. Thus in (129) above, two chain positions are ‘saved’ as a product of the DR, and thus the I-language of the acquirers generates modals under T rather than under V, in accordance with the LES.

Parameter Resetting is perhaps more familiar than the other two stages of syntactic change outlined above, and concerns “diachronic relations among I-languages” (Roberts 1993: 159). Thus, a younger generation of speakers may set a parameter differently from that of an older generation. By parameters, I mean those variety-particular “aspects of grammatical structure which children have to learn” (Radford 1997: 17) as part of the task of acquiring their native dialect/language. Such parameters are considered binary: thus a language can either have null-subjects (Italian) or it cannot have null-subjects (English); a language can permit *wh*-movement (English), or it cannot permit *wh*-movement (Mandarin Chinese). Relating this to the modals, Roberts (1993: 244) posits the following general parameter, given below as (132):

(132) For  $X^0$ , is there  $X^{-1}$ ?            Yes/no

The subcase of (132) which is of particular relevance to the modals is where  $X = T$ . In OE and early ME, speakers set the parameter of (132) to ‘yes’ for  $T^{-1}$ , on the grounds that, for instance, inflected infinitives are well attested in these periods, but not in the later stages of English<sup>33</sup>. As the *to* + infinitive structure gained ground in the course of ME, groups of speakers began to reset this parameter to ‘no’, since there was no morphological trigger for  $T^{-1}$ ; instead, the infinitive particle *to* was generated at  $T^0$ , and  $T^{-1}$  was no longer motivated. “This development made finite T a possible site for the insertion of modals and *do*” (Roberts 1993: 315).

I have tried, in the discussion above, to give a selective account of the history of the modals up to the present day in terms of the concepts of Steps, DR and PR which is presented in Roberts (1993). I end this section with a substantial quotation

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<sup>33</sup> Roberts (1993: 259ff) details other features surrounding changes in the infinitive in the history of English, which I do not have the space to go into here.

from Roberts (1993) which will be of central importance for an analysis of *on-going* change in modal verb usage in TE, discussed in chapters 5 and 6.

In terms of these ideas, the following postulate of the theory of change emerges rather naturally: structures are eliminated due to parametric changes only. Steps can and frequently do make certain constructions rarer, but they do not eliminate them totally, in the sense that the grammatical system still permits them. DRs act in such a way as to radically reduce the frequency of certain construction-types in the data, but nevertheless the constructions in question are not eliminated; DRs typically result in the innovation of new constructions alongside older ones. With parametric change, however, one of several constructions may be eliminated in the grammar altogether. Parametric changes may eliminate structures which were already obsolescent, but they may also eliminate otherwise perfectly viable constructions, or force them to undergo DR.

The LES is relevant for all three notions of change, since ... it is the sufficient condition for the move from one step to the next. Therefore, any DR will involve a reduction in the number of chain positions in the structure in question ... DRs frequently create the conditions for parametric changes, by removing the structural evidence for a given parametric setting.

(Roberts (1993: 159))

## §2.5 *Summary*

This chapter has presented a detailed analysis of the modal verbs of PDSE, the grammatical variables to be analysed in the speech of the informants later in this thesis. It has introduced and defined central concepts concerning the modal verbs, including *mood* and various different types of *modality*, such as *epistemic*, *deontic* and *dynamic*, in §2.1. Certain morphosyntactic features were used in order to establish a central core of the modals in the present day standard variety (§2.2). Once established, the semantics of this core set were discussed, focusing specifically on which types of modality were expressed by the various modals (§2.3). In §2.4, the syntax of the modals was addressed. This involved a lengthy discussion of the

principles behind the 'split-Infl' hypothesis, proposed by Pollock (1989), which in itself entailed a discussion of a wide range of issues, such as the position of TP in relation to AgrP, and the existence of NegP; these issues were further considered in relation to the analysis provided by Ouhalla (1991). Factors concerning the diachronic development of the modals were addressed within the syntactic framework presented in Roberts (1993). This last account provides a model for syntactic change which makes use of three distinct but related notions: Steps, Diachronic Reanalysis and Parameter Resetting. These notions relate not only to 'autonomous' syntactic change, but also consider lexico-semantic developments: thus the model accounts for change in syntactic distribution (why the modals became less and less 'verb-like' over time) and in lexico-semantic properties (why the modals have drifted towards epistemic modality away from root modality). It is envisaged that this model could be partly adopted and partly adapted to consider ongoing change and sociolinguistic variation in TE, though the details of such an adaptation have not been proposed here; rather, they are reserved until later in the thesis, once patterns of variation in the speech of the informants have been established.

### 3 Glottalling and glottalisation, with particular reference to TE

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This chapter will deal primarily with two related, but importantly distinct, phonological phenomena: **glottalling** and **glottalisation**. Glottal articulation takes place in the larynx. The glottal stop [ʔ] is a pulmonic sound, i.e. the pressure which is built up below the glottis is initiated by the lungs, but unlike most other pulmonic sounds, it is not phonated: since the production of a glottal stop, in terms of articulation, requires the glottis to be tightly closed, it cannot, by definition, be simultaneously (intermittently) open in order to produce voicelessness or voicing (cf. Catford 1988: 57). The production of [ʔ] requires the vocal folds to be fully adducted: "if pulmonic egressive effort is being continuously exerted, there will be a surge of transglottal airflow on the release of a short-term glottal closure, usually with a positive, low-level input of acoustic energy into the vocal tract in consequence" (Laver 1994: 187-8).

**Glottalling** (sometimes referred to in the literature as glottal replacement - cf. Giegerich (1992: 220)) is the term given to describe the occurrence of [ʔ] as an allophone usually of /t/ but in some accents, such as TE, also of the other voiceless stops /p/ and /k/, as in *butter* [bʊʔə], *kipper* [kɪʔə], *wicker*, [wɪʔə]. In cases where /p t k/ are all realised as [ʔ], that is, when *tat*, *tap* and *tack* are homophonous in [tʰaʔ], the contrast between the voiceless oral stops has been suspended (cf. Giegerich (1992: 225-6)) in that position; in other words, we have an instance of neutralisation rather than allophony. **Glottalisation** (sometimes referred to in the literature as glottal reinforcement) describes the co-articulation of a voiceless stop with a glottal stop, in (syllable) final position. This occurs in many accents of English, including Received Pronunciation (RP), in examples such as *beep* [bi:ʔp], *beat* [bi:ʔt], *beak* [bi:ʔk]. Giegerich (1992: 220) describes glottalisation as follows: "in syllable-final voiceless stops the bilabial, alveolar or velar closure is accompanied - *often slightly preceded* -

by glottal closure, so that a glottal stop [ʔ] is co-articulated with the [p t k] articulation” (emphasis added). In TE, however, Wells (1982: 374) suggests that the glottal articulation actually follows the bilabial, alveolar or velar release, so that there is “glottal masking of the oral plosive burst”<sup>1</sup>.

There are a number of important issues relating to glottalling and glottalisation to be considered with reference to both RP and TE. The central questions are as follows:

- (a) What is the phonological environment in which the rules of Glottalling and Glottalisation apply?
- (b) How do various phonological theories deal with the phenomenon?
- (c) Do those theories adequately encapsulate dialect (or more specifically, accent) differences within their frameworks?
- (d) How old are the phenomena of Glottalisation and Glottalling?

These issues will be considered below. The terminology used in the literature relating to these phenomena of glottalisation and glottalling can vary considerably, so that glottalisation, for instance, is also known as *glottal reinforcement* or *pre-glottalisation*. In the discussion which follows, both in this chapter and the remainder of the thesis, I use the following abbreviations: where the discussion applies to *both* glottalisation and glottalling, or where no distinction need be made between the two for the purposes of the point being made, the abbreviation ‘G’ is used; where the discussion applies to glottalisation/reinforcement alone, the abbreviation ‘GR’ is used; and where the discussion applies to glottalling alone, the abbreviation ‘GS’ (for glottal substitution) is used. This is done to maintain a consistent terminological labelling throughout this thesis: the terms I apply may not be identical to those used in works which I cite.

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<sup>1</sup> Docherty and Foulkes (1999) discuss the acoustic phonetics of glottalling and glottalisation in Tyneside English in some detail, with some evidence that runs counter to Wells’ claim.



### §3.1 *Environments in which G applies*

In this section, I consider the environments in which G applies in English, focusing predominantly on RP, since the next section deals with TE. The discussion of environment is a complex one, more complex, I think, than has previously been recognised, especially when regional varieties are taken into consideration. In what follows, the discussion considers the issue of ambisyllabicity, which will be seen to be very important for the analysis of the variable behaviour of the oral stops in TE.

#### §3.1.1 *Received Pronunciation (RP)*

In §3.0 above, it was noted that the articulation of glottalised stops in RP was different from that in TE. It should also be noted that there seems to be a wider range of phonetic environments in which G can occur in TE than there is in RP. The purpose of this and the following section is, then, to examine the precise range of phonetic environments in which G can occur (in both RP and TE).

Andrésen (1968: 9) notes three specific distributions of [ʔ] in what he calls ‘English Standard Pronunciation’. The first is when [ʔ] occurs as a syllable boundary marker: in his discussion of [ʔ] in RP, Gimson (1989: 169) states that “a hiatus of vowels belonging to different syllables (especially when the second vowel is accented), may in careful speech be separated by [ʔ] instead of being joined by a vocalic glide”. This occurs in words such as *re[ʔ]action*, *vanilla[ʔ]ice* and *Westminster[ʔ]Abbey*, all of which are Andrésen’s examples. The first example seems more plausible than the other two, since it might be expected - in allegro speech at least - that the liaison phenomenon of r-sandhi is likely to occur in *vanilla[r]ice* (‘intrusive’ /r/) and *Westminste[r] Abbey* (‘linking’ /r/) in current RP<sup>2</sup>. Andrésen’s second distribution of [ʔ] occurs in emphatic contexts: “any vowel that

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<sup>2</sup> Cf. Giegerich (1992: 66, 280-3).

occurs initially in the morpheme may have a glottal stop inserted before it for the sake of emphasis, eg [Its 'ʔæbsəlu:tɪr 'rɒŋ]]” (Andréson 1968: 9). Neither of these distributions will be discussed below: the sole focus will be on the third of Andréson’s distributions, which is as follows: “Many speakers use a glottal stop before, or even in place of, [p] [t] [k] in certain positions. For instance, the word *fortnight* may be pronounced [fɔ:ʔnait] or [fɔ:ʔnait]. This occurrence of the glottal stop is known as the ‘reinforcing glottal stop’ and the ‘replacing glottal stop’ respectively” (Andréson 1968: 9); that is, my GR and GS respectively.

Wells formulates the ‘effect’ of GR and a list of examples with environments as follows (Wells 1982: 260):

(a) *The ‘effect’:*

$\emptyset \rightarrow ? / V \text{ (L or nasal) } \_\_\_\_ \text{ [Voiceless Plosive]}$

(b) *Examples and environments*

		/p/	/t/	/k/
(i)	___ # true C	<i>stop talking</i>	<i>quite good</i>	<i>look down</i>
(ii)	___ # L or S	<i>stop worrying</i>	<i>quite likely</i>	<i>look worried</i>
(iii)	___ # V	<i>stop eating</i>	<i>quite easy</i>	<i>look up</i>
(iv)	___ pause	<i>Stop!</i>	<i>Quite!</i>	<i>Look!</i>
(v)	___ true C	<i>capsule</i>	<i>curtsey</i>	<i>picture</i>
(vi)	___ L or S	<i>hopeless</i>	<i>mattress</i>	<i>equal</i>
(vii)	___ syllabic nasal	<i>(happen)</i>	<i>button</i>	<i>(bacon)</i>
(viii)	___ V or [l]	<i>happy</i>	<i>bottle</i>	<i>ticket</i>

where, L = liquid and S = semivowel. Wells (1982: 260) defines a ‘true C’ as follows: “The expression ‘true C’ (true consonant) covers obstruents and nasals, but not liquids or semivowels”, though why a nasal is any more true a consonant than a liquid is not explained. These then are the phonetic *contexts* in which the rule of GR is operative in RP. In the case of (vii) various other phonological features come into play: for such instances to be relevant, the underlying /ən/ must have coalesced to [ŋ]



by what Wells terms Syllabic Consonant Formation<sup>3</sup> and in the case of *happen* and *bacon*, the [n], through the process of Progressive Assimilation, becomes bilabial [m] or velar [ŋ].

In discussing the phenomenon of GR in relation to RP, Wells notes that the degree of GR amongst RP speakers is not constant. He then suggests (1982: 260-1) that “no social value appears to attach to GR in the environments where it is very clearly audible [(iv) and (vi) GT]: English people do not have strong feelings about which is more elegant of [ˈhəʊpləs, ˈhəʊʔpləs], [ˈmætrəs, mæʔtrəs], [ˈi:kwəl, i:ʔkwəl]”. In their discussion of GS, Hughes and Trudgill (1987: 35) also note that the process “occurs much more frequently in some phonological contexts than others”: GS is most frequent finally before a consonant, then before a syllabic nasal, than finally before a vowel, then before a syllabic /l/ and least frequent medially before a vowel. It will be shown below that many sociolinguistic studies have indicated that the combination of social factors and linguistic environment can be seen to be very important in relation to speakers’ G patterns.

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<sup>3</sup> Carr (1993: 160) sees this as two separate processes: the first is Schwa Deletion, the second Nasal Syllabification. He argues that the contrast between /p t k/ is neutralised before syllabic nasals (if the preceding vowel is [+stress]) by a rule of Reduction (Carr 1993: 99) in TE; this rule is the last of a series of rules which derive the surface phonetic form [hɛʔm] from underlying /hɛpən/. The derivation is given below (cf. Carr 1993: 160):

Underlying:	/hɛpən/
RULES	
Schwa Deletion	hɛpn
Nasal Syllabification	hɛpŋ
Nasal Assimilation	hɛpŋ
Reduction	hɛʔm
Phonetic realisation	[hɛʔm]

Carr makes a distinction regarding the nature of sets of these rules. Schwa Deletion and Nasal Syllabification are intrinsically ordered: the second is not possible without the first having first applied; the ordering “arises from the intrinsic properties of the rules” (1993: 133). By contrast, Nasal Assimilation and Reduction are extrinsically ordered: while the second *could* apply before the first, in order to derive the correct output (the phonetic realisation), it must be ordered after the first; the ordering “is stipulated in the grammar, rather than arising naturally from the nature of the rules” (ibid.).

In his discussion of GS, Wells (1982) notes that [ʔ] can be observed for /p/ and /k/ as well as for /t/ in some accents, but not in the case of RP. GS of /t/ does occur in mainstream RP in environments (i) (ii) and (v) above, and for some younger speakers, in environment (iii) also.

In RP, glottal closure often accompanies bilabial, alveolar and velar stricture in syllable-final voiceless stops, as in (1) below (cf. Giegerich (1992: 218-221)):

- |     |    |             |         |    |             |        |
|-----|----|-------------|---------|----|-------------|--------|
| (1) | a. | <i>bit</i>  | [bɪʔt]  | b. | <i>bib</i>  | [bɪb]  |
|     |    | <i>luck</i> | [lʌʔk]  |    | <i>lug</i>  | [lʌg]  |
|     |    | <i>rope</i> | [rouʔp] |    | <i>robe</i> | [roub] |

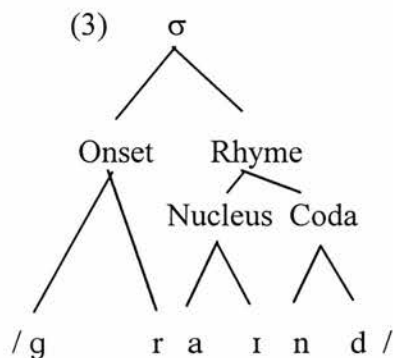
In (1b), the examples given illustrate the fact that glottalisation does not occur when the oral stops are voiced. Giegerich (1992: 221) notes that this allophony is a gradient phenomenon, dependent not only on both speaker and style (extralinguistic) variables, but also on “its segmental context in the syllable-final position: it is most easily perceived after short vowels (*cup*, *bit*) and probably weaker after consonants (*felt*)”. The situation is more complex when the oral stop is not word-final; that is, when it occurs medially. Consider the examples in (2) below:

- |     |    |                   |             |
|-----|----|-------------------|-------------|
| (2) | a. | <i>approve</i>    | [əpɹu:v]    |
|     |    | <i>patron</i>     | [peɪtrən]   |
|     |    | <i>micro</i>      | [maɪkɹou]   |
|     | b. | <i>replica</i>    | [reʔpɹɪkə]  |
|     |    | <i>metrical</i>   | [meʔtɹɪkət] |
|     |    | <i>Accrington</i> | [aʔkɹɪŋtən] |

In (2a), the sonorant following the voiceless stop is devoiced as a result of that stop being aspirated: by marking the devoicing of the sonorant, we are implicitly noting the aspiration of the stop.

In (2b), the stops are *ambisyllabic*. Ambisyllabicity is said to arise as a result of a conflict between two principles of syllabification. The first principle is that of ‘maximal onsets’ (cf. Vennemann 1972, Anderson and Jones 1974, Giegerich 1999). The onset of a syllable is the (optional) consonant (sequence) which precedes the

most sonorous element of a syllable (usually a vowel)<sup>4</sup>, called the nucleus. This nucleus can also be followed by further consonants, which form the coda. Together, the nucleus and coda of a syllable form the rhyme: this is illustrated in (3) below for the monosyllabic word *grind*, where the symbol ‘ $\sigma$ ’ represents ‘syllable’:



But consider the situation with polysyllabic words. If we imagine a segmental sequence ‘ $V_1C_1C_2V_2$ ’, we could posit three different syllabifications: the first possibility would be to syllabify the two consonants with  $V_1$ : this would maximise the coda; with the second possibility, we could syllabify  $C_1$  with  $V_1$  and  $C_2$  with  $V_2$ , which would maximise both coda and onset (in the sense that it would ensure at least one consonant is syllabified with each); and with the third possibility, we could syllabify both consonants with  $V_2$ , which would maximise the onset. The Maximal Onset Principle is one which argues for the third of these possibilities, on the condition that such a medial cluster does not violate any phonotactic constraint associated with initial clusters. By this I mean that if a consonant cluster is banned from occurring in morpheme-onset position, it will also be banned from occurring in morpheme-medial position. Let me illustrate this with an example. No morpheme of English begins with the sequence /nt/; as a result, in the word *pentathlon*, we cannot syllabify /nt/ in the second syllable: the nasal is syllabified in the coda of the first

<sup>4</sup> Clearly, sometimes syllabic consonants occur, as in *little*: /lɪtl/: there are two syllables in *little*, but only one vowel, so we need to suggest that the second lateral is syllabic. This issue is not pertinent to the main discussion, however, so I do not deal with it further.

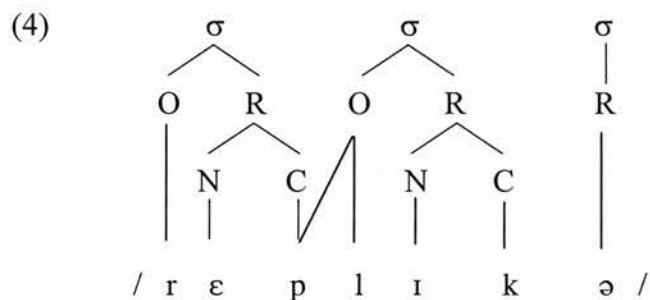
syllable, while the oral stop is syllabified in the onset of the second. By contrast, the /pr/ sequence can occur morpheme-initially in English (*prince*, *prize* etc.), so in *capricious*, that consonantal cluster can be syllabified in the onset of the second syllable. Assuming that this constraint is not violated, the Maximal Onset Principle seeks to syllabify any medial consonant (cluster) in the onset.

The second principle of concern to us with regard to ambisyllabicity is that which prevents forms such as /ka/, /mɒ/ and /pɛ/ surfacing as well-formed (stressed) monosyllabic words. Stressed syllables in English must contain (minimally) either a tense vowel or diphthong (as in *pea* or *pie*) or a lax vowel and a consonant in the coda (as in *pit*). I return to and expand the discussion of this issue later, but for now, I simply state this condition as the Complex Rhyme Condition (cf. Giegerich 1999: 272).

The two features of English syllabification relate to ambisyllabicity in the following way. In the word *witty*, the Maximal Onset Principle dictates syllabification of the oral stop in the onset of the second syllable; yet the Complex Rhyme Condition dictates syllabification of the stop in the coda of the first syllable. When such a conflict of syllabification arises, the segment is said to be both part of the coda of the first syllable, while simultaneously being part of the onset of the second; it becomes ambisyllabic. As shall be shown later in this thesis, the issue of ambisyllabicity is crucial for our understanding of the phonological behaviour of the oral stops in TE.

Having explained the concept of ambisyllabicity, I now want to reconsider the data in (2b) above. In these instances, the oral stops are part of the onset of the second syllable (note that /pl/, /tr/ and /kr/ are all well-formed onsets) as well as forming part of the rhyme of the initial stressed syllable. The fact that this initial syllable is the one that carries the stress would suggest that it is subject to the Complex Rhyme Constraint discussed above. Given that the vowels in the first

syllables of *replica*, *metrical* and *Accrington* are all lax, the following consonant must be associated with the rhyme of that syllable as well as the onset of the following syllable. This is represented in (4) below for *replica*:



where  $\sigma$  = syllable, O = onset, R = rhyme, N = nucleus and C = coda<sup>5</sup>. The ambisyllabicity of the stops in such positions as those in (2b) is borne out by their allophonic behaviour: they are both aspirated and glottalised (recall that aspiration is suggested by the fact that in such cases the following sonorant is devoiced). Giegerich's rule for GR then is as follows (cf. Giegerich 1992: 221):

/p/ → [ʔp] / \_\_\_\_ .

/t/ → [ʔt] / \_\_\_\_ .

/k/ → [ʔk] / \_\_\_\_ .

where '.' represents a syllable boundary. This analysis then supposes that the domain of GR is the syllable.

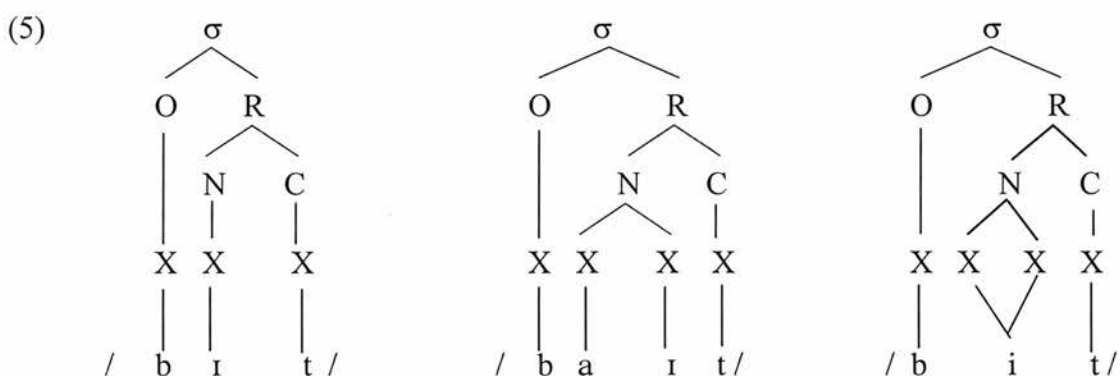
In (3) above, the phonological constituent 'syllable' was represented as being composed of two immediate constituents, the onset and rhyme, the first of which is optional, the latter obligatory (cf. *bile* /baɪl/, *aisle* /aɪl/, \**b* /b/); the same holds true of the constituency of the rhyme, except here it is the first constituent, the nucleus, which is obligatory and the second, the coda, which is optional (cf. *aisle* /aɪl/, *eye* /aɪ/, \**l* /l/<sup>6</sup>). The onset, nucleus and coda are themselves composed of various segments, the first and last consonantal, the second (prototypically) vocalic. Let us

<sup>5</sup> The issues of timing and melody tiers is not discussed here, but is addressed below.

<sup>6</sup> The picture is complicated slightly by 'syllabic consonants' such as [ŋ] in *button*, although it seems reasonable to suppose - for English at least - that such forms are derived from a vowel + consonant sequence such as /əŋ/ (cf. footnote 3 above).

consider the representation of several monosyllabic words in terms of tiers; the words to be considered are *bit*, *bite* and *beat*.

There are clear qualitative and quantitative differences in the vowel sounds in *bit*, *bite* and *beat*. /ɪ/ in *bit* is a lax monophthong, /aɪ/ in *bite* is a tense diphthong, and /i/ in *beat* is a tense monophthong. But in addition to the qualitative tense-lax distinction in *beat* and *bit*, there are further quantitative differences too: the duration of the vowel in *bit* is shorter than that in *beat*. It would be beneficial then, to invoke a phonological representation which could take account of both quality and quantity differences. This has led, in autosegmental representations, to the proposal of an additional tier<sup>7</sup>, which falls between the segment and the syllable tier levels. At the segmental level, the qualitative characteristics of the phoneme are displayed: this is the melody tier and segments appear in a melody slot; above this is the timing tier, where quantitative characteristics are displayed by means of timing slots (Spencer 196: 220) or X-positions (Giegerich 1992: 139 ff). In RP, each lax vowel and consonant is associated with one X-position; each element of a diphthong is associated with one X-position; and each long vowel is associated with two X-positions, as illustrated in (5) below:



<sup>7</sup> As Goldsmith (1990:8) notes, such tiers can vary depending on the phonological material each tier is designed to represent. In the model presented here, one tier concerns the melody of a segment, and the other its timing; other tiers can be invoked to indicate tone, for instance. "Each tier itself consists of a string of segments, but the segments on each tier differ with regard to what features are specified in them" (Goldsmith 1990:8).

Now, tense vowels - and geminate consonants (Spencer 1996: 220) - occupy one slot at the melody tier, but two at the timing tier; and Hayes (1986) argues there can be no rule that affects ‘half’ of a geminate<sup>8</sup>. It follows from this that phonological rules must take into account the structural characteristics of both melody and timing tiers. How does this fit in with the rule of GR?

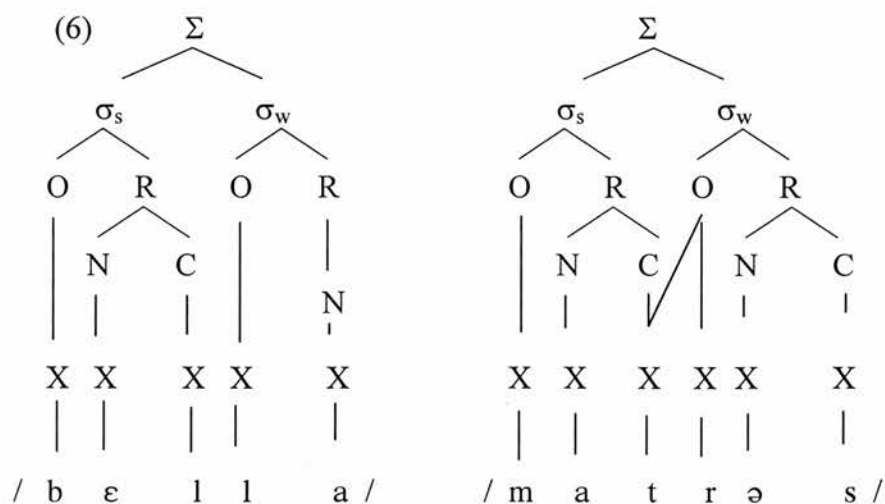
We have claimed that GR in words such as *mattress* applies because the /t/ is ambisyllabic: as such, it is part of the coda of the first syllable (and GR only applies to oral stops in syllabic codas, not to those exclusively in syllabic onsets). Ambisyllabicity means that one melody element is linked with two different syllables; but it does not mean that it is associated with two different X-positions (cf. Giegerich 1992: 183). There is no phonetic evidence to suggest that ambisyllabic consonants are longer than other intervocalic consonants. In this sense, then, ambisyllabic consonants are different from geminate consonants, which *are* associated with two different timing slots: consider the syllabification of the Italian word *bella*<sup>9</sup> and the English word *mattress* in this regard:

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<sup>8</sup> Hayes (1986) classifies this as Inalterability - though presumably the rule of Degemination affects only one half of a geminate?

<sup>9</sup> I have chosen an Italian word here as there are no geminate consonants in English; there are, however, ‘derived’ ‘long’ consonants, by which I mean consonants at morpheme boundaries in morphologically complex words, such as *dul+ly*, *whol+ly*, *pen#knife*. Interestingly, in careful speech at least, these ‘long’ consonants display predictable allophonic patterns given their position in the syllable: in *dully* and *wholly* for instance, the first l is ‘dark’ (velarised) and the second ‘clear’, since the first is *not* in a syllabic onset while the second is. Note further that *dully* does not rhyme with *sully*; and *wholly* is not homophonous with *holy*.





An ambisyllabic consonant should therefore be subject to rules that affect both segments which appear in syllabic codas and segments which appear in syllabic onsets. And this is exactly what happens in *mattress*: the stop is both glottalised and aspirated. We need to express the fact that /t/ is glottalised if it is associated with an X-position in a syllabic coda; and that it is aspirated if it is associated with an X-position in a syllabic onset. If that X-position is associated with both coda and onset, then the segment represented at the melody tier will be both aspirated and glottalised. In other words, the phonetic behaviour of /t/ when realised in words like *mattress* is entirely in keeping with its association with one timing slot that is in turn associated with two different syllables<sup>10</sup>.

### §3.1.2 *Tyneside English* (TE)

Carr's (1991) account of TE considers two separate but related phenomena, GR and Weakening. The former has been described extensively above; the latter affects the voiceless alveolar stop in intervocalic position in TE. In this phonetic environment, the stop is realised either as an alveolar tap [ɾ] or as a post-alveolar approximant [ɹ]. The details of this feature of TE are discussed later (see (12) below). The environments in which GR occurs in TE are morpheme-internal, across

<sup>10</sup> Cf. further Giegerich (1999: 271f).

a morpheme boundary and across a word boundary; there is therefore, according to Carr at least, no GR in initial position (cf. below and chapters 5 and 6 of this thesis for data which runs counter to Carr's claim). The environments in which GR does occur according to Carr are exemplified in (7) below (after Carr 1991: 44):

(7)	<i>Morpheme-internal</i>	<i>Across a morpheme boundary</i>	<i>Across a word boundary</i>
	stupid	clipper	clip her wings
	winter	chanter	chant it
	reckon	wrecker	wreck her

Carr's argument on GR in TE relies heavily on specific features of metrical structure as proposed by Giegerich (1985), which I detail in some depth now. These claims are:

- (a) English feet are trochaic;
- (b) monosyllabic words are metrically binary-branching, with  $\emptyset$  syllables as right sisters.

If English feet are trochaic, it means that the primary stress of English feet falls on the left-most syllable, and that feet are maximally binary; in other words, in terms of relative prominence of stressed and unstressed syllables in a metrical tree, we could suggest a stress contour [s w], where the constituent labelled s is more prominent than that labelled w. If we assume (following Giegerich 1985: 13f) that each word must contain at least one [s w] foot, how is a word like *bit* to be analysed? A potential analysis for cases such as this is provided by (b) above: the word *bit* has a metrical structure [s w], where the s node dominates *bit* and the w node dominates a zero syllable (cf. Giegerich 1985: 13, Hogg and McCully 1987: 228f).

There has been a range of evidence produced for the motivation for positing such zero syllables, which will not be extensively discussed here - since there is a

simpler way to account for the data, which I detail below - but some discussion is necessary in order to understand Carr's analysis more fully.

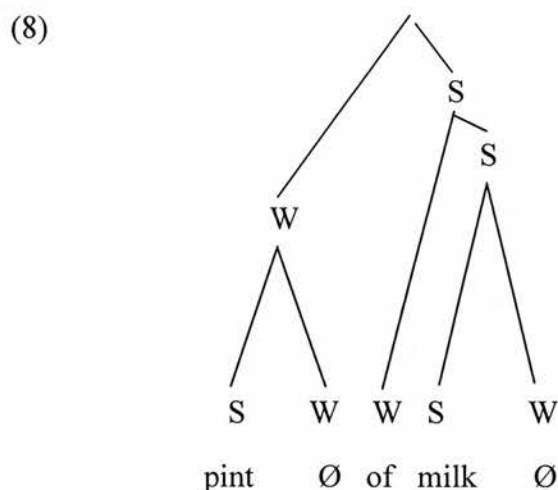
Some of the arguments proposed for the positing of zero syllables relate to the issues of *isochrony* and *enclisis*, and I deal with these separately here:

- *isochrony*: English is a stress-timed language. That is, the intervals between stressed syllables (i.e. feet) in English are roughly equal in time (between 488 and 566 msec (Gimson (1989: 264)); therefore, a monosyllabic foot should occupy the same phonological time as a disyllabic foot, so that *the 'rat 'fell* patterns with *the 'rattle 'fell*. Given that *rattle* has an [s w] contour, it has been suggested that a similar pattern should hold for *rat*; the constituent dominated by 'w' would then need to be specified, and it has been suggested that this constituent is the zero syllable.
- *enclisis* (cf. Giegerich 1985: 14f, Giegerich 1992: 268f, Hogg and McCully 1987: 231-2): examples of enclisis in English occur in phrases such as *lorra lorra laughs* or *Drinka pinta milka day*. This process relies on the principle that feet in English are left strong, and that unstressed syllables form part of a preceding foot, even if, in terms of syntactic constituents, it is more closely associated grammatically with the word or words in the following foot. For instance, in *lorra laughs*, the reduced form of the preposition *of* [ə] is attached to the foot containing the preceding word *lot*, even though it is head of the PP *of laughs*. This relates to zero syllables in the following way. Consider the phrase *pint of milk*; if we assume that lexical category words have a specific prosodic status<sup>11</sup> so

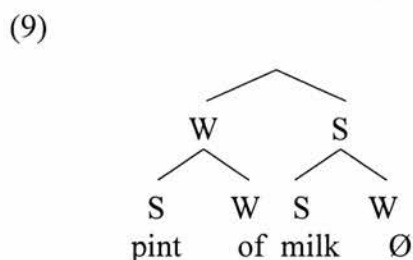
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<sup>11</sup> This prosodic status leads Lieberman and Prince (1977) to categorise such words as 'mots' or 'prosodic words' to distinguish them from function words in terms of stress patterns. I do not go into the detail of this argument.

that they have a stress contour [s w], we would have to assume that the tree structure for such phrases is as follows:



This is undesirable on the grounds that such a structure would suggest some kind of ‘pause’ between the lexical word and the preposition. The appropriate structure would be:

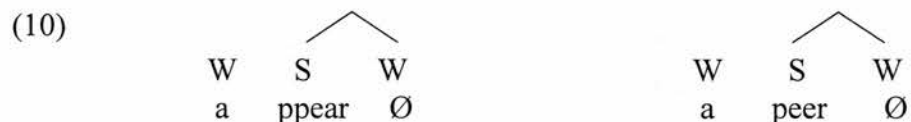


Giegerich (1985: 14) derives (9) from (8) by positing a zero syllable constraint<sup>12</sup>, whereby, if there are two terminal and adjacent w nodes, neither can be a zero syllable. This encapsulates features of enclisis in English: “Unstressed syllables get encliticised; zero syllables occur only in feet that don’t contain unstressed syllables” (Giegerich 1985: 15).

This lengthy exposition on certain features of Giegerich (1985) is necessary, given Carr’s reliance on the claims discussed above. Firstly, Carr argues that GR in

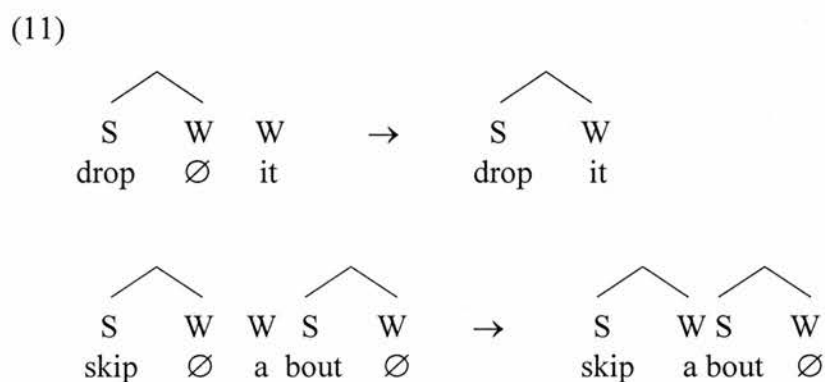
<sup>12</sup> In other words, a zero syllable is posited in (8) only to be removed in (9) by the zero syllable constraint; clearly it would be better if there was an analysis which could do without them altogether. Such an analysis is provided later in this section.

TE applies purely foot internally; that is, there are never any instances where GR in TE occurs foot-initially. He therefore predicts that there will be no GR in examples such as *appear, a peer; accuse, a cake*. The metrical structures of *appear* and *a peer*, following the argument discussed above, are as in (10) below:



In such instances, the target stop is foot-initial, and Carr's claim is that GR is blocked here.

Secondly, then, Carr notes that weak or unstressed syllables can undergo cliticization, as discussed above and exemplified in (11) for *drop it* and *skip about*:



Now feet so formed are important for Carr's analysis of Weakening and GR in TE as follows. The formation of these feet takes place after words are entered into syntactic structures; the phonological processes described in (11) above are not concerned with morphological processes, or the effect of suffixes on stress patterns and phonological alternations *within* the stem; indeed, the formation of the feet in (11) takes place after any word-formation or inflection has taken place; this is a postlexical rule. This concept is discussed in much more detail in §3.2 below; for now, let us designate those feet formed after the processes of inflection or word-formation have taken place as postlexical, while those formed as a result of morphological processes are

lexical. This then leads to a contrast between the foot structure of *fit her* and *fitter*; the formation of the foot in *fit her* is postlexical, while that in *fitter* is lexical.

Carr (1991) relates this to GR and Weakening in TE in the following way. Weakening is a process whereby the coronal voiceless stop is realised usually as a post-alveolar approximant [ɹ] or sometimes as an alveolar tap [ɾ] intervocalically, which is formulated as a rule in (12) below:

$$(12) \quad \textit{Weakening in TE}$$

$$/t/ \rightarrow [ɹ] \sim [ɾ] / V \_ V$$

Weakening occurs word-finally in all but a few cases and strictly intervocalically, but it does not occur in verbs which are not stressed on the last syllable (i.e. in cases such as *edit it* \*[ɛdɪtɪtʔ] or *interpret her* \*[ɪn'tɪz:prətə]) since specific metrical conditions are not met: Carr suggests that since a foot boundary intervenes between the target coronal voiceless stop and the following vowel, Weakening is blocked. This then suggests that both GR and Weakening are foot-internal processes; neither operate across a foot boundary. Furthermore, there are syntactic constraints on Weakening, since the rule does not (according to Carr (1991)) apply in nouns, adjectives and prepositions, even when they (like the verbs which do allow Weakening) are monosyllabic and are followed by an unstressed syllable. Carr gives examples (the transcriptions following these are my own) such as the following in (13):

$$(13) \quad \textit{a boat on the river} (* [\ə \text{b}uət \text{ɒn} \text{ðə} \text{rɪvə}])$$

$$\textit{was wet again} (* [\wəz \text{w}ɛt \text{ə}gɛn])$$

$$\textit{stayed out all night} (* [\text{s}tɛəd \text{u:r} \text{a:l} \text{nɪ:tʔ}])$$

In these cases, GR not Weakening occurs.

In summary, Carr (1991) claims the following about TE:

- Weakening affects a smaller number of phonemes than GR: the former affects the coronal voiceless stop only, the latter, all three voiceless stops;

- Weakening, not GR, is favoured in *fit her*; GR, not Weakening is favoured in *fitter*;
- Weakening applies to verbs (monosyllabic, or polysyllabic if stressed on the final syllable) but not to adjectives, nouns and prepositions; in these other cases, GR applies.
- no GR will occur in *accuse*, *a cake*, *suck oranges*;

Taking the first three of these claims together, Carr (1991: 49) goes on to argue that Weakening is a more specific rule than GR on the grounds that it (a) affects fewer input segments than GR does and (b) applies solely to feet formed post-lexically (while GR affects feet formed both lexically and postlexically); these rules therefore enter into an ‘elsewhere’ relation, whereby the more specific rule of Weakening applies first, “blocking the more general rule” (Carr 1991: 49) of GR. This explains why the /t/ in *fit her* surfaces as a tap or approximant, while the /t/ in *fitter* surfaces as a reinforced stop or a glottal stop: the metrical structure of *fitter* is formed in the lexicon; Weakening is blocked; GR applies as an ‘across the board’ rule, applying “regardless of whether the foot is formed lexically or not” (Carr 1991:48); on the other hand, the metrical structure of *fit her* is not formed in the lexicon, but post-lexically; Weakening, as the more ‘specific’ rule, applies first, and the output is a tap or approximant; and the application of Weakening blocks the application of GR.

There are, however, a number of problems with this account; these problems concern not only the claims made regarding the data for TE, but also the theoretical assumptions made. I want to deal with some of the theoretical issues first; but I will return to other theoretical problems after a discussion of the data, since it will be shown that the problems associated with Carr’s data have significant implications for his theoretical model.



The concept of a zero syllable seems (in and of itself) an undesirable one, and a model which could account for the data without invoking the zero syllable would be more advantageous. The issue of isochrony strikes me as something of a red herring in the present context: while there can be little debate about the fact that the intervals between stressed syllables in English are roughly equal in length, it is not clear to me how this motivates a zero syllable analysis: we *could* suggest that zero syllables exist; but why *must* we? Since the number of syllables in a foot can be three or two, why not allow for a monosyllabic foot? Isochrony *per se* is not concerned with the number of syllables, but with the interval of time between stressed syllables; and such stressed syllables can be adjacent, with no unstressed syllables intervening. I do not see why we need to invoke a zero syllable in such cases; and to do so only to then delete such zero syllables by a process of enclisis seems redundant - the issue of enclisis, while extremely important, does not need to be explained with reference to zero syllables, and an alternative analysis to zero syllables can account for both enclisis patterns and the isochrony issue at one and the same time, thus leaving the concept of the zero syllable redundant.

This alternative analysis relies on the concept of stray syllable adjunction discussed by Giegerich (1992: 267): his definition of foot formation suggests that stray syllables must be adjoined to the preceding foot. Let me illustrate this with a discussion of *fitter* and *fit her*, the examples given in the discussion of Carr (1991) above.

The foot and syllable structure of *fitter* is as given in (14) below:



This analysis, then, does not dispose of the concept of postlexical and lexical foot formation processes noted in the discussion of Carr (1991) above: as shown in (14), in *fitter*, the foot is formed lexically; and as shown in (15), in *fit her*, the complex (i.e. branching) foot is formed postlexically, after the words are entered into syntactic structure, and the unstressed pronoun is adjoined to the preceding foot. But crucially, there is no need for recourse to the zero syllable in this analysis; and the same principle of stray syllable adjunction can explain the enclisis phenomena (*lorra, cuppa* etc.) discussed above (for further details on this, see Giegerich 1992: §9.2.2.4).

The accuracy of the data (cf. Docherty *et al.* (1995), Docherty *et al.* (1997)) is a further problem for Carr's analysis as presented in Carr (1991). Firstly, in recordings of TE speakers analysed in Docherty *et al.* (1995, 1997), there are clear instances of GR in examples like *suck oranges* and *at Easter*, i.e. "in syllable-final, foot-internal positions specifically excluded by Carr" (Docherty *et al.* 1995: 33). This data suggests that GR in TE may occur in a wider range of environments than is the case in RP; Docherty *et al.* (1995: 33) provide further evidence for this, since they also note GR in syllable-initial position, such as *nineteen, three times* and *see you tomorrow*. With this evidence then, we are justified in claiming that GR in TE needs to be accounted for differently than in RP, as it clearly occurs in a range of different environments (i.e. in word- and possibly foot-initial position in examples such as *three times*).

Another potential data-related problem for Carr is highlighted by Docherty *et al.* (1995). They suggest that in instances of a similar type as *fit her*, either GR or Weakening can occur: word-finally, preceding a vowel, such as in *got a* (in *got a little dog*, for instance). There are extralinguistic factors which seem to determine which of the variants appears, and these work in tandem with other rules, rules which are determined entirely by phonetic environment and/or metrical structure. For instance, Weakening was rarely attested when speakers were asked to read word

lists (suggesting the rule is sensitive to stylistic variation); and the tap and retroflex variants were characteristic of males (suggesting the rule is sensitive to sociolinguistic variation<sup>13</sup>). This is explored further in the analysis of the speech of my own informants in chapters 5 and 6.

In the light of the data problems noted above, further theoretical issues are raised. In the discussion of Carr's theoretical framework, it was argued that the positing of zero syllables was redundant, given that the phenomena associated with these zero syllables can be dealt with via a principle of stray syllable adjunction; however, such an adjunction is a postlexical process, so a foot thus formed (as in *fit her*, for instance) would be postlexical. It would therefore still seem possible to uphold that part of Carr's argument concerning the rules of GR and Weakening which suggests that the rules enter into an elsewhere relation, the specific rule of Weakening blocking the general rule of GR, since Weakening applies only to segments in feet formed postlexically, while GR applies across the board, to feet formed both lexically and postlexically. However, the discussion of the data collected by the fieldworkers for Docherty *et al.* (1995, 1997) suggest that such a position is untenable: if Weakening and GR variably apply in postlexical feet (as was shown to be the case), it is not possible to claim that the rules enter into an elsewhere relation. There are further (sociolinguistic, stylistic) issues concerned which must be incorporated to give a full picture of the patterns of Weakening and GR in TE. All in all, there seems to be, in terms of both the theoretical apparatus and the data used to uphold the theory, some substantial problems with Carr's account of GR and Weakening in TE.

In addition to the rules of GR and Weakening in TE, there are further issues concerning the syllabification of the oral stops in this variety of English, particularly in light of their phonetic behaviour. In Docherty *et al.* (1995), the authors discuss the

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<sup>13</sup> On which see further §4.3.2.

data in (2) above, from Giegerich (1992), in which GR is not predicted for RP in words such as *apron*, *matron* and *micro*. Following the syllabification patterns of the medial consonants with regard to the Maximal Onset Principle and the Complex Rhyme Condition discussed above, the stops are unambiguously syllable initial here: the first syllable is stressed and heavy with a nucleus with two X-positions at the timing tier, so there is no motivation for ambisyllabicity. However, in TE, GR is common in words of this type, predominantly amongst male speakers, and even in word list style. This evidence is important for a number of reasons: it again militates in favour of an analysis which takes into account both structural and sociolinguistic factors; it suggests that structural factors governing GR in one accent may be different from those in another; and it provides further evidence that the environments in which GR takes place in TE may be different from that of RP. In RP, evidence clearly points to an account of GR in which the rule operates only on the oral stops when they appear in syllabic codas; in TE, there is a body of evidence - which, as shall be shown in chapters 5 and 6, includes the data gathered from informants for this thesis - in which GR is shown to occur in (a) what appears<sup>14</sup> to be syllable initial position (in *apron*) (b) what clearly *is* syllable initial position (in *see you tonight*) and (c) in syllable final foot-internal position (in *suck oranges*).

### §3.2 *G and phonological theories*

In this section, reference is made to aspects of three different phonological theories, all of which shed light on the phenomenon of G in different ways. The first two continue in part the discussion of the previous section. I begin (§3.2.1) with a discussion of G within a partly metrical, partly autosegmental account, but with more extensive reference to TE; in §3.2.2, the discussion moves on to G within the framework of Lexical Phonology, focusing specifically on G as a postlexical rule.

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<sup>14</sup> See §3.2.

The first section, then, focuses on a representational account of the phenomenon; the second on its derivation. I will show that both are of relevance to patterns of G in TE: the Metrical Phonology account is a means of neatly representing the differences between RP and TE, in terms of variable ambisyllabicity, while the manner by which such variable ambisyllabicity arises is handled using a Lexical Phonology model; this model is also of importance for our understanding of certain aspects of GS (specifically) in TE, which seems to be developing in certain environments as the result of a lexical rule. In what follows, therefore, I generally present only a selection of the relevant aspects of the theories, providing a more detailed description of specific aspects where necessary.

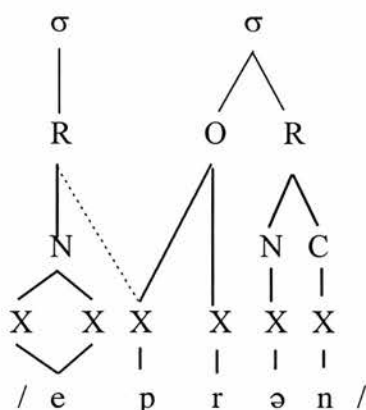
### §3.2.1 *Metrical and Autosegmental Phonology*

Much of §3.1 was couched within a theory of syllable structure drawn from a broadly metrical framework; what is intended in this section is a discussion of the Tyneside data outlined in the final part of that section. In §3.1.2, it was noted that research into TE had shown that a specific social group (working class males) had been heard to use glottalised variants of /p/ /t/ and /k/ in words such as *apron*, *matron* and *micro*. Such contexts are not potential sites for glottalisation in RP; the question remains as to how such variation is to be accounted for

What we need to account for is the presence of the oral stop in the coda of the stressed syllable. The Maximal Onset Principle predicts that the /p/ in *apron* to be syllabified with the second, not the first syllable, and therefore not to be glottalised; recall that the Complex Rhyme Condition is satisfied since the stressed syllable contains a branching nucleus (i.e. one associated with two X-positions), since the vowel is tense: there is no motivation for ambisyllabicity.

However, the syllable shape of *apron* in the speech of these working class Tyneside males is variably that shown in (16) below:

(16)



The broken line here represents a “structural change of a rule” (Goldsmith 1990: 13), the rule in question being the syllabification of the stop. The Maximal Onset Principle does not seem to be operative here, at least not in the same way as in RP. The timing slot associated with the stop appears to have become associated with the rhyme of the preceding syllable, as the result of some kind of spreading rule (Goldsmith 1990: 29); but there has been no delinking of the association of this segment with the second syllable, given the allophony of the sonorant (i.e. that it is devoiced). Put another way, the stop is ambisyllabic, despite the fact that there are no stress contour or syllable weight issues which require such a reanalysis. What this may suggest, then, is a sound change in which speakers favour maximally ambisyllabic consonants in these cases, through the adoption of a spreading rule. Such a change would seem to be an instance of change from below; certain social groups seem to be unaware of the change, and are not able to control it consciously, given its occurrence in word-list style, that is, in very formal speech (cf. Labov 1994: 78). In subsequent chapters of this thesis (particularly in chapter 6), based on the data collected from informants from this thesis, I investigate the possibility that such ambisyllabicity is more widespread than even Docherty *et al.* (1997) suggest in TE, and that ambisyllabicity of the type found in *apron* is also to be found across word-boundaries<sup>15</sup>.

<sup>15</sup> The issue of ambisyllabicity across word-boundaries is discussed in some detail in Kahn (1976).



In arguing for such an analysis, I adopt only in part the proposals put forward by Spencer (1996: 219f). He suggests that while the /t/ in, for instance, *night owl* and *wait reluctantly* is ambisyllabic, it is precisely because the segment is associated with both a coda and an onset that the segment is *not* subject to GR in General American. In other words, only those oral stops which are unambiguously in syllabic codas are subject to GR. However, later in the same work, Spencer notes a “curious fact” about GR, which he discusses “to show that not everything in phonology can be neatly accounted for” (Spencer 1996: 223). This fact refers to instances of GR in RP in *hopeless*, *mattress* and *equal*, where he argues that the Maximal Onset Principle would unambiguously syllabify the stop in the onset of the second (and unstressed) syllable. He then suggests that GR “never seems to occur” (*ibid.*) when the stop is a single medial consonant, as in *happy*, *dotty* and *lucky*.

As was the case with Carr (1991), I feel there are problems with the Spencer (1996) account in terms of theory and in terms of data, and again, I deal with the theory issues first. As noted above, during the course of this thesis, I will want to make use of the potential for ambisyllabicity across word-boundaries: but the motivation for such a phenomenon as presented in Spencer (1996) is somewhat lacking. For instance, in his discussion of GR in General American, he notes that no GR occurs in *wait eagerly*, yet GR does occur in *wait reluctantly*. He argues that the reason this is so is that, in *wait eagerly*, the stop is ambisyllabic (and therefore not subject to GR), yet in *wait reluctantly* it is wholly (and unambiguously) within the coda (and therefore subject to GR); but he does not provide any reason why this should be so (other than to preserve his GR generalisation). Moreover, if ambisyllabicity is posited in *wait eagerly* (presumably on the grounds that /t/ is a well-formed onset, and violates no filters for onset formation), then surely we *must* posit it for the stop in *wait reluctantly* (on the grounds that /tr/ is similarly a well-

formed onset). If this is the case - so that the stop in both cases is ambisyllabic - it is difficult to uphold the GR analysis presented here.

The analysis of the data which Spencer (1996) provides is also somewhat problematic. For instance, in the cases of *hopeless*, *mattress* and *equal*, the composition of the rhyme of the stressed syllable in (a) *hopeless* and *equal* is different to that of (b) *mattress*, in that in the (a) cases, the nucleus is complex (containing a tense vowel associated with two X-positions), while in (b) it is simple (that is, it contains only a lax vowel, associated with one X-position). If this is considered in relation to both the Maximal Onset Principle and the Complex Rhyme Condition discussed above, the stops in the (a) cases would be classified as unambiguously part of the onset of the second syllable, yet in the (b) case the stop would be ambisyllabic. So firstly, Spencer is not really comparing like with like; and secondly, with the (a) cases, we now need to account for (apparently) syllable initial GR. A further potential data related problem is the claim that there is no GR in words such as *happy*, *dotty* and *lucky* in RP, which, while it may be rare, is unlikely to be *categorically* absent for all RP speakers, particularly the younger ones.

### §3.2.2 *Lexical Phonology*

I give below a selective version of some basic principles of Lexical Phonology. Such brevity is warranted for two main reasons, namely (a) constraints of space and (b) relevance to the discussion at hand: not all aspects of the theory need to be investigated to illustrate the application of the model to the phenomena of G in TE. Specifically, in what follows, I attempt to present what differentiates lexical and postlexical rules within the framework of Lexical Phonology.

Lexical Phonology is a generative phonological model; that is, it is concerned with the relationship between the underlying representations of morphemes and their surface forms. The relationship between the underliers and their surface form can be

explained in terms of a set of ordered phonological rules. In this respect, then, Lexical Phonology is a direct successor to a general framework of Standard Generative Phonology (cf. McMahon 1989: 10); but it deviates from this standard in a number of significant ways. Crucially for present purposes, Lexical Phonology argues that phonological rules are split between two components: one set which operates within the lexicon, interspersed with (inflectional and derivational) morphological rules, and a second set which operates after such word-formation processes have taken place, and words have been entered into syntactic structure. Given this distinction, the first set of rules are commonly known as *lexical* rules, and the second set as *postlexical* rules. In order to understand this distinction fully, let us compare the rules of G<sup>16</sup> with the rule of Trisyllabic Laxing or Shortening (or TSS). There are a number of well-documented complexities associated with TSS (cf. Kiparsky 1982: 35f, McMahon 1989: 26-28) which are not dwelt on here; what is of importance are the principal characteristics of such a rule. TSS is a phonological rule which laxes or shortens a vowel that is “followed by at least two vowels, the first of which must be unstressed” (McMahon 1989: 26). This (in part) explains the vowel alternations in pairs of words such as *chaste-chastity*, yet we need to explain why TSS does not apply in the pair *waste-wastefulness*. The argument is that certain phonological rules apply at different levels or on different strata, and these rules interact with various morphological rules on these strata; affixes such as *-ity* are Class I affixes, applying on stratum 1, while affixes such as *-ful* and *-ness* are Class II affixes and apply on stratum 2. It is therefore suggested that a rule such as TSS applies on stratum 1, not stratum 2, as can be seen in the examples *chaste-chastity* and *waste-wastefulness*. Rules which apply at specific levels or strata are considered to be lexical rules - so TSS is lexical. There are, then, ways in which a rule such as TSS differs from G, and some of these are noted below in (17):

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<sup>16</sup> There is no need to differentiate substitution from reinforcement for present purposes, hence the use of the ‘cover-all’ symbol G.

- (17) (a) TSS applies word internally only, while G applies internally (*pity*) and finally (*cut*).
- (b) TSS applies in a restricted environment (i.e. in words with Class I affixes), while G will apply to any /t/ (and also to /p/ and /k/ in certain varieties) when it appears in the coda of a stressed syllable.
- (c) Following from the conjunction of (a) and (b) above, it is suggested that TSS is sensitive to morphological structure, while G applies across the board, blind to morphological structure.
- (d) TSS has exceptions: some complex words with Class I affixes do not undergo TSS (eg *obese-obesity*) - while G appears to be exceptionless: providing the right phonetic context exists, G will apply.

It seems necessary to posit two very different types of phonological rule, one which applies during word-formation, and one which applies after, when words have been entered into the syntactic structure. The first type, of which TSS is an example, is a lexical rule; the second, of which G is an example, is a postlexical rule.

An incorporation of this rule distinction into accounts of phonological variation and change would be desirable. Carr (1993: 197) notes that “speakers have some conscious awareness of distinctions introduced by lexical rules ... but they have little ability to perceive distinctions introduced by postlexical rules”. This would seem to suggest that there is a far greater level of salience with lexical rule addition (where a change, once established is “inserted as a phonological rule at the end of the native speaker’s rule system: it then moves gradually higher in the grammar as subsequent sound changes become the final rule” (McMahon 1989: 56-57)) than with postlexical rule addition. We might then want to set up a correlation between change from above (implemented by a high status group in the community, and with a high salience level (cf. §4.1)) and lexical rule addition, and change from below (implemented by a group at the interior of the social hierarchy, and at a low level of

salience: again cf. §4.1) and postlexical rule addition. But this would be erroneous, since there are instances of changes from below which are postlexical in some dialect areas and lexical in others, which complicates any such correlation.<sup>17</sup>

Another aspect of the ways in which the lexical/postlexical distinction are related to variation and change is the argument that postlexical rules can become lexical - when postlexical rules begin to diffuse, they gain lexical exceptions, and thus acquire the status of a lexical rule: one of the distinctive features of postlexical rules is that they do not permit exceptions, while lexical rules do have exceptions (cf. 17(d) above). Such a pattern has been attested for changes associated with the Scottish Vowel Length Rule (McMahon 1989: chapters 5 and 6, 1994), but I want to focus again on patterns of G in TE, in relation to the claims made by Docherty *et al.* (1997).

Docherty *et al.* (1997), as part of their overall analysis of patterns of G in TE, discuss properties of the Final Release Rule<sup>18</sup> (FRR) in some depth. The FRR states that in TE, neither glottal reinforcement nor replacement of /t/ occurs in pre-pausal or turn-final position; in such instances, a full [t] is released; this is also found in another north-eastern variety, Durham, about twenty miles south of Tyneside: Kerswill (personal communication) shows that GS was *not* found in pre-pausal position in his sample of Durham speakers. The evidence provided by Docherty *et al.* (1997) suggests that in conversation style, the FRR is rarely violated in pre-pausal

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<sup>17</sup> For instance, the rule of /æ/-Tensing variably tenses (or raises or diphthongizes) historically short /æ/ before certain consonants (e.g. *tap* [tæp] (lax before a voiceless stop) vs. *tan* [tɪ<sup>ɹ</sup>n] (diphthongised before a nasal)). In certain accents, such as Philadelphia, /æ/-Tensing has a range of specific properties which suggest it is a lexical rule: it has lexical exceptions (even though /d/ is not a tensing context (tensing occurs only before anterior nasals and anterior voiceless fricatives), *mad*, *bad* and *glad* surface with [ɪ<sup>ɹ</sup>]); and it is sensitive to morphological boundaries, in that *manning* (in e.g. *They were manning the desk*) surfaces with a tense vowel. But this is not the case in all dialect areas: in Chicago and Detroit, for instance, where lexical exceptions do not exist, and the rule applies across the board, irrespective of morphological boundaries. In such accents, /æ/-Tensing is a postlexical rule. Yet from observations in apparent time (see *Introduction*, footnote 3 for a definition) it is clear that /æ/-Tensing is a change from below (although in some areas of the northern US, it is nearing completion). So we cannot make a simple correlation between changes from above and below with lexical and postlexical change respectively. See further Labov 1981, 1994, Harris 1989 and McMahon 1989.

<sup>18</sup> I discuss the FRR again in chapters 5 and 6.

position, with 30 of their 32 speakers never or rarely using glottal or glottalised variants in such an environment. This is also true of turn-final variants; but where exceptions do exist, they seem to follow a certain pattern. Glottalised variants are occurring when the variable is preceded by a short vowel: long vowels seem to prohibit violation of the FRR. In fact, violation of the FRR is most frequent in the lexical items *that* and *it*. This leads Docherty *et al.* (1997: 297-8) to the following conclusions: “if we postulate that the rule for glottalisation is currently spreading into the turn-final environment in which it was formerly prohibited, it is observed in orthodox phonological terms to be spreading by lexical diffusion, with very frequent items such as *that* and *it* in the vanguard of the change ... the type of rule that accounts for this pattern appears at this stage of the argument to be lexical”.

I have tried to illustrate in this section the difference between lexical and postlexical rules, specifically as they relate to patterns of G in TE. That G is a postlexical rule in TE has been established by comparing this to a lexical rule such as TSS. Furthermore, data from a recent variationist study has shown that there is evidence to suggest that the postlexical rule of FRR has begun to acquire lexical properties: it has started to gain lexical exceptions. I explore these patterns more extensively in chapters 5 and 6 of this thesis, where I examine variation in the speech of a set of TE informants.

### §3.3 *Traditional dialect material*

In this section, I investigate what light an examination of traditional dialect material can shed on patterns of G in TE; I begin with data from the *Survey of English Dialects*, then move on to consider further data from O’Connor (1947).



### §3.3.1 Survey of English Dialects *data*

In order to attempt to discover more about the history of the phenomena of GR and GS in Tyneside, data was collected from the *Survey of English Dialects* (hereafter *SED*) (Orton 1962; (eds.) Orton and Halliday 1962-3). As Chambers and Trudgill (1998: 21-5) point out, it is important to note that the methods employed by a 'traditional dialectologist' are not in most cases directly comparable with those of a sociolinguist. The selection of informants is not the same, since traditional dialect surveys regularly rely on the speech of NORMs (non-mobile older rural males), and many sociolinguistic surveys attempt to record data from a more catholic sample of informants; the type of data collected is frequently different, given that traditional dialect surveys make use of questionnaires, often designed to elicit one word answers in fairly formal styles, while sociolinguistic surveys regularly aim to provide a recording of a range of different speech styles, from minimal pairs to casual speech; and the presentation of the data can also vary, with results from traditional dialect surveys presented in terms of maps, which chart the variants used in different locales, and data from sociolinguistic surveys presented in graph or table form, in an attempt to establish patterns of variation and change within the sampled speech community. The differentiation between traditional dialect material and sociolinguistic surveys is therefore well-established; but it is equally important to realise that such traditional surveys provide a wealth of information about earlier stages of a given dialect which might prove illuminating in terms of an explanation of current variation. So while there is no claim that the data presented in this section is of a similar nature - in terms of its collection or presentation - to those discussed above and in later chapters, the aim of investigating *SED* material is to establish the extent to which patterns of G are to be found in areas surrounding Newcastle in the mid-twentieth century.

To this end, data was collected from the *Introduction* (Orton 1962) and *The Basic Material vol 1*. (3 parts): *The Six Northern Counties and the Isle of Man* (Orton



and Halliday 1962-3). Two of the nine localities visited in Northumberland were selected, namely Earsdon (recorded in the *SED* as Nb 6) and Heddon-on-the-Wall (Nb 8). The reason for selecting these two particular areas was that they were the localities closest to Newcastle, and therefore most likely to provide the nearest approximations to Newcastle speech. A range of recent sociolinguistic data (for instance Eckert 1988, Trudgill 1986) has suggested that innovations often begin in urban centres and then diffuse to outlying rural areas<sup>19</sup>. If GR and GS are urban innovations, we might expect Earsdon and Heddon to be marked by more extensive glottalisation of the oral stops than other communities in Northumberland, given that Newcastle is the only major conurbation in the area: Berwick-upon-Tweed is not comparable in size, and Sunderland, while another large town, is further south and east of Newcastle, and therefore unlikely to influence the speech of NORMs in Heddon and Earsdon as markedly. Given the recent urban sprawl in the north-east, Heddon and Earsdon at the end of the twentieth century are now practically suburbs of Newcastle, but in the earlier part of the century were small, separate communities in their own right. Heddon lies approximately 10 miles west of Newcastle city centre, near a major road linking Newcastle with the west of England; Earsdon is a similar distance, but in a north-easterly direction, from the centre.

Five informants were recorded in these communities, two in Earsdon and three in Heddon. I have attempted to draw a brief profile of the informants based on the information provided by Orton and Halliday (1962-3: 13-4):

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<sup>19</sup> Other surveys have suggested that some innovations do not diffuse in this way, but rather 'hop' from one urban centre to another (see for instance Milroy (1996) on TH-Fronting in Derby).

	<i>Earsdon</i>		<i>Heddon-on-the-wall</i>		
<i>Sex</i>	male	female	male	male	male
<i>Class</i>	WC	WC	WC	WC	WC
<i>Year of birth</i>	1883	1874	1891	1886	1874
<i>Place of birth</i>	Earsdon	Earsdon	Carlerton Moor	Heddon	Gilsland

By and large, then, these informants conform to the NORM stereotype. There are the following exceptions, of course: one of the five is female, and two had moved to Heddon from elsewhere in the north-east. But the overall pattern with this small sample is that they reflect the general type of informant used in the *SED*.

Responses were collated for each of the localities for any possible glottalling, glottalisation or weakening site for any of the voiceless stops.

All potential sites were recorded and scores tallied for the following variants: fully released stop; glottalised stop; glottal stop; weakened variants (approximant or tap). The phonetic environments were also considered (following Wells 1982 as given in §3.1.1), and scores recorded for the following: before a word or morpheme boundary; before a consonant; before a non-syllabic liquid, or before a semi-vowel; before a syllabic liquid or nasal; before a vowel. The total number of potential sites for the Earsdon data was 303, and for Heddon was 424, and the percentage scores, correct to two decimal places, for each variant in each context is given below for both Earsdon and Heddon.

(18) *Voiceless stop variants in Earsdon*

	(p): N = 47			(t): N = 159				(k): N = 97		
	FR	GR	GS	FR	GR	GS	W	FR	GR	GS
#	57.45	0.00	0.00	76.73	0.00	0.00	0.63	64.95	0.00	0.00
C	0.00	0.00	0.00	0.63	0.00	0.00	n/a	1.03	0.00	0.00
L/S	4.26	0.00	0.00	1.89	0.00	0.00	n/a	3.09	0.00	0.00
sL/N	19.15	0.00	0.00	8.47	0.00	0.00	n/a	11.34	0.00	0.00
V	17.02	2.13	0.00	9.43	0.00	0.00	0.63	18.56	1.03	0.00

(19) *Voiceless stop variants in Heddon*

	(p): N = 63			(t): N = 224				(k): N = 137		
	FR	GR	GS	FR	GR	GS	W	FR	GR	GS
#	65.08	0.00	0.00	79.02	0.00	0.00	0.45	68.61	0.00	0.00
C	0.00	0.00	0.00	0.00	0.00	0.00	n/a	0.00	0.00	0.00
L/S	3.17	0.00	0.00	1.34	0.00	0.00	n/a	2.92	0.00	0.00
sL/N	7.94	0.00	0.00	9.38	0.00	0.45	n/a	6.57	0.00	0.00
V	22.22	1.59	0.00	7.14	0.45	0.89	0.89	21.90	0.00	0.00

Key: N: number of sites

FR: full release

GR: glottal reinforcement

GS: glottal substitution

W: weakened variant

\_ #: before a morpheme/word boundary

\_ C: before a consonant

\_ L/S: before a non-syllabic liquid, or before a semi-vowel

\_ sL/N: before a syllabic liquid or nasal

\_ V: before a vowel

The results indicate that there is very little evidence of any GS, GR or weakening in these localities at the time of recording. The vast majority of instances were word or morpheme final, where full release was the preferred variant; and indeed in all other contexts, the fully released variant was favoured. The results for GR, GS and weakening are not statistically significant. But this lack of glottalised or weakened variants is in itself significant in terms of the development of G and Weakening in Tyneside. There is not a single example of GS in Earsdon for any of the voiceless stops, and only 2 out of 303 potential sites show GR; similarly in Heddon, only 2 out of 423 sites show reinforcement, and only 3 show GS. How can these results be explained?

There are two important factors to consider relating to the data collection and transcription. Firstly, the nature of the interview for the *SED* material is very likely to have led to the recording of only a formal style; this might lead to the informants avoiding what they perceive to be stigmatised variants. Secondly, the fieldworker may either not have noticed the variant, and therefore not transcribed it, or may have ignored it as irrelevant: both of these could probably be discounted, certainly the latter. In the *Introduction*, Orton notes that “glottalised plosives have been symbolised by placing a linked (?) after the symbol concerned” (Orton 1962: 25-6). But given the data as we have it, we might suggest the following conclusions: that fully released variants are conservative forms (given that they are characteristically used by the NORMs) and that GR, GS and weakening are innovations; these innovations have as their origin the urban centre of Newcastle and diffuse outwards into the rural communities; and that the innovation is fairly recent, given the lack of any significant G in these localities relatively close to Newcastle.

But what of the qualitative data? Is there any indication of the nature of the potential diffusion, even given the small number of instances? Below are listed all the relevant instances of GS, GR and weakening from Earsdon and Heddon:

(20) *Examples of variants of (p) (t) and (k) in two Northumberland localities**Earsdon**Saturday* sɛkədə (Orton and Halliday 1962-3: §VII.4.5)*tinker* tɪŋkə (Orton and Halliday 1962-3: §VIII.4.9)*what have you done that for* wad hɛ jə djən ðat fɔ<sup>ɸ</sup>: (Orton and Halliday 1962-3: §VIII.8.6)*champion* tʃampjən (Orton and Halliday 1962-3: §VIII.8.5)*Heddon**haltershank* hɛlʔəʃaŋk' (Orton and Halliday 1962-3: §I.4.2)*water* wɑ:ɹə (Orton and Halliday 1962-3: §III.3.2)*wire-netting* waɪə nɛʔn (Orton and Halliday 1962-3: §III.7.11)*litter* lɪʔɔ<sup>ɸ</sup> (Orton and Halliday 1962-3: §III.8.3)*slaughterhouse* slɑʔtəhu:s (Orton and Halliday 1962-3: §III.11.4)*Saturday* sɛkədə (Orton and Halliday 1962-3: §VII.4.5)*slippy* slɪʔpɪ (Orton and Halliday 1962-3: §VII.6.14)

These data, though few in number, nonetheless provides some interesting and important examples, and I deal with each of them in turn.

- *Saturday*: this shows weakening of the stop to the distinctive uvular trill which is characteristic of rural Northumberland speech. Notice that both Heddon and Earsdon are marked for weakening in this one word, and provides a potential counterexample to Carr's claim (cf. §3.1.2) that Weakening does not apply to nouns. This is in itself problematic however, because it is clear that days share syntactic and semantic characteristics of both names and common nouns: *Saturday* in an utterance like *See you on Saturday* has the semantic characteristics of a name, in that it presupposes that the hearer is aware of which Saturday (of a potentially infinite range of Saturdays) is being referred to (cf. *Give it to Bob*); but *Saturday* in *next Saturday* has the syntactic characteristics of a common noun, in taking *next* as a determiner.

- *tinker*: this shows GR of the velar stop in what would appear to be syllable initial position (following the Maximal Onset Principle). This provides a useful instance to support a spreading analysis as discussed in §3.2.1; in this case, the speaker has favoured a maximally ambisyllabic stop, with the glottally reinforced velar associated with both the coda of the first syllable and the onset of the second. But this is an unusual example, in that there is evidence of apparent *pre*-glottalisation here ([ʔk]), which contrasts with the *post*-glottalisation ([pʔ]) in *champion*. This could be an instance of idiolectal variation between speakers in the one locality; or it could be a transcription error; whatever the reason, the transcription given suggests some sort of GR.
- *what have you ...*: this shows weakening of /t/ to a voiced stop (possibly a mistranscription of a tap, given the auditory similarity). This example concurs with Carr's (1991) argument, which suggests that weakening, not glottalisation, occurs in certain words belonging to what Docherty *et al.* (1997) describe as 'non-lexical categories', citing examples such as *not*, *but*, *what* and *that*.
- *champion*: what is distinctive about this example is the transcription which would seem to indicate a phonetically distinctive GR pattern, with the glottal being transcribed *after* the bilabial. This has been noted as a marked Tyneside form, as discussed in the opening section of this chapter.
- *haltershank*: this shows glottal replacement of /t/ in what would appear to be syllable initial position (according to the Maximal Onset Principle), similar to the *tinker* example discussed above.
- *water*: again, the data here is problematic for Carr (1991), given that *water* is clearly a common noun, but displays Weakening; but *water* is arguably not a count noun, given that it does not normally display properties of count nouns, such as the ability to be determined by the indefinite article (\**a water*) or be pluralised (\**waters*) without concomitant change of meaning. So in this sense, both

*Saturday* and *water* show certain idiosyncracies in their ‘nounhood’ - they are not prototypical count nouns - and this may in part explain why they do not behave as other nouns do with regard to Weakening.

- *wire netting*; *litter*: these examples display GS in very salient, stigmatised positions (before a syllabic nasal and intervocalically respectively), which again might suggest that the data collected in the *SED* does not display entirely ‘formal’ style features. Notice also that only the alveolar stop is replaced: there is no evidence of glottaling of /p/ or /k/.
- *slippy*: here the glottalisation pattern is different from that in *champion*, with reinforcement occurring before the release of the oral stop, suggesting two types of reinforced variants operative in these localities near Newcastle: one RP-like, another very localised.

So while the number of glottalled, glottalised and weakened variants in Heddon and Earsdon is insignificant quantitatively, there are still two important points to be made. The first is that qualitatively, there is some evidence to suggest that the phonology (as well as the phonetics) of GR, GS and Weakening is distinctive in the north-east, given the syllabic structure of words in which this variation takes place. The second is that the significant numbers of fully released variants might well suggest that this was the older form, and that the weakened innovations have spread from the urban centre outwards into the rural communities. Further evidence to support this comes from the fact that - as can be seen in the instances quoted in (20) above - the Earsdon and Heddon communities are variably (non-)rhotic, to a greater degree than some of the other Northumberland localities. Admittedly, rhyme /r/ is lost most frequently in unstressed syllables following schwa, an environment which is likely to promote r-loss, but this is not the only position, given that *wire* is [waɪə]. It would seem reasonable to suppose that this non-rhoticity was also an urban innovation, and has spread to the surrounding rural areas. The method of



transmission of such innovations cannot be unambiguously ascertained from the data available here, but patterns of dialect contact in other areas of Britain (cf. Britain 1997, Trudgill 1986) might lead to the hypothesis that inhabitants of these localities had established weak links with urban speakers from Newcastle, and that innovations actuated in that centre exerted pressure on outlying dialect areas, perhaps attracting covert prestige, diffusing geographically, socially, and perhaps lexically. There is not sufficient evidence to prove this is the case - and clearly the diffusion of innovations was not widespread in the 1950s when the data was collected: the hypothesis is merely presented by analogy with other similar changes.

### §3.3.2 *O'Connor (1947)*

O'Connor (1947) provides a brief sample of data collected from boys aged between 12 and 18 resident in the Benwell and Elswick areas of Newcastle. These localities lie to the west of the city centre (towards Heddon) and are strongly WC. The data do not allow for any significant generalisations, but, like the *SED* material, provide some potential evidence for glottalisation and weakening patterns in the middle of the present century. There seem to be clearly marked instances of tapped or retroflex variants of (t) in O'Connor's transcription: O'Connor provides transcriptions of apparently 'devoiced' voiced alveolar stops, transcribed as  $\underset{̣}{d}$ , in certain words, viz. *talk it himself* ta:ʔk əḏ ʊs'ɛ:l; *at him* əḏ əm; *you've got to be* jəv gɔḏə bi; *that a* ðəḏ ə. I wonder whether this symbol is meant to represent a tapped variant, so that the diacritic is not marking a devoicing but rather a change in manner of articulation: a tap rather than a stop. The  $\underset{̣}{d}$  symbol here appears in words in which the tapped variant is regularly found in TE (in prepositions, pronouns and monosyllabic verbs); this is conjecture, of course, but it would fit with aspects of the present-day variety. Other variants of *at*, *it* and *that* appear as 'əʔ, əʔ ~ ət and ðəʔt respectively. Variability in glottalisation of the voiceless stops is also in evidence,

with variants of (k) ranging from full release in *back* and *book*, through reinforcement in *looks*, *talk* and *talking*, and substitution in *thinks*. The examples *aunty* *an?ti* and *people* *pi:ʔpəl* again suggest interesting possibilities concerning syllabification of the voiceless oral stops in this variety of English. It seems as if there are two possible analyses. If we assume initially that the syllabification of the stops in Tyneside English is isomorphic with that of RP, then in instances such as *aunty* and *people*, the stops are syllable initial, following the Maximal Onset Principle discussed above: there is no motivation - on this analysis - to argue that the stops are ambisyllabic, since in these examples the stressed syllables contain two X-positions in the rhyme excluding the oral stop. Yet a reinforced variant occurs in these examples. These data provide yet further evidence to suggest that different or additional processes of syllabification may be operative in TE (as opposed to RP), which leads to the second, alternative analysis.

Suppose an additional, postlexical ambisyllabic process is operative in TE, which does not apply in RP, and that this ambisyllabicity is the result of speakers favouring maximally ambisyllabic stops. Such an analysis could be invoked to explain reinforcement patterns in what appear to be syllable initial position, such as *people* and *aunty* described above, as well as in instances such as *apron*, *matron* and *micro*, discussed in §3.2.1, and some of the examples which arose from the *SED* discussed in §3.3.1: this analysis can account for data gleaned from localised network surveys in the 1940s, traditional dialect surveys in the 1950s and present day sociolinguistic investigations. There are clearly instances of ambisyllabic stops which are reinforced or replaced if the stop follows a stressed short vowel in a polysyllabic word such as *pepper*, *butter*, *picky*: and variation in stop allophony is by no means restricted to Tyneside in this regard, with reinforced and replaced variants occurring in such words in a range of different varieties. But this ambisyllabicity follows from a clash between the Maximal Onset Principle, favouring syllabification of the stop in

the unstressed syllable, and stress assignment rules which favour - indeed require - syllabification of the stop in the stressed syllable, since stressed syllables containing a lax vowel must be closed. What is distinctive about TE is that ambisyllabicity seems to occur variably when there is no motivation for it in terms of stress assignment. In the distinctive Tyneside examples discussed in the preceding sections as well as this one, the stressed syllable contains either (a) a tense vowel or diphthong or (b) a lax vowel and a consonant before the stop. There is no need to assign the stop to the coda of the first syllable to satisfy stress assignment rules. So what seems to be happening here is that ambisyllabification occurs in a wider range of environments in TE than is the case in RP. It will be the purpose of parts of the following chapters of this thesis (a) to attempt to establish the phonological nature of these environments, based on the data collected from the informants, and (b) to establish how such a postlexical ambisyllabicity should be formulated.

#### §3.4 *Diachronic development of G in RP*

The *SED* material and O'Connor (1947) provide some brief information on G patterns in earlier stages of TE; but there is little material on the age of the phenomenon in RP. Collins and Mees (1996) attempt to provide evidence to suggest that glottal reinforcement is "of some antiquity" (1996: 175), possibly common in RP from the mid-nineteenth century onwards. Using the concept of apparent time<sup>20</sup>, Collins and Mees used tape recordings of RP speakers born between 1848 and 1888 to illustrate patterns of G in the later part of the nineteenth century in two linguistic environments: pre-consonantal (*hot coffee*) and pre-pausal (*I'm hot*). The data presented in their article illustrates patterns of G for H.C. Wyld, Daniel Jones and Ellen Terry, all of whom they consider to be U-RP speakers; the formality of the discourse is high (reading passages for Wyld and Jones, Shakespearean soliloquy for

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<sup>20</sup> See *Introduction*, footnote 3.

Terry); and yet the pervasiveness of GR - and in some cases GS - of /t/ is quite marked. My presentation of the data is slightly different from that found in Collins and Mees (1996: 183-4), since I give additional scores on both replacement and reinforcement, and I give here scores for /t/ only.

(21) Reinforcement and replacement of /t/ in H.C.Wyld's speech

Number of potential G sites:	14
Number of G variants:	14
Ratio:	100%
GS:	4 (28.57%)
GR:	10 (71.43%)

Reinforcement and replacement of /t/ in Daniel Jones' speech

Number of potential G sites:	13
Number of G variants:	11
Ratio:	84.62%
GS:	4 (30.77%)
GR:	7 (53.85%)
Full release:	2 (15.38%)

Reinforcement and replacement of /t/ in Ellen Terry's speech

Number of potential G sites:	11
Number of G variants:	5
Ratio:	45.45%
GS:	2 (18.18%)
GR:	11 (27.27%)
Full release:	6 (54.54%)

The sample here, though small, nonetheless suggests that there is some evidence to support the hypothesis that patterns of G were in evidence in the speech of RP speakers at the turn of the century, though to varying degrees, with the male speakers favouring glottal variants to a greater extent than the female speaker does in this case. There are no data here which could shed light on the syllabification issues discussed above; but it is necessary to square the fact that GR and GS were in evidence in the

RP of the late nineteenth century with the fact that there seemed to be a notable lack of glottalisation recorded for the *SED* informants from Northumberland. As discussed earlier, it could well be the case that the researcher failed to mark glottalised or glottalled variants; but this runs contrary to the principles stated in the *Introduction* to the *SED* basic materials. It may simply be the case that G was not common with these rural speakers, occurring only sporadically through contact with speakers from the urban centre of Newcastle.

Andréson (1968) presents a copious, detailed review of evidence for the age of the phenomenon in (proto-)RP, which I summarise here. The sixteenth and seventeenth century examples from Matthews (1938), Wyld (1936) and Dobson (1957) fail to provide evidence for any lenition process: *ffleestreet* 'Fleet Street', *fonstone* 'font-stone' and *wisntide* 'Whitsuntide', as Andréson (1968: 12) argues, display elision of [t] rather than any GR or GS. Obviously the matter is complicated given that there is no orthographic symbol for the glottal stop; but the evidence presented for the early Modern English period is not conclusive, giving no *positive* evidence for [ʔt] or [ʔ]. The best evidence for GS and GR comes from prescriptivists in the late nineteenth<sup>21</sup> and early twentieth century. Bell (1860: §137) suggests a Scots origin for this phenomenon: "The Breath Obstructive Articulations, especially the letter T, are, in the West of Scotland pronounced without any articulative action, but with a mere glottal catch, accompanying the articulative position". Bell, like O'Connor on Tyneside, seems to be suggesting a reinforced variant here, rather than the stereotypical substitution associated with present-day west Scotland accents (cf. Macaulay 1977), but in a later work (Bell 1867: 60, 93) notes GS as a variant for /t/ in west Scotland varieties. The situation is further confused by Sweet, who in 1877 describes the glottal catch, symbolized as [x], as a substitute for voiceless stops in

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<sup>21</sup> The fact that prescriptivists in the late part of the nineteenth century were commenting on such variants is further evidence to suggest that GR and GS were widespread by this time. As a change from below, the stigmatisation of the new forms is usually in evidence only when the change is near completion.

Glaswegian English, giving examples such as (waxehrr) for *water* and (bæxehrr) for *butter* (Sweet 1877: §19), and this would seem to support Bell's later assertion; but four years later, Sweet (1880-1: 231-2) reverts to the claim that GR, not GS, is the typical Glaswegian pattern. Such seemingly contradictory evidence would really seem to suggest that both GS and GR were common in mid-nineteenth century Glaswegian, just as they are on Tyneside today. Ellis (1889) notes a spread of GS variants eastward across the Central Belt into Fife, and Wright (1905) considers GS to be widespread across the whole of central Scotland.

Evidence for the phenomenon in England seems to be confined to the earlier part of the twentieth century, with Sweet (1908) noting the glottal stop (symbolised now as [!]) in intervocalic position in Northern England (as in wa!ər for *water*) and Jones (1909: §35,47) illustrating GS of /t/ with [ʔ] in London English. Jespersen (1909) argues that both GS and GR are widespread throughout England, and that such variants are not restricted to dialect speakers. Hirst (1914) provides some evidence for GR in Lancaster English, arguing that the GS variants are particularly common among the young. He suggests that in Liverpool, Wigan and Ormskirk, GS occurs only in the context of a following consonant, but (and Andrézen does not pick up on this) his transcriptions for northern English generally tend to be odd, suggesting *not coming* as [nɔʔ kʌmin] and Lancaster *matter* as [mɑʔə]; the first is odd in terms of phonemic inventory, suggesting that northern English has a six way contrast in lax vowels (i.e. /ʌ/ is part of the phonemic inventory for northern English); and the second is odd in terms of lexical incidence (i.e. /ɑ/ in *matter*) and phonotactics (the transcription given here suggests non-rhoticity in this rhotic area of England). These may be slips, or the informant may be accommodating or hypercorrecting; but these factors in themselves may bring into question the reliability of Hirst's evidence. Andrézen (1968: 25-36) provides further evidence for



the later twentieth century development of the phenomenon which I do not go into here: but the findings of his own study are discussed briefly below.

The informants for Andréseu's survey - 45 in total, 20 men and 25 women aged between 16 and 30 - were all RP or 'near' RP speakers (though what constitutes 'near' is not explicitly considered). In his analysis, he makes no distinction between GS and GR, grouping all glottal variants together. The motivation for so doing is wanting, since he claims that [ʔp], [ʔt] and [ʔk] "cannot be heard" (Andréseu 1968: 43). His results suggested that there was no evidence to suggest G was more common among women than men; but there was an implication that the *range* of G was greater for men than for women, that women's scores tended to be more even. My own analysis of Andréseu's data does not seem to suggest any noteworthy sex distinctions; the only discernible pattern which emerged was that a third of both male and female speakers show decreasing glottalisation scores for the stops in this order: t > p > k; but this is hardly a revelation. The analysis in terms of phonetic environment is slightly more promising. Andréseu compares patterns of G in words like *Patricia*, *democratic* and *fatuity* with those in words like *Patrick*, *democracy* and *fatuous*. Given our knowledge of present-day RP we would expect variable GR in the second set, but categorically no GR in the first set (given that the stop is foot-initial). Andréseu (1968: 104) comments: "Pre-glottalisation does not take place, *or is very rare*, if the voiceless plosive is preceded by an unstressed vowel and followed by a consonant (or semi-vowel) belonging to the same word, with which consonant it forms a cluster that can occur word-initially" (emphasis added). Andréseu is therefore suggesting that even in RP, in careful style, foot-initial GR is not categorically disallowed, so that while GR is not expected in *Patricia*, *democratic* and *fatuity*, it can sometimes occur. No statistics are provided for this - it is unlikely to be significant statistically - but its appearance in this style in this variety might alert researchers to potential foot-initial GR in vernacular speakers in casual style,



where it might be more likely to occur. And finally, Andréseu (1968: 112) notes that all writers on the subject agree that “*in non-dialectal speech*”<sup>22</sup> preglottalised /p/, /t/ and /k/ never occur before a vowel sound belonging to the same simple word, e.g. *water*, or to a ‘bound’ morpheme, e.g. *looking*” (emphasis added), whether or not that morpheme is inflectional or derivational. Andréseu here seems to be suggesting that dialectal variation in G patterns may be subject to constraints imposed by morphological bracketing, with G applying in some varieties in some contexts, but not in others. Such variation is not common with prototypical postlexical rules; this again needs to be considered in relation to the dialectal material collected in Newcastle which forms the basis for chapters 5 and 6.

### §3.5 *Summary*

This chapter has attempted to consider some of the complexities surrounding the phonology of GS and GR. The environments in which G applies in RP were considered; certain problems seem to have emerged when this was applied to data from TE. These problems related to the range of variants of the oral stops in TE, and the concomitant issue of Weakening in that variety. This led to a consideration of syllabification of the oral stops in polysyllabic words, and it was shown that either (a) syllabification seemed to be variable, with some speakers in some words favouring maximally ambisyllabic stops or (b) glottalisation patterns in TE allowed variable glottalisation of syllable initial stops. Further, such syllabification patterns and changes affecting the postlexical rule of GR led to a discussion of the phenomena within metrical and lexical frameworks of English phonology, and an attempt to investigate the diachronic development of this lenition process in both Tyneside English and RP. Material from the *SED* suggested that neither GR nor GS were common in rural localities close to Newcastle even in the early part of the century,

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<sup>22</sup> i.e. in the speech of Andréseu’s RP speakers.

suggesting that the change was urban in origin, and diffusing slowly out to the rural communities; however, even in these traditional dialect surveys, isolated examples from both Tyneside and Northumberland seemed to suggest a phonological process concerning syllabification and G different from that in RP. These were considered to be distinctive characteristics of this variety of English, and therefore to be investigated further in the analysis of the speech of the informants in chapters 5 and 6.

## 4 The gender debate

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Until recently, it was an apparent truth almost universally acknowledged that “[i]n virtually all sociolinguistic studies that include a sample of males and females, there is evidence for this conclusion about their linguistic behaviour: women use fewer stigmatized and non-standard variants than do men of the same social group in the same circumstances” (Chambers 1995: 102). This was for many years the received opinion on the differences between men and women’s speech (see for instance Wolfram (1969: 76) and Hudson (1996: 195) for different expressions of the same general sentiment). This view has latterly been debated by a number of scholars; in particular, the role of women in the propagation of innovative (and often, though by no means always, non-standard) variants has been seen as an important topic in sociolinguistic research that has focused on differences between male and female speech. In this chapter I propose to cover three main topics which concern the relationship between sex-based variation in language and linguistic change:

- the first subsection of this chapter will consider some of the principles and theories surrounding the gender debate, and its implications for an understanding of language change. Clearly there are a number of other important areas in which an understanding of gender-based variation illuminates specific linguistic behaviour: such topics include, for instance, politeness in discourse (e.g. Holmes (1995)), or male dominance in mixed talk (e.g. Leto DeFrancisco (1998)). However, these will not be discussed here. My specific (and admittedly more traditional) interest concerns an approach to linguistic change which is related to differences in women’s and men’s usage of certain linguistic forms. To that end, the discussion in this section will draw heavily on the following: Labov (1990); Eckert (1989, 1998); and James (1996),

looking particularly at the (related) notions of standard and prestige forms, and how they relate to male and female usage.

- In the second section, I intend to review two ‘classic’ sociolinguistic studies (Trudgill’s survey of Norwich English, as presented in Trudgill (1974) and the Milroys’ survey of Belfast English, as presented in (among others) Milroy (1981) and Milroy and Milroy (1978, 1985, 1992)), focusing particularly on variables which were seen to be significant sex markers, specifically those involved in linguistic change. This will provide the opportunity to investigate the validity of some parts of the traditional sex/prestige pattern with which I opened this chapter.
- In the final section, I will move on to look at the more recent challenges to the established notions of differences in women’s and men’s language, which will include both a discussion of the concept of gender itself, its role in the analysis of linguistic variation and also empirical evidence from a variety of sociolinguistic studies, including some carried out in Newcastle, New Zealand and the Fenlands.

The reason I provide such an extensive review of analyses of gender-based patterns of variation and change is concerned primarily with the social characteristics of the sample of TE speakers who make up the informants for this thesis. I discuss these characteristics in some detail in the next chapter; as will be shown there, in addition to being an apparent time study (since the sample is divided along an age parameter), the survey is also an investigation into the speech of male and female Tynesiders, in order to investigate the extent to which established patterns associated with male and female speech hold true in this community. In order to investigate this, there is a

need to provide a discussion of what such established gender-based patterns are, and this is what I attempt to do in this chapter.

#### §4.1 *Gender-based variation in speech: a discussion of the principles*

Labov (1990) discusses three main principles concerning the linguistic distribution of variants between the sexes, and it seems appropriate to take each of these principles in turn as the basis of the present discussion:

Principle I (Labov 1990: 210): *For stable sociolinguistic variables, men use a higher frequency of non-standard forms than women.*

The evidence for this pattern seems to be quite widespread. For instance, the (ng) variable in words such as *hunting*, *fishing* and *shooting*, seems to show a regular stable stratification in terms of social class, and a regular differentiation between the sexes: men have a tendency to use (ng):[ɪŋ] (the non-standard variant) to a much higher degree than women do (assuming speech style and social class to be constant), with women favouring (ng):[ɪŋ], the standard form. This pattern has been shown to be the case in a variety of studies which involved a variety of speech communities (including, to name but three, N(ew) Y(ork) C(ity) (Labov 1966), Detroit (Wolfram 1969) and Norwich (Trudgill 1974)). But even this classic sociolinguistic marker might not be as straightforward as it might first appear. Amongst other things, spelling, historical and dialectal evidence (from accents in the West Midlands and north-west of England: see Giegerich (1992: 297)) suggest that words such as those cited above may have been pronounced [hʊntɪŋg] [fɪʃɪŋg] and [ʃu:tɪŋg] in a rather more widespread geographical area than is currently the case. In other words, while the (ng) variable might now appear to be a stable sociolinguistic marker in some communities, it has not always been so, and in other communities, the simple binary

division into (ng):[ɪn] as the non-standard form and (ng):[ɪŋ] as the standard one will not work. Indeed, Chambers' work in Ottawa and Toronto (as reported in Chambers (1995: 108-9) suggests that a further variant [ɪn], that is, one with a tense vowel, is a marked variant in formal M[iddle] C[lass] speech. This too would presumably need to be classed as a non-standard variant, even though it is used by a prestigious social group in a careful speech style.

But to return to the situation in present-day British varieties, it would be profitable to see the nature of the gender distribution of the variants [ɪŋ], [ɪŋg] and [ɪn] in accents where all three exist: if two non-standard variants of (ng) exist within a speech community, and if males favour non-standard forms, do the male speakers favour [ɪn] (which would seem then to function as a geographically widespread variant) or [ɪŋg], the highly localised form? The situation with (ng) is complicated further by evidence presented by Wyld (as cited in Strang (1970: 79-80)). Strang argues that the extension of /ŋ/ into unstressed syllables is "quite recent, and has spread from the middle class into general usage under the influence of spelling (or so the expression 'dropping the g', for the older pronunciation, indicates)" (Strang 1970: 79-80). Does this suggest that the historic 'standard' or 'prestige' variant was [ɪŋg]: it could not have been [ɪŋ], since auditorially the 'g' has already been 'dropped', if one assumes that 'dropping the g' does indeed mean absence of the voiced velar oral stop? But as Wyld argues (as late as 1936) the [ɪn] variant was "still widespread among the large classes of the best of speakers, no less than among the worst" (Wyld 1936: 283, as quoted in Strang 1970: 80). The notion that the [ɪn] variant was a prestige form is reinforced by the popular stereotype of the *huntin', fishin' and shootin'* set as inherently U(pper) C(lass). If Wyld's evidence is accurate, then, we cannot assume that the change involving (ng) - where one variant is perceived as stigmatised, avoided in formal speech and characteristically male in terms of gender distribution - had been stabilised before the middle of this century. This in turn

might suggest that for some of Trudgill's informants for the Norwich survey (those who were in their 80s or 90s at the time of recording in the late 1960s), (ng) might not have been a stable variable at the time of their sociolinguistic maturation (usually taken to be during adolescence (see Kerswill and Williams (1994))). A more detailed discussion of Trudgill's survey - specifically on some vocalic variables involved in linguistic change - can be found in §4.2.1: what I note here is that some speakers who formed part of his sample may well have been caught in the tail end of a linguistic change, and as we shall see below, patterns of distribution between the sexes vary considerably in circumstances of linguistic change. I am not disputing Trudgill's findings as they are presented in Trudgill (1974): what I am suggesting is that the pattern of stable stratification may not have appeared had different age groups been stratified separately (specifically, stratified separately by class). Indeed, in Chambers and Trudgill (1998: 78), the scores for the (ng) variable in Norwich are stratified by age and style: Chambers and Trudgill (1998: 79) suggest that the resulting graph displays a curvilinear pattern "typical, it seems, of linguistic variables not involved in linguistic change", whereby higher scores (i.e. more [ɪn] variants) are recorded for those at either end of the age spectrum. Chambers and Trudgill (*ibid.*) argue that the curvilinear pattern is due to (a) younger speakers being influenced by their peer group, with a weakened influence from the standard, and therefore more likely to produce vernacular variants and (b) older speakers for whom "social pressures are again less, success has already been achieved (or not, as the case may be), and social networks may be narrower" (*ibid.*). This seems ultimately unsatisfactory: the claims made about older speakers are too broad and generalised, and specifically, fail to account for the possible repercussions of the recent history of this particular variable, since the curvilinear pattern may not be the result of typical stable sociolinguistic stratification at all.



Despite this potential problem with the (ng) variable, there are nonetheless other variables which do seem to uphold the principle discussed above: (th) and (dh) in Belfast (Milroy and Milroy 1978) and NYC (Labov 1966); negative concord in NYC (Labov 1966) and Detroit (Wolfram 1969); and simplification of final consonant clusters in Detroit (Wolfram 1969). These variables have a high level of social consciousness, in that many speakers are consciously aware of 'prestige' variants; such prestige forms have a tendency to a high level of occurrence in formal styles (in all social classes); and stereotyped forms are common. In these three regards, stable sociolinguistic variables display similar characteristics with variables involved in change from above, defined by Labov as follows (Labov 1994: 78):

*Changes from above* are introduced by the dominant social class, often with full public awareness. Normally, they represent borrowings from other speech communities that have higher prestige in the view of the dominant class. Such borrowings do not immediately affect the vernacular patterns of the dominant class or other social classes, but appear primarily in careful speech, reflecting a superposed dialect learned after the vernacular is acquired.

What differentiates variables undergoing change from above from stable sociolinguistic variables can crucially be explained in terms of stylistic variation: the former will be subject to hypercorrection<sup>1</sup> (predominantly by L(ower) M(iddle) C(lass) speakers, and predominantly *female* LMC speakers) in more formal styles, while the latter do not display such a pattern; thus hypercorrection is crucial as a marker of change from above. However it is clear that stable sociolinguistic variables and those involved in change from above do share a number of properties; on these grounds, Labov suggests that a second principle of gender-based variation, should be labelled IA:

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<sup>1</sup> Hypercorrection occurs when speakers display a marked effort to avoid socially stigmatised forms, to the extent that they will often, in formal speech styles, use a greater frequency of incoming variants than the social class above them. Evidence to suggest that (ng) is not a universally stable variable in Toronto English might be drawn from the hypercorrect behaviour of the LMC who favour the [in] variant (with a tense vowel): see Chambers (1995: 108), as noted above.

Principle IA (Labov 1990:213): *In change from above, women favour the incoming prestige form more than men*

In other words, Labov claims that men favour non-prestige forms and women favour prestige forms in cases where a variable is stable *and* in cases where a variable is involved in change from above. We will consider the evidence for Principle IA before commenting on arguments relating to *why* women and men behave in these ways.

Labov suggests that evidence for Principle IA comes from a variety of sources. For instance, in the case of (r) in NYC (Labov 1966), there is evidence to suggest that women are leading in the change (where [r] is being re-introduced into syllabic rhymes in that speech community). More specifically though, it seems fair to suggest that it is the women in the LMC who are leading in this change: in this respect (as the Labov 1990 article insists) what is crucial is the *intersection* of gender and social class; how these two speaker variables interrelate is what needs to be taken into account. But for this to be meaningful, the genders have to be equally represented in the differing classes: Nichols (1998: 57) points out that in the NYC survey, L(ower) W(orking) C(lass) women outnumbered men by the ratio of two to one, while in the UMC men outnumbered women by the same ratio. This clearly suggests a degree of bias in the class/gender cells. Nonetheless, other variables (such as (ɛ) in Belfast, on which see §4.2.2, and the raising of final /a/ to /o/ in the variety of Spanish spoken in Uceda, as reported by Labov (1990: 214), do seem to show the pattern established under Principle IA. Accepting for the moment that these principles are accurate, a natural question to consider is why the sexes should pattern in this way.

Answers to this question, as Labov (1990: 214) notes, have tended to focus on women's speech, rather than men's speech. It has been suggested that, since women possess less material power than men, they use more expressive symbols than men to assert their position. One such expressive symbol is the use of standard language. But this seems to me to be potentially problematic, particularly since what is perceived as 'standard' or 'prestigious' is based on class norms (i.e. the speech associated with the MC) rather than gender norms (i.e. the speech associated with women). I shall have more to say about this later in this chapter; for now, it is sufficient to point out the following anomaly. If it is assumed that (a) men are relatively 'more powerful' materially than women and (b) men use more non-standard norms, surely women should, in their attempts to show an association with the more powerful social group, accommodate to their speech patterns; in other words, use more *non*-standard forms. In this status-based explanation, women's patterns of speech divergence would indicate a willingness to be *dis*associated with the more materially powerful social group. There are also many other weaknesses associated with the status model: it assumes that all women share the same goals (James 1996: 106); and it fails to account for women's marginalization in the vernacular market as well as the standard language market, where it is claimed that stereotypically female jobs involving cleaning, cooking and sewing are less highly valued than (equally stereotypical) male jobs such as those of mechanics, electricians and plumbers (Eckert 1998: 68).

Attempts to explain the behaviour of men with regard to Principles I and IA have suggested that such patterns are derivable only by means of inference (Labov 1990: 214): that men are influenced by covert norms, and respond to covert prestige. This theory has been put forward by Trudgill (1972), and is clearly linked to the status explanation briefly discussed above: men are drawn to behaviour associated with the vernacular, since the vernacular is associated with the working class, and the

working class is associated with ‘toughness’, a positive characteristic for men. Evidence to support this claim is provided by Trudgill through an analysis of under- and over-reporting between the sexes in his Norwich survey. A speaker is said to under-report if he or she claims to use non-standard variant X, while actually using standard variant Y; a speaker is said to over-report if he or she claims to use standard variant Y, but actually uses non-standard variant X. The tables below, taken from Trudgill (1998: 26), which is an edited reprint from his 1983 book *On dialect*, show the scores for over- and under-reporting of (er) in *ear, here* and (o:), in *moan, nose*. For (er) the standard variant is [ɪə] while the non-standard variant is [ɛ:]; for (o:), the standard variant is [əʊ] while the non-standard variants are [ʊ ~ u:]:

Percentage of informants over- and under-reporting (er)

	<i>Total</i>	<i>Male</i>	<i>Female</i>
over-reporting	43	22	68
under-reporting	33	50	14
accurate	23	28	18

Percentage of informants over- and under-reporting (o:)

	<i>Total</i>	<i>Male</i>	<i>Female</i>
over-reporting	18	12	25
under-reporting	36	54	18
accurate	45	34	57

Trudgill (1998: 27) comments on these data as follows: “there are more male speakers who claim to use a less prestigious variant than they actually do than there are who over-report, and for one of these variables [(o:)] the difference is very

striking: 54 per cent to 12 per cent". But only half of the men under-reported for (er): this in turn suggests that the other half were either right in their self-analysis, or over-reported. Similarly, only a quarter of the women over-reported for (o:). Without wanting to labour the point about this, it seems to me that what Trudgill's data show is the following: 'if an informant over-reports, that informant is significantly likely to be female; if an informant under-reports, that informant is significantly likely to be male'. What it does *not* consistently show (and this is what Trudgill 1998: 27 claims) is the following: 'if an informant is female, that informant is significantly likely to over-report; if an informant is male, that informant is significantly likely to under-report'.

More exceptions have been found to Labov's Principle II, which I give below:

Principle II (Labov 1990:215): *In change from below, women are most often the innovators*

Changes from below are defined as follows (Labov 1994: 78):

*Changes from below* are systematic changes that appear first in the vernacular, and represent the operation of internal, linguistic factors. At the outset, and through most of their development, they are completely below the level of social awareness. No one notices them or talks about them, and even phonetically trained observers may be quite unconscious of them for many years. It is only when the changes are nearing completion that members of the community become aware of them. Changes from below may be introduced by any social class, although no cases have been recorded in which the highest status group acts as the innovating group.

Labov (1990: 216) cites a number of cases in which women seem to be in the vanguard of such changes. In his own NYC survey, he noticed that women were using more innovative variants than men were in many cases, including the fronting of the first element of (aw), the backing of (ah), and the raising of (æh) and (oh); in a major phonological change affecting the northern cities in the United States, such as

Buffalo, Chicago and Detroit, in which there appears to be a “rotation” (Wolfram and Schilling-Estes 1998: 138) involving the low long vowels (oh) and (æh) and short vowels (o), (e) and (ʌ) - the Northern Cities Shift - women seem to be leading in some respects: while they are markedly innovative in the use of fronted variants of (æh), (o) and (oh), Eckert (1989) argues that girls are not using the newer variants of (e) and (ʌ) (on which see further below, §4.1.2); and in a change to the allophonic distribution of /aw/ (where traditionally [əʊ] occurred before voiceless consonants, and [aʊ] elsewhere) in Toronto and Vancouver - known as Canadian Raising (see further §4.3.5) - women are innovating by using [aʊ] in all positions to a greater extent than men are.

There are, as noted above, some clear exceptions to Principle II. I discuss two in detail in §4.2 - the unrounding of (o) in Norwich and the backing of /a/ in Belfast. Labov (1990: 218) also mentions the case of (ay) in Martha’s Vineyard. In this study, reported in Labov (1978, chapter 1), men are in the vanguard of change, in that they use a centralised variant of (ay) - [əɪ] - more regularly than women do. Labov demonstrates that such a change is indeed an innovation (rather than a retention of an early Modern English variant) and that such variants characterise not only men, but more specifically, those speakers who value the traditional island way of life, and who object to the increasing incursion of visitors from the mainland who are seen to disrupt - and corrupt - Vineyard ways. But more importantly, in terms of language change and effect on the system, this change is minor. It is not included in a chain shift (of the type associated with the northern cities, mentioned above); it is an isolated, local change. The Vineyard men are using a marked local variant, which is distinctive of that community: in §4.3.2, another study is discussed which seems to indicate that men do indeed lead in these highly localised changes, which do not have a widespread geographical distribution, and consequently rarely have an effect on the linguistic system as a whole.



Labov's attempt to explain why women should lead in these changes from below (Labov 1990: 219) concerns the role of the woman in the provision of child care: "children learn the rudiments of their native language from their primary caregivers, who are women". But again this makes very broad assumptions, and does not really explain the exceptions to Principle II (and Labov himself notes that there are more exceptions to Principle II than to either I or IA), some of which have been noted above. It is interesting to speculate on the likelihood that such widespread changes to language systems stem from "the fact that the first steps in language learning are dominated by women" (Labov 1990: 219), but far more convincing arguments come from analyses which have attempted to consider gender-based variation within a wider social spectrum: where language change is seen to be a product of a variety of extralinguistic factors. It is to the 'intersection' of gender with other social variables that the discussion now turns. In the next two sections, I look at the intersection of gender and class, and gender and social category affiliation. Equally important is the intersection of gender and social network: but this is discussed in detail in §4.2.

#### §4.1.1 *Gender and social class*

The linguistic behaviour of women and men is not always constant when social classes are analysed in isolation. There is a significant body of evidence which suggests that the patterns which emerge from the second highest status group (normally the LMC) show a clear differentiation in the variants used by men on the one hand and the variants used by women on the other; Labov (1990: 222) has argued that it is significant that it is this social class which shows the highest degree of linguistic insecurity (and tends toward noticeable hypercorrection) and similarly a marked tendency to style shift. It follows, then, that if women and men are not showing consistent patterns irrespective of social class, there can be no biological



property of men or of women which determines such a pattern: we must look to a social (or gender) based explanation, not a sex-based one (on which see further §4.3.1 below). Labov (1990: 224) goes on to detail a finding concerning the (oh) variable in NYC, in an attempt to discover whether the hypercorrect behaviour of the LMC is due solely to the women in that social class. Innovative variants of (oh), such as [ʊ:°] (which had an index score of 10), in *lost* and *coffee*, for instance, were characteristic of younger speakers in less formal speech; older speakers tended toward the more conservative [ɔ:°] (score = 30); but when the women's and men's scores for the LMC were analysed separately, it was shown that the pattern of hypercorrection was really only a characteristic of women's speech: they had scores ranging from 17 in C(asual) S(tyle) to 31 in W(ord) L(ist) S(tyle), while men's scores ranged from 20 (CS) to 26 (WLS). Evidence from the other social classes suggests that differences in male and female speech do exist across the class spectrum; these differences are most marked, though, in the LMC.

As an instance of the intersection of sex and social class in a change from below, Labov cites current variation in the vowel system of Philadelphia (Labov 1990: 227ff). One of the newest changes concerns the quality of the first element of the diphthong (ey) in words like *wait* and *reign*. Historic evidence suggests there is a trend towards dissimilation with this diphthong: the two elements are becoming less and less similar phonetically, moving from General American [eɪ] towards [aɪ]<sup>2</sup>. But what is innovative in Philadelphia is that this historic trend is being reversed in checked environments (that is, where the vowel is followed by one or more consonants in the syllabic coda). In terms of co-variation with social variables, this new change is showing a very interesting pattern: there seems to be no difference between the sexes, and this is true of speakers in the so-called 'unskilled' class as well as those in the clerical, managerial and professional groups. This contrasts

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<sup>2</sup> This pattern can also be seen in Cockney English as opposed to RP: *David* is [daɪvɪd] in Cockney, [dɛɪvɪd] in RP.

strongly with older changes (such as the height of the vowel in words like *ham*, *pass* and *bad*) in which frequently (though not exclusively) women show a clear preference for innovative variants. Labov suggests, then, that there is no intersection of class and sex at the actuation of a sound change; but as a sound change develops, men and women use increasingly different variants, with women tending to favour the new forms. At the point at which such innovative forms become salient - and in those cases where once salient, the new variants become stigmatised and rejected by the wider community, concomitant with an increasingly sharp class stratification - the rejection of the new forms is not just a characteristic of women; men reject certain changes in the mechanism of their language, but they do so to a lesser degree than women do. When the roles of the two sexes are considered in relation to language change, then, what is really being considered is the embedding and the transmission of an innovation, rather than the source of an innovation itself.

This analysis attempts to understand the motivation for change as being related to both class differences and gender differences; but in many other instances, there is clearly a bias towards an explanation in terms of social class: "linguistic change is in general led by the working class" (Labov 1990: 227). Eckert (1989) has argued that one of the reasons that our understanding of the relationship between linguistic variation and social class is so developed is because that is where the main focus of earlier sociolinguistic investigation lay. The key problem (as noted by Milroy and Milroy (1993: 58)) is that researchers have tended to assume that class is primary, and that gender differences (as well as other differences, such as age and ethnicity) are to be explained by reference - and in relation - to that. It is clear, however, that some variables which have previously been analysed as class markers (such as (dh) in NYC) are actually more stratified by gender (as Fasold (1990: 101) shows). It was generally accepted that the speech community, so crucial to the Labovian method of analysis, "emphasises shared values throughout the community

where speakers are said to agree on the evaluation of these very linguistic norms which symbolise the divisions between them” (Milroy and Milroy (1993: 59)), but many communities display patterns of conflict and sharp division rather than consensus; it is this model of conflict which Milroy and Milroy consider to be crucial in an account of linguistic change (on which see further §4.2, §4.3.2).

#### §4.1.2 *Gender and social category affiliation*

The relationship between gender and social category affiliation is primarily explored in Eckert (1988, 1989) in her study of adolescents in Detroit and Cheshire (1982) in her study of adolescents in Reading. Eckert’s study involved a number of years of participant observation of high school students who fell into three social categories: the Jocks, who were students who typically excelled at sports and/or achieved high grades in academic work, but who primarily endorsed the corporate norms of the school, and whose networks were hierarchical in structure and were competitive; the Burnouts, who were students whose loyalties lay outwith the school, who adhered most strongly to the metropolitan environment, and whose networks were egalitarian in structure; and the In-betweens, who, as their name implies, either associated with neither of the other two social categories or shared social characteristics of both. The phonological changes investigated were part of the Northern Cities Shift, noted in §4.1. This chain shift involves the fronting of the low back vowel, raising of the low front vowel, and backing and lowering of the mid vowels. One of the newer changes in this shift concerns (uh) with variants [ʌ] (conservative), [ɔ] and [ɑ] (innovative), and very rarely [ʊ], another innovative variant: this variable showed interesting patterns of gender and social category affiliation in Detroit.

In attempting a correlation of (uh) scores with social class<sup>3</sup>, no clear patterns emerged: there was no clear preference for MC speakers to use conservative forms or for WC speakers to use innovative forms, or vice versa; neither was there any clear gender-based pattern: indeed both the lowest (most conservative) *and* the highest (most innovative) scores came from UMC girls. But with the correlation of (uh) scores and social category affiliation came a much clearer pattern. Burnout scores were generally higher (suggesting that this change might have originated with speakers in the urban centre of Detroit, with whom the Burnouts contracted a number of network ties); but yet again, within each social category, both the highest and lowest scores came from girls. This contrasts with other changes in the Northern Cities Shift - such as those involving (a) and (oh) - in which the girls are shown to be in advance of the boys, in each of the social categories investigated. Eckert (1989: 261-4) suggests an explanation for this pattern in terms of the age of the changes involved. The variables which show the most marked differentiation between the genders are the older, less vital, ones, such as (a) and (oh). In the newest changes, such as those involving (uh), social category differences are more marked, with Burnouts using newer variants than Jocks or In-betweens. In both sets of changes, girls are using a greater range of variants than the boys are. In the newest changes, the pattern of variation with the girls shows a greater differentiation between Jocks and Burnouts than does that with the boys; and in the oldest changes, the girls make far greater use of variables not associated with social category affiliation. These results would seem to question the claim that women lead in linguistic change: in the most recent changes affecting the Detroit vowel system, the innovators are not a particular sex, but rather a particular social category; but this is not to suggest that gender is not important in terms of an explanation of the development of such changes; indeed gender and social category interact in a significant way: “category

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<sup>3</sup> The social class of the students' *parents* was established, and it was with this that the correlation was attempted.

membership is more salient to members of one sex than the other; girls are asserting their category identities through language more than are boys” (Eckert 1989: 265).

Cheshire’s (1982) survey of Reading adolescents focused on grammatical variation in WC speech. It did not directly address the issue of linguistic change, and as a result, will not be discussed at length. Nonetheless, it showed some revealing patterns of the relationship between gender-based variation and social category membership that may prove illuminating. The study investigated speech patterns in three gangs, two male and one female, at two adventure playgrounds in a WC area of Reading. Membership of the peer group was seen to be important to many of the informants, but the structure of the boys’ groups was distinct from that of the girls’. One of the boys’ groups, at Orts Road, showed clear divisions into core, secondary and non-members; yet not all of the linguistic variables marked this peer group status. For instance, consider the percentage scores for *ain’t* as a variant of negative auxiliary *have* (as in *I ain’t got one single flea in my hair, they’re all married* (Rob)), negative auxiliary *be* as a marker of progressive aspect or passive voice (as in *How come that ain’t working?* (Benny)), and negative intensive or copular *be* (as in *You ain’t no boss* (Rob)) given in the table below (Cheshire 1982: 91):

<i>Variant</i>	<i>Core</i>	<i>Secondary</i>	<i>Peripheral</i>
<i>ain’t (aux have)</i>	80.85	66.67	100.00
<i>ain’t (copular be)</i>	92.31	77.77	86.36
<i>ain’t (aux be)</i>	64.86	75.00	100.00

The boys’ scores here are not showing any regular pattern of use of non-standard variants correlating with social category affiliation: indeed, for each of the functions of *have*, a different pattern emerged, so that it is clear that not all non-standard forms mark peer group status. When Cheshire attempted to correlate the

linguistic scores with another social variable, the vernacular culture index (calculated using indicators such as weapon carrying, criminal activities, and skill at fighting), a similar pattern emerged: while some variables (such as the non-standard morphological structure of present tense verbs) showed a correlation with vernacular culture, others (such as *ain't* for copular *be*) did not. While the girls' socialisation patterns were clearly different - they did not attach the same sort of prestige to criminal activities as the boys did - some girls adhered more to vernacular culture than did others; this broad division into 'good' and 'bad' girls yet again failed to show the stereotyped pattern of 'bad' girls using non-standard forms more frequently than 'good girls': while *ain't* as a variant of copular *be* was a marker for the 'bad' girls, there was little to distinguish 'good' from 'bad' girls in the use of non-standard *never* (where *never* means 'not on one particular occasion', in addition to standard English 'not on any occasion') (Cheshire 1982: 108). In other words, some features of non-standard English function as markers of vernacular loyalty for girls, some for boys and some for both; other non-standard features function solely as sex markers; and some function as both markers of vernacular loyalty *and* sex.

Social network is clearly associated with social category affiliation. For instance, the nature of the networks contracted by the Jocks, Burnouts and In-betweens differed markedly in terms of their structure: Jock networks are dense, multiplex, hierarchical and suburban; Burnout networks are dense, multiplex, egalitarian yet widespread (in that Burnouts contracted network ties in the inner city); and In-between networks tend to sparseness and uniplexity. And just as gender enters into a relationship with the phenomenon of social category membership, so it enters into a relationship with social network structure: and the details of this are discussed in §4.2.2.



## §4.2 *The 'traditional' approach*

In this section, I discuss some of the findings from two early sociolinguistic studies into British English: the work of Peter Trudgill in Norwich (Trudgill 1974), and the work of James and Lesley Milroy in Belfast (Milroy. (1992), Milroy (1987b), amongst others). The review of these studies is by no means exhaustive. It does not comment on many of the sociolinguistic patterns that emerged from the data, and restricts discussion to a couple of variables for each study. This is because the purpose of the review is to present some of the findings of early studies which correlated gender with linguistic variation, thereby allowing for potential comparison with newer approaches to be discussed later.

### §4.2.1 *Norwich*

The Norwich survey followed the Labovian model of the NYC survey (Labov 1966): informants were gathered by means of a quasi-random sample, and interviewed, asked to read a passage, word lists and a list of minimal pairs. The informants were then stratified by social class, sex and age. There were some clear problems with the stratification. Having decided on a class index based on a variety of social factors, including education, housing and employment, and having scored the informants for each of these factors, Trudgill still needed to establish a correlation between score and class: what range of scores would classify the M[iddle] M[iddle] C[lass], for instance? In order to establish where such cut-off points should lie - that is, why scores that marked speakers as MMC should be 19 or above, and not, for instance, 25 - Trudgill used results from an experiment involving morphological variation in Norwich English. A characteristic of this variety is that many Norwich speakers do not have the {S} morpheme as a marker of III singular



present tense on (non-modal) verbs, so that *he like* is well-formed in this variety. The informants were scored for their use of this dialectal feature. Anyone who had a score of 80% or above (that is, who used the non-standard variant in at least four of every five possible occurrences) was classified as L[ower] W[orking] C[lass], and this happened to correlate with speakers whose social class index was between 3 and 6. Therefore, the LWC were speakers whose social class scores were between 3 and 6, and the rest of the class divisions were made on a similar basis. There is therefore a potential problem of circularity, since as Wardhaugh (1992: 147) argues, Trudgill “uses linguistic behaviour to assign membership to social class”: in other words, Trudgill uses linguistic data to classify certain social groups, which will then be used to comment on linguistic phenomena relating to the sociolinguistic distribution of variants in Norwich English.

Many of the vocalic variables selected for analysis by Trudgill seemed to be undergoing change at the time of his survey. One such variable was (o), the vowel in *top*, *hot* and *box*, with variants (o)-1: [ɒ] and (o)-2: [ɑ ~ ä ~ ä]. The first variant, [ɒ] is the vowel found in RP for these words, the second set, which I group collectively under [ɑ], a variant similar to the General American vowel in this lexical set, are local, non-standard variants. This variable showed strong stylistic variation, and Trudgill’s (1974: 88) initial hypothesis was that unrounding “appears to be a feature only of Norfolk speech in East Anglia”. Furthermore, the variable was “not the subject of any overt comment in Norwich” (Chambers and Trudgill 1998: 73), which might indicate a change from below nearing completion. But if this was the case, and the change involving (o) was indeed nearing completion, we would expect that the innovative variants would be salient, stigmatised and avoided by the MC. However this social class are readily adopting the variant. So perhaps this is a change from above. But this is still problematic: there is no evidence of hypercorrection from the LMC (probably because the UMC use (o)-1 nearly 100% of

the time in all speech styles, which would presumably make hypercorrection impossible), and as was just noted, the variants do not seem to be salient, which is unusual for a change from above. This seems to make (o) a rather unique sociolinguistic variable.

To adequately explain the patterns with (o), it is useful to consider the following data (Chambers and Trudgill 1998: 84) on male and female scores for (o) by class in formal style: a score of 0 indicates consistent use of (o)-1, while a score of 100 indicates consistent use of (o)-2:

	<i>Male</i>	<i>Female</i>
MMC	1	0
LMC	11	1
UWC	44	68
MWC	64	71
LWC	80	83

While the MC scores seem to pattern in the ‘traditional’ way, with men favouring the Norwich vernacular form and women the RP prestige form, the WC scores seem to reverse this pattern, with the most marked difference being found at the heart of the social hierarchy, in the UWC. However, regional dialectal evidence is useful in explaining such an apparently aberrant set of statistics: the neighbouring county of Suffolk has a variant of (o), namely [ɒ], which is phonetically identical to the RP form<sup>4</sup>. So while MC women seem to be adopting the RP variant, WC men seem to be adopting the same variant, but not because it is the nationally prestigious form. The WC men seem to adopt [ɒ] through the geographical diffusion of a linguistic innovation from a neighbouring *low-status* variety. In other words, even though the

<sup>4</sup> That Suffolk variants are diffusing into Norfolk (and specifically to Norwich) is also in evidence with the (a) variable, the vowel in *bat, bag, carry*.

variants are identical, it seems as if the MC women are innovating by their use of a supra-local (here nationally prestigious) form, while the WC men are innovating by their use of a local (and perhaps - though this is not explored in the literature - a locally prestigious) form, despite the fact that on first reading, it seems as if the WC women are the group most likely to use a vernacular variant. The importance of local and supra-local forms is considered in more detail in §4.3.2.

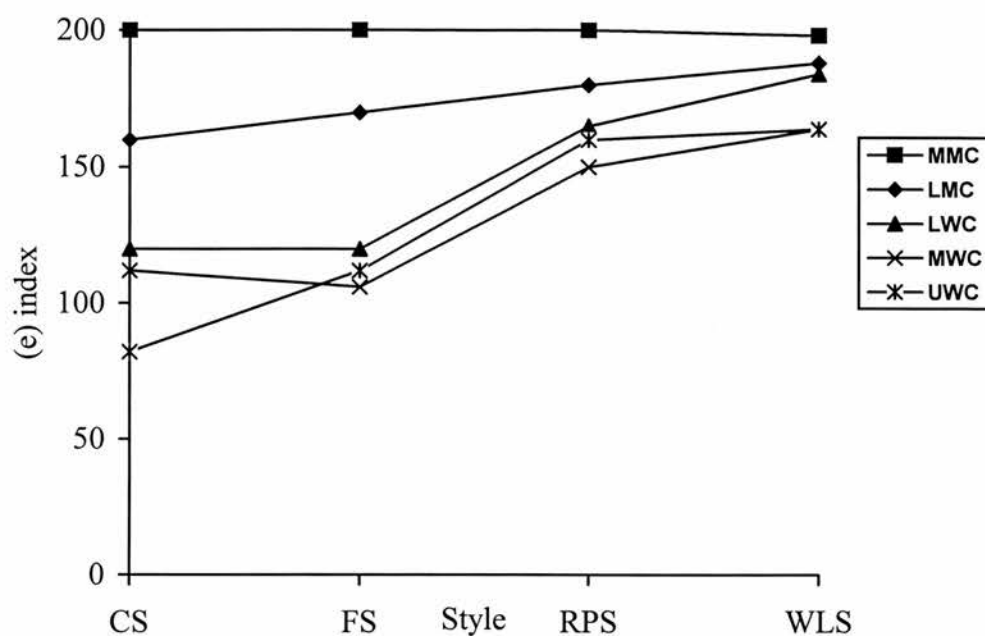
A similar pattern was discernible for the (e) variable in Norwich. There were two specific environments in which this vowel was undergoing change in Norwich English (see Trudgill 1974: 87). The first was when the vowel was followed by the lateral /l/ as in *tell*, *bell* or *wealthy*; the second was in the following environment, where the vowel is stressed: [bilabial C] \_\_ [ʔ] [V -stress], as in *better*, *metal* or *betting*. The variants of this variable were as follows:

(e)-1: [e ~ ε]

(e)-2: [ĕ ~ æ ~ ɜ]

(e)-3: [ɐ ~ ʌ ~ ʌ]

A linguistic change was again noted: centralization of the vowel was becoming more and more noticeable in the Norwich speech community, and this was a marked innovation. Regional dialect surveys had noted some opening to [æ] in Norfolk, and backing to [ʌ] in Suffolk, but marked shifting to [ʌ] had not previously been considered a pattern in Norwich English. Trudgill (1974: 104-5) notes that high (e)-scores, indicative of centralisation, the new variant, was particularly marked for the under 30 year olds, and the adolescents and young adults (aged 10 to 19) in particular. Especially noteworthy were the scores for the youngest males, whose average score was the maximum 200 (i.e. consistent use of the centralised form). This gender pattern, as noted by Coates (1993: 174-5), helps to explicate the seemingly unusual class stratification:



Firstly, in casual style the UWC is using a greater number of the incoming variants than the MWC and the LWC. This supports the view that change from below is led by those not at the periphery of the social spectrum but at its heart. But there are some oddities when it comes to looking at the style stratification within the working class groups, since in all styles, the LWC has a higher score than the MWC and the UWC. Such irregular style stratification is further evidence of a change from below: but only when the scores are analysed by sex of speaker can we clarify which social group in particular is crucial in the propagation of the change. The evidence from gender-based co-variation with linguistic variants suggests that the change is being spearheaded by young UWC males.

In terms of the Labovian principles discussed above, then, it seems that the Norwich evidence might indicate the need for some revision. With (o), two distinctive changes were seen to be taking place: a change from above, led by MC females, using a nationally prestige variant, which was phonetically identical to the innovative variant of the second change, a change from below. This second change was related to the diffusion of a local pronunciation (in WC Suffolk speech), and was led by WC males. UWC young males were also seen to be instrumental in the

transmission and propagation of the innovative centralised variants of (e), another change from below. These examples, then, run counter to Labov's Principle II, though as Labov himself notes, there are more instances of exceptions to this principle than to the others.

#### §4.2.2 *Belfast*

Whether the Milroys' study of the vernacular of speakers from Belfast can be considered traditional is a moot point, but the purpose of the present review is to examine what evidence the Belfast study provides for research into gender-based linguistic variation by analysing the sociolinguistic patterns discernible from two specific variables.

There is much to be said about the general principles of the social network, both as a method of speaker sampling ("a quantitative speaker variable" (Milroy 1992: 84) and as a tool for the analysis of linguistic variation ("an interpretative category" (*ibid.*)). But the discussion here is restricted to the most central ideas, and does not explore the notion of network in great detail. Firstly, the concept of the social network is one which focuses on micro-level social relationships, which does "not need to accept any prior assumption about how society at large is structured" (*ibid.*). In this sense, social network analysis is independent of social class. But more specifically, the model of social networks proposed by the Milroys does not reject the importance of class out of hand: indeed, Milroy and Milroy (1992) is an attempt to find some ways of integrating the two models of the structure of society. The Milroyan analysis seeks to reconsider Labov's notion of a speech community based on the idea of a consensus of norms operative across a broad social spectrum. By contrast, the network model highlights the conflict and division which characterises many of the societies into which sociolinguistic research has been undertaken.

Central to the concept of social network is the fact that speakers have a range of social contacts with other speakers, contacts which are not dependent on pre-defined social concepts of class or ethnicity (though these may be factors in the *nature* of those contacts). It has been especially noticed that dense and multiplex networks function as norm-enforcement mechanisms. Relative density and multiplexity are terms used to describe certain characteristics of social networks. Density refers to the number of ties within a network; multiplexity refers to the nature of those ties. A dense network is one in which all members of the network know one another; a sparse network is one in which the centre of the network (known in the literature as “ego”) knows everyone else, but the other members do not know one another. A multiplex network is one in which members of the network are linked to one another in a number of ways (kinsmen, friends, workmates). This is discussed further below. As a norm-enforcement mechanism, dense and multiplex networks have been seen to be highly influential in the preservation of non-standard dialects, despite widespread standardisation in the recent history of English.

This then leads to a discussion of the roles of weak ties within the network model. Milroy and Milroy (1993: 66-7) have argued that “where ties are relatively loose-knit, communities will be susceptible to change originating from outside localized networks”. Such change, when it is related to language, has been both away from and towards the standard variety. The closer the ties of a speaker to the local community network, the greater the likelihood of that speaker using vernacular variants, promoting the continuation of conservative, often non-standard variants; conversely, weak ties, where the social bonds between two points in a network are not numerous, promote innovations and new influence, often from the standard variety, but as noted above, not always from this standard. The notion of network, then, is an aid to our understanding of how innovative variants are transmitted from one group of speakers to another: notice that the term ‘group of speakers’ is



deliberately vague, given the vast number of social links contracted by many speakers on a day-to-day basis.

Nonetheless, even here we must consider some aspects of the Milroys' method, as it has been claimed (Coates and Cameron 1989) that the calculation of an informant's N[etwork] S[trength] S[core] can be seen to be androcentric. The following criteria were used to establish the NSS of each of the Milroys' informants: the first was used to calculate the density of the informant's network; the remainder to calculate its multiplexity.

1. Membership of a high-density, territorially based cluster.
2. Having substantial ties of kinship in the neighbourhood. (More than one household, in addition to his own nuclear family.)
3. Working at the same place as at least two others from the same area.
4. The same place of work as at least two others of the same sex from the same area.
5. Voluntary association with workmates in leisure hours. This applies in practice only when conditions three and four are satisfied.

The main criticism voiced against these criteria is that they are based around male socialisation patterns: they list activities that would signify a dense and multiplex social network *for men*, and a different set of criteria might be more applicable for women. By using the same criteria for men and women, the Milroys have used male socialisation patterns as the yardstick by which to measure the relative density and multiplexity of their female informants, which, it is claimed, will lead to bias in the results.

But this critique of the calculation of NSS seems to miss the point entirely. What is of crucial importance in the Belfast study is the changing gender roles in the various communities under investigation. For instance, the socioeconomic climate in Ballymacarrett, a Protestant community in east Belfast had not changed as drastically



as it had in the Clonard, a Catholic community in the west of the city. In the former community, unemployment, although still a problem, had not forced the male residents to find work elsewhere in Belfast: it was much more common for the men to go out to work and the women to stay at home, thus maintaining the traditional gender roles. In the Clonard community, massive unemployment had not only meant that the men might have to leave the local area to find work (thus weakening their own networks) but that women (particularly young women) had also left the home to find work of their own (thus realigning the gender roles traditionally associated with working class communities). The calculation of NSS then is a neat way of *avoiding* bias in the results, since the creation of a new set of criteria for women may not have been applicable for all women in the survey: certainly, the application of such a new set of criteria to both the Ballymacarrett and Clonard women's social networks would have been highly inappropriate, given their very different social roles in the communities in which they lived and worked. Recent work (cf. James 1996: 119 and references therein) in language and gender has highlighted the importance of 'looking locally' - attempting to understand the roles played by men and women at a small scale community level, rather than (erroneously) assuming that the same behaviour patterns should be applied to all men and all women everywhere. This can be achieved by adopting methods akin to those employed by the Milroys in Belfast.

These social roles are the key to explaining some of the crucial findings of the Milroys' survey, which I summarise here (for further detail, see Milroy (1987a, 1987b), Milroy and Milroy (1985)) for two of the variables investigated: (a) and (ε)<sup>5</sup>. These two variables showed significant results when correlated with both social network and gender. Briefly, backed variants of (a), i.e. [ɑ] and [ɔ], the latter often with an in-glide, are associated with the Belfast vernacular, and were shown to be the

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<sup>5</sup> There is clearly much that can be said about the linguistic distribution of variants: for instance, pre-velar /a/ does not undergo backing in the Belfast vernacular, while /a/ before non-velars is never raised. These are important aspects of the Belfast survey, but since the present discussion focuses on gender-based variation, they will not be discussed in detail here.

target variants for the Ballymacarrett men. Raised and lengthened variants of (ɛ), ie [ɛ·ə], in monosyllables particularly, were associated with out-of-town speakers and targeted by some of the women. This, however, does not do justice to the complexities of the correlations with network and gender. For instance, the (a) variable functioned primarily as a network marker for women; “among women a relatively large amount of /a/ backing is likely to be associated with a high level of integration into the network than is the case among men” (Milroy and Milroy 1985: 360), so that women with a high proportion of [ɑ] and [ɔ] variants were much more likely to contract dense and multiplex network links. This is particularly true of the young Clonard women, whose NSS was 4.75 (out of a maximum possible 5). That these women were part of a dense and multiplex network is crucial to our understanding of how the change (which is referred to in Milroy and Milroy (1985) as *Backing of /a/*) spread through Belfast. For these women, while maintaining dense and multiplex network links among themselves, also contracted weak links with those who were in contact with the Protestant males from Ballymacarrett in east Belfast, who were shown to be the innovators of the change *Backing of /a/*, since they had the highest index score for (a). Further evidence to support the claim that the young Ballymacarrett males are innovators comes from data that suggests it is this social group which uses backed variants of (a) in a wider range of phonetic environments than many other speakers. Backed variants are favoured in the Belfast community as a whole generally when the vowel is followed by a fricative or non-velar voiced (oral or nasal) stop. Ballymacarrett males additionally favour the backed variants in voiceless stop environments in words such as *tap* and *that* (see Milroy 1992: 115), and a more extensive phonetic diffusion of incoming variants is characteristic of innovative groups. But to return to the transmission of new forms from one community to another, weak ties - along which transmissions spread rapidly - arose from the contact the young Clonard women had with East Belfast speakers at

their place of work, on the sectarian interface between East and West Belfast, and it may be surmised, “given the number of service encounters in the shop in any one day, that weak-tie encounters with back [a] (*sic*) users who transmit the innovation will greatly exceed in number strong-tie encounters with non-back [a] users” (Milroy and Milroy 1985: 373). Hence, while the young Clonard women are not the innovators, they are certainly the early adopters, whose role is crucial in the diffusion of linguistic changes. Note that backed variants of (a) are stigmatised in Belfast speech. I will return to this later in the chapter.

In contrast, the raised and lengthened variants of (ε) are seen to have *overt* prestige in Belfast speech, being primarily associated with out-of-town speakers. Indeed, the (ε) variable generally shows a converse pattern to that of the (a) variable. While the change *Backing of /a/* is led by (young) men (in Ballymacarrett) and backed variants of (a) are correlated with dense and multiplex networks for women, the change *Raising of /ε/* is led by (young) women (in Ballymacarrett) and low variants of (ε), namely [a] and [æ], are correlated with dense and multiplex networks for men.<sup>6</sup> In their summary of the historical, geographical and sociolinguistic evidence regarding the change *Raising of /ε/*, Milroy and Milroy (1985: 354-5) argue that

the low realizations of /ε/ (conservative English in background) are giving way in a linguistically ordered way to the long mid realizations characteristic of present-day Scots. It is clear that this change carries prestige in Belfast in terms of social class hierarchy and status, as it is the more prestigious groups that tend to adopt it and the more ‘advanced’ (generally female and younger) group who introduce it to the conservative inner-city communities (which are characterized by dense and multiplex network ties that tend to resist innovation and maintain conservative forms).

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<sup>6</sup>Some of the variants of both the variables discussed here are favoured in certain linguistic environments: for instance, in the Belfast vernacular, it is common for /a/ to be realised as [ε:] before velars, so *bag* [bε:g], *bang* [bε:ŋ]. The account given here has not gone into any detail regarding this, since the purpose of the summary was to look at the relationship between gender roles in Belfast and the changing vernacular there. Further detail on the vowel system in Belfast can be found in Milroy (1981) and Milroy and Milroy (1985).

It would seem, then, that the two changes *Backing of /a/* and *Raising of /ε/* are both linked to gender and social network structure, but in quite different ways. *Backing of /a/* concerns the spread of low-status variants (i.e. those associated with the vernacular as opposed to the standard) from one inner-city Belfast community to another, across the sectarian divide (from the Protestant east to the Catholic west), while *Raising of /ε/* concerns the infiltration of high-status variants from outside the city into the Belfast vernacular. Crucial in both of these changes are the young females, those from Clonard with regard to *Backing of /a/* and those from Ballymacarrett with regard to *Raising of /ε/*. From this it would seem that women are central in changes involving both overt and covert prestige, and the relationship between women's language and prestige forms is a major part of the following sections. Nonetheless, it is important to be aware of the fact that the nature of the diffusion of innovative variants of (a) is different from that of (ε). Backed variants of (a) are solely associated predominantly with vernacular speech. A very gradual social diffusion is taking place, with some MC males in out-of-town communities showing a degree of backing with a realisation at [ä]. By contrast, the diffusion of innovative variants of (ε) works across a wider social spectrum: "[ε] raising is characteristic both of low-status female speech and more generally of higher-status speech" (Milroy 1992: 121). This may lead to a more rapid diffusion of new variants of (ε). But crucially, we have seen that the *transmission* of new variants - for both (a) and (ε) - is seen to be dependent on the sociolinguistic behaviour of women.

This section has not attempted to review all the important findings of Trudgill's work in Norwich and the Milroys' work in Belfast: its purpose was to examine the distribution of some linguistic forms related to gender-based variation and language change in two of the earliest and most influential studies of sociolinguistic patterns in the British isles. The next section concerns more recent

attempts to evaluate the relationship between men and women's language and linguistic change.

### §4.3 *Challenging the traditional approach*

#### §4.3.1 *Gender and linguistic variation*

The concept of gender has been widely debated in many academic fields including sociology and psychology, but only relatively recently has the distinction between sex and gender been noticeably addressed in the field of linguistics, particularly sociolinguistics. Indeed, it is a complaint voiced by members of the 'feminist school' of linguistics (e.g. Cameron (1996)) that the role of gender has been too blithely accepted by sociolinguists generally. One of Cameron's arguments is that, in their desire to explain linguistic variation in terms of general societal factors, linguists have failed to deal with the complexities of the societal factors themselves. Because many sociolinguists have insisted that the aim of "sociolinguistics proper" (Trudgill 1978) is shared with other areas of linguistic science - namely, to identify "inherent properties of language systems" (Cameron 1996: 32), they have tended to be less rigorous about the 'socio-' aspect of their work. This, of course, is an untenable position: without a fully developed and empirically justifiable sociological theory, any claims that sociolinguists make about linguistic variation could be seen to be ill-founded.

These problems that sociolinguists face in general are particularly central to researchers in the field of language and gender (Cameron 1996: 33). Firstly, there is the matter of distinguishing sex from gender, and the implications of such a distinction. The differentiation seems quite straightforward: while sex is a biological given, gender is a sociological construct. Yet clearly certain aspects of this societal differentiation are based on biological differentiation: as Chambers (1995: 103)

argues, the “social role of mothering is traditionally assumed by women as a consequence of their biological functions in carrying and nursing their children. Intensely physical labor is traditionally done by men because on average they are bigger than women”. Nonetheless, these social roles are tendencies rather than absolutes. In sociolinguistic analyses of male and female language, there has been some degree of failure in attempts to distinguish biological sex from sociological gender. Eckert (1989: 247) encapsulates this problem: she argues that sociolinguists

have been examining the interaction between gender and variation by correlating variables with sex rather than gender differences. This has been done because although an individual’s gender-related place in society is a multidimensional complex that can only be characterized through careful analysis, his or her sex is generally a readily observable binary variable, and inasmuch as sex can be said to be a rough statistical indication of gender, it has been reasonable to substitute the biological category for the social in sampling. However, because information about the individual’s sex is easily accessible, data can be gathered without any inquiry into the construction of gender in that community. As a result, since researchers have not had to struggle to find the categories in question, they tend to fall back on unanalyzed notions about gender to interpret whatever sex correlations emerge in the data and not to consider gender when there are no sex correlations.<sup>7</sup>

This failure to distinguish sex and gender has led in some cases to misinterpretation of linguistic variation, with the result that sex differences, “being visible, are taken as the independent variable to be correlated with linguistic variables regardless of gender roles in the community” (Chambers 1995:104). Such a distinction is vital to a clear understanding of, for instance, the variation in the vernacular speech of Belfast as discussed in §4.2.2 above and also in Eckert’s study of adolescent and gender variation in suburban Detroit speech (Eckert 1989), as discussed in §4.1.2 above. In Belfast, the changing economic climate led to a realignment of gender roles in communities which were particularly hard hit by

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<sup>7</sup> Labov (1990: 209) points out that a simple substitution of *gender* for *sex* is not beneficial, and goes on to suggest that a “biological bias is not avoided by dropping the category of men and women as independent variables, but rather by tracing the differential behavior of men and women through a wide variety of social factors” (1990: 242).



unemployment, so that the traditional pattern of women staying at home while the men went out to work locally changed drastically: the men were often the ones who stayed at home, while the women were employed outwith the immediate area, thereby contracting the loose ties which facilitated the spread of backing of /a/ from east to west Belfast, as discussed above.

Secondly, there is the problem of identifying women as a particular social group. Coates (1993: 7) makes the following observations about what she calls ‘normal’ social groups, groups like working-class residents of Belfast and adolescents in Reading which have been the objects of study in other sociolinguistic research (Milroy (1987a) and Cheshire (1982) respectively):

1. **Either** they live near each other in a recognised neighbourhood ... **or** they have recognised meeting places.
2. They have a recognisable and distinctive sub-culture.
3. Members of the group acknowledge the existence of the group: belonging to the group is part of their identity

Women, Coates argues (1993: 7-8), do not meet all of these criteria as a ‘social group’, though she does suggest that women increasingly are acknowledging ‘the existence of the group’, fulfilling criterion 3 above. If this is the case, is it possible to use ‘men’ and ‘women’ as extralinguistic variables following the same criteria as that employed for ‘class’, ‘network’ and ‘age’? This seems to be at the heart of the complaints voiced by feminist linguists: previous research into the correlation of linguistic variation with gender has failed to question “certain common-sense propositions *about gender*” (Cameron 1996: 49). It seems vital, then, to address the *concept* of gender itself when discussing this type of sociolinguistic variation: gender is not the solution to questions about certain types of linguistic diversity; rather, it is a problem which such diversity only serves to highlight. In the next four subsections, I give a brief analysis of recent sociolinguistic studies (all within the 1990s) which display interesting patterns of gender-based variation. These patterns prove useful in



an attempt to reconsider some of the received notions about gender and language change.

#### §4.3.2 *Glottalling and glottalisation in Tyneside English*

In chapter 3, I looked at some of the features of glottalisation in TE. Here, I intend to develop this further with a review of a study carried out by James Milroy, Lesley Milroy and Sue Hartley (1994), hereafter MMH, which sees some of the features of the distinctive patterns of glottalisation in that accent as only explicable from a sociolinguistic perspective. The central theme of MMH's article is the motivation for phonological change. Whereas traditional dialectologists have seemed to think that, in terms of dialect contact, modifications enter rural dialects by a direct influence from the standard language, MMH argue for the need of some intermediary category to facilitate such a change, suggesting that "linguistic variables operate at different levels of generality in terms of their territorial spread" (MMH 1994: 2), so that while some variables (such as (ng) with its variants [ɪŋ] and [ɪn] discussed above) operate across the whole of the English speaking world, some are very restricted in locus (such as the [e'] variant of (a) before velars in Belfast English discussed previously). The opposition 'standard vs. non-standard', then, is too simplistic and fails to reveal any fine gradation: recall that this was shown in chapter 1 of this thesis, which provides a selective account of the development of the TE accent. With regard to gender-based variation, MMH hypothesise that "males and females will show relative differences in their use of localised and supra-local variants, with males preferring the more local variants" (MMH 1994: 2).

In TE the pattern of glottal substitution (that is, the [ʔ] variant of, for instance, (t) - henceforth GS, as in chapter 3) can be said to be *general* (that is, there is little to distinguish TE from other varieties of the language in terms of the auditory distinctiveness of the glottal or in the range of environments in which glottalling

occurs), while the process of glottal reinforcement (ie the [tʔ] variant of (t), henceforth GR<sup>8</sup> as in chapter 3) can be said to be *local*. Furthermore, GS and GR are also distinct in terms of their sociolinguistic patterning: “they do not exhibit the same sociolinguistic patterns in co-variation with extralinguistic variables, and they are not socially evaluated in the same way, or on the same scale of evaluation” (MMH 1994: 7). For instance, GR is the predominant pattern amongst elderly Tynesiders, i.e. they are characterised by a low incidence of GS. How, then, do the processes of GR and GS tie in with gender-based variation?

One of the central tenets of the MMH thesis is that the general trend among sociolinguists of seeing gender marking in language as somehow ‘falling out’ from social class marking is erroneous: the Sex/Prestige pattern outlined at the beginning of this chapter sees class as primary and gender as secondary, since the pattern presupposes that prestige variants (used by the upper and upper-middle classes) and vernacular variants (used by the working class) are established first and then generalisations about male and female usage are based on the former’s targeting of vernacular variants and the latter’s attempts to emulate the prestige, standard variants. MMH claim this is erroneous, based on their interpretation of various studies of G in TE (particularly Hartley (1992)). These ideas were developed further in Docherty *et al.* (1995), and Docherty *et al.* (1997), as discussed in chapter 3.

Hartley (1992) recorded 16 WC Newcastle schoolchildren of two age groups (5 years and 10 years old) with 4 boys and 4 girls in each age group. While two styles were recorded for the 10 year olds (‘peer interaction’ and ‘word list style’), 5 year olds were not given a reading list, so the only style recorded for them was ‘peer interaction’. Below I give some statistics regarding GS and GR based on Hartley’s (1992) findings as reported by MMH (1994):

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<sup>8</sup> Where no distinction between substitution and reinforcement is necessary, I use the symbol G, again as in chapter 3.

Percentage use of **GS** for individual variables by age, sex and style in the speech of 16 Tyneside schoolchildren (after Hartley (1992))

Age	Sex	Style					
		<i>Peer interaction</i>			<i>Word list style</i>		
		/p/	/t/	/k/	/p/	/t/	/k/
5 years	Male	11	35	4			
	Female	8	43	6			
10 years	Male	5	36	4	5	12	6
	Female	19	43	1	6	31	4

Percentage use of **GR** for individual variables by age, sex and style in the speech of 16 Tyneside schoolchildren (after Hartley (1992))

Age	Sex	Style					
		<i>Peer interaction</i>			<i>Word list style</i>		
		/p/	/t/	/k/	/p/	/t/	/k/
5 years	Male	56	28	73			
	Female	40	14	61			
10 years	Male	55	25	73	43	21	41
	Female	48	21	52	37	16	20

These data reveal much about the processes of GS, GR and gender-related variation in TE. The scores suggest that females are favouring the GS variant, while males are favouring the GR variant; put another way, women are leading a supra-local change (the increase in GS, especially amongst MC women, is a countrywide phenomenon: cf. Mees (1987)), while men are in the vanguard of a local change (the dissemination and preservation of a distinctive phonetic variant which characterises Newcastle speech). Furthermore, the data reveal that certain segments are more likely to be glottalled than glottalised, irrespective of the sex of the speaker: /k/, for instance, is

rarely glottalled by either the boys or the girls: both sexes favour GR in items such as *lucky* [lʊkʔi].

More data reveal that phonetic environment is also a factor in the prevalence of one variant above another. Below are the percentage scores for the GS variant of (t):

Percentage use of GS of (t) in peer interaction by age, sex and phonetic context (after Hartley (1992))

<i>Age</i>	<i>Sex</i>	<i>Within-word</i>	<i>Word-final</i>
<i>5 years</i>	<i>Male</i>	34	35
	<i>Female</i>	55	35
<i>10 years</i>	<i>Male</i>	25	40
	<i>Female</i>	51	39

Of particular interest here is the scores for girls of both age groups in the 'intervocalic context' (55% for 5 year olds, 51% for 10 year olds). Firstly, it is this environment which accounts for the high GS scores for the girls. Secondly, there is a (very slight) decrease in use of GS from the 5 year old group to the 10 year old group. The importance of this will become clearer after the following section on gender-related variation in New Zealand English, but suffice it to note here that: (i) it is in the intervocalic context that GS variants of (t) are most salient and (arguably as a result) most highly stigmatized in British English; and (ii) it is not surprising to see a small decrease in the use of GS amongst the 10 year olds, since it has been argued that, in Britain at least, it is at around this age that children become particularly aware of the social significance of certain (stigmatized) variants (cf. Romaine (1978)).

Data for word-final (t) suggests that different sexes favour different variants in different contexts. Before a pause, both sexes favoured non-glottal, fully released variants (cf. chapter 3), so for instance *Out!* [u:t]; before a consonant, the boys tended to favour GS (though this is a characteristic of British English generally in this context), so for instance *he hit Peter* [hi: hi? pitʔe] for boys generally; and most

interestingly before a vowel, most speakers tended to favour a voiced and/or tapped realisation (following the ‘T-to-R rule’ (Wells 1982)) as in *he hit Anne* [hi: hɪr ən].

Hartley’s statistics for this final environment are given below:

Percentage use of voiced variants of (t) word-finally before a vowel in peer interaction (after Hartley (1992))

<i>Age</i>	<i>Sex</i>	<i>% [ɾ ~ɹ] variants</i>
<i>5 years</i>	<i>Males</i>	82
<i>10 years</i>		55
<i>5 years</i>	<i>Females</i>	60
<i>10 years</i>		44

As MMH note, a bigger sample is needed to make statistically defensible predictions regarding this phenomenon, but it seems as if the use of tapped and voiced variants word-finally before a vowel is a change that is being led by the boys.

MMH draw the following conclusions from these and other data:

- (1) “evidence from the various studies reviewed in this paper suggests quite strongly that gender marking may over-ride class marking as the underlying social mechanism whereby linguistic change is implemented and diffused in the speech community” (MMH 1994: 26);
- (2) the generalisation made here, then, is not that females favour certain forms which are prestigious, but that they create them; that is, if females favour certain forms, then those variants will become marked as the prestige ones;
- (3) males favour features which are more localised (GR, not GS, variants; tapped variants of (t) word-finally before a vowel); females favour forms which are supra-local or generalised, in this instance the GS variant.

### §4.3.3 *The formation and development of New Zealand English*

This section reviews the research carried out by Woods (1997) into the formation and development of New Zealand English and its links with gender-related variation and linguistic change. It involved a real-time study of change in New Zealand English through analysis of speakers recorded in 1948 and also recordings of their descendants which had been carried out ‘recently’ (Woods 1997: 95). It focused on the vowels in *mouth*, *trap* and *dress* which I will hereafter refer to as (au), (a) and (ε), though Woods herself does not label the variables as such. Woods makes the following hypothesis: women will lead in linguistic changes that are dynamic or new yet lag behind men in the use of variants which represent older changes; prestige, then, will not suffice as an explanation for such a change, and subsequently, developments should be analysed in terms of dialect contact, specifically the patterns of discourse employed by women in contact situations. The linguistic behaviour of women may appear self-contradictory, as Labov (1990) has pointed out, since women’s speech is often characterised as being both innovative and conservative. This apparent contradiction may lie in “women’s tendency to lead in new changes, but to withdraw from the use of changing features as soon as they become stigmatized” (Woods 1997: 96). The following data will illustrate whether such a claim is valid, and also whether some of the conclusions drawn by Milroy, Milroy and Hartley about gender-based variation are applicable to the New Zealand situation.

#### (i) *The (au) variable*

The (au) scores were as follows:

(au)-0 for variants [äʊ ~ aʊ]

(au)-1 for variants [aʊ ~ æʊ]

(au)-2 for variants [æʊ ~ εʊ]



Scores were calculated by finding the average for each informant and multiplying by 100, so that consistent use of [äu] would give a score of 0, while consistent use of [ɛu] would result in a score of 200. The following results obtained for the first generation speakers, which showed that the choice of variant was correlated quite strongly with phonetic context. In words of more than two syllables (such as *empowering* or *cowardice*), the first generation speakers would rarely have (au)-2. The same holds true for words in which (au) follows /h/ directly (*howl*, *hound*). However, when (au) occurs before an alveolar stop then (au)-2 variants *are* favoured (*about*, *loud*). This is also the case where (au) occurs in open syllables (*row*, *allow*). For second generation speakers, the results were not as systematic, and a further variant [a:] was noted. A comparison of the results for the two groups seemed to suggest that “the articulation of the nucleus of MOUTH has become closer in New Zealand English” (Woods 1997: 103). More crucially to the present discussion, it was found that among female speakers of the first generation, a closer variant was favoured when compared to the average male score for that age group (female mean score, first generation: 147; male mean score, first generation: 111.4); however, the reverse was true of the second generation speakers, so that the men were on average using closer variants than the women (female mean score, second generation: 115.6; male mean score, second generation: 171.4). “First generation women were in the vanguard of the change. However, fifty years later when this feature has become a more established variant of New Zealand English, women are using a less ‘advanced’ (i.e. less close) form” (Woods 1997: 105). These data, then, will not tie in with Labov’s theory on women’s language, namely that women lead in linguistic changes until the variant they use is overtly recognised, at which points complaints surrounding the use of that variant lead to its stigmatisation; once this point is reached, women, being more sensitive to the social prestige associated with language, use fewer of the recognised, stigmatised and innovative variant. We would expect



that the first generation women would show a low incidence of the advanced [ɛʊ] variant, given that they would have grown up at a time when such a variant was highly stigmatised. However, these data suggest that the reverse is true.

(ii) *The (a) and (ɛ) variables*

The (a) and (ɛ) scores were as follows:

(a)-0 for variant [æ]	(ɛ)-0 scores for variants [ɛ ~ ɛ]
(a)-1 for variant [æ̘]	(ɛ)-2 for variants [ɛ̘ ~ ɛ]
(a)-2 for variants [ɛ̘ ~ ɛ]	(ɛ)-3 for variants [ɛ̘ ~ ɛ̘]

Looking first again at phonetic environment, with the (a) variable there seems to be little consistency with regard to the first generation speakers. In other words, the (a) variable displayed considerable intra-idiolectal variation; it was not uncommon for speakers to use all three variants during the recordings. There was notably far less variation within the idiolects of the second generation speakers.

This was not the case with the (ɛ) variable. With first generation speakers, a whole range of variants, including some not listed in (ii) above, was used, but with certain restrictions and weightings dependent on environment. For instance, [ɛ̘] occurred only before [v] (*never, seven*); the close variants [ɛ ~ ɛ̘] were rare generally, and appeared usually before [s] (*guess*); a further diphthongal variant [ɛə] occurred in the speech of one female speaker in one lexical item (*yes*). With the second generation, the following patterns emerged: categorical absence of [ɛ̘], even before [v] - in this environment, close non-front forms [ɛ̘ ~ ɛ̘] were favoured; the broken variant [ɛə] was more common, particularly among women and particularly in one lexical item (*yes*). As a result of these specific constraints, the mid-low non-front variant and the broken variant were discounted in the calculation of scores.

The results illustrated that women are favouring a higher/closer variant of (a) than men. The mean score for first generation women was 121.00, and for first generation men 81.2. The same is true for the second generation (women's mean score 145.2, men's 118.6) though it is important to note here that the discrepancy in scores between the sexes is far smaller with the second generation than with the first: in other words, second generation males are using variants that are much closer to those used by women than the first generation males were. Should this continue (and further research, perhaps into third generation speakers, would be beneficial here) we might expect to see the crossover pattern that occurred with the (au) variable.

Results for (ε) showed that although there is "some evidence of an innovation towards a raised vowel in the 1940s speakers [those of the first generation GT] (presumably enough movement to avoid a merger with raised TRAP), in general 1st generation speakers display a relatively low index ... In 2nd generation speakers, however, clearer trends in the use of close variants emerge and, while patterns of individual variation are considerable, women have a higher index than men" (Woods 1997:109). The (ε) shift is then a more recent development than the (a) shift: both are led initially by the women, but then men show a more rapid increase in use of the innovative closer variant. This would suggest that there is a chain shift in progress in New Zealand English, affecting the short vowels, and (given that the evidence presented above indicates that the (a) variable has been undergoing raising for a longer period of time than the (ε) variable) that this is a push chain.

How, then, do these data fit with a pattern of gender-related variation and, specifically, change led initially by women? Rather than attempt to analyse the findings in terms of simple prestige (which, as we saw with the (au) variable, is not particularly useful in this instance), Woods seeks answers in the fields of dialect contact, accommodation and women's discourse strategies. The central points of Woods' argument regarding New Zealand women's use of certain variants are:

- (a) first-generation women lead in the use of variants which are stigmatised
- and (b) second-generation women are not consistent in their use of stigmatised variants (cf. Woods 1997: 116)

Why should this be so? Initially, it is important to look at the formation of this particular variety of English. It has been variously suggested (e.g. Bauer 1994, Harkness and Crawford 1912) that the context in which New Zealand English was formed was one of notable dialect diversity; in such situations, dialect levelling and convergence are commonplace. However, such a process of levelling will consist of speakers “co-operating with, and conforming to, the speech styles of others” (Woods 1997: 116). As Holmes (1995) has shown, women speakers in her corpora of New Zealand English have a greater tendency than men to be listener-orientated in their discourse, and to pay greater attention to their interlocutor’s face<sup>9</sup>. “In other words, women’s speech is characterised by precisely those features which come into play when speakers of different dialects interact” (Woods 1997: 117). Bauer (1994) has argued that Australian English was a major influence on the development of New Zealand English, and it is noticeable that Australian English is similarly characterised by close variants of vowels in MOUTH, TRAP and DRESS (cf. Giegerich 1992: 77-8). Woods (1997: 117, footnote 18) notes evidence which suggests that a significant Australian contingent was integral in the formation of early NZE: women, therefore, may have accommodated to the speech of the Australians who were characterised at the turn of the century by their use of close variants of (au), (a), and, although this change is relatively recent, perhaps also (ɛ). Such a phenomenon is attested elsewhere, and has even been discussed briefly earlier in this chapter: in Belfast, for instance, women’s use of raised variants of (ɛ) has been associated with accommodation to the speech of those resident in the out-of-town communities. It is

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<sup>9</sup>The term is borrowed from Deuchar (1989) and refers to politeness strategies adopted in discourse, where respecting face is defined as a linguistic strategy used to show “consideration for people’s feelings” (Coates 1993: 85).

important to stress that much of this does have links with the concept of prestige; but Woods argues that prestige alone will not suffice as an explanation for all of the changes witnessed in her analysis of the data, and that explanations which draw on other types of sociolinguistic patterns, such as discourse strategies, dialect levelling and accommodation, can help to make some of the issues relating to linguistic change and gender-based variation much clearer. Woods concludes that “changes which are male-dominated are ‘relatively isolated’ in linguistic systems, while women are more likely to lead in major - e.g. chain shift - linguistic developments” (1997:118). This ties in with some of the observations made by Milroy, Milroy and Hartley about TE, discussed earlier: widespread and major changes seem to be led by women; a variety of factors, including accommodation and levelling, are influential in the initial stages of the change; other factors, such as prestige and stigma, may then check women’s use of the innovative variants, and the change is then carried on by men. It is only at this stage that the variants are accorded some sort of prestige, be it overt or covert, and subsequently stratified in terms of social class.

#### §4.3.4 *The Fenlands*

Britain (1997) collected data from 81 WC informants in the Fenland area of eastern England, an area of previously uninhabited marshland reclaimed in the seventeenth century (see Britain 1997: 18). Dialects spoken to the east and west of this area were historically very distinct, and even early inhabitants of the reclaimed land were very isolated, living on a few islands on higher ground<sup>10</sup>. But once the land had become habitable, there was a period of mass migration, with speakers moving from east and west to create new communities. Britain’s investigation focuses on a salient variable for these Fenland speakers, the vowel in words such as *right* and *line*, i.e. (ai). This diphthong shows predictable allophony in a number of

<sup>10</sup> Derek Britton (personal communication) has informed me that there is evidence to suggest some contact between speakers in the area in days prior to reclamation, by means of navigation. But clearly the extent of face-to-face contact would be minimal.

varieties of English. In Canada, as well as the Northern United States and in the Falkland Islands and Bermuda, amongst other areas, a diphthong with a central or close first element is found in syllables ending with a voiceless consonant; this also occurs with the (au) vowel, and is commonly referred to as 'Canadian Raising', hereafter CR, giving rise to pairs such as *rice* [rəɪs] vs. *rise* [raɪz] and *mouth* [maʊθ] vs. *mouths* [maʊðz]. Chambers (1989: 84-5), as quoted in Britain (1997: 24), suggests that "there is no English source for Canadian Raising, or, at least, none is recoverable from orthoepy, philology or dialect surveys", while Britain himself argues that "it [CR] has normally been reported as NOT occurring at all in the UK" (*ibid.*).

There are a number of accounts which attempt to explain this phenomenon. The Failure to Lower hypothesis is proposed by Lass (1987: 285), with support from evidence from Scots dialects. It is suggested that, as part of the S(cottish) V(owel) L(ength) R(ule), [aɪ] for /aɪ/ appears before voiced fricatives, before /r/ and at a morpheme and syllable boundary (the 'long' environments), while [əɪ] appears elsewhere (the 'short' environments). This pattern displays an arrested development of ME /i:/, in which the gradual dissimilation of two elements of the diphthong is increased before voiced consonants, schwa and at morpheme boundaries to a greater extent than before voiceless consonants. CR is therefore a development of the SVLR pattern, since the 'long' allophone [aɪ] occurs before all voiced consonants, not just the fricatives and the retroflex approximant, and occurred in North America following the high level of Scots and Scots-Irish immigration to Canada (Gregg 1973).

There are, however, a number of problems with this analysis. Firstly, the phonetic environment for the SVLR allophones is not isomorphic with that of the CR, so that the argument for a 'simple' expansion of the rule is questionable. Secondly, there were also a great number of Scots-Irish who settled in other areas of



North America - including the southern states of Tennessee and Kentucky (Wolfram and Schilling-Estes 1998: 98ff) - and in the South, not only does CR not occur, but more importantly an entirely different allophonic pattern exists: raised onsets occur in voiceless environments, with low monophthongs elsewhere (*flight time* [flɪɪt ta:m]).

Another explanation reported by Britain (1997: 33-4) is Chambers' (1973, 1989) Raising hypothesis, in which the characteristic features of CR are explained as innovations, rather than as instances of 'colonial lag'. Claims that this feature is an innovation are based on early dialect records - Chambers argues that there are no pre-twentieth century records for CR, though Britain cites the work of Thomas (1991: 148) who argues that raising of /aɪ/ occurs in the speech of Ontarians born in the latter part of the nineteenth century, based on evidence from linguistic atlases of the north USA and Canada - and on the geographical diffusion of CR from Canada into the northern US cities.

But neither of these explanations can fully account for the data collected by Britain. Britain's survey involved data collection from three distinct areas in the Fenlands (the Western, Central and Eastern Fens); the results from this survey were then compared with older dialect material (as presented in the *SED*). The *SED* material showed that speakers from the communities in the Western Fens (Crowland and Little Downham) displayed no allophony with /aɪ/: in all contexts they favoured a diphthong with an open first element, generally [aɪ]. By contrast, those in the Central Fenland area (Outwell and Wisbech) showed a marked allophony, with [ʌɪ] allophones before voiceless consonants, and [aɪ] before voiced consonants, schwa, and at a morpheme boundary. Such patterns are similar - but not identical - to the patterns which emerge from Britain's data. Britain showed that different allophonic patterns were operating across three distinct geographical areas noted above: in the Western Fenlands (that is, in communities such as Peterborough and Spalding), there

was no evidence of allophony with any group of speakers, speakers preferring monophthongal variants or those with low (and often back) first elements; in the Eastern Fenlands (in urban centres such as King's Lynn and Ely), there was evidence of some allophonic patterning, but this was not marked, and generally in the east, diphthongs with close onsets were preferred. Distinct allophonic variation was only to be found in a north-south band in the Central Fenland area (which included the communities of Wisbech and Chatteris): this allophonic variation was most clear in the speech of the older (45-65 year old) informants, but was also displayed by the younger (15-30 year old) group. Crucially, gender-based variation was also in evidence in this study. While monophthongal variants were widespread in the Western Fenland communities, with all speakers having at least some monophthongal forms in all phonological positions, only the women in the Central Fenlands commonly had monophthongal forms. Further, in terms of the spatial distribution of the allophonic patterns, it was shown (Britain 1997: 27-30) that distinct allophony was found in women's speech over a much wider geographical area than it was in men's speech. This was shown to hold true for both age groups: although "the area in which Canadian Raising is used is shrinking ... speakers in some areas, principally the Wisbech-March area, are more resistant to losing it than those in other parts of Central Fenland" (Britain 1997: 29).

There are a number of conclusions to be drawn from Britain's data. Firstly, the emergence of allophony across the Fenlands seems to be related to dialect contact. Recall that this area is one which was only inhabitable relatively recently: in the post-reclamation period, the inhabitants of the Fenlands enjoyed increased mobility, coupled with more extensive contacts with new in-migrants, from both east and west. Such migratory patterns would have led to a range of variants of (ai) in the Fenlands, some conservative<sup>11</sup> (from the east, where close onsets were favoured) and

<sup>11</sup> The classification of east Anglian varieties as conservative and east Midlands varieties as innovative is from Britain (1997: 36), where he refers to East Anglia as "well-known" to be sociolinguistically conservative.



some innovative (from the west, where open onsets were favoured), and given that no one migratory group is seen to be dominant in terms of the demographic history of the area, there would be no variety of English that “could ‘swamp’ other varieties” (Britain 1997: 36). Over a number of years, increased intercommunication led to the establishment of regularised networks, displaying relatively dense and multiplex links, with subsequent regularisation of variants and increased focusing, with speakers simplifying the variants “by reallocating them according to principles of phonological naturalness - [əɪ] before voiced<sup>12</sup> consonants, and [ɑɪ] in other environments” (Britain 1997: 37). Secondly, the specific gender patterns displayed in this study are similarly related to patterns of dialect contact. The reclamation and subsequent habitation of the Fenlands was a major social change, one in which previous network ties were broken, and new network ties brokered. The re-establishment of network ties is seen to be a product of increased “routinization” (Britain 1997: 39), a sociological term which highlights the importance of day-to-day routines as a cohesive force in communities typified by strong network ties (though it is not restricted to such communities); such re-routinization will affect the dialect of those involved in the re-establishment of network ties; dialect changes are likely to be most marked by those most regularly involved in such contact, and those regularly involved in such contact are likely to be convergent accommodators. Recent studies into such patterns of dialect contact have shown that adolescents tend to accommodation more readily than adults (as noted for instance in Kerswill and Williams (1992)); and in such dialect contact scenarios, females lead in the convergence on new levelled variants: in the Milton Keynes study (see §4.3.5 below). Women’s speech behaviour in these dialect contact situations is further evidence to support the claim that they will be in the vanguard of linguistic changes.

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<sup>12</sup> Britain has ‘voiced’ here, but this would seem to be an error: his argument clearly suggests that the [əɪ] variant would appear before *voiceless* consonants.

#### §4.3.5 *Milton Keynes*

I conclude this chapter with some brief comments on Kerswill and Williams work (1992, 1994) on Milton Keynes. This study, like Britain's discussed above, did not focus on gender-based variation: indeed, its primary aim was to consider the role of children's speech in the resolution of dialect conflict in new settlements. The Milton Keynes project attempted to look at the outcomes in post-contact scenarios, but unlike other studies which used data collected from informants who were the grandchildren of original migrants, this survey investigated the speech patterns of the 'first generation' - children born in Milton Keynes - in an effort to establish sociolinguistic patterns of dialect contact in its initial stages (Kerswill and Williams 1992: 68-9).

In terms of gender-based variation, two variables displayed results significant for the present discussion. The first is (ou), for which four variants were recorded:

(ou)-0: [o: ~ ou]

(ou)-1: [ɸʊ ~ ɸö]

(ou)-2: [æʏ]

(ou)-3: [æɪ]

(ou)-0 is characterised as a Northern English or Scottish variant<sup>13</sup>; (ou)-1 as a London form; (ou)-2 is an innovation with fronting of the onset and the offset (both rounded), and (ou)-3 another innovative variant, with fronting of the onset and unrounding of the offset. In terms of age distribution, the children had a much narrower range of variants than did their mothers. This is evidence to support the claim that focusing occurs in dialect contact scenarios: rather than tolerate a wide range of variants, new dialect speakers have a general "reduction in the amount of variation" (Kerswill and Williams 1994: 9). More important for the present discussion, however, is that the girls are leading in this focusing: pre-adolescent girls were adopting new fronted

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<sup>13</sup> That such diverse dialectal variants should be represented is a reflection of the diversity of origins of many of the Milton Keynes residents.

variants of the (ou) variable, (ou): [æy] in words such as *coke* and *blow*, more readily than the boys were, and also more readily than the younger girls were: this latter group seemed more heavily influenced by the speech of their caretakers (usually their parents). This might suggest that females tend to define which variants are characteristic of new dialects.

Another characteristic of many dialect contact scenarios is that, in the new variety, marked regional forms are disfavoured, and certain of the vocalic variables investigated in Milton Keynes (such as (a), the vowel in the TRAP lexical set) support this. However, in some cases, it was possible to see a co-variation of localised and non-localised variants, and these localised variants tended to be London forms. This is not surprising, given that 35.2% of the incoming residents moved from the capital. One such variable is (au), the vowel in the MOUTH set (Kerswill and Williams 1994: 20). This variable had the following variants:

(au)-1: [ɛ:]

(au)-2: [a:ə]

(au)-3: [æʊ]

(au)-4: [aʊ]

The first two variants were classified as east London forms, the third as general London or south-eastern, and the last RP. There were certain expected patterns with the mothers: those born in London favoured either (au)-1 or (au)-2, while those born in Milton Keynes favoured the RP form, (au)-4. But there was a slightly different pattern with the children, with the older girls favouring (au)-4 and the older boys favouring (au)-2. This pattern might yet again be seen to support one of the central arguments of the revisionist view of gender-based variation: that male speech is associated with local forms, and female speech with supra-local forms, in the establishment of a new dialect.

#### §4.4 *Summary*

In this chapter I have discussed aspects of a range of sociolinguistic studies, many of which made reference to linguistic variation and its potential correlations with the sex of the speaker; particularly, I have tried to focus on those parts of the studies which attempted to consider linguistic change with regard to gender-based variation. In §4.1, I tried to outline some of the general principles associated with gender and language change, focusing particularly on the principles of Labov (1990), which argue that women tend to lead in changes both from above and below; in §4.2, I reviewed the early approaches taken by Trudgill and the Milroys in their analyses of Norwich and Belfast English respectively - the former attempting to provide an account of gender-based variation within a class-based, consensus model of society, the latter attempting to do the same within a network-based, conflict model of society; and in §4.3 I looked at more recent studies, which challenged some of the received wisdom on the role of gender in linguistic change. Many of these more recent analyses of the role of women and men in the promotion of innovative forms have made reference to sociolinguistic factors other than gender (such as dialect contact and accommodation, for instance), which imply a more holistic approach to the role of gender in the propagation of linguistic innovations. We therefore need to consider gender-based variation in a wider social context. Further issues concerning gender-determined linguistic patterns - and their role within a wider theory of social variation - are noted by Chambers (1995: 125-6):

In societies where gender roles are sharply differentiated such that one gender has wider social contacts and greater geographical range, the speech of the less circumscribed gender will include more variants of the contiguous social groups.

Chambers' comments here are based primarily on patterns emerging from modern data collected in Detroit and Belfast, but it is clearly applicable to the other studies

discussed in this chapter. Chambers here implicitly links gender issues with other sociolinguistic phenomena, particularly patterns in dialect contact scenarios, and accommodation theory; and as Chambers goes on to argue (Chambers 1995: 126):

One clear advantage that this explanation has over previous attempts to explain linguistic differences between men and women is that it attributes nothing at all to gender itself. It does not claim that women are likely to be “more conservative” than men, or “prestige-oriented”, or anything else. As Eckert (1989: 248) says, “Not only is it a mistake to claim that women are more or less innovative than men, but at this point in our research it is a mistake to claim any kind of constraint associated with gender.” Instead, the dynamic variable is mobility. Sociolinguistic patterns, according to the principle of gender-based variability, are to some extent determined by the breadth of social and geographical contacts.

This holistic approach may be seen as the way forward for an examination of gender-based linguistic variation; and this is considered further in the following chapters of this thesis.

## 5 The Newcastle survey: method and data

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This chapter attempts to provide a complete account of the methodology for the collection of the data used in the remainder of this thesis. The framework for the presentation of the method follows the five stages which Hudson (1996: 150-1) considers both “important and problematic at all stages in a sociolinguistic text study”, namely:

- selecting speakers, circumstances and linguistic variables
- collecting the texts
- identifying the linguistic variables and their variants in the texts
- processing the figures
- interpreting the results

The first four of these are each considered in the remainder of this chapter, and the last is considered both in this chapter and in chapter 6: in this chapter, the results for each specific variable are interpreted, while in chapter 6, a synthesis of the different variations is attempted, in order to provide an overview of the (socio)linguistic patterns in the speech community investigated, particularly with regard to those patterns associated with gender-based variation considered in chapter 4, and how these might relate to the ‘structural’ issues considered in chapters 2 and 3.

### §5.1 *Selecting speakers, circumstances and linguistic variables*

In order to record as much informal speech as possible, the speaker selection for this thesis largely followed a modified social network, participant observation approach based on methods favoured by the Milroys in Belfast (Milroy and Milroy 1978), Cheshire in Reading (Cheshire 1982) and Labov in Harlem (Labov 1972), among others. Twenty informants were recorded; these twenty fell into four pre-existing social groups, so that each cell contained 5 speakers, of the same sex and

roughly the same age, as illustrated in Table 1. It should be stressed at the outset, however, that there will be no attempt to establish a network strength score for each of the informants, for reasons detailed below. The purpose of adopting this approach was to gain access to large amounts of spoken discourse during which the informants might talk more with each other than with me, and to allow the group to operate as a norm-enforcement mechanism, allowing for as close an approximation to natural speech as possible. It should also be remembered that the central sociolinguistic focus of this thesis is to investigate differences between male and female speech on Tyneside; ideally, we are looking for networks which have a similar structure in terms of their density and multiplexity.

Table 1

## Informants by age and sex

Older males		Older females	
Name <sup>1</sup>	Age	Name	Age
Ronnie	55	Eileen	44
Vince	46	Theresa	49
Neil	54	Doris	52
Clive	53	Nancy	50
Scott	60	Florrie	58
Younger males		Younger females	
Name	Age	Name	Age
Robbie	24	Monica	20
Kevin	24	Gail	20
Tom	24	Fiona	20
Chris	20	Melanie	14
Steve	20	Sam	20

The informants were contacted through my father's cousin; I had met most of the informants in each network many times before over a number of years, so was familiar to members of each group, but always as a friend of a friend. This was of

<sup>1</sup> The names given here are pseudonyms in order to preserve anonymity, following the general consensus among sociolinguists (cf. Milroy (1987: 91)).



great benefit when the recording of each group took place, as all of the participants were very much at ease, and little attention was paid to the tape recorder (on which see further §5.2 below). The fact that I was born and brought up on Tyneside until I was eighteen also helped to overcome some of the issues of the Observer's Paradox (cf. §5.1.2), since there was thus at least some degree of common ground with all of those participating in the recordings, reducing the social distance between the observer and the observed.

The social characteristics of members of each of the networks is detailed below, but prior to this, it is important to establish the general social context in which these networks operate.

The ward of South Gosforth is a mainly residential area of Newcastle upon Tyne approximately two miles to the north of the city centre. The *South Gosforth Ward Poverty Profile* (SGWPP 1995) provides important sociological and socio-economic information about the area. This report was based (among other things) on the data provided by the 1991 census, and gives the impression of the South Gosforth ward as a very middle class area. It has a population of 9,869 and a total of 4,171 households (SGWPP 1995: §4), with 84% of those households being owner-occupied. "This figure is two thirds higher than the city average, and is the highest in the city" (SGWPP 1995: §6.5). Roughly one-third of such private housing is semi-detached. In terms of social class, the ward has "one of the highest proportions of residents classed in the top socio-economic group. 58% of men and 30% of women are professionals or managers compared to the city-wide average of 25% and 12% respectively" (SGWPP 1995: §4.3), and the unemployment rate in this area is also comparatively low, with 5% unemployment in the ward, compared to a rate of 13% in the city, and 11.9% in Tyne and Wear as a whole (SGWPP 1995: §6.1).

The perception of this area as very middle class is prevalent throughout Newcastle, and reflected in the following three quotations from various chapters of Colls and Lancaster (eds.) (1992):

As the region's pitmen are few in number, and as the dialect refuses, as usual, to be pinned down to some linguistic ghetto, and as even in Gosforth there are not many outspoken Hanoverians left (some would disagree), now seems as good a time as any to reclaim 'Geordies' for the whole North East.

(Colls and Lancaster 1992: xi)

Although middle-class ourselves, we don't stray too close to middle-class, ex-urban or suburban gentilities in Hexham or Gosforth.

(Colls 1992: 7)

A walk through gentrified Jesmond, past the Real Tennis court into well-manicured Gosforth, or a journey to 'Little Chigwell' at Darras Hall makes it abundantly clear that the region does not lack a well-heeled middle-class. Moreover, like middle-class suburbs in other cities, they have their cordon sanitaires to ensure spatial separation from the lower orders. Jesmond has the canyon of the Dean (*sic*) to define clearly its border with working-class Heaton; Gosforth sits beyond the Town Moor; while the suburban enclave of Darras Hall is over the fields and far away.

(Lancaster 1992: 65)

But this, to a certain degree, gives a false impression of the social mix in the South Gosforth ward as a whole, and is certainly not representative of many of the informants used in this study. Indeed, some of the conversations with the older males and females specifically focused on differences between 'South Gosforth' and 'Gosforth', the first being a mixture of upper-working and lower-middle class (with a bias towards the former), and the second a clearly middle-class ghetto. The precise delineation of this area is not clear; but in general, those areas in which most of the informants lived, at the north and the east of the ward, closer to the broadly (upper-) working class areas of Longbenton and High Heaton, were perceived by those same

informants as less middle class. So the networks investigated here were situated in a generally high-status community, though, as discussed below, the precise socio-economic status of many of the informants in fact suggests a mixture of upper working-class and lower middle-class speakers. In many respects South Gosforth is very much like the Braniel and Andersonstown areas of Belfast, which were investigated by the Milroys and others between 1975 and 1981. Indeed, some of the comparisons are quite striking. Milroy (1987a: 75) states that “Braniel and Andersonstown are both located on the outer edges of the city and might be described approximately as upper-working to lower-middle class”. The geographical location of South Gosforth in relation to Newcastle has been mentioned above; and as also mentioned above, in discussion with the older informants, it emerged that South Gosforth (in contrast to Gosforth) was broadly conceived of as lower middle or upper working class<sup>2</sup>. Milroy (1987a: 75-6) goes on to state that many of the Braniel and Andersonstown informants had either originated from or had family ties with the Clonard and Ballymacarrett areas of Belfast, areas of much lower status within the city centre. Similarly, many of the older informants for this survey who lived in South Gosforth now had actually been born and brought up in the traditionally working class communities of Byker, Walker and Wallsend, in or near the shipbuilding areas of the Tyne. This contrasts with the younger informants who were born and brought up in the Gosforth area. The details of birthplace and residence are given in Table 2:

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<sup>2</sup> In fact, the older females tended to view the area as lower middle class, while the older males saw South Gosforth as upper working class.

Table 2

Informants by birthplace and current residence

Older males			Older females		
Name	Born	Resides	Name	Born	Resides
Ronnie	Washington	S. Gosforth	Eileen	Walker	S. Gosforth
Vince	Walker	S. Gosforth	Theresa	S. Gosforth	S. Gosforth
Neil	S. Gosforth	S. Gosforth	Doris	Byker	S. Gosforth
Clive	Wallsend	S. Gosforth	Nancy	Byker	S. Gosforth
Scott	Heaton	S. Gosforth	Florrie	Byker	S. Gosforth
Younger males			Younger females		
Name	Born	Resides	Name	Born	Resides
Robbie	S. Gosforth	S. Gosforth	Monica	Gosforth	S. Gosforth
Kevin	Gosforth	N. Gosforth	Gail	Gosforth	Wansbeck
Tom	Wide Open	Wide Open	Fiona	S. Gosforth	S. Gosforth
Chris	Gosforth	W. Gosforth	Melanie	Gosforth	S. Gosforth
Steve	Gosforth	W. Gosforth	Sam	Gosforth	S. Gosforth

Finally, Milroy (1987a: 76-77) uses employment as a means of establishing the relative status of the communities in Belfast. The informants in the outer areas of Braniel and Andersonstown have occupations such as “skilled tradesmen (an electrician and a fitter), a school meals’ supervisor, a bricklaying instructor at a government training centre, a shop assistant at Boots, a primary school teacher, students and unemployed ex-students and apprentices”. Such occupations are very similar to those of the South Gosforth networks, as shown in Table 3 below:

Table 3

## Informants by occupation

Older males		Older females	
Name	Occupation	Name	Occupation
Ronnie	Sales rep	Eileen	Personnel
Vince	Engineer	Theresa	Computing
Neil	Video engineer	Doris	Secretary
Clive	Engineer	Nancy	Cashier
Scott	Retired	Florrie	Sales assistant
Younger males		Younger females	
Name	Occupation	Name	Occupation
Robbie	Estate agent	Monica	Student (part-time customer service)
Kevin	Customer service	Gail	Waitress
Tom	TV repairman	Fiona	Student
Chris	Student	Melanie	At school
Steve	Customer service	Sam	Student

In summary, the general social characteristics of the networks as a whole are representative of a community located at the interior of the social class hierarchy, with both upper working and lower middle class members. It was generally the case that the older speakers had been raised in a more working class environment and had moved into a higher status community.

#### §5.1.1 *Social characteristics of the networks*

A potential problem with such a network approach for speakers who are comparatively socially and geographically mobile is highlighted by Milroy (1987a: 107-8), whose comments are worth quoting at length:

Looseknit networks are hard to deal with chiefly because a multi-valued speaker variable like social network involves comparing speakers who differ from each other in certain respects - let us say in respect of the multiplexity of the ties which they have contracted at the workplace - but are still similar enough to each other

in other related respects to make such a comparison meaningful.

Milroy (1992: 104) also notes that it is difficult to operationalize the measures of network strength used in the inner-city communities for speakers in the higher-status areas, because of the differences in social structure. These problems are addressed in this section.

Recall from §4.2.2 that the Belfast projects used the following criteria to establish NSS (Network Strength Scores):

1. Membership of a high-density, territorially based cluster.
2. Having substantial ties of kinship in the neighbourhood. (More than one household, in addition to his own nuclear family.)
3. Working at the same place as at least two others from the same area.
4. The same place of work as at least two others of the same sex from the same area.
5. Voluntary association with workmates in leisure hours.

Clearly such criteria are well-suited to an investigation of communities such as Ballymacarrett in Belfast; but not all of these criteria are suitable for the networks investigated in South Gosforth, as they fail to reflect the nature of the social ties which operate on such networks and can still exert significant normative pressure (linguistic or otherwise). The social characteristics of the communities are significantly different; while the north-east of England generally has areas of very high unemployment, South Gosforth is not one of those areas with the high degree of social malaise that characterised the inner-city Belfast communities. Yet there was clear evidence from some of the recordings - particularly with the younger groups - that Gosforth networks still function as norm enforcement mechanisms.

Cheshire's (1982) survey of Reading adolescents adapts a general network approach in part of the analysis of sociolinguistic correlates of grammatical variation in that community. An initial approach adopted by Cheshire was to consider

reciprocal naming as friend as a method of calculating degree of integration into the network, but in practice this only worked as a means of differentiating one of the boys' groups, the Orts Road peer group, who were thus subdivided into core, secondary and non-members. The sociolinguistic correlates so obtained were, however, inconclusive, since not all of the non-standard features functioned as markers of peer group status. Cheshire's subsequent approach was to look beyond the composition of the peer group, and to consider the issue of vernacular culture, and how certain covert norms functioned to constrain or promote the use of non-standard grammatical constructions. By vernacular culture, Cheshire was referring to the degree to which the children took part in non-mainstream activities such as arson, theft, vandalism and fighting, and the construction of the vernacular culture index reflected these activities. But again, it was difficult to apply the same criteria for all three of the adolescent groups investigated: specifically, what constituted vernacular culture for both Orts Road and Shinfield boys was not comparable for the socialisation patterns of the girls at Shinfield, as they did not attach the same value to these criminal activities. While it was possible to separate 'good' girls - those who adhered to mainstream culture - from 'bad' girls - those who did not - the actual construction of a vernacular culture index for the girls was both impractical and redundant. This again highlights the importance of establishing the right sort of social index to evaluate what can broadly be conceived of as network strength, and its association with loyalty to the vernacular.

Milroy (1987b: 141) discusses the precise indicators necessary in constructing a scale of network strength: they must reflect conditions which predict the application of norm-enforcement mechanisms, and they must be transparent to the fieldworker as part of the data collection. It was clear from the recordings that a direct application of the network strength scale used in the Belfast survey would be of little benefit for the South Gosforth networks. These networks were not



isomorphic with any of those in the localities investigated in the Belfast survey - that is, they were like neither the inner-city networks of Ballymacarret, the Clonard and the Hammer, which tended to have relatively denser, more multiplex networks<sup>3</sup>, nor those of the out-of-town communities of Braniel and Andersonstown, where networks tended to sparseness and uniplexity. In fact, they seemed to fall somewhere in-between these two sets: this is best illustrated through discussions of specific examples, which I provide below.

Recall from §4.2.2 that network density is calculated by the following formula:

$$D = \frac{100 N_a}{N} \%$$

where  $N_a$  is the number of actual links, and  $N$  the number of possible links, between those people in the network under consideration. Applying this formula to the South Gosforth groups, for the older males and females,  $D = 70\%$ ; for the younger males and females,  $D = 100\%$ . We could clearly expect the younger networks to be strong norm-enforcement mechanisms, and even the score for the older groups is relatively high. However, it would be difficult to argue that these networks form territorially-based clusters, in the sense employed by Milroy (1987b: 141) or Milroy (1992: 104), where people have close relatives living on the same street or on the next street. There were sets of relatives recorded as part of the networks, as shown in Table 4 below, but these relatives lived within about a five minute drive from each other, and although they met on a regular basis, the territorial nature of the Belfast networks was in part absent from these South Gosforth groups. Nevertheless, there was a degree of territoriality in sectors of the networks. As detailed further in §5.1.2 below, all the recordings took place in Ronnie and Robbie's house. Neil was born in that house, before his parents moved to the house opposite and Neil and Theresa were currently living with Neil's mother in that house, before a move to another country. Vince and

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<sup>3</sup> For a definition of network density and multiplexity, see chapter 4, §4.2.2.

Eileen were Ronnie's next-door neighbours; and Fiona lived a couple of doors up the street. Of the twenty informants interviewed in total, then, seven lived in the same street. Within the young male networks, Colin and Steve were flatmates, and at the time of recording, Tom was about to flatshare with them; Robbie still lived at home with his parents, as did Fiona, Monica and Melanie in the young female networks.

Table 4

Family relationships within and between the networks

<i>Husband and wife</i> Vince - Eileen; Neil - Theresa; Clive - Doris; Scott - Florrie
<i>Parent and child</i> Ronnie - Robbie; Nancy - Monica and Melanie
<i>Siblings</i> Nancy - Florrie; Monica - Melanie; Kevin - Steve
<i>Aunt/uncle and nieces</i> Florrie/Scott - Monica and Melanie

Such characteristics of the density of the South Gosforth networks would thus suggest a pattern identical to neither the inner-city nor the outer-city Belfast communities, but somewhere in-between.

In terms of the multiplexity of these network links, it would be fair to say that they are closer to those of the Braniel/Andersonstown communities than those of the inner-city Belfast communities. It was rarely the case that the informants within the networks knew each other in capacities other than friend and neighbour. There are some exceptions to this: Kevin and Steve, who were brothers, were also workmates in a financial company in the area, and this company also employed Monica on a part-time basis; Sam and Fiona were also working together, but only on a temporary summer job. More crucially, what seemed to characterise many of the informants was a degree of mobility; most of the older informants, as noted above, had been born in traditionally working-class areas of Newcastle, before settling in South

Gosforth, while some of the younger informants, though born and raised in the area, had worked or studied away from the area - Robbie had worked as a salesman in Tenerife, while Kevin and Fiona had studied in Glasgow and London respectively, though all three had decided to move back to the area. This will be crucial for an understanding of potential influences on speech patterns, since social mobility of this type can lead to increased standardisation (cf. Trudgill 1986, Milroy 1992).

In summary, the speaker selection for this project was based on entering a series of interlocking social networks, members of which I had known for a great many years, all of whom were totally at ease during the recordings. While a network strength score has not been calculated, since this is difficult to operationalize with higher-status communities, the social characteristics of the networks suggested that, in terms of density and multiplexity, the structures in South Gosforth were not isomorphic with either the high-status or the low-status communities in Belfast, though the patterns were closer to those of the former. In terms of overall structure, each of the four networks were very similar to each other, with a general mix of relations, friends and neighbours (not always as non-discrete groups). Another defining characteristic of the informants was their general social and geographical mobility. It might therefore be expected that there are two (potentially) opposed forces at play on the speech of these informants: the pressure to conform to group norms on the one hand, and standardising influences through a range of contacts with speakers outside the networks on the other. The opposition comes into play only if the group norms tend towards use of the vernacular: if the speech of the members of the networks is generally standard, that is, if the group norms themselves dictate an adoption of standard variants, then there is no opposition. But given the social characteristics of many of the informants, we can expect there to be a degree of conflict between use of the local vernacular and the (local) standard. The linguistic ramifications of this conflict are to be examined in this and the following chapter.

### §5.1.2 *Constraining the circumstances of the recordings*

The research was presented to the informants as an attempt to find out more about the relationship between language and the community, particularly whether the language of the community had changed over the years (specifically with the older networks) and also whether significant differences existed between the way men and women talked. All the recordings took place in one home, that of Ronnie and his wife, a regular gathering point for most members of all the networks, so that the surroundings were very familiar for the informants. Although I had known most of the informants as a friend of a friend for many years, I decided not to record a number of shorter sessions, but rather recorded one session per network for a number of hours, resulting in approximately fifteen hours of recording. Using only one session is a departure from normal practice in sociolinguistic studies which involve participant observation, and clearly has its drawbacks, the most obvious of which is the extent to which such a recording could really be seen to overcome 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 this data by systematic observation" (Labov 1970: 32). Yet the recordings collected, taken as a whole, are genuinely of a very high quality, with the informants all very much at ease with being recorded. Some sessions were better than others: the older networks required more input from me as the fieldworker, though even in these cases my contribution was not great; some of the younger females had previously taken part in a recording for an A-level English Language project, so were used to the recording equipment; and in the younger male group, my input was practically zero. Occasional reference to the fact that the sessions were recorded were made by the younger males, specifically when the discussion turned to illegal and sexual activities, but all of the informants knew that the tapes were confidential and would not be heard by anyone but me. A larger sociolinguistic project would ideally have

recorded more data from these groups, and from others in different areas of Newcastle; but the data collected for this project suffices to make certain claims about aspects of phonological and syntactic variation - and their sociolinguistic correlates - and how this relates to linguistic structure, and practicalities (in terms of time and manpower) meant that fifteen hours of recording were all that were feasible for the purposes of this thesis.

A potential problem with having just one fieldworker for a variety of different social groups is mentioned by Milroy (1987a: 79-81), concerning the social role played by that fieldworker. There is a general consensus (though cf. Milroy 1987a: 81 for an important rider to this generalisation) that the more social attributes the fieldworker shares with the informants (in terms of age, sex, and occupation, for instance), the more likely he or she is to be successful in breaking down certain social barriers, with a concomitant greater likelihood of a successful recording. This was a potential problem for me as a young(ish) male, no longer resident on Tyneside; and clearly the fieldwork had to be carried out by me alone as part of the thesis, since gaining the experience of sociolinguistic data collection is an important part of the project. But in reality such problems were overcome by the fact that many of the informants in each of the four networks knew of me prior to the recordings, and that I was obviously familiar to the area (cf. Hudson 1996: 159); and the fact that the recordings took place in groups of five, with regular interruptions from neighbours calling round, meant that my presence during the sessions was at best overlooked entirely, and at worst marginalised. Furthermore, the fact that only one fieldworker was used might also be considered as a means of constraining the circumstances of the recordings, since it keeps the role of observer constant in each session. More details of the recordings are given in §5.2.

### §5.1.3 *Selecting the linguistic variables*

One of the central features of the present study is that it attempts to examine (socio)linguistic variation at various levels of the grammar. Two broad sets of variables were chosen to represent these levels. The phonetic and phonological variables are (p) (t) and (k): aspects of variants of these variables were discussed at length in chapter 3. The classification of (p), (t) and (k) as phonetic *and* phonological variables stems from the distinction made in part by Hudson (1996: 170). He classifies (t) as a phonetic variable on the grounds that the range of its variants, such as [ʔ], [ɾ], [ɻ] and [t], among many others, all function as pronunciations of the same phoneme. He contrasts this, however, with phonological variables, “where the same lexical item has alternative phonological structures” (Hudson 1996: 170), and cites examples such as alternating stress patterns on words like *controversy*, and the (h) variable affecting the lexical set containing *hot* and *horrible*. But as the discussion of glottalling and glottalisation in chapter 3 showed, it is clear - in certain environments at least - that variants of the voiceless stops can indeed occur in varying phonological structures (in terms of their syllabification) in TE, and sociolinguistic research by Docherty *et al.* (1997) has suggested that this variation is socially sensitive. The purpose of the present study is to investigate

- (a) the range of phonetic variants of the voiceless stops in certain environments in the speech of the South Gosforth informants;
- (b) the implications for the syllabification of these variants in certain positions;
- (c) the extent to which the sociolinguistic distribution of the variants - in terms of both (a) and (b) above - correspond with the patterns established by Docherty *et al.* (1997).

The second set of variables works at the morphological, syntactic and semantic levels of the grammar. The variables to be considered in this regard are modal verbs. An



attempt to define criteria to establish a core set of modal verbs in standard English, along with the syntactic and semantic characteristics of such a set, was presented in chapter 2; the present study will attempt to investigate:

- (a) variation in the use of modal verbs, comparing the results with patterns of use in both standard English (as established in chapter 2) and existing surveys of Tyneside English (Beal (1993); McDonald (1981));
- (b) the implications of such variation for aspects of syntactic theory;
- (c) where possible and relevant, the sociolinguistic distribution of the variants.

Finally, an attempt will be made to compare the patterns of variation of both 'sets' of variables, to see whether the phonological and grammatical variables correlate with social variables in the same way; this entails a broader discussion of the role of sociolinguistic data in formal linguistic theory.

A real problem exists when attempting to establish what constitutes a variable at certain levels of the grammar. Purely phonological variables (cf. Romaine (1980)) seem to present few problems for the quantitative paradigm established by Labov (1966). For instance, the variation between the alveolar and velar variants of (ng) are widespread and salient throughout many English dialect areas. In any given text, it is possible to identify a range of variants of (ng): the number in the range will be finite, and the variants themselves salient to the researcher, if not to the informant. The quantification of each of the variants of such a discrete variable might then be presented as a percentage of the total number of variants noted, whether for each speaker or for each group (cf. Hudson 1996: 177-81). But the application of this seemingly straightforward procedure to other types of linguistic variation is more complex.



Consider the utterances in (1) below:

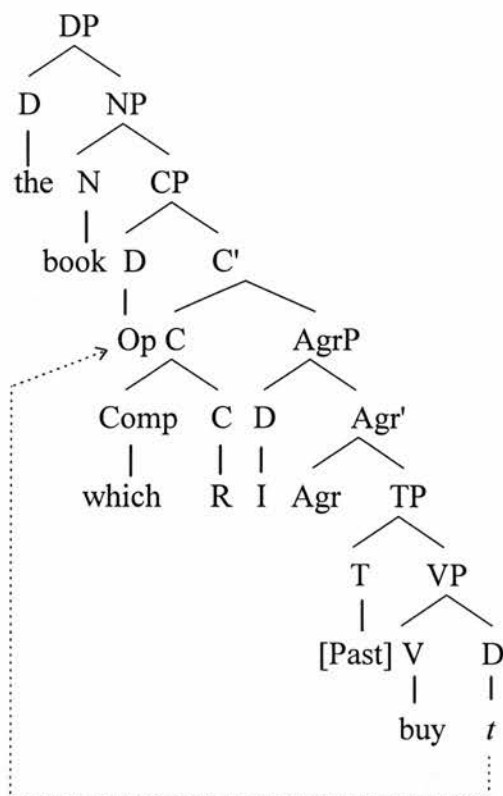
(1) *John may kiss Sandra*

*John can kiss Sandra*

Imagine an (admittedly unusual) piece of spoken discourse in which these utterances appeared 3 and 97 times respectively. How is the researcher to analyse such variation in modal verb usage, and what potential problems exist for such an analysis?

Firstly, there is a problem in establishing the level of the grammar on which such variation is taking place. Hudson (1996: 171) describes a syntactic variable as one where the same meaning is expressed by different syntactic structures, and gives examples concerning relative clause markers such as *which*, *what*, *that* and zero, as in *the book which/what/that/Ø I bought*. But the extent to which these represent different structures is not clear. One possible analysis of *the book which I bought* is given as (2) overleaf:

(2)



where Op is a null relative pronoun operator, and R a null relative clause affix (cf. Radford 1997: 307). In such instances, Op has a head feature [rel], and R a specifier feature [rel]: movement of Op to spec-C' is motivated by enlightened self interest (Lasnik 1995), where constituents “move in order to satisfy the morphological requirements of other constituents” (Radford 1997: 506). But the structural representation of *the book what I bought*, *the book that I bought* and *the book I bought* would be identical to the structure presented above. In each case, a Comp (*what*, *that* or  $\emptyset$ ) merges with the null relative clause affix R whose specifier feature is checked against the head feature of Op. The variation here is not structural; it depends only on the extent of the range of elements which can appear under Comp in such structures. This is a very theory-specific point; in order to claim that such variation is or is not structural depends on the theory of linguistic structure adopted.

For instance, a Word Grammar analysis of relative clause *I bought* in the construction *the book I bought* is presented by Hudson (1986: 1068) as follows in (3):

(3) (adjunct of noun) is (a ((tensed verb)((whose visitor) is (a noun)))

The term 'visitor' is applied to a constituent which is not in its normal position by virtue of some sort of extraction (here, *wh*-movement); crucially, the structure of the relative clause is "all done without reference to any expressions longer than single words"; and a relative clause which contains a relative pronoun of some sort will clearly be structurally different in this theory of grammar. Hudson (1986: 1069) goes on to explain how such a theory of language structure can accommodate social constraints as part of the structural representation of utterances, which is clearly advantageous for the development of a theoretical model which can account for sociolinguistic phenomena. These issues are considered further in the final chapter. However, it is important to note here that our understanding of what constitutes an instance of structural variation is unambiguously allied to whichever theory of language structure is being adopted.

However, returning to our first example, at a semantic level, it is not clear that the two utterances in (1) are indeed expressing the same meaning: out of context, *may* is indeterminate between an epistemic and root reading, while *can* is clearly root. The importance of semantics - particularly the concept of semantic equivalence - has for many years been at the centre of the debate about syntactic variation, and I do not propose to rehearse that whole debate here<sup>4</sup>. The crux issue is the extent to which we can apply the concept of the linguistic variable to syntactic variation (Lavandera 1978, Cheshire 1987). There is, as Milroy (1987a) and Romaine (1984) point out, a significant problem in attempting to constrain the number of syntactic variants which can be said to be semantically equivalent ways of doing or saying the same thing. Lavandera (1978: 175) argues "*laughing* and *laughin'* or [ga:d] and [go:d] can more convincingly be shown to be used to say referentially the same thing

<sup>4</sup> For a fuller discussion of the issues, see Milroy 1987a: §7.5 and references therein.

than any pair of postulated synonymous syntactic constructions such as *the liquor store was broken into* versus *they broke into the liquor store*". Weiner and Labov's (1983: 30) claim that "active and passive normally have the same meaning in a truth-conditional sense" cannot be upheld given pairs of constructions such as (2) below:

- (2) Every boy kissed some of the girls  
       Some of the girls were kissed by every boy

More problematic is the relationship between standard and non-standard forms in this regard (Harris 1984, Milroy 1987a). In the claim that standard and non-standard variants are semantically equivalent lies an even more controversial claim that both the standard and non-standard lect share a structurally identical grammar. On this traditional sociolinguistic approach to syntactic variation, Harris comments

Of necessity the model encourages an atomistic view of variation, whereby apparently alternating standard and non-standard forms are studied in isolation from other forms in the grammatical subsystem. This 'worm's eye' approach encourages the impression that differences between the standard and a particular vernacular are merely superficial and tends to obscure whatever deep-seated divergences there might exist between the two varieties.

(Harris 1984: 304)

The analysis of grammatical variation in this thesis attempts to bear these issues in mind; indeed, one of the central areas of investigation is the extent to which sociolinguistic material can be used to examine structural variation between standard and non-standard forms. But the overall ethos of the present approach most closely follows that of Cheshire (1987) who rightly argues that too little is known about the nature of syntactic variation even to begin to worry about the application of the concept of the sociolinguistic variable to explain specific patterns. Cheshire advocates an approach which attempts to examine what types of syntactic variation exist. Until this is achieved, efforts to establish sociolinguistic correlates may prove fruitless. The method adopted in this thesis, then, is first to establish whether a system of variation in modal verb usage exists within the speech of the informants;

attempts to find trends in this variation which seem to indicate co-variation with extralinguistic variables will also be made, but always bearing the caveats discussed in this section in mind.

### §5.2 *Collecting the texts*

Each of the groups discussed in the previous section was recorded once only, in sessions ranging from three to four hours. That each group was recorded once only is a disadvantage: it would have been profitable to have recorded the groups on a number of occasions, to have been able to collect more data to process, but the limited time and resources available to me meant that this was not possible. The amount of data recorded is particularly a problem when it comes to an attempt to analyse syntactic variation, as is well-documented (cf. Labov 1972; Cheshire 1982; Milroy 1987a; Miller and Cann 1994; Cornips 1998); this issue is discussed in more detail in §5.3. That each group was recorded for over three hours is a distinct advantage: the length of recordings allowed for a range of different topics of conversation to emerge, and for speakers to settle into something approximating their normal interactional style (cf. Douglas-Cowie 1978).

Milroy (1987a: 79) refers to a paper by Labov (1981a: 4), in which he argues that the technical quality of participant-observation recordings is often of poorer quality than that obtained from a traditional interview method. I did experience certain technical problems - the session with the younger males ended after about four hours when another group returned to the house from the pub and started singing and playing an accordion; and clearly, when a number of speakers are all talking at the same time, it is not always possible to analyse all of the data - but such interference is a minimal drawback given the other advantages of participant observation discussed above. Normal domestic background noise and regular interruptions by third parties are part and parcel of this method of data collection, and

were not significant problems for the purposes of the present study. A further problem was the amount of unanalysable data in a small section of the recording with the younger females, where two of the informants had begun speaking to me about Edinburgh, while the other three were discussing another topic. Because the tape recorder was closer to the former group, it was not possible to transcribe material from the other conversation. But in practice this was not a significant problem, as it only occurred on this occasion, and even then only lasted for about ten minutes.

In terms of general atmosphere, it is genuinely the case that all of the informants were very relaxed about being taped, with speakers coming and going over the period of the recording, and regular interruptions from others. Many of the informants wanted to know what the purpose of the project was, and some speakers wanted to know if I had any specific questions to ask. This varied from group to group: the younger males asked what the project was about initially, but then immediately began recounting a story about what had happened in the pub the previous night, while the older females seemed more anxious to provide me with the 'right' sort of information for the project. I attempted to avoid direct questions as much as possible to avoid the atmosphere of an interview; and even when such questions arose, the fact that the sessions were not one-on-one interviews meant that much of the discussion of the topics was carried out *between* the members of the groups rather than between me and the group.

#### §5.2.1 *Elicitation tests*

For the grammatical variables, I decided not to attempt any elicitation tests, as the format of the tests results in utterances which are not representative of speech used in everyday conversational interaction, and would have been markedly out of place in the participant-observation setting described above. While such tests are a valuable means of filling the gaps in corpora where few or no examples of a given



construction occur, there is the potential “mismatch which sociolinguists have consistently found between what speakers *claim* when they are directly questioned, and what they actually *do*, as evidenced by their linguistic behaviour in naturally occurring conversation” (Milroy 1987a: 149).

A modified elicitation test was attempted for the phonological variables, which, it was hoped, might be a useful method of examining stylistic variation. The traditional model, implemented by Labov and used to investigate a range of speech styles, divides the style continuum initially into two broad categories: conversational style and reading style. As Milroy (1987a: §8.2.1) argues, there are a number of problems, both practical and theoretical, concerning this division, and more crucially, concerning the notion that conversation and reading aloud are indeed on the same style continuum. On a practical level, Milroy notes that in the Belfast surveys, a number of the inner-city informants were semi-literate at best. These speakers felt awkward at the prospect of the reading passage or word list, resulting in “a halting, dysfluent manner” (Milroy 1987a: 173) of speech. Such a scenario is clearly counterproductive to the whole exercise. On a theoretical level, the concept of the style continuum fails to consider a range of psycholinguistic issues<sup>5</sup>. It assumes that the mental processes of the production of a word in conversation are identical to those when reading, and this is by no means clear. It does not consider the issue of “grapheme to phoneme mapping” (Milroy 1987a: 176) in the construction of the phonological shape of the written word. It also fails to address the potential differences between the phonological production of written words in isolation (in word lists and minimal pairs) and in connected prose (in the reading passage). In short, there is a danger that the traditional view of stylistic variation is an oversimplification: “recognizing and producing words in conversation is a very different activity from recognizing and reading aloud written words” (Milroy 1987a: 178). I had hoped to circumvent these problems, while still investigating some

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<sup>5</sup> Milroy (1987a: §8.2.1) provides a much fuller discussion of these issues.



aspects of style shifting, by using a series of photographs. I had taken a series of photographs of a range of (what I thought were) well-known buildings and monuments in and around Newcastle, and the names of each of these constructions contained the phonological variables under investigation in a range of phonetic contexts (for instance *Civic Centre* (k): V \_\_\_ # true C<sup>6</sup> and *St. James's Park* (k) \_\_\_ Pause). Some of the photographs were of more obscure landmarks (for instance, Bessie Surtees' House on the Quayside) or of sites further afield than central Newcastle (for instance, the Stadium of Light in Sunderland or the Souter Lighthouse at Marsden), but most were in central Newcastle. The aim of this part of the recording, carried out at the end of each session with each informant individually, was to examine how a different setting (the informant recorded alone) and a different style (elicitation rather than natural conversation) might affect the speech of the informant - *but crucially without any recourse to the written medium*.

At the experimental design stage, this had seemed like a good idea, but there were a number of unforeseen problems. Some informants had not brought their glasses and found it difficult to see the photographs clearly, while others, who could see well enough, did not recognise the buildings. This had two adverse effects: the first was that it did not provide sufficient data for a satisfactory comparison across all of the groups recorded; the second was that some of the informants were surprised and slightly embarrassed that they had not recognised certain areas of their city. While in many instances the photographs prompted discussion of the area and attitudes towards Newcastle, which was both interesting and informative, this part of the recording sessions did not work anywhere near as well as I had hoped, and the types of data gathered for each informant are so variable that no meaningful comparisons could be drawn. It would have been possible to consider results for a small selection of words, but this was not what was intended.

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<sup>6</sup> See above §3.1.1 and below §5.3.3.

The recordings were made using a Marantz PMD201 cassette recorder. Only rarely did any of the informants make reference to the tape recorder. The amount of input to the sessions by me varied from group to group; generally, though, my input was low, and in the case of the young males, practically non-existent.

### §5.3 *Identifying the linguistic variables and their variants in the text; processing the figures*

#### §5.3.1 *Grammatical variation*

At many points in the following discussion reference is made to McDonald (1981), a PhD thesis which investigated modal verb usage on Tyneside. McDonald's methodology was very different from the method adopted in this thesis, and is summarised in McDonald and Beal (1987: 45-6). Her corpus (150,000 words each of 'Tyneside' and 'non-Tyneside' speech) consists of a range of different types of data. The non-Tyneside material is an eclectic mix, gathered from the *Survey of English Usage*, non-localised speakers who took part in the *Tyneside Linguistic Survey*, and radio programmes; the Tyneside material comes from spontaneous conversation with "relatives, friends and neighbours", and "conversation between speakers on a less intimate footing" (McDonald and Beal 1987: 45). To supplement this data - to "provide invaluable nuggets of information which are maddeningly absent from even the largest corpus" (McDonald and Beal 1987: 45) - reference is made to attested utterances<sup>7</sup> and the results of elicitation tests. Overall, this allowed McDonald to analyse a larger number of utterances containing modal verbs than is the case in the present study, since her method did not require her to analyse only the forms which occurred *naturally* during tape-recorded sessions. She was able to note down any examples she heard uttered by Tyneside speakers, as well as using examples of

<sup>7</sup> Attested utterances are utterances which did not occur in the speech of the informants during the course of recording, but which informants considered to be characteristic of the vernacular, forms they had heard from local speakers.

speech believed to be characteristic of Tyneside English, even if it was not used by the informant who provided the example. For instance, McDonald (1981: 186ff) discusses the use of double modal constructions in Tyneside English, and cites a range of examples which she includes in her Tyneside corpus. However, only *one* instance of a double modal construction actually appeared in the speech of one of her informants (*Oh no! They're double-glazed. They wouldn't could [break]*). The rest of the examples are all attested utterances. Furthermore, the social characteristics of the speakers (whether part of the core sample, or those who produced the attested utterances) were not made explicit, and no sociolinguistic correlations were attempted. It is therefore not possible to compare the data in McDonald (1981) *directly* with those presented here; however, patterns of modal verb usage which were considered to be characteristic of Tyneside English by McDonald will be made use of where relevant.

#### §5.3.1.1 *Can*

As is well documented (McDonald 1981, Beal 1993), *can* in Tyneside English is used as a means of expressing deontic possibility (permission) to the exclusion of *may* (cf. §5.3.1.3 below). No instances of permission *may* occurred in the corpus for this thesis, while many instances of deontic *can* could be heard:

- (3) *Older males*  
 Can I tell you when the rot set in?  
*Older females*  
 You can call her Louise cos that's her name  
*Younger males*  
 you can leave now Dave  
*Younger females*  
 You can have which room you want

In addition to its use as a deontic marker, *can* has another function (in both Tyneside and Standard English) as a modal of dynamic possibility, as witnessed in the following:

- (4) *Older males*  
 I can see what Neil's getting at  
*Older females*  
 I honestly don't understand how that can work out more expensive  
*Younger males*  
 I like the fact that we can look out of wor window and say Peter get  
 the beers in  
*Younger females*  
 at least you can sort of see why Chris Evans is famous

The total number of instances of *can* is given in the table below:

**Table 5**  
**Total instances of *can* by group**

Modality	Older males	Older females	Younger males	Younger females
Deontic	4	14	22	16
Dynamic	37	44	38	54

The preference of *can* over *may* as a root modal in this variety of English is but one stage of a long semantic rivalry between the two (Traugott 1972) during which *can* has gradually encroached on the various semantic fields of *may*. In Old English, *mæg* had an 'ability' meaning, as instanced in the following from Denison (1993: 308):

- (5) Ac þæt hie magon þæt hie þas tida leahtrien  
 but that they have-power that they these times blame (SUBJ)  
 'but all they can do is blame the times'

In the transition to Middle English, it is possible to find evidence of *cunnen* undergoing some kind of semantic shift from 'know' to 'have-ability':

- (6) þatt I shall cunnen cwemenn Godd  
 that I shall have-ability please God  
 'that I shall have the ability to please God' (Denison 1993: 310)

This dynamic sense of *can* has continued to the present day, as is shown in (4) above. The OE use of *may* in (5) above has ceased to be productive in present-day English, but in ME a 'permission' sense of *may* developed, as in:

- (7) and wiþ her feyned disputacions and false exposicions scleen it in  
and with their feigned disputacions and false expositions destroy it in  
hemsself and in opere as miche as þei may  
themselves and in others as much as they may (Denison 1993: 304)

while Traugott (1972) notes that the deontic meaning of *can* only developed in Modern English. In other words, the increase in the range of modalities expressed by *can* mirrors the decrease in the range of modalities expressed by *may*. In §5.3.1.3 it will be seen that *may* (in the data collected for this thesis) is very rare even as a marker of epistemic modality, and it will be interesting to see, in future studies of modality in TE, whether *can* comes to be used epistemically. If it does, it will have gradually taken over from *may* as a marker of dynamic, deontic and epistemic modality. In his discussion of Standard English modality, Palmer (1990: 72) states that it “would even be possible to suggest that CAN is, in fact, never deontic in its basic meaning, that the only deontic possibility modal is MAY”. Evidence from the use of *can* by these speakers suggests that this is not the case for TE; and the discussion of *may* in §5.3.1.3 provides further evidence against the application of Palmer’s claim to the expression of deontic possibility in TE.

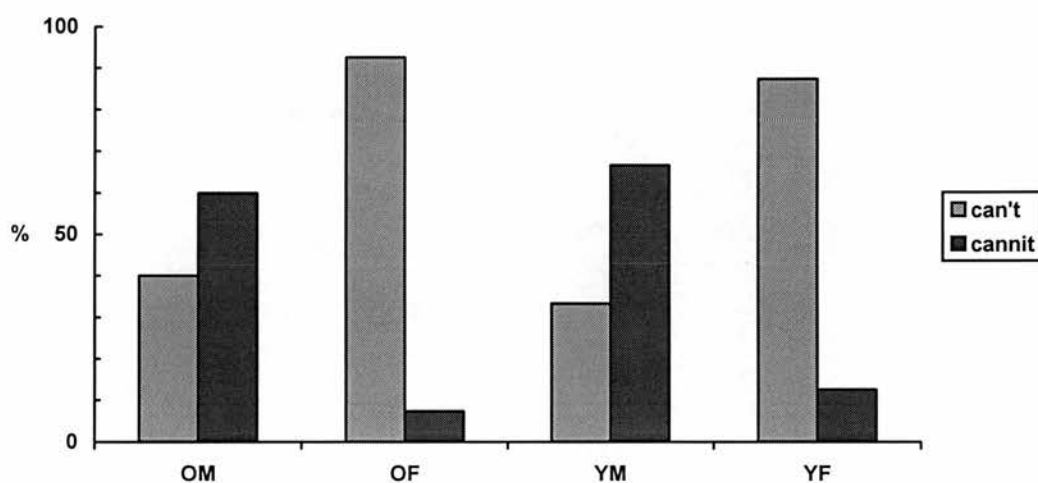
A further distinctive characteristic of TE (cf. Beal 1993: 199) is the uncontracted negative form of *can*, [kanət], which I represent orthographically as *cannit*, to distinguish it from the standard uncontracted form *cannot*. Denison (1993: 309) states that the “primary pattern of negation has changed from OE *ne* + finite verb to ModE operator + *not*” and provides the following information about the history of modal negation:

Contraction of a modal with a following *not* is first recorded from the fifteenth century with assimilation or elision of part of the modal. Forms showing loss of the vowel in *not* are first recorded at the very end of the sixteenth century. The dates of earliest occurrence in the following lists are in some cases considerably earlier than those usually cited ... 1597 *can't*

He later argues that negative sentences in English “sort the lexical sheep from the operator goats” (Denison 1993: 448). Crucially, though, the uncontracted form *cannit* in TE is a highly localised form, distinct from the standard variant *cannot* and other non-standard variants (cf. Scots *cannaie* [kane]) The total number of variants for each is illustrated in the chart below:

Graph 1

Negative variants of *can* by group<sup>8</sup>



The distribution of these variants in the corpus is shown in the tables below with scores correlated with age and sex:

Table 6

Negative variants of *can* by age

Variant	Older	Younger
<i>can't</i>	33	44
<i>cannit</i>	14	23

<sup>8</sup> In this and other graphs, OM = older males, OF = older females, YM = younger males and YF = younger females.

Table 7

Negative variants of *can* by sex

Variant	Male	Female
<i>can't</i>	17	60
<i>cannit</i>	30	7

Although the frequencies here are very small given the number of informants in each age and sex group, it is nonetheless possible to apply statistical tests<sup>9</sup> to try to establish trends which could then be followed up in further research (cf. Cornips 1998). Applying the  $\chi^2$  test to these 2x2 contingency tables, the score for age is 0.2598, which is not significant, while that for sex is 35.91, which is very highly significant ( $p < 0.001$ ,  $df = 1$ ). It is possible to suggest that negative variants of *can* function as gender-markers, with females favouring a supra-local, standard form, and males a local, vernacular variant.

§5.3.1.2 *Could*

I begin this section with a lengthy quotation from Palmer (1990: 185) on problem cases concerning *can* and *may* in their conditional uses:

There are some instances of CAN in the Survey that look as if they are epistemic:

This could be the all important round. (S.10.3.99)

Well, now we're coming to this big fight of the evening described in the programme right here in front of me as 'Eliminating World Heavyweight Contest'. Well, it could be that, and it probably is. (S.10.3.1)

The banging on the ceiling could have been water in the pipes or the central heating or something. (S.5.8.39)

<sup>9</sup> I am grateful to the Statlab in the Department of Mathematics and Statistics at the University of Edinburgh, and to Astrid Scheppman, research associate for statistics and experimental design in the Department of Linguistics, University of Edinburgh, for their advice on which statistical tests to use. I am also extremely grateful to the creators of the following web-page, for their on-line chi-square calculator - [http://www.georgetown.edu/cball/webtools/web\\_chi.html](http://www.georgetown.edu/cball/webtools/web_chi.html) - which has saved me many hours of complex calculations.



These all appear to refer to what is conceptually possible, and it might be thought that in all cases *could* is replaceable by *might*. But it can equally be argued that they indicate what would be (or have been) experientially possible, and so are dynamic. In the context, there is little difference between conditional dynamic and non-conditional epistemic modality: what would be possible in the world of experience is also conceptually possible for the speaker. There is a near equivalence between 'It would be possible for x to occur/have occurred' and 'It is possible that x occurs/occurred'.

Certain such problem cases occasionally appeared in this corpus:

- (8) *Older males*  
 There's a chance that he could be kept on then?  
*Younger males*  
 she could have been mine

Such problem cases are rare. Yet, if we follow Palmer's classification of such forms as dynamic rather than epistemic, it is possible to make the following generalisation about the semantics of *could* for these Tyneside speakers: the only modality expressed by *could* is that of dynamic possibility. Very frequently, there is a clear past time reference, as can be seen in the following set of examples:

- (9) *Older males*  
 we bought a house in 1968 because we couldn't find one to rent  
*Older females*  
 you couldn't see in here for beer  
*Younger males*  
 all you could hear was Jimmy Hendrix  
*Younger females*  
 some kid who did a swimming gala but couldn't actually swim.

Also very frequent were hypothetical (often counterfactual) or tentative uses of *could*:

- (10) *Older males*  
 it could be the Vaux Tavern  
*Older females*  
 it could be quite close to St. Mary's Island  
*Younger males*  
 you could tattoo a little face on it  
*Younger females*  
 you could spend a lot of time doing something more worthwhile

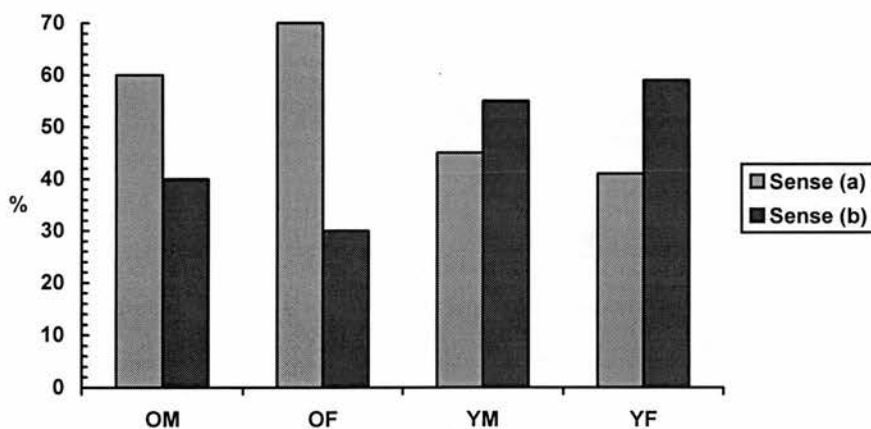
and *could* in combination with *have* + *past participle* also functions to mark both past time and unreality, as can be seen in the following:

- (11) *Older males*  
 he could have been made redundant  
*Older females*  
 we could have done it  
*Younger males*  
 she could have been mine  
*Younger females*  
 it could have been worse

The past tense form on its own (that is, when it is not followed by *have* and a past participle) has therefore two specific functions or 'senses': sense (a) as a marker of past time; and sense (b) as a marker of unreality, or tentativeness. The graph below shows the number of instances of each for each sense:

Graph 2

Percentage of occurrences of 'past time' *could* (sense (a)) and 'unreal/tentative' *could* (sense (b)) by social group



The numbers involved are small, but still large enough to apply the  $\chi^2$  test to establish whether there is significant correlation between different senses of *could* on the one hand and age or gender on the other. The correlation with age is very highly significant ( $p < 0.001$ ), but not significant with gender. This would suggest that the use of the past tense form *could* as a marker simply of past time is in fact a characteristic of older speakers, with younger speakers favouring *could* as a marker

of unreality or tentativeness (that is, where it has lost any sense of temporal reference).

### §5.3.1.3 *May*

Instances of *may* were extremely rare in the speech of the informants: no examples of *may* were found in the speech of the older males or younger females, and only two instances occurred during the recording of the older females:

(12) We may be erased by the end

(13) if you gotta park all the way down there, you may as well get the Metro  
and one during the recording of the younger males:

(14) she may as well drop one off at the flat

(12) is an instance of epistemic possibility, while (13) and (14) show collocation of the modal with the AdvP *as well*; Hoyer (1997: 275) considers this collocation to be found “in idiomatic expressions typically used to make a circumspect or sardonic recommendation”.

The rarity of *may* in this corpus is linked to a number of factors, some of which are characteristic of PDSE as much as TE. For some, possibly many, speakers of PDSE no contracted negative is possible, particularly if *may* is expressing deontic modality. Nor does *may* appear in epistemic questions. Coates (1983: 147) notes that work on child language and regional varieties of English indicates that “*might* is superseding *may* as the main exponent of Epistemic Possibility”. Denison (1992), in a paper on counterfactual *may have*, refers to a small study in which he assesses the acceptability of the construction. Several of the informants considered *may* to be “‘old-fashioned’ or ‘formal’ or ‘written, not spoken’”. There are, then, both language internal and language external factors which might help to explain the rarity of the form. The formal irregularity (relating to negation and inversion, as noted above) is

coupled with a possible avoidance of a stylistically (and socially) marked form, leading to an (at best) sporadic use of this modal.

#### §5.3.1.4 *Might*

*Might* functions as an expression of epistemic possibility across all of the groups, with no significant differences in terms of age or sex. Examples of epistemic possibility *might* are given in (15) below:

- (15) *Older males*  
 You might as well gan and put it back on the shelf  
*Older females*  
 we might have time to fit something else in before we get the train home  
*Younger males*  
 these might welcome you to the family  
*Younger females*  
 think I might go out of my head

Palmer (1990: 58) suggests that *might* is used “exactly as *may* is. It merely indicates a little less certainty about the possibility”. This is not the case in TE (cf. McDonald (1981:281)), where *might* is not just a tentative form. Instead, given the increasing obsolescence of *may*, as discussed in §5.3.1.3, *might* functions as the main marker of epistemic possibility in this variety. It *can* be used tentatively, but is not restricted to this function. McDonald’s (1981) non-Tyneside corpus reveals a roughly 50:50 split between uses of *may* and *might* as markers of epistemic modality<sup>10</sup>, whereas the Tyneside corpus shows a 90:10 preponderance towards *might*: this is almost identical to the findings of the present study, which shows 91% use of *might* as opposed to 9% use of *may*.

We can therefore suggest that the gap left by the increasing obsolescence of *may* has been filled by two separate verbs in TE. While in the standard variety, *may* functions as both an epistemic and a deontic modal, on Tyneside epistemic possibility is expressed with *might*, while deontic possibility is expressed with *can*.

<sup>10</sup> McDonald (1981) does not specify whether or not any such instances might be classed as tentative.

And while it is possibly the case (cf. Coates 1983, Denison 1992) that *may* is *gradually* becoming obsolete in the standard variety, its demise is more marked in this non-standard dialect, as both the present study and McDonald (1981) indicate. Furthermore, findings from the present study indicate that this development is well established in all of the four social groups investigated: there is no firm evidence of this being a new or vigorous change in the community. It might therefore be possible to suggest that the change affecting *may* in standard English is the result of a change from below, with non-standard varieties leading the way in the marginalisation of *may*. Clearly further investigation would be needed to establish whether this change is as marked in non-Tyneside non-standard English; but the evidence from the present study would seem to suggest that, even in a relatively high-status community such as South Gosforth, the obsolescence of *may* and the increasing semantic scope of *might* and *can* is pretty well established, to a greater extent than is the case in the standard variety. Beal (1993: 191) makes an important point regarding her discussion of grammatical variation in TE, namely that our understanding of the sociolinguistic distribution of the variants is limited. However, she goes on to cite examples of the double modal construction and a morphologically ‘irregular’ past tense formation uttered by “speakers higher up the social and educational scale than might be expected”. Certainly the evidence provided by parts of this thesis suggest that specific features of this variety of English, even though they are non-standard, can be characteristic of speakers from a high-status community.

#### §5.3.1.5 *Shall*

There were no instances of *shall* at all in the fifteen hours of recorded material analysed for this thesis. It is true that non-occurrence should not be considered as non-existence of the form within the grammar of the dialect, let alone the idiolect of each of the informants. However, McDonald (1981) notes that

instances of *shall* are infrequent in her Tyneside corpus also. She argues that *shall* is rare, but sometimes occurs in positive interrogatives, with first person subjects, which involve the volition of the hearer (1981: 100), thus suggesting that even in her large corpus, the frequency of *shall* is low and restricted by clause and subject type. The absence of *shall* in the grammar of TE is also noted by Beal (1993), and its absence from the grammar of Broad Scots is discussed by Miller (1993: 116f). Even in surveys of standard English usage, *shall* is reported as infrequently occurring. Hoyer (1997), basing his analysis on data collated from the *Survey of English Usage*, points out that after *ought to*, “*shall* is the least frequent of all modal expressions (3 per cent relative frequency) ... and operates as a formal suppletive of *will*” (1997: 120). In standard English, *shall* is often used as a marker of futurity, often with a sense of volition on the part of the hearer, and the variation between *will* and *shall* as markers of futurity is subject to prescriptive rules. These issues are discussed further in §5.3.1.10.

#### §5.3.1.6 *Should*

In standard English (Palmer 1990: 49, 81, 122) *should* functions as a marker of three types of necessity, epistemic, deontic and dynamic. This is not the case in TE: unequivocally non-root uses of *should* were very rare in this corpus. In two cases, the sense was indeterminate between an epistemic and a dynamic reading, even taking the context into consideration. My feeling is that the first is epistemic, and the second dynamic, but it is difficult to prove either way:

- (16) *Younger females*  
 this should be fun  
*Younger males*  
 she should have been swinging from the trees her like

In all other cases (49 in total), a root meaning is expressed:

- (17) *Older males*  
 I think the reporting should be stopped

*Older females*

we should go out and get to know more about the area

*Younger males*

that's who he should be going out with

*Younger females*

you should have done what I did

*Should* then seems to be a marker of root modality, a sort of 'weak necessity' in TE. An interesting pattern has therefore emerged when we consider *shall* and *should* alongside *may* and *might*. In both sets, the present tense form appears to have become obsolete in this variety (based on the data collected both in this thesis and in McDonald (1981)). This obsolescence is very possibly related to sociolinguistic factors: both *shall* and *may* are perceived as 'formal', almost hyper-standard forms, a sign of ultra-genteel speech. It might be expected, on these grounds, that the fact that these forms are clearly standard might lead to *increased* usage amongst what amount to relatively high-status speakers in the Tyneside community. However, it might also be the case that such forms are considered so 'posh' and/or archaic that they would be avoided even by lower middle class speakers (cf. Denison 1992). Specifically, they might not be considered to be characteristic of standard Northern English. But crucially, *shall* and *may* both seem to be increasingly obsolete in this variety, highly constrained in terms of morphosyntax (e.g. no contracted negation with root *may* (\**He mayn't come in*); no I-to-C movement with epistemic *may* (*May he be in his office by now?*) and meaning (only ever functioning epistemically in the case of *may*, on the rare occasions it arises in natural speech; and the same is true for *shall*)). This increasingly obsolescence has led to *might* and *should* becoming morphologically marked: they no longer form the past form of a 'modal pair' (cf. *will-would*, *can-could*), and in this regard, they pattern like *must* (which is itself the historical past tense form of OE *mot*). This morphological markedness seems to have had a repercussion on the range of modalities which these forms can exhibit: *might* has become the only marker of epistemic possibility in TE (following the demise of *may*), and has lost the 'tentativeness' which characterises its use in standard English,



according to the account presented in Palmer (1990), while *should* has become a clear marker of root modality, its expression of epistemicity being very rare in this variety. In other words, there is a clear interplay of sociolinguistics, morphosyntax and semantics in the use of these four modal forms in this variety. The hypothesis for this development is therefore as follows. An extra-linguistic force (a reaction to prescriptivism, and not necessarily the adoption of the ‘correct’ prescriptive form) has led to increased obsolescence of the present tense form of a pair of modal verbs; this obsolescence leads to the past tense form becoming marked, since it no longer forms part of a pair; and this markedness is concomitant with a restriction in the type of modality that is expressed. The validity of this theory would be strengthened if, in this variety, *must* is restricted in the range of modality it expresses, since we could then suggest a parallel development for the range of forms which fail to enter into a tensed pair<sup>11</sup> (namely *must*, *might* and *should*): once ‘isolated’ in this way, the forms can only express either epistemic or root modality, but not both. In §5.3.1.9, it will be shown that this is indeed the case for *must*, at least in the case of the female informants.

#### §5.3.1.7 *Will*

As discussed in §5.3.1.10 below, instances of *will* are very rare in the whole corpus. Other than (18) below:

(18) I think people with children will get that

every instance of *will* in the corpus occurred in one of the NICE instances, as the following examples show:

---

<sup>11</sup> In classification of *can* and *could*, for instance, as different tenses of a single lexeme, I follow Denison (1993: 337), who argues that although “there are merits in such approaches, there is sufficient evidence for treating them as different tenses of a single lexeme CAN, and historically that is the only defensible position to take”.

- (19) *Older males*  
 They **will not** speak to you (emphatic)  
*Older females*  
 you won't go anywhere  
*Younger males*  
 he won't have anyone tell him what he's gotta wear  
*Younger females*  
 I won't mind doin' the horror one again

This pattern was also found in the speech of the informants from Gateshead, analysed in Trousdale (1994). The pattern of negation with *won't* is an interesting one, specifically when it is contrasted with *'ll not*, as in (20) below:

- (20) *Older males*  
 I'll not tell you the answers  
*Older females*  
 Now Abbie'll not know one football team from the other  
*Younger males*  
 People tell me a secret, I'll not tell a soul  
*Younger females*  
 no recorded instance

Generally, *won't* tends to negate the 'factual conditions expressed in the clause', i.e. Halliday's (1970: 343) definition of modulation, whereas *'ll not* negates the assessment of probabilities the speaker gives, i.e. Halliday's definition of modality. That is to say, *won't* is the negative form of 'volitional' (root) *will*, while *'ll not* is the negative form of 'future' or epistemic *will*. On arguments for and against interpreting future *will* as epistemic, see Palmer (1990: 161-3), Jenkins (1972: 73), Huddleston (1976: 69) and Coates (1983: 177-83). In terms of the root-epistemic opposition, it seems logical to assign future *will* to the epistemic category, since both the *will* of futurity and the *will* of 'speaker's knowledge' are both centrally concerned with prediction. Palmer (1990: 163), however, suggests that in its strictest sense, epistemic *will* and future *will* are different, as evidenced by the following:

- (21) she will be arriving in JFK about now (epistemic)  
 she will be twenty next birthday (future)

The first relates to a judgement based on the speaker's knowledge of events, and is therefore clearly epistemic, while the latter lacks this sense of judgement. However,

the distinction is fairly minimal, and both are more like the other than either is to volition *will*. Further discussion of ‘*ll*’ forms - which far outnumber *will* or *won’t* forms - can be found in §5.3.1.10.

#### §5.3.1.8 *Would*

Patterns of use of *would* with all of the speakers seem very similar to the patterns which characterise standard English usage as documented by Palmer (1990) and Hoyer (1997), amongst others. *Would* appears very frequently as the modal form in the apodoses of unreal conditionals:

- (22) *Older males*  
 I wouldn’t have been here if Doris hadn’t reminded me  
*Older females*  
 if I didn’t know that was your house, there’s no way I would drive into your estate  
*Younger males*  
 If I was a lass and went back there like I would just walk out straight away  
*Younger females*  
 if they didn’t you would come out with it

As Palmer (1990: 155) and Anderson (1971: 84-5) both point out, in standard English, subject-oriented *would* can have a ‘past habitual’ sense, and more specifically, an implication that the action is iterative. This is also a characteristic of the form for some of these Tyneside speakers:

- (23) *Older males*  
 How many houses would you go to on a New Year’s Eve?  
*Older females*  
 Every year, I would order my turkey  
*Younger males*  
 (no recorded instances)  
*Younger females*  
 then they would die in their matchboxes

That this ‘past habitual’ use of *would* is absent from the speech of the younger males may be an indication that such a form is becoming old-fashioned and thus tending to

obsolescence: but generally, then, the uses of *would* for these Tyneside speakers largely conform to patterns for standard English.

#### §5.3.1.9 *Must*

The total number of instances of *must*, as a marker of epistemic and deontic necessity are given in the table below:

*Table 8*  
Number of instances of *must* by group

Modality	OM	OF	YM	YF
epistemic	11	7	10	6
deontic	4	0	7	0
dynamic	0	1	0	0

These raw data are, however, slightly misleading. *Every* instance of deontic *must* in the male groups (i.e. both older and younger) occurs as part of the construction *I must admit*, as in the examples in (24) below:

- (24) *Older males*  
 I must admit I agree with you  
 I must admit it's a bit like that in this street  
*Younger males*  
 I must admit she should have been swinging from the trees her like  
 I must admit I trust youse all

Palmer (1990: 73-4) states that "MUST is often used in a rather weaker sense with a limited set of verbs all related to the act of conversation - *I must say / admit / be honest / ask you / reiterate / confess / concede / mention* and *you must remember / admit / realize / understand*, etc. With these there is still an element of discourse orientation; the speaker either imposes the obligation on himself and by so doing actually performs the act (*I must admit = I do admit*), or else asks his hearer to behave in a similar fashion". There were no instances of this construction in the speech of the female informants. If we exclude what might therefore be seen as some

sort of male discourse marker from the data given in Table 8 above, it would seem that *must* functions almost exclusively as a marker of epistemic necessity for these informants: other than the *I must admit* construction in the speech of the males, only one instance of root *must* is to be found in the whole corpus, as a marker of dynamic necessity:

(25) we must go there

McDonald (1981) provides further evidence in support of parts of this claim, though her corpus makes no reference to patterns within specific social groups. She notes (1981: 231) that root uses of *must* are rare in her Tyneside corpus, and that the “tendency in the Tyneside corpus seems to be not to employ *must* in ‘root’ uses except in ‘personal’ set expressions of the *I must say* type” (1981: 237).

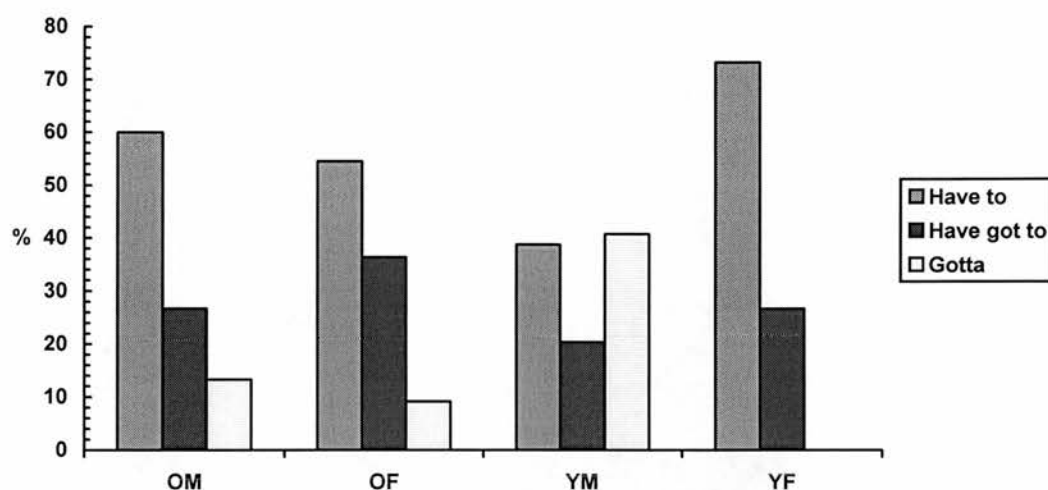
It could therefore be suggested that what appear to be deontic instances of *must* are rather occurrences in fossilised or set expressions: in fact, in this corpus, instances of deontic *must* are restricted to just one expression; and such a usage is associated with male speech in TE. It is perhaps the case that this absence of root instances of *must* is linked to its defective morphology (since *must* has no past tense form, unlike the other core modals). Perhaps these Tyneside speakers are attempting to regulate the grammatical system in the following way: *must* is marked within the groups of core modals because it lacks a past tense pair; additionally, the speakers are restricting its use to express epistemic modality rather than root modality. So the pattern which was characteristic of *might* and *should* has been extended to *must*, as predicted in section §5.3.1.6. A research program which has access to a larger corpus might be able to investigate the pervasiveness of this trend in the Tyneside community.

However, it is clear that in PDSE, *must* does function as a marker of root necessity, as examples such as (26) below show:

(26) I really must go home now

The question then remains as to how such modality may be grammaticalised in TE, if not by means of the modal verb *must*. This is explored with reference to variants of *have got to* (specifically *have got to*, *have to* and *gotta*). Consider the distribution of variant forms of *have got to*, as in Graph 3 below:

*Graph 3*  
Percentage of variants of *have got to* by social group



There are a number of distinctions between these variants, some of which relate to sociolinguistic factors, and others which are concerned with language-internal constraints. In terms of the language-internal constraints, Palmer (1990: 114) notes that there are no non-finite forms of *have got to* (and presumably therefore *gotta*, though Palmer does not list this as a separate form), and that in his corpus, past tense forms of *have got to* (i.e. *had got to*) are rare, *had to* being used in such circumstances. Myhill (1997: 4) distinguishes *must*, *have to* and *(have) got to* on the following semantic grounds. He suggests that *have to* displays objective modality, while *(have) got to* and *must* are subjective<sup>12</sup>. In terms of sociolinguistic variation, *gotta* is marked as non-standard, but is likely to be considered part of a widespread non-standard variety: it is not localised to the Tyneside area. It may perhaps be perceived as a stigmatised form, which attracts covert rather than overt prestige; this

<sup>12</sup> Myhill (1997) introduces further semantic distinctions concerning the modals, principally between group orientation and individual orientation; these issues are not addressed here.

might in part explain why this particular variant is more frequently used by the male speakers rather than the female ones.

In her discussion of the syntactic patterning of *have (got) to*, McDonald (1981: 236) suggests that its behaviour in standard English is like that of an “auxiliary”<sup>13</sup> in negatives and “also in interrogatives without *do*, eg *If you have a button off, has Norman to sew it on*”. I feel this oversimplifies the matter somewhat. It is not clear as to whether McDonald considers both *have to* and *have got to* to display this pattern. My impression for standard English is that while *have to* is a *variable* operator, *have got to* must *always* be an operator except in ‘code’ positions, where it is variable, as the examples in (27) below illustrates:

- (27) Norman has to sew it on  
 Norman hasn’t to sew it on  
 Norman doesn’t have to sew it on  
 Has Norman to sew it on?  
 Does Norman have to sew it on?  
 Norman has to sew it on and so have I  
 Norman has to sew it on and so do I  
 Norman HAS to sew it on  
 Norman DOES have to sew it on
- Norman has got to sew it on  
 Norman hasn’t got to sew it on  
 \*Norman doesn’t have got to sew it on  
 Has Norman got to sew it on?  
 \*Does Norman have got to sew it on?  
 Norman has got to sew it on and so have I  
 Norman has got to sew it on and so do I  
 Norman HAS got to sew it on  
 \*Norman DOES have got to sew it on

Further complications arise here concerning negation. *Norman hasn’t to sew it on* does not mean the same thing as *Norman hasn’t got to sew it on*. In the first, the proposition is negated, not the modality, i.e. [HAVE TO] [~ NORMAN SEW IT ON], while in the second, the modality is negated, not the proposition i.e. [~ HAVE TO]

<sup>13</sup> I think it is better to use the term operator here, as not all auxiliaries display the NICE properties (cf. §2.2.1). For instance, *get* is used in standard English as a passive auxiliary, yet requires *do*-support in negatives and interrogatives.



[NORMAN SEW IT ON]. In other words, *Norman hasn't to sew it on* = necessary not, while *Norman hasn't got to sew it on* = not necessary. In fact to express the notional equivalent of *Norman hasn't got to sew it on* using *have to*, *do* support is obligatory, i.e. *Norman hasn't got to sew it on*  $\equiv$  *Norman doesn't have to sew it on*.

The situation with regard to TE is more complex, as Beal (1993: 197) notes:

where standard English uses *mustn't* to mean 'it is necessary not to ...', Tyneside uses *haven't got to*. Here, misunderstandings could easily arise: a Tynesider, saying:

You haven't got to do that!

means, not that you are not obliged to do it, but that you are obliged *not* to do it.

In the present corpus, the negated variants (of which there were only three) all displayed negation of the modality (= not necessary) and involved *do*-support plus *have to*:

- (28) *Older females*  
 they don't have to pay forty or fifty pound  
 you don't have to travel far to do anything  
*Younger females*  
 you don't have to apologise

This is in accordance with the patterns described by Beal (1993: 197). The five inverted variants also showed *do*-support plus *have to*:

- (29) *Younger males*  
 Do you have to write anything about it do you?  
 Did you have to smoke a pipe before you did it like?  
*Younger females*  
 Do you have to wash their arm?  
 Do you still have to do RE?  
 Do they have to work like 12 hours a day?

The data here is too scant to make even tentative claims about the operator status of *have to* in current Tyneside English, though research which has access to a larger corpus may provide some illuminating results.

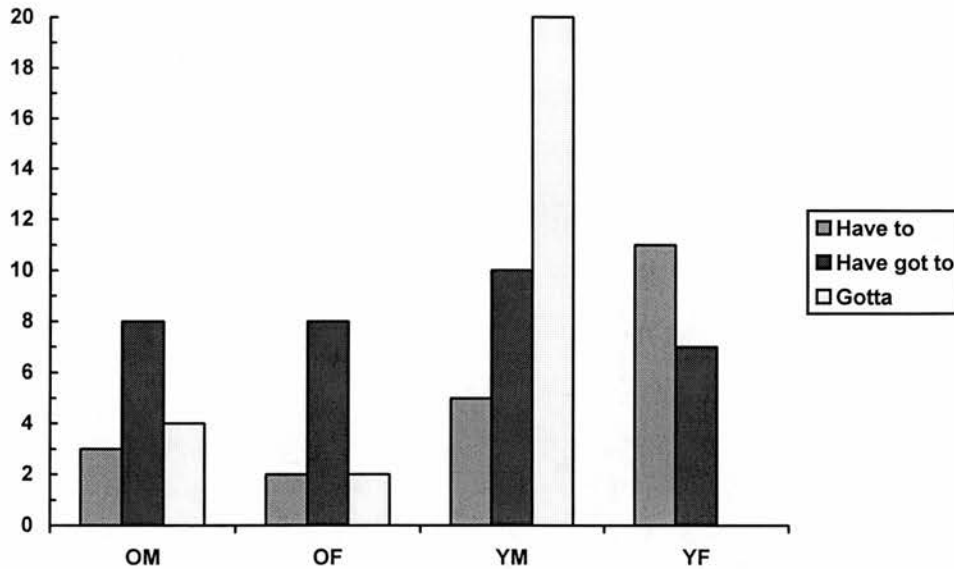
The absence of non-finite forms of *have got to*, *gotta* (and *must*) means that no auxiliary (including other quasi-modals such as *be going to*) can occur to the left of these forms, in contrast with *have to*, as the following instances from the present corpus show:

- (30) *Older males*  
 You'll have to sit over there  
 the way you think you might have to talk to people  
*Older females*  
 You might have had to stop  
 I'm going to have to go round our town and open me eyes  
*Younger males*  
 Why he'll have to tell her  
 I used to have to share a room with him  
*Younger females*  
 you'll have to see this  
 you're gonna have to stop wanking in the office

Furthermore, as Palmer notes for Standard English (1990: 114), *have got to* is much rarer in its past tense form than *have to* is; and *gotta* (like *must*) has no past tense pair at all. In fact, all past tense forms in the present corpus took the form *had to*. Palmer (1990: 114) also argues that *have to* is more formal, with *have got to* belonging to "a more colloquial style". In constructions in which the variants appear in the present tense, that is, where there are no constraints as to which of the variants can or cannot appear, *have to* is the least favoured variant for all groups other than the younger females, who favour this form above the others, as the graph below shows:

## Graph 4

Number of instances of *gotta* and present tense forms of *have to* and *have got to* by social group



$\chi^2$  tests<sup>14</sup> here show age not to be significant, while gender is significant at  $p < 0.001$ , with the younger males showing a clear preference for the vernacular variant *gotta*, an issue discussed further in the following chapter.

In addition to expressing root necessity, variants of *have got to* can also express epistemic necessity in the speech of the informants, as the following examples show:

- (31) *Older males*  
it has to be Gosforth  
*Older females*  
there's got to be a lot of bother in that Tescos  
*Younger males*  
It's got to be scum city  
*Younger females*  
this has got to be another one of those parents and Sundays things

However, such instances are relatively rare, as the table below indicates:

<sup>14</sup> It must be remembered that the numbers here are very small, however.

Table 9

Epistemic and root uses of *must* and *have got to*

	Epistemic	Root
<i>Must</i> <sup>15</sup>	44	1
<i>Have got to</i>	9	123

$\chi^2$  (applying Yates's correction (cf. Butler (1985: 122)) = 127.1 (very highly significant); df = 1.

Myhill (1997) suggests a similar development with American English modals: root (specifically deontic) instances of *must* have decreased significantly in the course of the last century, as root instances of what he classifies as *gotta* have risen; Myhill (1997) also notes a rise in usage of epistemic *must*.

#### §5.3.1.10 'll and 'd

This section attempts to investigate patterns of variation in the use of the clitic forms 'll and 'd (hereafter L forms and D forms respectively), specifically to examine their status in the speech of these informants in comparison with *will* and *would* (W1 and W2 forms) and *shall* and *should* (the S1 and S2 forms). McDonald (1981: 12, 13) makes two important points on this subject. Firstly, she discusses the importance of prescription for the use of W and S forms. Prescriptive grammars such as Fowler (1968: 548-51, 713-4) attempt to regulate the use of the W and S forms in the following manner. They broadly distinguish two ways of referring to future time in the use of the W and S forms, which I will classify as *marked* and *unmarked* respectively. The 'unmarked' interpretation is one in which the reference to future time is not coloured by the speaker's volition or obligation; 'marked' is the converse of this. In unmarked cases, the S forms are used with first person subjects, and W forms with second and third person subjects; in marked cases, the W forms are used

<sup>15</sup> This excludes the fossilised use of root *must* in the male discourse marker *I must admit*, discussed above.

with first person subjects, and S forms with second and third person subjects, as in the examples in (35) below (not from the speech of the informants):

(32) *Unmarked*

I shall be in my room

He will be arriving on the 13.30 from Heathrow

*Marked*

I will get this report to you on time (volition)

You shall be here by nine in the morning (obligation)

It should of course be stated that such examples are based on what prescriptivists feel *should* be the correct usage, rather than what is actually used, even by Standard English speakers. The second important point made by McDonald concerns what the L and D forms stand for; she argues that “it is most usual to take ‘*ll* for *will* and ‘*d* for *would*” (McDonald 1981: 13). The findings of McDonald (1981) as regards TE will be discussed in relation to my own data below.

The following tables show the number of occurrences of L and W1 forms (no S1 forms occurred anywhere in the present corpus: cf. §5.3.1.5) by clause type and subject type.

Table 10

Number of L and W1 forms by clause type

Clause type	OM		OF		YM		YF	
	L	W1	L	W1	L	W1	L	W1
PD	23	0	8	1	57	0	12	0
ND	1	1	1	2	3	6	1	2
fragment	na <sup>16</sup>	0	na	0	na	3	na	0
PtagQ	na	0	na	0	na	1	na	0
NtagQ	na	0	na	0	na	0	na	0
Total	24	1	9	3	60	10	13	2

Key:

PD = positive declarative

ND = negative declarative

fragment = sentence fragment<sup>17</sup>

PtagQ = positive tag question

NtagQ = negative tag question

<sup>16</sup> ‘na’ means not applicable: in sentence fragments (see next footnote), uncontracted forms never appear (e.g. \**aye it’ll*).

<sup>17</sup> By fragment, I mean an utterance with either (a) the auxiliary in code position (e.g. *aye it will*); (b) the auxiliary plus substitute *do*, e.g. *I will do*) or (c) no subject (e.g. *will be*).

Table 11  
Number of L and W1 forms by subject type

Subject type	OM		OF		YM		YF	
	L	W1	L	W1	L	W1	L	W1
Personal pronoun	22	1	5	2	50	8	9	1
That (dem)	1	0	1	0	2	0	2	0
There	1	0	0	0	1	0	0	0
Simple DP <sup>18</sup>	0	0	4	0	5	1	2	1
Complex DP	0	0	0	1	1	0	0	0
Relative pronoun	0	0	0	0	1	0	0	0

These data suggest interesting patterns of use of the L and W forms in Tyneside English, and also between the different social groups investigated. Table (10) suggests that L forms are greatly favoured in positive declaratives over W1 forms for all speakers. In fact, W1 forms seem to be rare generally. McDonald (1981: 94) gives the following data for her Tyneside corpus:

L-forms ('ll and 'll not): 674 (84.04%)

W-forms (will and won't): 113 (14.09%)

S-forms (shall and shan't): 15 (1.87%)

This compares with the following from this study:

L -forms: 106 (86.89%)

W-forms: 16 (13.11%)

S-forms: 0 (0%)

Although the numbers in the present study are much smaller, there is a clear similarity - in terms of proportion - between the sets of data from the two studies. The question, then, is why the L form should be preferred over both the W and S forms. McDonald (1981: 98), referring to Strang (1970: 98), suggests that in TE, as

<sup>18</sup> For present purposes, I take a simple DP to be one which contains either a noun only or a noun with a preceding determiner; a complex DP is one with pre- or post -modification of the noun.

well as in Scottish, American and Irish English, *will* has been “expanding at the expense of *shall*”, but this still does not explain the preponderance of L forms. Clearly the type of discourse recorded here is important: L forms are generally considered to be on the casual end of the style continuum (cf. Hudson 1996: 147), and the method of the data collection for this thesis was one which actively attempted to collect only the most casual speech possible; and regular use of full forms would certainly seem marked and out of place in this type of discourse. But it is also possible that prescriptivism has a role to play here. I would suggest that for speakers of many non-standard varieties, there are two processes working in tandem, one which could explain the absence of any S forms in this corpus, and another which could explain the preponderance of L forms. The first is that unmarked *shall* certainly, and marked *shall* probably, like *may* (cf. §5.3.1.3), might be perceived as very formal and old-fashioned, which could lead to its obsolescence. The second is that the prescriptive rule outlined above, concerning marked and unmarked uses of ‘future’ *will* and *shall*, is not operative in the speakers’ grammars. Instead, the speakers have selected a neutralised form. The L form is ambiguous as a cliticisation of either *will* or *shall*, and the use of the L form bypasses any potential misapplication of the prescriptive rule.

Table 11, which illustrates uses of L and W1 forms by subject type, shows some potential trends which future research might be able to develop further. The majority of instances of L and W forms which occurred in the corpus occurred with personal pronoun subjects. However, in the case of the younger males, a wide range of subjects were found to occur with the L form, including a complex DP and a relative pronoun, illustrated in (33) and (34) respectively:

- (33) students who are gonna move out next year’ll have bought and rented the properties for next year already
- (34) anything that’ll soak up piss



Clearly the numbers of instances involved are too small to make generalisations, but further research might profitably consider the range of subject types which allow cliticisation in this way; if it is the case that the younger males show this tendency, it might be possible to suggest that this is a change from below which is spreading slowly into different syntactic environments, as a syntactic equivalent of lexical diffusion (cf. Tagliamonte 1998). I discuss this issue further in the following chapter, where data of this type are considered with regard to head-to-head adjunction and case checking.

The pattern with D and W2 forms is also revealing, as the following two tables show:

*Table 12*  
Number of D and W2 forms by clause type

Clause type	OM		OF		YM		YF	
	D	W2	D	W2	D	W2	D	W2
<i>PD</i>	6	39	5	19	28	19	30	29
<i>ND (e)</i>	0	0	0	4	0	0	0	0
<i>ND (n)</i>	0	19	0	12	0	15	0	11
<i>PI</i>	0	3	0	0	0	1	0	1
<i>PtagQ</i>	na	0	na	0	na	0	na	1
<i>NtagQ</i>	na	0	na	0	na	0	na	1
Total	6	61	5	35	28	35	30	43

Key:

PD = positive declarative

ND (e) = negative declarative (emphatic)

ND (n) = negative declarative (neutral)

PI = positive interrogative

PtagQ = positive tag question

NtagQ = negative tag question

*Table 13*  
Number of D and W2 forms by subject type

Subject	OM		OF		YM		YF	
	D	W2	D	W2	D	W2	D	W2
<i>Personal pronoun</i>	6	54	5	33	24	27	24	34
<i>DP</i>	0	6	0	0	4	8	4	6
<i>That (dem)</i>	0	0	0	0	0	0	1	0
<i>There</i>	0	1	0	2	0	0	1	3
<i>Relative pronoun</i>	0	0	0	0	0	2	0	0

As was the case with the L-forms, I begin with considering total numbers of D and W forms irrespective of clause or subject type. As the totals stand in *Tables 12 and 13*, the results may not seem significant, but it is possible to regroup this raw material into scores for both age and sex, following the procedure applied with negative forms of *can* in §5.3.1.1 above, with the following results:

*Table 14*  
D and W2 forms by age

	Older	Younger
D forms	11	58
W forms	96	78

df = 1,  $\chi^2 = 30.85$  ;  $p < 0.001$  (highly significant)

*Table 15*  
D and W2 forms by sex

	Male	Female
D forms	34	35
W forms	96	78

df = 1,  $\chi^2 = 0.69$  (not significant)

This would seem to suggest that there is a significant difference in the use of the clitic form between the two generations, but not between the sexes. However, if we return to an examination of the scores for the individual groups, further patterns emerge. Consider first *Table 12* which shows the scores by clause type. The first point to notice is that for all four groups, D forms occur only in positive declaratives: no D forms are found in negative declaratives, whether emphatic or neutral. This is consistent with the findings of McDonald (1981: 133) who notes that this is not peculiar to Tyneside: in her *non-Tyneside* corpus, no instances of '*d not* were elicited. The second point to consider is the difference in use of D and W forms in positive declaratives between the four groups. Applying a  $\chi^2$  test to these frequencies gives a value of 27.6, which for three degrees of freedom is higher than the critical

value of  $p$  at 0.001 (16.27), suggesting that the difference in scores is highly significant. Specifically, the older speakers, both male and female, favour *would* in positive declaratives; the younger females show a roughly equal split between *would* and the clitic form; but the younger males seem to favour the clitic form over the *W* form. Again, it is important to be cautious about making large claims on relatively small numbers of examples per speaker. But these results would seem to indicate that the younger males are instrumental in the diffusion of this clitic form.

The table which illustrates use of *D* and *W* forms by subject type also reveals interesting patterns between the groups. The older informants, both male and female, only use the *D* form with personal pronoun subjects. Indeed, taking the two tables together, it seems to be the case that use of the *D* form among the older speakers is restricted to its appearance in positive declaratives with personal pronoun subjects. McDonald (1981: 133-4) argues that “contracted forms tend to be avoided with non-monosyllabic subjects and are used with monosyllabic subjects, especially pronouns and particularly first person pronouns”. This is not the case in this corpus of TE, as the following instances from the younger speakers, given below in (35), would seem to suggest:

- (35) *Younger males*  
 everyone'd be jealous as fuck  
 My dad'd twat me if I called him by his first name  
 Tommy'd get in like  
*Younger females*  
 I wish my mum'd let us  
 cos then people'd watch it  
 everyone'd be totally hammered all the time  
 mam and dad'd be pissing themselves  
 the mature students'd be so keen

With these examples from the younger females, we can see instances of the *D* form used with complex DP subjects and co-ordinated DP subjects.

We might therefore suggest the putative development of these cliticized forms as follows:

1. *Shall* is perceived as a very formal (hyperstandard?) means of marking future time; this is combined with confusion over the prescriptive rules used to regulate the use of *shall* and *will* as future time markers and leads to:
2. Neutralisation of the opposition between *will* and *shall* as markers of futurity to the clitic 'll (as a regular pattern of grammaticalisation, from members of a functional category 'modal' to clitics), but with restricted distribution by clause type (positive declarative) and subject type (personal pronoun), leading to:
3. An analogous spread of this change from *would* to 'd and:
4. The diffusion of the use of the L forms (a) with other non-personal pronoun subjects and (b) into other non-positive declarative clause types.

### §5.3.2 *Summary of grammatical variation*

In chapter 2 of this thesis, I attempted to provide a set of core modals for standard English, based on morphosyntactic and semantic properties. The seven members of the core were: *can, could, may, might, shall, should, will, would* and *must*. The data collected for this thesis - and other reports of modal verb usage in TE (McDonald (1981), Beal (1993)) - would suggest that the TE core is not identical to that of the standard variety<sup>19</sup>.

Firstly, some members of the standard English core are either marginal or have a more restricted syntactic distribution in the Tyneside variety:

*May*: only three occurrences of *may* are attested in the whole corpus, two of these were collocated with the AdvP *as well*, which Hoyer (1997: 275) classifies as idiomatic.

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<sup>19</sup> I am not suggesting that non-occurrence of any given form in the data collected here is *proof* that the form is not part of the grammar of these speakers; I am claiming that these data - and others collected by McDonald and Beal - suggest that if such forms do exist, they are very marginal, given their sporadic or non-existent appearance in a set of relatively lengthy recording sessions.

*Shall*: no instances of *shall* were attested

*Will*: only one instance of *will* was attested in a positive declarative; the rest were restricted to NICE contexts.

It would be right to suggest that *may* is marginal or distributionally restricted in the (spoken) standard variety also, since for many standard speakers *may* does not take the negative clitic when used deontically (*\*He mayn't come in yet*); not does it appear in epistemic questions (*\*May he be wrong?*), as discussed in §5.3.1.6 above. But even if this is the case, it would still be fair to argue that TE is more advanced than the standard variety in this regard: standard English speakers still use *may* as a marker of epistemic and deontic possibility, even though this might be on the wane. To all intents and purposes, *may* is not part of the modal system of TE at all: *might* is the usual marker of epistemic possibility, and *can* the marker of deontic possibility. In this sense then, as noted by McDonald (1981), TE, far from being a conservative variety, is in fact more innovative than the standard: the gradual erosion of *may* from the modal system is taking place more rapidly on Tyneside than is the case in the standard dialect.

The rarity of *shall* (and to a lesser extent *will*) is perhaps more surprising. Recall from §5.3.1.5 that *shall* is rare in Scots also, and that in the standard variety it is (a) rare and (b) used predominantly as a formal suppletive of *will* (Hoye 1997: 120). But again, this change, taking place in both the standard and the Tyneside variety, seems to be developing more rapidly in the latter than in the former. The view that TE is unequivocally a conservative variety is not the case. And what is more, these changes are characteristic of a relatively high-status group in the Tyneside community: the absence of *may* and *shall* is not simply a vernacular feature, but one which is typical of upper-working and lower-middle class Tynesiders. The sociolinguistic implications of such a distribution are discussed in the next chapter.

Secondly, some 'verb' forms which do not form the core of standard English modals seem to be more core-like in TE:

*'ll*: in some ways, this clitic form is characteristic of the standard variety too. But writers on the subject (Palmer 1990, Coates 1983, Denison 1993) have tended to describe the clitic as a reduced form of *will* (and sometimes *shall*). For TE, it seems more productive to describe *will* (and potentially *shall* if and when it appears in this variety) as expanded forms of *'ll*: that is, the clitic is the most regularly appearing form in positive declaratives, with *will* appearing in ICE contexts (sic - negation patterns are different, and discussed above in §5.3.1.10). A similar pattern seems to be on-going with *would* and the reduced form *'d*. Numbers of instances per speaker were too small to make definite predictions of which specific social groups were implementing any change, but it seemed to be the case that the clitic form is distributed over a greater number of clause types and with different subject types with the younger groups, with older speakers restricting its appearance to positive declaratives with personal pronoun subjects.

*gotta*: as a further marker of root necessity along with *have to* and *have got to*, this form may develop as a new member of the core. It currently has the modal-like morphosyntactic characteristic of being finite only (*\*you'll gotta go*; *\*I'm gottaing to go to London each week*); it does not take an {S}inflection with third-person singular (3S) subjects, but this is because it never appears with 3S subjects - 3S subjects take *have to* or *have got to* (*\* he gotta go*) in this variety. However, *gotta* is not an operator (*\*gotta he go?*, *\*he gottan't go*).

Notice then, that it seems to be the case that the present tense forms are becoming obsolete: of our original present-tense core of *may*, *shall*, *will*, and *can*, only *can* remains as a full member (i.e. one which regularly appears in all clause types) for TE.

Thirdly, some modal variants were clear sociolinguistic markers. The uncontracted negative variant *cannit* was a marker of male speech, with females favouring the *can't* variant; 'past time' *could* was a marker of the older speakers, with 'unreal/tentative' *could* more regularly correlated with the younger groups; and the spread of the clitic forms L and D to a range of different syntactic contexts (both in terms of clause type and subject type) was also characteristic of the younger speakers, particularly the younger males. I think these sociolinguistic correlations are important, given the relatively few sociolinguistic studies that attempt to consider morphological and syntactic variation.

Fourthly, of the modals that remained as part of the core, it seemed as though the type of modality they displayed was increasingly uniform. By this I mean that, whereas in standard English, it is common for the modals to be polysemous, the Tyneside modals showed an increasing tendency to monosemy, at least as far as the epistemic and root divisions are concerned. While *should* in standard English can potentially be root (as a marker of weak obligation) or epistemic (as a marker of possibility), in TE it was overwhelmingly root; this was also true of *could*; and *might* similarly was monosemous, although in this case it expressed epistemic rather than root modality, as did *must*. Now, a few caveats are necessary at this point. Firstly, simply because a form does not turn up in any given data set does not mean to say it does not exist, even within the grammars of the informants who provide the data; we cannot absolutely claim that *should* is *never* epistemic in TE. All we can suggest is that epistemic uses, based on the recordings made, seem to be very marginal, and statistically, we would want to claim that this was not merely down to chance. Secondly, in many cases, the number of instances of forms occurring is quite low, and ideally, a larger corpus is needed to provide more statistically significant claims concerning the hypothesis. And thirdly, with a larger corpus, we could examine the speech of a greater number of TE speakers from a broader social background than is



the case for this thesis, so that we could investigate the extent to which this monosemy is characteristic of Tyneside as a whole, rather than just a small section of the community.

These general patterns are summarised in Table 16 below. Attempts at explaining why these patterns should exist can be found in the next chapter; I now turn in this chapter to consider the results for the phonological variables.

*Table 16*  
Patterns of modal verb usage in TE

<i>Becoming core-like</i>	<i>Remaining as core</i>	<i>Leaving core</i>
<b>gotta</b> (root)	<b>can</b> (root)	<b>may</b>
<b>‘ll</b>	<b>would</b>	<b>shall</b>
	<b>should</b> (root)	<b>will</b>
	<b>could</b> (root)	
	<b>must</b> (epistemic)	
	<b>might</b> (epistemic)	

### §5.3.3 *Phonological and phonetic variation*

This section concerns the variables (p) (t) and (k) in TE, specifically patterns of glottal substitution (GS cf. chapter 3) and glottal reinforcement (GR): where no distinction between substitution and reinforcement needs to be made, the symbol G is used, as in chapter 3. The initial aim was to consider variation in eight different phonetic contexts, similar to those listed by Wells (1982: 260), discussed in §3.1.1 above. I attempted to record thirty tokens<sup>20</sup> of each variable in each context for each speaker. Had this been achievable, this would have meant collection of 14, 400 tokens; however, it was not always possible to collect a sufficient number of tokens

<sup>20</sup> Milroy (1987a) suggests that thirty tokens should be collected for each speaker for each variable; if this is not achievable, the researcher should attempt to record as many variants as possible, but at least ten for each speaker.

from each group to provide a satisfactory set of data for analysis. In the table below, I give the total numbers of variants collected for each of the variables in each context.

*Table 17*

Number of variants of (p), (t) and (k) by phonetic environment

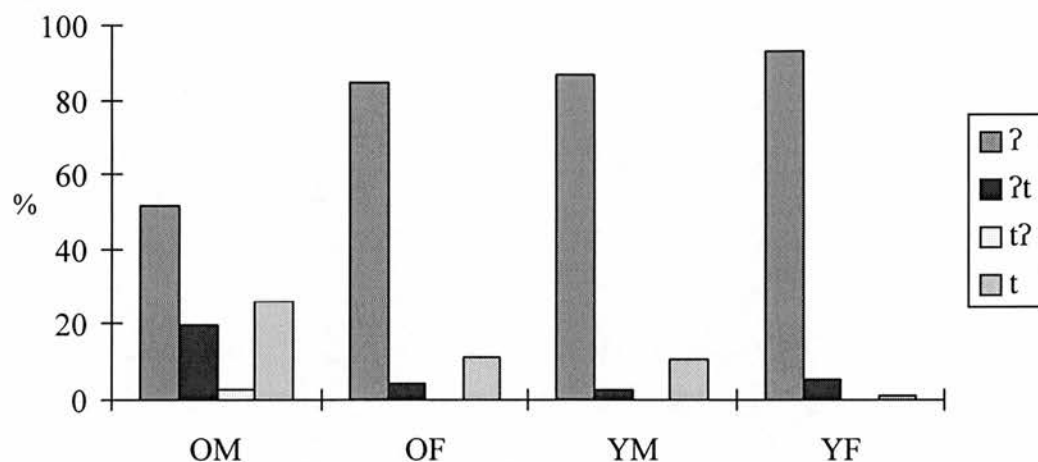
Environment		(p)	(t)	(k)
1	___ # C	117	543	394
2	___ # L/S	34	319	129
3	___ # V	94	565	361
4	___ Pause	63	482	270
5	___ C	90	189	303
6	___ L/S	151	288	66
7	___ N	55	209	228
8	___ V	166	468	297
Total		770	3063	2048
Grand total		5881		

This means that the total number of variants collected is only about 40% of the intended number. However, time and resources prevented an attempt to collect, transcribe and analyse at least as much data again in order to achieve the initial aim. This shortfall in the number of tokens means that the scores for (p) cannot be analysed individually in each context (given that the absolute minimum number of tokens is 200 (an average of 10 tokens for each of the 20 informants)), and that (t) \_\_\_ L/S, (k) \_\_\_ # L/S and (k) \_\_\_ L/S must also be discounted. As a result, the statistical analysis of patterns of G is restricted to two of the three oral stop variables. However, when relevant, the data for (p), and for (t) and (k) in those environments where the number of instances was too small to represent a reasonable sample will be used to discuss aspects of syllabification patterns in TE in §5.3.4.

Initially, however, a discussion of patterns for each variable in each environment is given, where possible.  $\chi^2$  tests were applied to each set of data, except those discussed above in which the number of variants were too small; the test was carried out for gender and age independently, and unless otherwise stated, p is less than or equal to 0.001 for both gender and age; in other words, the effect of

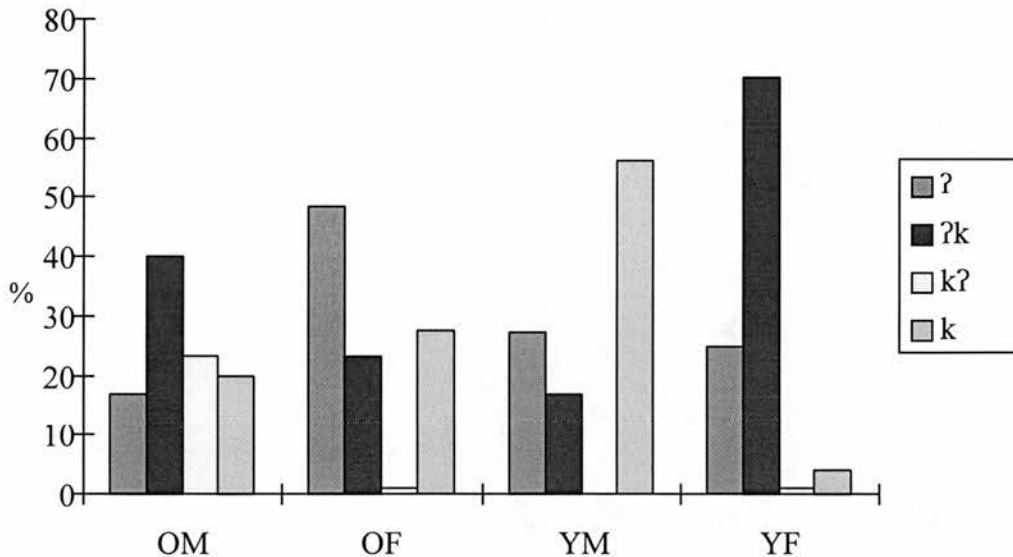
gender and age can be seen as very significant in determining which variant is used. To repeat for convenience's sake, in the following discussion and graphs, OM stands for older males, OF for older females, YM for younger males, and YF for younger females, as before.

§5.3.3.1 (t) \_\_ # C (e.g. *hit Peter*)



It is in this environment, according to Hughes and Trudgill (1987: 35), where GS is most frequent in British English generally. The data indicate that GS is not uniform across the board, however, being most favoured by YF, then YM, then OF and least favoured by OM. Notice particularly that this pattern is the converse of that for full release as a variant of (t) in this position. This variant is favoured most by OM, then the OF, then YM and lastly YF. As shall be seen throughout the following discussion, these initial data suggest that GS is particularly marked as a characteristic of younger females, though it is by no means exclusive to this group: it is a form favoured by younger males and older females too. But crucially, the high incidence of this variant among the young female group might be an indication that they are the innovators of this variant in this group of TE speakers. Such a pattern would lend weight to the argument that females - particularly younger females central in the class hierarchy - favour supra local forms such as GS, as discussed in the previous chapter.

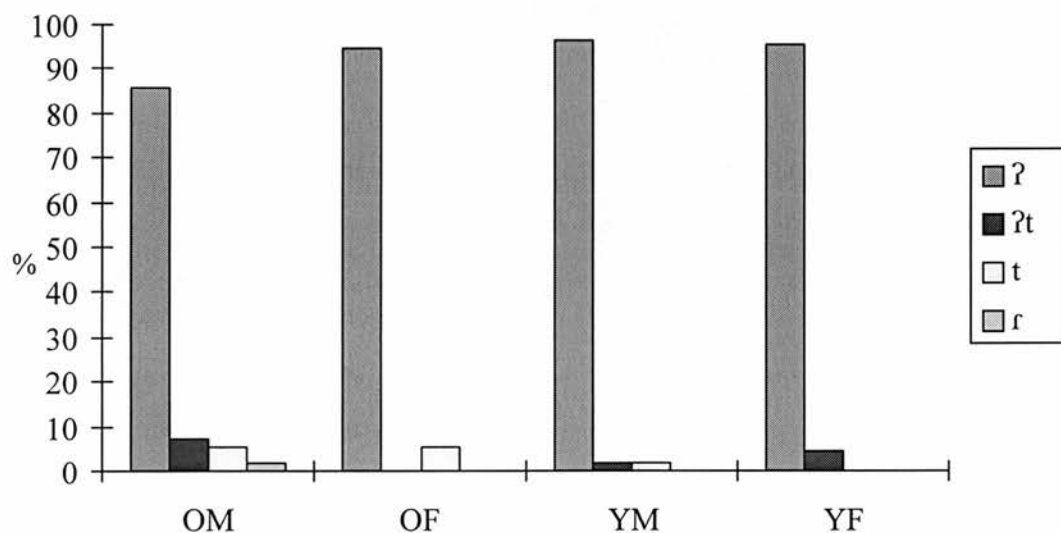
§5.3.3.2 (k) \_\_ # C (e.g. *thick soup*)



Patterns for (k) in this position are far less clear cut than is the case for (t), and regular stratification across the social groups here seems difficult. Nonetheless, some interesting distributions exist. For each social group, one variant is favoured above all others - for OM and for YF [?k], for OF [?], and for YM [k]. However, other variants are well represented in the speech of OM, including the highly localised GR variant [k?]. Indeed, this is possibly the most significant pattern for this variable in this context: while there seems to be a wide spread of variants, each fairly well represented, for the OM group, the OF, YM and YF groups - particularly the last - seem to be focusing on one particular variant over and above the others: notice that the difference in percentage points between the most favoured variant and the next most favoured becomes increasingly larger as we move left to right across the groups. Such a pattern might be explained with reference to behaviour in dialect contact scenarios, in which there is “a move from diffusion to focusing” (Kerswill and Williams 1994: 9); however, this is usually applied to speech communities as a

whole, and this is clearly not the case here, as quite a wide range of variation still exists. However, we could modify this by suggesting that focusing is occurring *more rapidly* in the younger groups than is the case for the older groups; and that the YF group is the ‘most focused’ and the OM group the least. This again lends weight to the claim that YF groups are often at the leading edge of change in dialect contact scenarios (cf. Britain 1997, Watt 1998). Furthermore, the highly localised pattern of GR is only found significantly in the speech of the OM. That the YM group favour full release is not easy to explain in this case; however, as shall be seen later, full release as a variant of (k) in many different environments does seem to mark out the YM group.

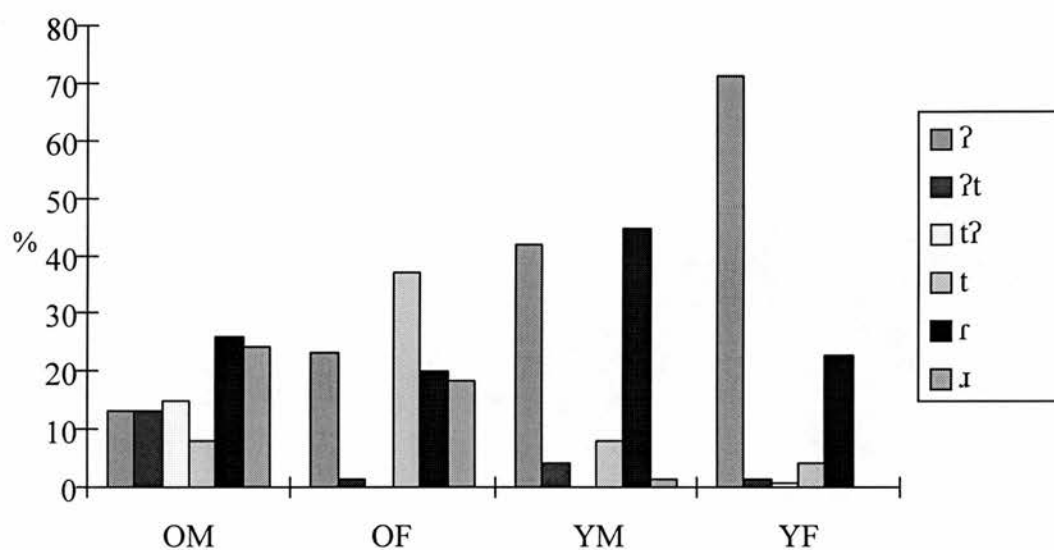
§5.3.3.3 (t) \_\_ # L or S (e.g. *hot roll*)



Despite the claim made by Hughes and Trudgill (1987: 35) discussed in §5.3.3.1 above with regard to (t) finally before a ‘true’ consonant, it seems as if this is the most favoured environment for GS of (t). The scores are very high for all social groups, nearly categorical for the younger speakers. Again, there is most diffusion (albeit minimal) within the OM group; but generally speaking, this environment is the one in which, taking the speech community investigated as a whole, we could

come closest to suggesting that the glottal stop is an allophone of /t/ as opposed to a variant of (t). Interestingly, the effects of gender and age are less prominent here than for any other of the variables in any other environment: for age,  $\chi^2 = 7.81$  ( $p = 0.05$ ), while for gender,  $\chi^2 = 1.19$ , which is not significant.

§5.3.3.4 (t) \_\_ # V (e.g. *get Alan*)



The range of variants of (t) is greater in this environment than in the other two environments discussed above: in addition to GS, the two types of GR, and full release, there is evidence - for most of the groups at least - of two further variants, a tap [r] and a retroflex approximant [ɽ]. The group in which all variants are most evenly represented is OM, then OF, then YM and lastly YF, which again seems to display the most focusing, clearly favouring [ʔ] above all other variants. Notice that the increase in use of GS from left to right in the graph above (i.e. from OM through OF through YM to YF) mirrors the decrease in the use of the approximant [ɽ], to the extent that it is not recorded for the YF group. Full release is the most frequent

variant for the OF group; perhaps this is a response to a perceived ‘stigma’<sup>21</sup> for GS in this environment (i.e. intervocalically, both finally (as here) and medially (cf. §5.3.3.12 below).

Recall from chapter 3 that the use of the tap or retroflex variants are considered to be examples of Weakening (as opposed to GR) in the sense used by Carr (1991). Docherty *et al.* (1997) summarise Carr’s (1991) analysis of where Weakening and not GR will occur as follows:

- (i) in certain words belonging to non-lexical categories:
  - (a) *not*: *not a chance, not altogether*
  - (b) *but*: *but he wouldn’t*
  - (c) *what*: *what a night, what is he doing, what about Jim*
  - (d) *that* (as a complementiser or determiner): *I knew that he would, eat that egg*
- (ii) in verbs:
  - (a) monosyllabic: *put it down, put in front, met him, hit him, get away, get up, got a light I thought I had, fit her*
  - (b) bisyllabic, with stress on the second syllable: *allot it, delete it, incite it, excite her* but not when stressed on the first syllable: *\*edit it, \*elicit it, \*interpret it*

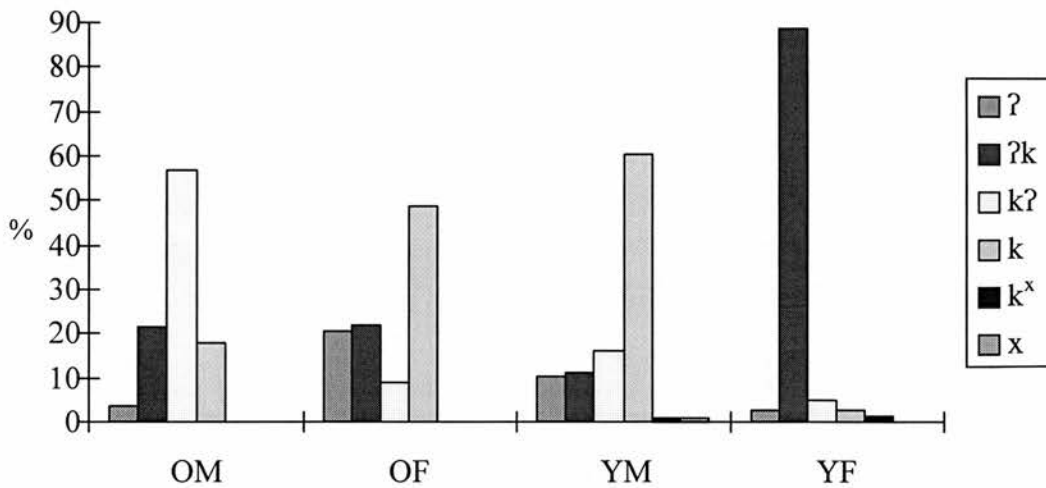
Docherty *et al.* (1997), taking their data in part from Hartley (1992), note that, in contrast to Carr’s claims, “individual speakers produce either the glottal variant or [ɹ] on a monosyllabic verb across word boundaries”, so that *get it* surfaces as both [geʔtɪʔ] and as [geɹɪʔ] within the same idiolect. Evidence from this thesis supports this claim made by Docherty *et al.* (1997): “Carr’s claim about rule-ordering in which weakening bleeds the condition for ‘across the board’ glottalisation fails to meet the requirement of even observational adequacy; both constraints can apply in given identical contexts” since speakers display both weakening and G (either GR or GS) in *not, but, what* and *that*, in addition to monosyllabic verbs such as *put* and *get* when they occur before a word beginning with a vowel. Furthermore, in this corpus, a

<sup>21</sup> In previous (unrecorded) discussions with members of this group, it was clear that this group above all others were aware of what for them were stigmatised variants of TE particularly (for instance, intrusive /r/ in *drawing*); but no overt comment was made on GS as a stigmatised form.



different constraint seems to be operating on monosyllabic verbs ending with (t), and this is not noted by Docherty *et al.* (1997): those with a lax vowel (such as *get*, *split* and *put*, for instance) display weakening and G, as discussed above; but those with a tense vowel (such as *bought*, *brought* and *fart*) only ever surfaced with G - specifically with GS.

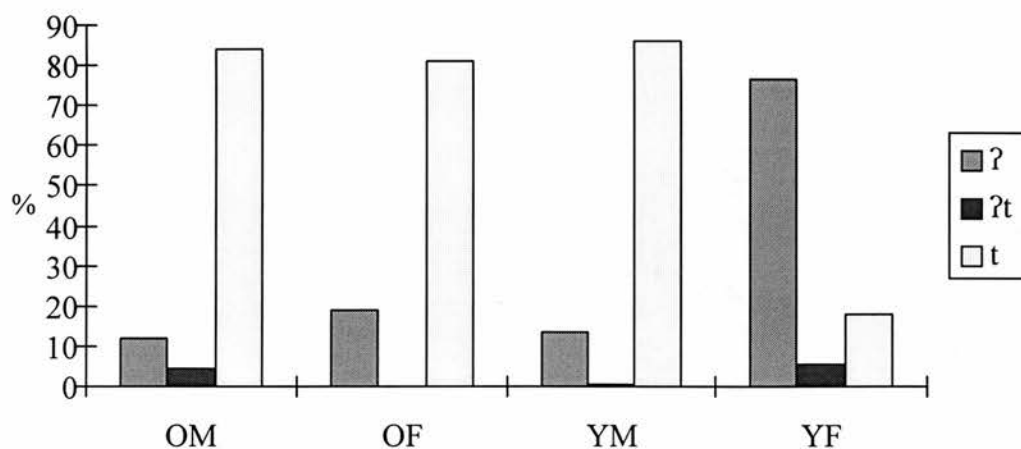
Milroy, Milroy and Hartley (1994) consider tapped variants of (t) in this environment to be local rather than supralocal forms, and as such, like the highly localised pattern of GR in TE, are more likely to be favoured by (and therefore to mark) male speakers, and specifically young males, based on Hartley's data. This prediction is borne out by the data collected for this thesis, as illustrated in the chart above. But I am less confident that we should think of tapped (or retroflex) variants of (t) as local to TE, in the way that the specific pattern of GR is, and this opinion is confirmed by a reading of various chapters of (eds.) Foulkes and Docherty (1999), which provides a range of data from a number of British urban centres. Stoddart, Upton and Widdowson (1999: 76) suggest a retroflex variant is a characteristic variant of (t) intervocalically in Sheffield speech; Mathiesen considers the tap to be characteristic of (older) male speech in Sandwell, West Midlands (Mathiesen 1999: 110); Tollfree notes tapped (as well as GS and GR) variants of (t) across a word boundary and internally as frequently occurring in her corpus of south-east London English (Tollfree 1999: 170-1), as does Stuart-Smith in Glasgow (Stuart-Smith 1999: 209) and McCafferty in Derry (McCafferty 1999: 249).

§5.3.3.5 (k) \_\_ # V (e.g. *sick animal*)

The distribution of G and full release variants here is different to that for (t) \_\_ # V, especially in relation to GS, which is (a) generally low and (b) very low for the YF group, in contrast to patterns in other environments. The OM group generally favours the highly localised GR pattern here, with YF favouring the supralocal GR variant. Unusually for TE, as far as I am aware<sup>22</sup>, there were a couple of instances of audible velar fricatives/affricates from the younger speakers, restricted to the words *back* and *fuck*, but such instances were very rare in this environment, and though they do appear more regularly in other environments (cf. §5.3.3.11, §5.3.3.13 below), they are never the favoured variant. It may be interesting to see if and how such variants do develop in TE; it is possible that such variants may form part of a supralocal, non-southern, non-standard variety, given their saliency in the dialect of another major northern urban centre, Liverpool, and the surrounding area (cf. Newbrook (1999)). Whether such a development follows the ‘city-hopping’ pattern of TH-Fronting (cf. Milroy (1996)) - in which innovative variants are found in a range of urban centres without having diffused into nearby rural communities first - remains to be seen.

<sup>22</sup> I know of no other study which indicates this weakened variant in Tyneside; and it is very uncommon, even in these groups.

## §5.3.3.6 (t) \_\_ Pause



The data here relate to variants of (t) pre-pausally. This variant in this environment has been frequently discussed in relation to accents in the north-east of England (cf. Kerswill (1987), Docherty *et al.* (1997)), to the extent that a rule affecting (t) in this position in these varieties has been proposed, namely the Final Release Rule or FRR (cf. §3.2 of this thesis). The FRR states that glottal variants of (t) do not occur finally before a pause or at the end of a conversational turn<sup>23</sup> in TE. Recall that Docherty *et al.* (1997: 294ff) suggest that in TE, glottal variants of (t) are very rare in pre-pausal and turn-final position: pre-pausally, 30 of their 32 speakers never violated the FRR, and turn-finally, exceptions to the FRR occur only after short vowels, and most frequently in the words *that* and *it*. This led Docherty *et al.* (1997: 297-8) to the claim that GS is spreading by a process of lexical diffusion, and that as such the rule is a lexical rule.

Clearly the patterns for this group of TE speakers are greatly different, both between each group, and between this community as a whole, and the informants who provided the data for Docherty *et al.* (1997). Firstly, while three groups (OM, OF and YM) clearly favour full release pre-pausally - in marked contrast to patterns

<sup>23</sup> Whether the precise positional constraint should be stated as turn-final or pre-pausal is a matter of debate (cf. Docherty *et al.* (1997: 295)).

for this variable for these groups in other environments - there is some evidence for GS in all three of these groups. Secondly, the fourth group, namely YF, favour GS above full release pre-pausally. Why should these patterns contrast so markedly with the data provided by Docherty *et al.* (1997)?

There are a number of potential answers to this question. The first concerns the type of discourse which constitute the data for Docherty *et al.* (1997) on the one hand, and this thesis on the other. Docherty *et al.* (1997: 299-300) consider a wide range of features associated with different types of conversational analysis, which collectively constitute “a field described by Local, Kelly and Wells (1986) as PHONOLOGY OF CONVERSATION”. Specifically, Docherty *et al.* (1997) consider the application of the FRR to be a means of turn-delimitation, a phonetic signal that a speaker is prepared to yield the floor. This does not seem to be the case for the data considered in this thesis, for some instances of violation of the FRR occurred turn-medially, with no indication that another speaker wanted the floor. But more importantly, the present data and the data for Docherty *et al.* (1997) were collected under totally different circumstances. While the information concerning the methodology used in Docherty *et al.* (1997) is necessarily brief, there is enough to show a significant contrast with the method used in this thesis. “Informants were recorded first *in a (usually single sex) dyadic conversational exchange for around 50 minutes*” (Docherty *et al.* (1997: 288), emphasis added). These data were also used by Watt (1998) for his analysis of TE vowels, and his work notes that, while the data collector was present in the room at the time of the recordings, she played a minimal part in the discourse, and the informants were recorded without significant interruption from her. Recall that, in the recording sessions used to collect data for this thesis: (a) much larger groups were recorded; (b) every group was single sex; (c) contributions from me were variable, occurring as and when it was natural for me to take part in the conversations; and (d) the sessions each lasted for a number of hours.

While all of these factors may play a part in relation to violation of the FRR, I think the crucial distinction might be the number of participants in the discourse. Specifically, demarcation of turn-taking in dyadic exchanges may not be isomorphic with those in sessions of a greater number of participants, an issue raised by Duncan (1973: 45). If this is the case, then we cannot claim that FRR violation in dyadic sessions can be accounted for in the same terms as violations in sessions with a larger number of speakers.

Secondly, the greater number of violations of the FRR by the YF group might also be accounted for by claiming that these innovators are in the vanguard of a change in which GS is spreading to a greater number of environments; in other words, this particular YF group is more innovative than those in the Docherty *et al.* (1997) corpus. For instance, as discussed above, the Docherty *et al.* (1997) corpus suggests that when violation of the FRR does occur, it occurs after short vowels, and most frequently in *that* and *it*. In this corpus, such a claim needs modification. Violation of the FRR in the YF group is most frequent after short vowels, and *that* and *it* do account for a large number of the violations; but violation following long vowels and diphthongs, and in complex codas (where the underlying /t/ is preceded by another consonant) is also common, as the table overleaf indicates:

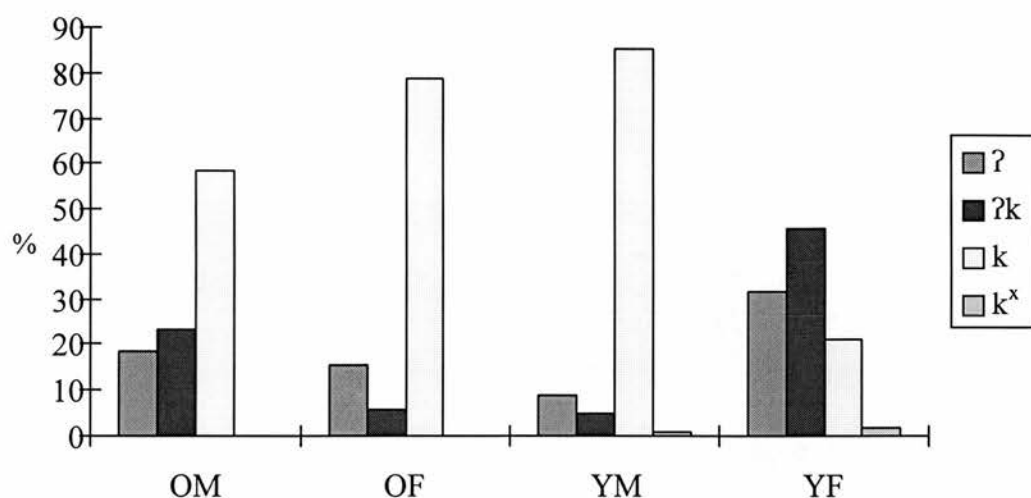
Table 18  
Violation of FRR in YF group

SPEAKER	VIOLATION OF FRR IN:
<i>Monica</i>	<i>about, night, alright, out, inflate, arrogant, different</i>
<i>Gail</i>	<i>about</i>
<i>Fiona</i>	<i>right, night, separate (vb.), might, boot, out, about, quite, cute, sweet, agreement, faint, blatant</i>
<i>Melanie</i>	<i>right, might, sweet</i>
<i>Sam</i>	<i>start, Kate, arrogant, disappointment</i>

Thus we could argue that the lexical diffusion of GS into pre-pausal position is very advanced in this group of speakers by the standards of the TE speakers in Docherty *et al.* (1997); furthermore, this diffusion is not simply one which takes place in words which have a 'short vowel + /t/' in the stressed syllable - all environments are affected.

The data for (t) pre-pausally in this corpus has therefore provided a different picture to that in Docherty *et al.* (1997), suggesting that violation of the FRR, taken as evidence for the development of a lexical rule, is more frequent in certain groups of TE speakers than for others, and that the YF group in particular are in the vanguard of the change. The diffusion is taking place at different rates in different parts of the TE-speaking area; the lexical rule is more established in some speech communities than in others, potentially as a result of dialect contact.

## §5.3.3.7 (k) \_\_ Pause

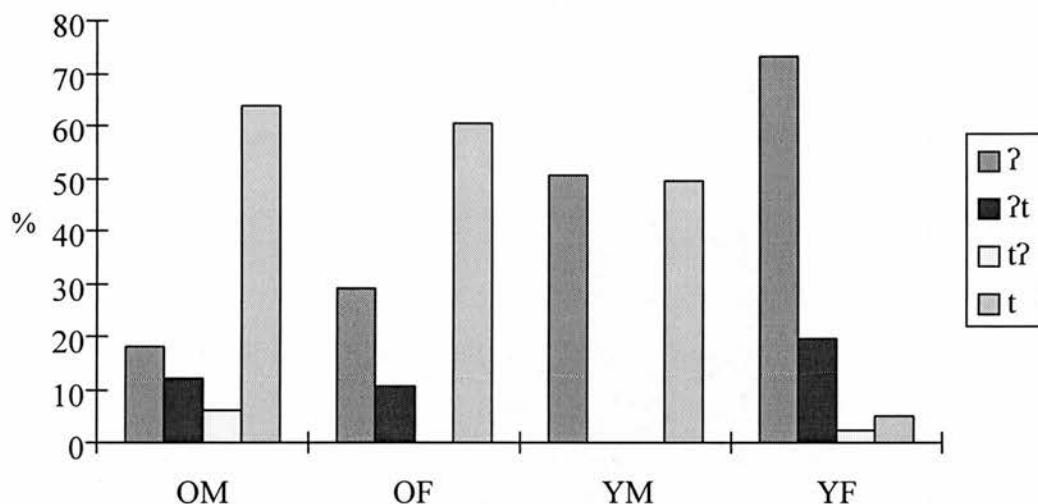


For at least three of the social groups investigated, (k) before a pause - like (t) before a pause - is generally realised as full release. For the YF group, a pattern of GR rather than GS is favoured, though again, the GS score is higher than the full release score for this group. Thus it would seem as though the YF group, instead of displaying the most focusing, as it seems to do in most other cases, is actually showing tolerance for a greater range of variants. Given that the YF group seems to be favouring GS in a range of different environments, we need to explain (a) why GR is favoured by the YF group here and (b) why the pattern for (t) is different from that of (k) for YF. Possibly the weakening process in this position is a gradient phenomenon, and that lenition to GR is an intermediate stage, before GS (though cf. Docherty *et al.* (1997) for arguments against this 'gradience' proposal). The evidence so far seems to suggest that the YF group are favouring GS for both (t) and (k) in a range of environments, but not all environments are affected to the same extent; similarly, there is no reason to suggest that (t) and (k) must be affected at exactly the same rate, so that speakers may favour GS of (t) in one environment more regularly than they do GS of (k). This certainly seems to be what is happening here. If GS is an *established* variant of (t) pre-pausally for the YF group, it is likely to



show a much higher frequency of use than any other variant does; if GS is an *innovative* variant of (k) pre-pausally for that same group, we might expect a more equal distribution of the variants, which is exactly what happens in this case.

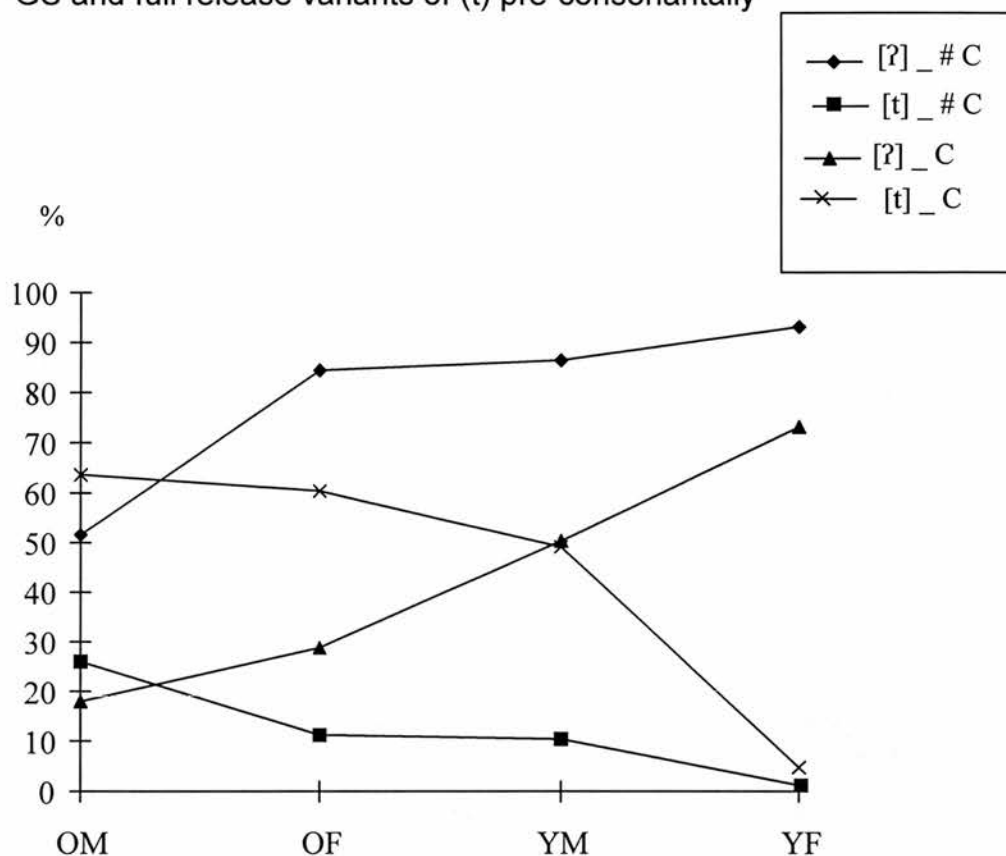
§5.3.3.8 (t) \_\_ true C (e.g. *football* or *boots*)



In some ways, the pattern for (t) word-medially<sup>24</sup> before a consonant (in, for instance, *the cats wallowed*) is similar to that of (t) word-finally when a consonant begins the following word (*the cat swallowed*, as in §5.3.3.1): patterns for GS and full release are summarised in the graph overleaf:

<sup>24</sup> The classification of (t) in *cats* as ‘word-medial’ is perhaps controversial, given that it is questionable as to whether the addition of an inflectional suffix creates a new word. But that is not of concern here. What matters is the extent to which morphological structure affects patterns of GS: if GS is sensitive to morphological structure, then there would be some evidence to suggest it is acquiring properties of a lexical rule.

## GS and full release variants of (t) pre-consonantly



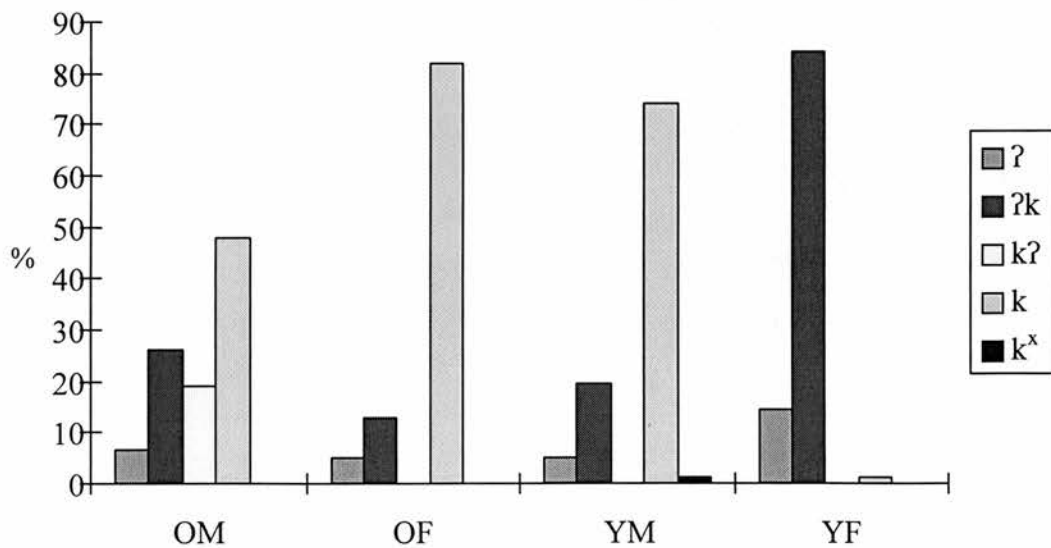
Thus we can see that the YF group is favouring GS in both environments, that OM have a higher incidence of full release in both environments than any other group, and so on. But there are important distinctions to be made too.

The scores for GS are markedly greater in \_\_ # C position than they are in \_\_ C position, and this is true for all groups; by corollary, scores for full release are generally greater in \_\_ C position than they are in \_\_ # C position. So there is evidence to suggest that the environment may be affecting the likelihood of GS occurring. But this is clearly not an across the board phenomenon, affecting each group of speakers in the same way; the differences in scores in the groups suggest that there may be a sociolinguistic correlation.

If the data for (t) \_\_ C is examined more closely, it becomes apparent that there are in fact two broad 'sets' of words within this one environment. With the exception of one word (*atmosphere*), all of the words were either (a) compounds and

words with derivational suffixes, where the variable appeared at the *derivational* boundary or (b) inflected<sup>25</sup> forms where the variable appeared at the *inflectional* boundary. Examples of the (a) set included *football*, *nightclubbing*, *flatmate*, *whiteheads*, *allotment* and *sit-com*; examples of the (b) set included *darts*, *reports*, *gets*, *mates*, *shorts* and *louts*, to name but a few. With the older groups (both male and female), where the variable was at a derivational boundary, the GS variant was selected; at the inflectional boundary, the full release variant was selected. With the YM there was increased variability for *both* sets of words: within the (a) set, GS is favoured, but some instances of *flatmate* had full release, others had GS; *whitehead* had full release, but *Batman* had GS; within the (b) set, full release is favoured, but *shorts* and *dates*, for instance, both had GS. Within the YF group, however, GS applied across the board, irrespective of whether the variable occurred at a derivational or an inflectional boundary.

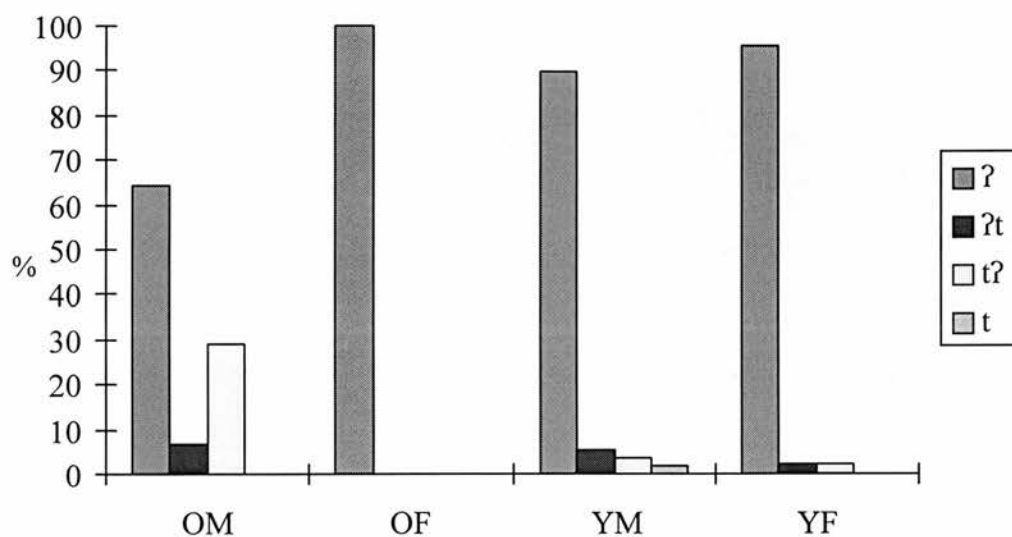
§5.3.3.9 (k) \_\_ true C (e.g. *Parkside*, *mix* or *takes*)



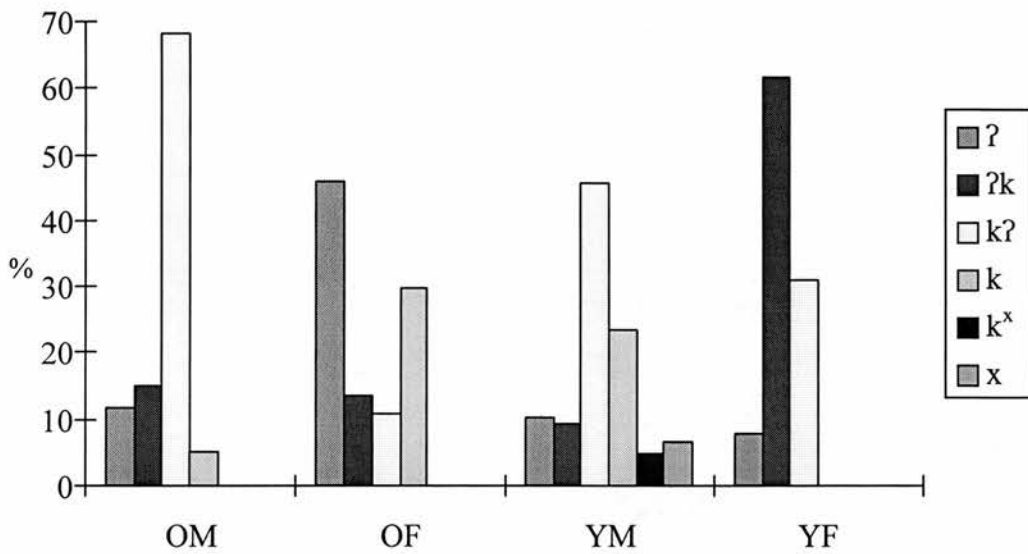
<sup>25</sup> Some of the words were inflected for number (e.g. nouns like *suits*), some for tense/person/number (e.g. verbs like *gets*).

Patterns of (k) before a consonant are slightly different from those of (t) in the same environment, as the graph above indicates. Firstly, the incidence of GS is much lower, with supralocal GR being favoured by the YF, full release by the YM and OF, and a variety of forms being used by the OM group. However, of these variants, the localised GR pattern is evident only when the following consonant is /t/ in e.g. *factory* or *Proctor*. Again, it is possible to suggest that the spread of GS is affecting different variables at different rates in different environments with different groups: here, GS is not as favoured as was the case with (t) in the same environment.

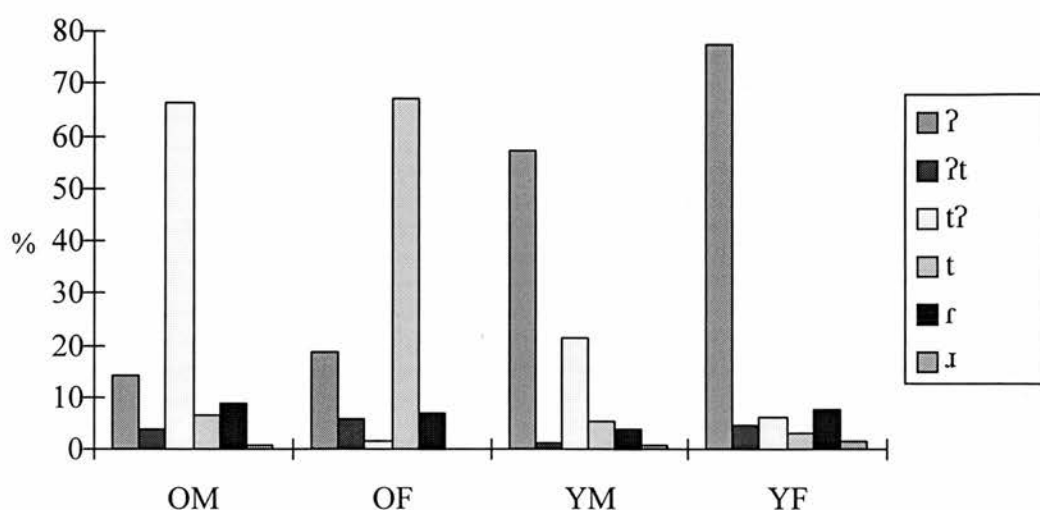
#### §5.3.3.10 (t) \_\_ N (e.g. *Britain*)



As was the case with (t) \_ #L/S, there is a very high use of GS for all speakers here, with a clear indication of allophony rather than variability for the OF speakers in this environment. This is a frequent site for GS in British English generally, according to Hughes and Trudgill (1987: 35). Again, the OM group retain some use of the highly localised GR pattern in this environment, but this is levelled by all other groups, whose use of variants other than GS is fairly minimal.

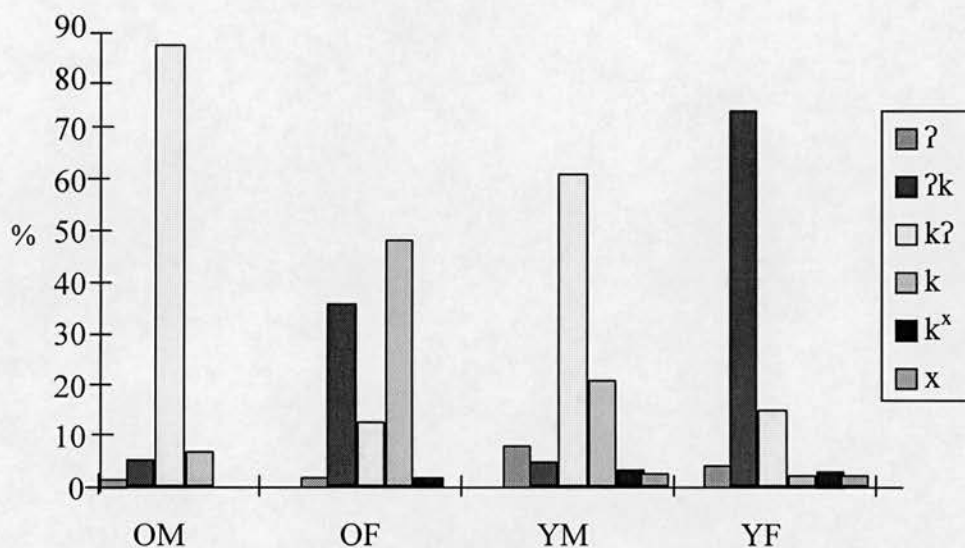
§5.3.3.11 (k) \_\_ N (e.g. *bacon*)

A much wider range of variants is found with (k) in this position than is the case for (t), as a comparison of the previous graph with the one above indicates. The local GR pattern is very much a characteristic of male groups, with female groups again favouring supralocal - but different - variants. Again, there is some evidence, albeit small, of velar fricatives and affricates as variants for the younger speakers, though here it is restricted to the YM and indeed restricted to its occurrence in one word - *fucking*, regularly used by the YM during the recording session.

§5.3.3.12 (t) \_\_ V (e.g. *lighter*)

Carr's (1991) claim that weakening is restricted to functional categories and monosyllabic and polysyllabic verbs with stress on the final syllable (cf. §5.3.3.5 above) is brought into question by the data provided by these informants, since evidence of weakening occurs in adjectives, both uninflected (*beautiful, pretty*) and inflected (*better, later*), proper nouns (*Butty, Butterworth, Porter, Peter*) and count nouns (*butter, quarter, matter, property, computer, photos*). Tapped rather than retroflex variants are favoured here: retroflex variants are found only in *whatever* and *better*.

In contrast to the pattern where (t) appears at a word boundary, each of the social groups favours a specific variant in intervocalic position. The OM group are characterised by use of the localised GR variant, the OF by full release and the younger speakers of both sexes by GS, though there is some evidence of the localised GR pattern among the YM group. This is further evidence of the spread of GS occurring at different rates in different environments with different social groups; in the vanguard of the change, again, are the YF group.

§5.3.3.13 (k) \_\_ V (e.g. *jacket*)

With intervocalic (k), we can see the OM favouring the local GR pattern, as they did with intervocalic (t); however, the progress of GS is much less marked in this environment; it represents only a small proportion of the variants in all social groups, even the YF, who frequently use this form in a wide range of environments, as has been shown throughout the above discussion. Note that the YM group favour the local form (local GR) with intervocalic (k), but the supralocal form (GS) with intervocalic (t), so that again, it is difficult to make watertight generalisations about the correlation of local and supralocal forms with specific social groups: it is not the case that, in every environment, males favour local forms and females supralocal forms; however, there is a preponderance of environments in which this is the case, and it is therefore possible to conjecture that innovations develop at differential rates with different variables in different environments.

There is some further evidence here of the velar affricate and fricative variants, and in this environment there are a number of different words in which such forms appear, namely *lucky*, *knackered*, *shocker*, *weekend*, and *wicked*. Kerswill (personal communication) notes that fricative variants of (k) are found in Durham (though he notes them in pre-pausal position), particularly among young people.



There is therefore the possibility of some sort of diffusion (geographical, lexical, sociolinguistic or a combination of any or all of them) taking place from Durham into Newcastle. However, a larger corpus would be necessary to justify this claim.

#### §5.3.4 Syllable initial glottalling and glottal reinforcement

In the discussion of patterns of G in RP in chapter 3, it was suggested that the domain of G in that variety was the coda. However, a small number of instances in this corpus suggest that this is not always the case in TE. In all but one of the four social groups (the OF group being the exception) there was evidence to suggest that G occurs unambiguously syllable initially, as the following table indicates (the figures indicate the number of instances of G in each environment):

Syllable initial G in TE

'Environments'	OM	YM	YF
<i>to/til</i> (e.g. <i>went to, here til</i> )	5	0	9
<i>time(s)</i> (e.g. <i>sometimes, three times</i> )	2	1	4
<i>-teen</i> (e.g. <i>nineteen, fourteen</i> )	1	4	1
<i>-(i)ty</i> (e.g. <i>celebrity, identity</i> )	2	1	2
<i>-ton</i> (e.g. <i>Washington, Cramlington</i> )	4	0	0

Furthermore, where the variable was preceded by a stressed vowel and a sonorant consonant, and followed by an unstressed vowel, there was evidence of GS: for instance, *contact* surfaces as [kɒnʔaʔk], *altar* as [ɒlʔə] and *presenter* as [pɹæzɛnʔə]; and there were even some instance of foot-initial GS as in *settee* [sɛʔi:] and *tattoo* [təʔu:]. While such instances are rare, they nevertheless need to be accounted for in any theory which attempts to be descriptively adequate. And furthermore, such patterns were as much a feature of the younger groups as they were of the older, so that it cannot be suggested that syllable-initial - and occasionally foot-initial - patterns of G are recessive, or not favoured by younger groups.

The issue is further complicated by patterns of marked ambisyllabicity (on which recall the discussion in chapter 3), and I intend to illustrate this pattern with reference to a specific environment, not detailed in the section above, namely, where the stops appear medially before a liquid or a semi-vowel (i.e. (p/t/k) \_\_ / L or S)<sup>26</sup>.

Consider the data in the following table:

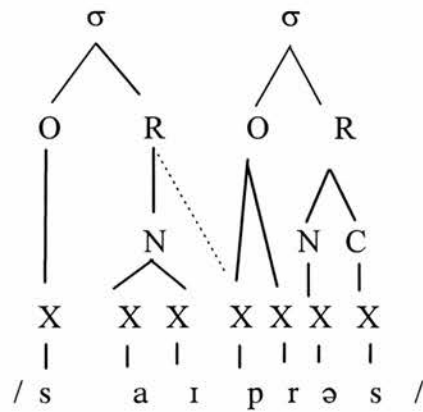
<i>Group</i>	<i>Word</i>	<i>Variant</i>
OM	<i>Cyprus</i>	[pʔ]
	<i>country</i>	[ʔ]
	<i>introduced</i>	[tʔ]
	<i>secret</i>	[kʔ]
	<i>Auckland</i>	[kʔ]
	<i>local</i>	[ʔ]
	<i>equal</i>	[kʔ]
OF	<i>central</i>	[t]
	<i>country</i>	[t]
	<i>Auckland</i>	[k]
YM	<i>country</i>	[ʔ]
	<i>secret</i>	[kʔ]
	<i>sacred</i>	[k]
YF	<i>waitress</i>	[ʔt]
	<i>country</i>	[ʔt]
	<i>matrix</i>	[ʔt]
	<i>Oklahoma</i>	[ʔk]
	<i>lycra</i>	[ʔk]

<sup>26</sup> It is necessary to illustrate this point with reference to the behaviour of the stops in this particular environment, for reasons given in the next footnote.

We can see here that in three of the four social groups (again, the OF group is exceptional here), there are patterns of G (both GS and both types of GR, local and supralocal) which show a marked departure from patterns in RP as discussed in chapter 3. In each of these instances the first syllable is the one which takes the primary stress, except *introduced* where the first syllable has secondary stress; and each of these syllables is heavy (as we would expect if we assume that only heavy syllables can be stressed), in that it has a complex rhyme consisting of at least two X-positions (recall the discussion in chapter 3). But crucially, there is no motivation - in terms of stress - for the stops (whose variants are given in the third column) to be considered as 'part' of that stressed syllable. This contrasts with instances such as *metro* or *macro*, where the first syllable is stressed, but contains only a lax vowel, which is associated with only one X-position: in order to satisfy the condition that stressed syllables have rhymes with at least two X-positions, the stop (which forms part of the second unstressed syllable under the Maximal Onset Principle) must be ambisyllabic. But in *Cyprus*, *central* and *Auckland*, this is clearly not the case. In all of the above examples for the OM, YM and YF groups, the stops undergo either GS or GR and yet are clearly aspirated since the following sonorant is devoiced<sup>27</sup>. In other words, the syllabification of the stops for the majority of these speakers is not identical to that of RP; we need to suggest that the syllable shape for these words for these speakers is different from that of speakers of RP. In *Cyprus*, for instance, the syllable shape is:

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<sup>27</sup> This is why this environment is so crucial for purposes of exemplification based on phonetic behaviour: if I had provided evidence from the environment '\_\_\_ V' it would not have been possible to provide phonetic evidence that the stop is also syllable initial (and thus aspirated), because the key to proving that the stop is aspirated is the devoicing of the sonorant. The phonetic behaviour of the following segment is central for determining whether or not the stop is ambisyllabic; however, that should not prevent us from suggesting that all stops in TE are maximally ambisyllabic, as shall be shown later.



But there are a number of questions still remaining. The first is this: are the patterns with unambiguous syllable-initial G (*times*, - *ton* etc.) part of the same general phenomenon as the patterns with foot initial G (e.g. *tattoo*, *settee*), where ambisyllabicity might be considered even more unlikely, and are either or both of these linked to the ambisyllabicity described above? Secondly, if the patterns *are* linked, how are we to account for this phenomenon in terms of a generalised phonological structure? And thirdly, how are we to link this to the distribution across various social groups? Such questions are investigated in the following chapter.

#### §5.4.4 *Summary of phonological variation*

In chapter 3, I attempted to provide a discussion of issues pertaining to patterns of G in TE and RP; in this chapter, I have provided a range of evidence which suggests that patterns of G (and Weakening) in this corpus are (a) different to RP and (b) different to some existing accounts of TE. Violation of the FRR, for instance, was more widespread in this corpus than is the case for the corpus of TE data used in Docherty *et al.* (1997); and the phonetic behaviour of the stops in certain contexts seems to suggest a different syllabification process in TE than in RP, though the precise nature of the difference is not discussed until the next chapter. However, such differences were not identical in each social group, so that there is indication of potential sociolinguistic conditioning.

In terms of the general distribution of variants, it seems that the YF group in particular favoured GS in a wide range of environments for both (t) and (k), with the OM group favouring very localised patterns of GR (and also Weakening in the case of (t)). However, the following patterns also emerged:

- patterns of GS with (t) were more regular than those with (k) in all environments, while localised patterns of GR were more regular with (k) than with (t) in most environments (cf. Milroy, Milroy and Hartley (1994));
- not all of the environments favoured GS to the same extent: for instance, GS was very regular as a variant of (t) finally before a liquid or a semi-vowel for all social groups, yet medially before a ‘true’ consonant, it was only a marker of the YF group;
- sporadic fricative or affricate variants of (k) were occasionally found in the speech of the younger groups;
- Weakened variants were found to occur (a) not just at word boundaries and (b) in a wider set of lexical items than those predicted by Carr (1991), particularly with the younger groups.

As with the modal verbs, an explanation of these patterns as a whole is reserved for the final chapter; this chapter is intended to present the results for the variables in each environments, and to establish what the general patterns seem to be.

### §5.5 *General summary*

In this chapter, I have provided a detailed description and critique of the methodology I used to collect the data from the TE informants. This included a discussion of the sampling method used, and the social characteristics of the informants and the nature of the recording sessions. I have then presented the results of the analysis of the recordings, beginning first with the modal verbs, and then the

oral stops. In both cases, there were interesting correlations with both social groups and with linguistic environment.

As we shall see in the next chapter, the sociolinguistic patterns are vitally important in explaining the way in which the linguistic structure of some aspects of TE are different from those in standard English and RP. For this reason, I have delayed a discussion of the general patterns - sociolinguistic and structural, at all levels of the grammar - so they can be dealt with in the final chapter, which attempts to provide a synthesis for the findings outlined in this chapter.

## 6 Synthesis

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In this chapter, I aim to provide an explanation of the (socio)linguistic patterns derivable from the data presented in chapter 5. Particularly, I want to examine the extent to which we can - and indeed should - synthesise the sociolinguistic patterns, especially as they relate to gender-based variation as discussed in chapter 4, with the structural issues which comprise chapters 2 and 3. The first section of this chapter examines the modal verb variations; and the second concerns the glottalisation, glottalling and other weakening phenomena found in the corpus. Throughout these two sections, I will be attempting to show how and why it is necessary to consider the data in the light of both structural and sociolinguistic frameworks, and to provide a view of linguistic variation and change which takes on board the sociolinguistic variability in its account of language structure. The third section considers some further sociolinguistic issues, particularly the distribution of both the modal verb variations and the phonological variants by gender and age, specifically in relation to the variable use of localised forms, and how this relates to patterns of dialect levelling.

### §6.1 *The modals*

Perversely, perhaps, I want to begin this section with a discussion of a feature (or a specific set of features) which did *not* appear in this corpus, but which has been reported in previous accounts of TE modals (such as Beal (1993) and McDonald (1981)), namely instances in which certain of the modals appear to behave, for a variety of reasons, more like lexical verbs. Consider the following examples, from McDonald (1981):

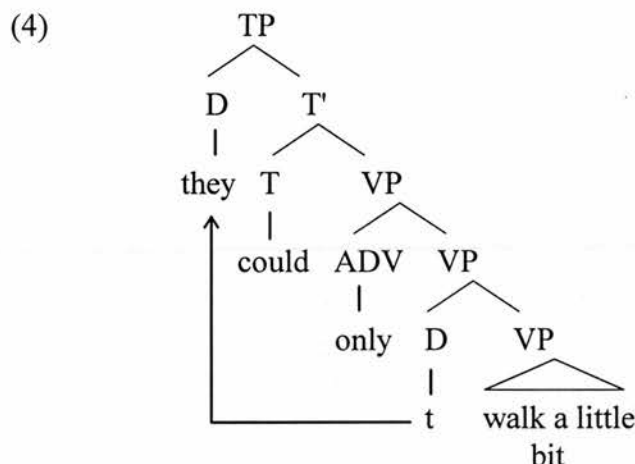
- (1) if they only could walk a little bit, they should thank God
- (2) Oh no! They're double-glazed. They wouldn't could [break GT]



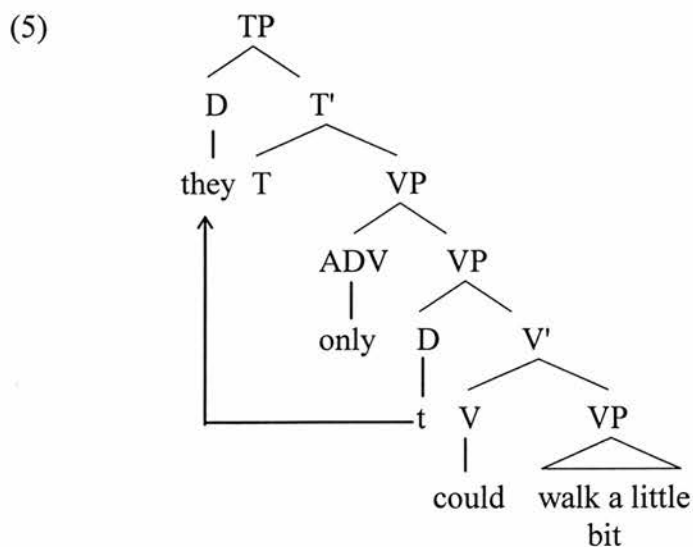
In (1), the placement of the adverb *only* before the modal is not characteristic of standard English, in which dialect such an adverb would appear before the lexical verb<sup>1</sup>:

(3) if they could only walk a little bit

In (3), *only* behaves like other adverbs such as *hardly* and *barely*; let us class such forms as VP adverbs, adjuncts which attach to VP in the following way as illustrated<sup>2</sup> in (4) for the standard English example *they could only walk a little bit*:



This is structurally different from the TE pattern, however, and the analysis of *they only could walk a little bit* is given in (5):



<sup>1</sup> The adverb can also appear directly after the conjunction (as in *if only they could walk a little bit*), though I do not deal with this here.

<sup>2</sup> I am ignoring the Agr projection here, as it is not relevant to the present discussion

The position of *could* in relation to the VP adverb *only* is a reflection of its syntactic status in the two varieties: in standard English *could* is generated under T; in the TE example, it must be generated under V; the tense properties of V (here [Past]) percolate up to T via a process known as attraction (Chomsky 1995: 139, 297ff).

Further verb-like properties of *could* are shown in (2) above, where the modal appears in a non-finite position. These double modal constructions are said to be widespread in the TE vernacular (cf. Trudgill 1990, Roberts 1993, de la Cruz 1994, 1996); however, it should be remembered that only one instance of a double modal construction arose in that part of McDonald's (1981) corpus which consisted of naturally occurring Tyneside speech: all of the other instances were attested utterances, i.e. reported occurrences of dialect features said to be characteristic of the local vernacular. Such a data set must be treated with caution, as the extent to which the form is regularly used in TE is by no means clear.

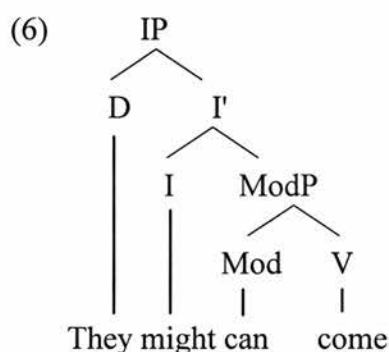
Nonetheless, the form is reported as having been a feature of TE until fairly recently, and double modals seem to be fairly widespread in other areas of the English-speaking world, as noted by de la Cruz (1996: 76-77):

The areas of the English-speaking world where double (or multiple) modals are found are Scotland, the Northumbrian borderline and Tyneside, in the British homeland, and the South-East of the United States, across the Atlantic. They are also known to have existed in Northern Ireland, but their occasional occurrence there nowadays is probably due to contemporary sporadic Scottish influence.

This construction therefore needs to be accounted for within the framework presented elsewhere in the thesis. Roberts (1993) does not deal in detail with the double modal construction in his treatment of the diachronic development of the modals in general - and indeed there is some debate as to whether the construction is an innovation in some varieties, rather than a retention of an earlier trait (on which see further de la Cruz (1994), Nagle (1989)) - but such issues are really peripheral to the heart of the

matter. What we can see in instances such as (2) above is further evidence for *could* in TE behaving more like a verb. But how are such constructions to be analysed?

In his analysis of double modals, Radford (1997: 203-4) argues for the adoption of a Modal Phrase, presenting the following analysis for *They might can come*:



But such an analysis seems highly awkward: generating *can* under neither I nor V for this particular dialect seems stipulatory rather than explanatory, especially given that in *They can come*, *can* would be generated under I (cf. Radford 1997: 178). Labov (1972), Plank (1984) and Miller (1993) all favour an analysis in which *might* in a construction such as (6) would be more favourably analysed as an adverb, but as Roberts (1993: 333) points out, the “problem with this is the fact that negation can associate with *might* (as well as with *could*), while this of course is impossible with adverbs” and gives the following evidence from American English: *They might not could have gone over the state line*, and *I was afraid you might couldn't find this address*, where the first example illustrates the association of the negative with *might*, and the second with *could*.

The issue of finiteness is crucial, since the second element of a double modal construction is non-finite, and non-finiteness is not usually associated with forms generated under T. The non-finite issue is a very complex one, and one which is not dealt with in any depth here; in what follows, my discussion of the historical materials is very selective. The following data is taken from Denison (1993: 310-1), which he in turn cites from Visser (1963-73).

In earlier stages of the language, in addition to double modals of the type shown in (7), from Denison (1993: 310)

- (7) þatt I shall cunnen                      cwemmen      Godd  
       that I shall have ability (INF) please(INF)      God  
       ‘that I shall have the ability to please God’

there are instances of what appear to be participial forms of the modals, as in (8) and (9), from Denison (1993: 311):

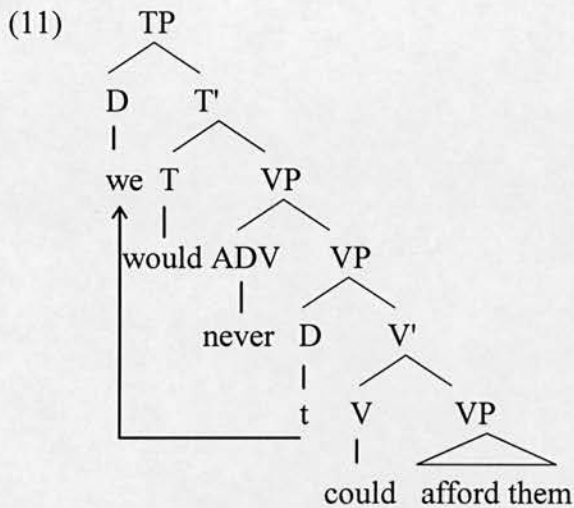
- (8) yf we had mought                      conuenyently come togyther  
       if we had might (PAST PTCP) conveniently come together
- (9) maeyinge                      suffer no more the loue & death of Aurelio  
       may (PRES PTCP)              suffer no more the love and death of Aurelio

I classify all of these - infinitival forms and both participial forms - as part of the same phenomenon: non-finite forms of the modals (as does Denison 1993). The extent to which each of the modals retained non-finite forms is variable diachronically, and also clearly diatopically. Warner (1982) argues that no non-finite forms of *mot* and *shal* are to be found in OE or ME; participial forms of *might* and *may* by contrast continue into eModE, as examples (8) - from 1528 - and (9) - from 1556 - illustrate; Denison (1993: 336) quotes Roberts (1990: 47), who suggests that *will* and *can* are the last English modals to lose non-finite forms.

To return to the present-day example given for a double modal construction in TE, the appearance of *can* as a non-finite form lends weight to the argument that *can* in (6) should be generated as a verb. Furthermore, that the second element of a double modal construction always displays some sort of root modality (cf. Roberts 1993: 317) would suggest that these elements have the power to assign  $\theta$ -roles, which is again more a characteristic of substantives than of functional heads, as discussed in §2.4; and in examples such as (10), which is a further attested utterance from McDonald (1981), the position of the VP adverb *never* again suggests that the second element is more ‘V-like’ than ‘T-like’:

(10) they had velvet curtains, and, oh, we would never could afford them

A potential structure of *we would never could afford them* is given in (11) below:



This is a very complex issue, which I do not pretend to have resolved here; my aim was to show that, in certain constructions, forms which are regularly generated under T in TE seem to be much more ‘verb like’ than their standard English counterparts. Crucially, however, the data provided by McDonald (1981) and Beal (1993) suggest that this ‘verb-like’ property is only really characteristic of *can* and *could*, and not of any of the other modals. And here we can bring in a feature of *could* characteristic of the informants for this thesis: recall in §5.3.1.2, the tense-time relationship between *can* and *could* was a feature of the older informants in this study; that is, for the older informants, *could* functioned regularly as a marker of past time for the 45 to 60 year old age group, while for the younger informants, it was a marker of unreality or tentativeness.

These features would all seem to point to the same thing - in terms of the Diachronic Reanalysis of the modals (cf. §2.4.4), we must conclude that the DR has not been *fully* implemented in the sociolects of these groups. Roberts (1993: 315) schematises the DR as follows ((129) in chapter 2, repeated here for convenience):



construction, in a set of texts”, and that the drift from root to epistemic modality in the history of the English modals can be thought of as one such Step. Evidence of this semantic drift is clear with two of the TE modals. Both *must* and *might* are clear markers of epistemic modality: there were no instances of root *might*, and all instances of root *must* were formulaic, and functioned as male discourse markers. This raises two related questions: why should such a change take place, and why should there be some indication of a gender difference, where some instances of *must* - the only root instances - seem to be a characteristic male discourse marker? This pattern, I think, provides an excellent example of why it is necessary to synthesise the findings of structural and sociolinguistic research, as I demonstrate below.

In Coates (1989), the author discusses some patterns of language use in all-female groups. This research addresses the issue of the function of gossip in women’s speech, suggesting that its chief goal is the “maintenance of good social relationships” (Coates 1989: 98) and that it promotes co-operativeness between the participants. Issues such as turn-taking and minimal responses are considered, and Coates illustrates specific discourse patterns of female speech in this regard. One of the specific patterns addressed is women’s use of epistemic modal forms. Now Coates does *not* make reference to modal verbs: she illustrates her argument with reference to modal adverbs such as *possibly*, and tags such as *you know*, among many others. Such patterns are shown in the following examples in (13) below, from Coates (1989: 114):

(13) *I mean I think it really depends on the attitude of the survivors who are there*

*she looks very sort of um kind of matronly really*

All the italicised forms in (13) above are classed as epistemic modal forms by Coates; and Coates argues that women use such forms “to mitigate the force of an utterance in order to respect addressees’ face needs” (Coates 1989: 114). Later in the



same paper, Coates goes on to suggest that these forms are “used to respect the face needs of all participants, to negotiate sensitive topics, and to encourage the participation of others; the chief effect of using epistemic modal forms is that the speaker does not take a hard line” (Coates 1989: 119).

But it would clearly not be unreasonable to suspect that women could make use of any linguistic item which can express epistemic modality to fulfil the discourse functions discussed above; and we could therefore suggest that women might exploit the use of the epistemic interpretations of the modal verbs in their negotiation of certain discourse topics. If, as Coates suggests, males in all-male conversations tend to talk more about topics which “do not trigger the use of epistemic modal forms because they are not so face threatening” (Coates 1989: 115), we could expect male discourse to be characterised by root interpretations of the modals; and this is certainly the case with *must* in this corpus: the root interpretations (though rare generally) were characteristic of the male groups, both older and younger.

I am not suggesting that the semantic drift in the modal system of English, from root modality to epistemic modality, can be explained simply and wholly in terms of women’s exploitation of certain discourse markers; but what I am suggesting is that such discourse features may be a *factor* in the development. The role of women as innovators in changes in phonology was extensively discussed in chapter 4; there is no reason to suppose that women do not function as innovators in syntactic changes also. And if we assume that the change is toward an epistemic reading, with root variants of *must* becoming more and more marginalised, we can suggest that TE is more innovative in this regard than is the standard variety, by comparing the data used in this corpus with those used in Coates (1983) and Hoyer (1997), both of whose corpora focused on standard English. Hoyer (1997) examines the use of modal forms in combination with certain adverbs, and finds that in his

corpus, from the Survey of English Usage, adverbial combinations with *must* “encode epistemic and non-epistemic modal meanings in roughly equal proportion, ... (53 per cent and 47 per cent respectively)” (Hoye 1997: 101), while Coates found epistemic *must* to be *rare* in her corpus.

There were no indications of a difference in the modal meanings of *might* which could be correlated with the different genders; that is, the male groups did not use root interpretations of *might* as a distinctive discourse feature as they did with *must*. All instances of *might* were epistemic. But *might* is seen in the standard variety as a more tentative marker of epistemic possibility than *may*; so use of *might* as opposed to *may* could therefore be seen as a further means of protecting an addressee’s face; *might* functions as a more salient hedge than *may*, and could therefore be exploited in discourse as such. This is clearly different from the discussion of *must* above; with *must*, the change concerns epistemic and root interpretations of the same verb; with *might*, the change concerns epistemic interpretations of that verb with epistemic interpretations of another (*may*). But nonetheless, discourse factors - in terms of face protection, for instance - may come in to play. However, there is no evidence in the present corpus that gender is a significant factor in the development of *might* as the TE marker of epistemic possibility to the exclusion of *may*. The rise of *might* is most likely linked to the rise of *can* as a marker of deontic possibility: *may* has suffered a pincer attack to both fronts of its modality, from the forces of *can* on one (root) side and the forces of *might* on the (epistemic) other, not helped by insurrection within its own ranks: the failure of epistemic *may* to undergo I-to-C movement (*\*May he be in his office?*) and the failure of root *may* to host the negative clitic (*\*He mayn’t come in until I’m ready*). It looks as though *may* - in TE at least - is heading for a dishonourable discharge from the modal core/corps.

There are, however, other changes ongoing in the TE modal system which are not as clearly linked to the ‘root-to-epistemic Step’ which Roberts (1993) proposes for the history of the English modals, and I deal with these in the remainder of this section. The first is the ‘monosemy’ of *can*, *could* and *should*; the second is the development of *gotta*; and the third is the use of the clitic forms ‘*ll* and ‘*d*. Again, I think the most profitable way of thinking about these variations is to attempt a synthesis of sociolinguistic and structural knowledge and theory.

As discussed in chapter 5, *can*, *could* and *should* in this corpus of TE were all markers of root modality, and clearly we want to find out why this is so. Firstly, evidence from the standard variety would suggest that epistemic readings of *can* (especially) and *could* are rare<sup>5</sup> anyway; so in terms of dialect contact, speakers of TE regularly exposed to standard English, whether it be through the media or through face-to-face contact with standard English speakers<sup>6</sup>, are much more likely to be exposed to root rather than epistemic variants of *can* or *could*. Furthermore, as discussed above, *can* and *could* in TE are the two forms most likely to show more ‘verb-like’ patterns than any of the other modals; and root modals are much more ‘verb-like’ than epistemic modals are, as discussed in §2.4.4. So in terms of contact with both vernacular TE and standard English, the speakers of TE who formed the sample for this survey are far more likely to be exposed to root meanings of *can* and *could* than they are with epistemic ones, irrespective of whether that contact be with more vernacular or with more standardised speakers.

This may not be the case with *should*, however. As discussed in §§2.3.2.7 and 5.3.1.6, *should* in the standard variety is a marker of both epistemic and root modality, as can be seen in the following examples - *should* in (14a) is epistemic, but root in (14b):

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<sup>5</sup> See Høye (1997: 85ff) for a useful recent summary of the controversies surrounding *can* and *could* within the standard variety.

<sup>6</sup> The issue of the media in the development of a favourable attitudinal environment for the adoption of in-coming variants in dialect contact scenarios has recently been debated in a number of works, such as Trudgill (1986), Docherty and Foulkes (1999) and Williams and Kerswill (1999).

- (14a) It was not the prospect of being deprived of money, keys, wallet, letters, books, long-playing records, drinks, the opposite sex, and other solaces of adulthood that upset me (I should have been about eleven), but having to put up indefinitely with the company of other children  
(Larkin: 1983: 111)

- (14b) You should read more

In this corpus of TE, however, there were no clear examples of (14a) - one example was indeterminate between a root and epistemic reading, even within the context of the discourse, and the rest were clearly root. This pattern is harder to explain than that of *can* and *could*; nor can we explain the TE uses of *should* as some sort of periphrastic subjunctive (cf. Anderson 1991), since the data would not support such a reading of *should*. It may be that, if root meanings of *should* are the majority form in the standard variety, then those in contact with that variety should adopt that form more regularly than the epistemic meaning, given that one of the principles of dialect contact scenarios is that majority forms win out (cf. Kerswill and Williams 1994). But why *should* should behave as it does in TE is by no means clear; it may be that, given the fact that *shall* no longer functions as a modal in TE - so that *should* stands alone, not part of a tense-related pair - *should* therefore falls in with *must* and *might* in expressing either root or epistemic modality but not both. But why this should be is not clear either. Again, one possible explanation is concerned with dialect contact scenarios, in which there is a move from diffusion to focusing (cf. Kerswill and Williams (1994: 9)); TE speakers, in their contact with the standard variety, focus on one meaning of *should* (the root interpretation) above others (the epistemic and periphrastic subjunctive readings); but the motivation for such a selection remains unclear.

The pattern with *gotta*, by contrast, is much clearer. Recall from §5.3.1.9 that *gotta* seems to be developing alongside *have got to* and *have to*, where all three in TE are markers of root necessity. In non-finite positions only *have to* can appear, whereas *gotta* is restricted to finite position: in this sense, *gotta* and *have got to* are

more 'modal-like' than *have to*. In finite positions, all three are possible, but *gotta* does not occur with third-person singular subjects (*\*he gotta go*). In addition, *gotta* is not an operator (*\*I gottan't go*, *\*Gotta I go*) - yet it cannot take *do*-support either in this variety (*\*I don't gotta go*, *\*Do I gotta go*). This contrasts with some other varieties of English, such as American English, in which *gotta* does appear in negatives and interrogatives, but not as an operator (that is, it requires *do*-support). In other words, *gotta* is restricted by clause type to positive declaratives. Furthermore, in TE, in positive declaratives with non-third-person singular subjects (i.e. constructions with no syntactic constraints as to which form can be selected), there was a significant correlation<sup>7</sup> between the variant selected and gender, with males favouring *gotta*, and females favouring *have to* (with frequencies for *have got to* roughly equal for each gender). The correlation with age was not significant. Particularly, the younger males seemed to favour *gotta* more than the other groups, in noticeably marked contrast with the younger females, from whom not one instance of *gotta* was collected. This could signify a change from below, with the younger males in the vanguard of the change. Here, then, is an example of a change whose development is constrained by both structural and sociolinguistic factors: on the structural side, *gotta* is restricted (a) by morphosyntactic features (to finite position); (b) by clause type (to positive declaratives) and (c) by subject type (to non-third-singular subjects); and on the sociolinguistic side, it is more regularly a feature of the young male group than of any other.

Clause type and subject type are also constraints for the distribution of the clitics 'll and 'd. Recall from §5.3.1.10 that the L and D forms ('ll and 'd respectively) are not distributed uniformly by clause and subject, nor by social group. The L forms were favoured above the full forms *will* or *shall* where the subjects were personal pronouns; but there were some instances in which a non-personal-pronoun

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<sup>7</sup> Recall, however, that the numbers involved were very small (a total of 79 variants).

subject could host the clitic, as in (15) below ((33) in chapter 5, repeated here for convenience):

- (15) students who are gonna move out next year'll have bought and rented the properties for next year already

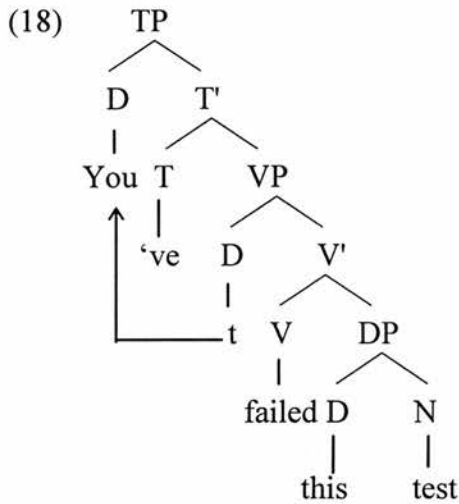
Now this is only one isolated example; but it is important, given claims like those made in Radford (1997: §8.5), which I discuss here in some detail. I present all of Radford's claims first, and then point out some of the problems with the analysis. In this section of his book, Radford discusses the ways in which case-features of subjects can be checked in the course of a derivation, and specifically attempts to explain why constructions such as (16) are grammatical, while those like (17) are not:

- (16) You've failed this test  
 (17a) \*Bob and you've failed this test  
 (17b) \*The Geordie'll fail this test  
 (17c) \*Guy'd fail this test

In his discussion, Radford separates auxiliary clitics out into two discrete groups: *reduced* forms such as [əl] for *will* and [əv] for *have*, and *contracted* forms such as [l] and [v] for the same auxiliaries. In other words, reduced forms consist of a vowel plus a consonant, while contracted forms consist solely of consonants. His discussion concentrates exclusively on the contracted forms: thus *you've* in (16) rhymes with *move*, *Geordie'll* in (17b) rhymes with *raw deal*, and *Guy'd* in (17c) is homophonous with *guide*.

Radford proposes two potential explanations for the grammaticality of (16), one traditional and uneconomical, the second innovative and economical. The traditional explanation is that the subject of *fail* raises from spec-VP to spec-TP and that "(for phonological reasons) the contracted auxiliary subsequently attaches to the pronoun in the PF component" (Radford 1997: 330), where PF stands for Phonetic Form, the PF component being that component in the grammar in which the

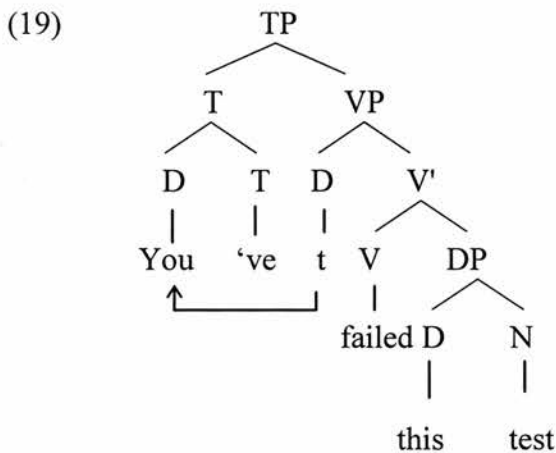
specification of the pronunciation of any given sentence is determined. In other words, we have a derivation such as that in (18):<sup>8</sup>



Here, then, case is checked via the specifier-head relation, not via head-to-head adjunction; this latter is, according to Radford, an alternative analysis, and is more economical than the specifier-raising illustrated in (18). In this adjunction process, the contracted auxiliaries are bound forms, “affixal heads which are only legitimate PF objects if another head attaches to them”. Radford argues that the head to which these contracted forms attach are the subject pronouns: the “subject pronouns (by virtue of being heads) check their nominative case by adjoining directly to T” (Radford 1997: 330). Thus the derivation of (16) according to this new proposal is as given in (19):

<sup>8</sup> I ignore the issue of vp shells here as they are not relevant to the present discussion.





In other words, auxiliary contraction is not merely associated with PF, but takes place at an earlier stage of the derivation of the sentence; it is only possible “as part of a checking via head-adjunction process which ... is designed to check the case-feature of *you* and the agreement-features of *have*” (Radford 1997: 331).

The ungrammaticality of sentences such as those in (17) is explained as follows: since the contraction involves checking via head-adjunction, contracted auxiliaries can only attach to heads, not phrases; given that *Bob and you*, *the Geordie* and *Guy* are all DPs (the last headed by a zero determiner), checking here must be via movement to spec-TP, which blocks contraction.

I have tried to summarise Radford’s analysis as clearly and concisely as possible; but I think there are a number of problems with his proposals, which I deal with in turn below.

Firstly, how is *you* in (16) a head? No justification is given for this claim; and in most other analyses within this syntactic framework, it would be classified as a specifier (as Radford himself does earlier in his work (1997: 90-1)). Of what phrase is it the head? None of this is made clear, and it surely should be clarified, given the seemingly radical reanalysis of the constituent from specifier to head.

Secondly, how is the head adjunction analysis more economical, given that all other subject types (i.e. full DPs) must check features via specifier movement? It is

not clear what is gained by making the subject pronouns exceptions in this regard, other than an attempt to explain why the examples in (17) are ungrammatical.

Thirdly, the division of the clitics into reduced and contracted forms is somewhat arbitrary, a stipulatory rather than an explanatory analysis; and furthermore, I would question the claim that full DPs cannot host cliticized auxiliaries (be they reduced or contracted). The example from the TE corpus (i.e. (15) above) is a complex DP, where the noun is postmodified by a relative clause: and yet the auxiliary is cliticised. More specifically, the auxiliary in this instance was contracted, not just reduced, so that *year'll* rhymed with *earl*. So Radford's claim that sentences such as those in (17) are ungrammatical is factually incorrect for TE: not only is (15) a counter example but so is (20) below, again from the younger males:

(20) Tommy'd get in like

Clearly Radford's analysis is inadequate for the grammar of these TE speakers: not only are there contentious issues surrounding the grammatical analysis of some parts of the constructions, but also there are certain subject types which host the contracted auxiliaries in TE but cannot do so in Radford's conception of the standard variety.

There is a different proposal which could be made, however. I restrict the following discussion to the L and D forms discussed above. Let us assume that all subjects are specifiers of VP, and raise to spec-TP (and thence to spec-AgrSP) to check features, whether they be pronouns or full DPs. Let us further assume that, when they are generated, *will*, *would*, *'ll* and *'d* are *all* generated under T (in TE at least; the standard variety is of no concern here) - in other words, contraction is not simply a property of the PF component - but that the extent to which they are so generated is sociolinguistically and structurally determined. That is, a young male TE speaker regularly generates L forms under T when the subject is a personal pronoun, as do most other TE speakers; but in addition to this general TE pattern, the

young male generates L forms with a wider range of subject types (including DPs). Similarly, with the D forms, the older TE speakers in this sample only generate D forms under T when the subject is a personal pronoun (cf. §5.3.1.10, table 13), generating W forms with all other subjects. By contrast, the younger speakers of TE, both male and female, generate D forms with pronominal and DP subjects. This could potentially be seen as evidence for a type of Parameter Resetting in (21) below:

(21) For every contracted T, is the subject a pronoun only?      Yes/No

For the older TE speakers in this sample, where T is D, the parameter is set at Yes; for the younger speakers it is set at No. But this does not do justice to the range of variation in the use of L and D forms (and for the whole host of other contracted auxiliaries not investigated here). What we really want is an analysis which can account for the variation in terms of *weighted* parameters, rather than binary yes/no choices, so that account could be taken of what T is, what clause type is involved, and what the structure of the subject is. The idea that clause type and subject type are central factors in the progress of syntactic and morphological change is well known, and can be seen in the discussions of historical syntactic change (cf. Kroch (1994) and Hudson (1997a, b) on the development of *do*-periphrasis) and on-going changes in British English, such as are described in Tagliamonte's (1998) analysis of *was/were* variation in York, and Cheshire's (1982) survey of grammatical variation in the speech of Reading adolescents. But in addition to this, we need to consider how the variants are distributed in the speech community, so we can take account of structural and sociolinguistic factors in the linguistic analysis that is proposed.

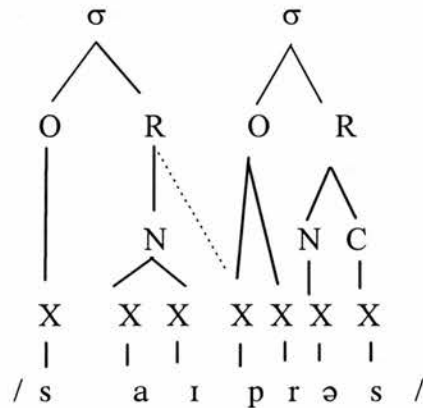
## §6.2 *The oral stops*

As with the modals in the section above, in the following discussion of variants of the oral stops, emphasis is placed on providing an account which attempts to marry aspects of phonological structure (which are more readily associated with

phonological theory) with the sociolinguistic distribution of the variants. There are three main areas to be discussed: the syllabification of the stops, given the evidence provided by the phonetic characteristics of the variants; the notion that glottalling of (t) when that variable is pre-pausal is developing in the speech community by means of a lexical rule; and the distribution of tapped and retroflex variants of (t), that is, [ɾ] and [ɽ]. I deal with each of these in turn below.

Firstly, then, the issue of syllabification is to be discussed. In chapter 5, I argued that the phonetics of *Cyprus*, as produced by one of the older male informants, with glottalisation and aspiration of the stop (the latter indicated by devoicing of the following sonorant) suggested the following syllabification pattern:

(22)



and I suggested that such a pattern was different from RP. We therefore need to explain how such syllabification arises, in terms of principles of syllable theory, and also how such syllabifications are not characteristic of every social group; again, we need to make reference to structural and sociolinguistic theory.

Let us assume that ambisyllabicity in TE applies at both the lexical and the postlexical levels; to put this another way, let us suggest that ambisyllabicity in TE is both a lexical and a postlexical process. The lexical ambisyllabicity in TE is exactly the same as RP and other varieties of English, and is invoked to explain why /t/ in *pity* is associated with both the coda of the first (and stressed) syllable as well as the

onset of the second unstressed syllable. This syllabification is discussed immediately below; it is classed as lexical because it predicts why forms like \*/pɪ/ are ill-formed as lexical words, and as shall be shown, this is linked to the ambisyllabicity of /t/ in *pity*.

Two specific rules are of importance here; the first is the Maximal Onset Principle (cf. chapter 3), and the second is the Complex Rhyme Condition:

(23) Maximal Onset Principle

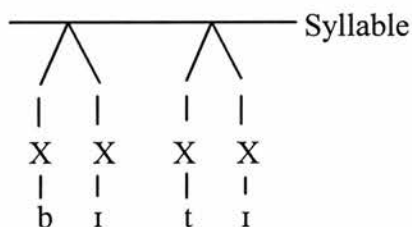
On the condition that no phonotactic constraint is violated, syllable boundaries are to be placed at that point at which onsets are maximised.

(24) Complex Rhyme Condition

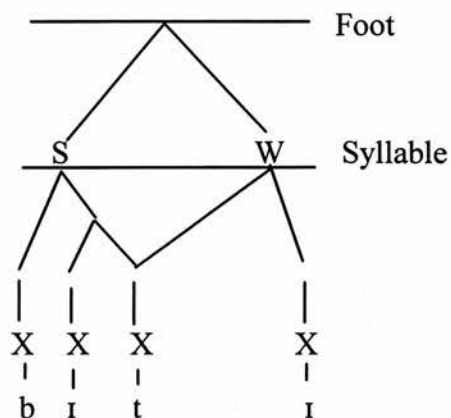
Stressed syllables must contain rhymes associated with at least two X-positions

Application of (24) blocks the formation of \*/pɪ/ as a well formed lexical word, in TE as in any other variety of English; and the formation of *bitty* and *Beatie* are illustrated in (25) and (26) respectively below:

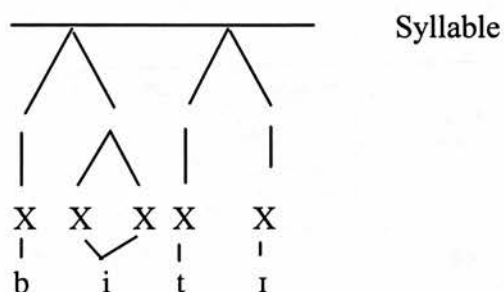
(25a) *bitty*: after application of (23):



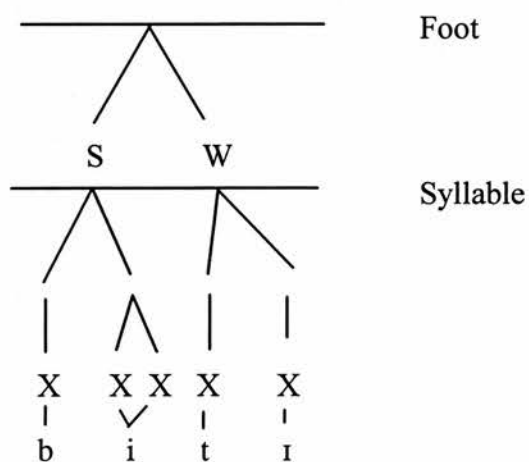
(25b) *bitty*: after application of (24):



(26a) *Beatie*: after application of (23)



(26b) *Beatie*: after application of (24)



There is nothing to differentiate RP and TE with regard to this lexical ambisyllabicity: in both accents, the motivation to ambisyllabify the stop in (25) is

provided by the constraint in (24); by contrast, in (26), the rhyme is already complex, so no ambisyllabicity is motivated.

However, additionally in TE, there is a postlexical ambisyllabification process, which I formulate in (27) below:

(27) Postlexical Ambisyllabification (PA) (TE, not RP)

Ambisyllabify all voiceless oral stops<sup>9</sup> in intersonorant position, irrespective of boundaries

(27) would then account for the analysis of *Cyprus*, given in (22) above, which is not accounted for by (23) and (24) - the lexical process would fail to ambisyllabify the stop, suggesting that the stop would be syllable initial, and thus liable to undergo aspiration but not glottalisation or glottalling. Furthermore, (27) can account for other patterns too: glottalisation of (t) in (a) *three times* and (b) *tattoo* are both motivated by (27) despite the fact that in (a) the ambisyllabification is across a word boundary and in (b) the target stop is foot initial. All the patterns of glottalisation and glottalling in this corpus of TE given in §5.3.4 can be accounted for by (27).

But (27) as it stands is too powerful, in that it would also predict *this room is a tip* as potentially surfacing in TE as (28):

(28) \*[ðɪs ɹu:m ɪz ə ʔɪʔ]

and this is clearly undesirable. We therefore need a set of constraints for (27). Ideally, we need a ‘cline’ of environments in which the PA takes place, from least frequent to most frequent; this is proposed in (29):

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<sup>9</sup> Whether ambisyllabification of segments other than the voiceless stops takes place in such an environment is outwith the scope of this thesis.



(29) *Environments for PA in TE*

Increasing likelihood of ambisyllabicity	↓	(a)	stop is both word and foot initial
		(b)	stop is foot initial, but not word initial
		(c)	stop is word initial, but not foot initial
		(d)	stop is medial; coda of the stressed syllable is complex <sup>10</sup>
		(e)	stop is medial; onset of the unstressed syllable is complex
		(f)	stop is medial, and is immediately preceded by tense vowel or diphthong, and immediately followed by unstressed vowel

In other words, (29) suggests that glottalisation of the stop is least likely in environment (a), in instances such as a *tip* or *three times*, and the likelihood increases in instances such as *thirteen*, *tattoo*, and *settee* (environment (b)), *listen to* (environment (c)), *introduce* (environment (d)), *Cyprus* (environment (e)) and *Peter* (environment (f)). This order is suggested by the frequency of glottalisation of the stops in these environments based on the data collected for this thesis: for example, the only instance of environment (a) was in the phrase *x times*, where *x* is a numeral (eg *two times*, *four times* etc.), so ambisyllabicity of the stops in environment (a) is practically non-existent (hence the ill-formedness of (28) above). The same is true of (b) and (c) where there are only a handful of words which display this pattern. Clearly word and foot initial glottalisation is highly constrained in TE, especially word initial position, being restricted merely to *time(s)*, *til* and *to*; thus we could reformulate (27) as (30) below:

## (30) PA (TE only)

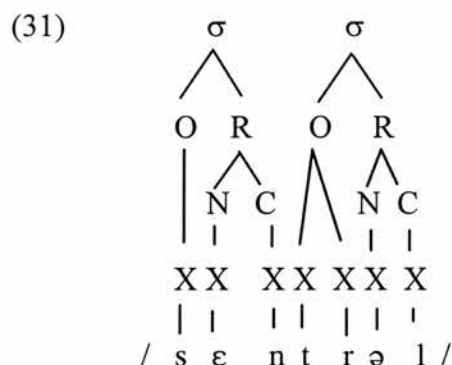
Ambisyllabify all voiceless oral stops<sup>11</sup> in intersonorant, non-word initial position.

Exceptions<sup>12</sup>: *to*, *til*, *time(s)*

<sup>10</sup> 'Complex' in (29) (d) and (e) refer to the structure of the coda and onset respectively after the incorporation of the stop.

<sup>11</sup> Once more, whether ambisyllabification of segments other than the voiceless stops takes place in such an environment is outwith the scope of this thesis.

However, even these constraints are insufficient, because the conditioning is not simply linguistic, but sociolinguistic also; we need to incorporate the sociolinguistic distribution of such variants to achieve a fully adequate picture of this phenomenon in TE; again there is a need to synthesise the structural and the sociolinguistic patterns. Specifically, the speech patterns of the older female group stand out here; the environments in which PA seems to have taken place for this group are environments (e) and (f); in other positions, no PA takes place. This accounts for the data in §5.3.4, where it was shown that in *central*, *country* and *Auckland*, the stops were not glottalised, yet the following sonorants were devoiced. This suggests, for the older females, a syllabification pattern of *central* as that given in (31) below:



Thus the PA specified for TE in (30) may be variable across the TE speaking community; certainly in this corpus, the older females had a more restricted application of (30) than is the case for the other groups.

Clearly we want to explain why this should be: why should the older females show a restricted application of (30)? A possible reason for this might well be tied in with other features of glottalling, glottalisation and weakening in TE discussed in chapters 4 and 5. Recall that previous investigations of glottalisation etc. (Milroy, Milroy and Hartley (1994), Docherty *et al.* (1997)) suggested that women -

<sup>12</sup> That a postlexical rule should have lexical exceptions may be an indication that it is becoming lexical, though there is no apparent time evidence for this based on the data collected here, ie there is no indication of a wider set of lexical exceptions being used by the younger speakers in comparison with the older ones.

particularly young women - were favouring supralocal variants (in the TE case GS variants, cf. chapters 3 and 5), while the males were favouring the localised forms (GR (local glottal reinforcement) and weakened variants). Given that the PA is a localised pattern - different from the lexical ambisyllabicity which characterises both TE and RP - we might expect the female groups in this sample to avoid this pattern, at least to some extent; and that seems to be what is happening with the older females: they have a restricted application of a localised postlexical rule.

However, this could raise a potential problem for an analysis of the speech patterns in the younger female group; if the use of non-localised variants is a characteristic of female speech in general, surely we should expect the younger females to display a restricted application of the postlexical rule in (30) above, and this is not the case. Members of the younger female group produced forms like *beautiful* [bju:ʔɪfəl] (Gail), *thirties* [θɜ:ʔɪz] (Fiona), *quarter* [kwɔ:ʔə] (Sam), *motors* [mɔ:ʔəz] (Monica) and *water* [wɔ:ʔə] (Melanie), all of which clearly show the application of the PA rule characteristic of TE. But notice that the variant of (t) used in each case is the glottal stop, the supralocal form. In other words, while the ambisyllabification may be local, the variant selected is supralocal.

Furthermore, this local vs. supralocal distinction could be tied in with the ordering of the postlexical processes of glottalling/glottalisation (hereafter abbreviated as G as in previous chapters) and ambisyllabicity. If we assume that in order for G to apply, the target stop must be at least partially associated with a syllabic coda, then PA must be ordered before G: in order to predict the correct output for G, the stop must be in a syllable coda, and in cases such as *motors* and *water*, this is only the case after the rule of PA in (30) has taken place. The claim that PA is ordered before G would be further enhanced if there were evidence to suggest that PA is an older rule than G, and this seems to be the case. Details concerning the historical evolution of G in TE and RP were discussed in chapter 3,

where it was suggested that G is a late Modern English phenomenon; by contrast, Jones (1989: §3.5) presents a range of data from OE onwards suggesting that the operation of a PA similar to that discussed here - though (a) not classified as postlexical by Jones, though the data clearly indicates that the ambisyllabicity is postlexical, given the phonological structure of the words in question; and (b) not restricted to the oral stops - is central to the production of a specific syllable shape: “speakers tend to ‘avoid’ syllable interfaces whose terminal points or contiguous edges show unique syllable domain reference. Overlapping at such junctures seems to be ‘preferred’” (Jones 1989: 189)<sup>13</sup>. Thus, while the young girls apply the local rule of PA first, the ‘localness’ is diluted by favouring a supralocal output of the second rule, G.

Such a pattern is partially upheld by an examination of the behaviour of the males, since both the older and the younger males show regular application of PA. But the older males more readily favour the local variants of G, with the younger males using both local and supralocal forms (thus, for instance, *country* [kʊnʔi] (supralocal) but *secret* [sikʔɪʔ] (local)). In other words, the older males favour local patterns in terms of PA and G, while the younger males clearly favour local patterns with PA but partly local and partly supralocal patterns of G.

Above, I made regular reference to the notion of G as a postlexical rule; however, some of the patterns of (t) in pre-pausal position (cf. §5.3.3.6, and Docherty *et al.* (1997)) would seem to motivate the concept of G being implemented in this environment as a lexical rule. Docherty *et al.* (1997) were the first to make this suggestion regarding TE, suggesting that (t) in pre-pausal position is subject to the Final Release Rule or FRR, where glottalled variants rarely surface. Docherty *et al.* (1997) relate this finding to conversational structure also, suggesting that full release is a signal that the speaker is prepared to yield the floor. The data collected for this

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<sup>13</sup> A detailed analysis of the historical material is outwith the scope of this thesis, so the reader is referred to Jones (1989) and the references therein.

thesis showed a different pattern, particularly in the behaviour of the younger female group. For this group, full release did not signal a willingness to yield the floor; and in terms of the lexical diffusion of the glottalled variants, the development of lexical exceptions seems to be much more rapid than is the case for the informants for Docherty *et al.* (1997). A large number of words were produced which seemed to display violation of the FRR (for a full list, see Table 18 in §5.3.3.6), suggesting that if the progress of G into this environment is spreading via lexical diffusion, it is spreading more rapidly in the lexicon of these younger females than was the case for the young TE female informants for Docherty *et al.* (1997). There are a number of possible explanations for this, but these must necessarily be tentative, as the precise social characteristics of the informants who provided the data for Docherty *et al.* (1997) are not given<sup>14</sup>. Given the high proportion of GS variants of (t) for the younger females in so many other environments, it is not surprising that the form has diffused into this position in this group specifically; but again this provides evidence that the young female group is favouring a supralocal form more readily than the others. Recall from chapter 5 that the particular area of Newcastle in which this YF group live is of a relatively high status in the Tyneside area as a whole, and that the area of residence of the YF group in Docherty *et al.* (1997) may be of lower status: crucially, the Docherty *et al.* (1997) informants may be less exposed to non-local influences on a regular basis than is the case for this YF group: recall that three of the five were university students, one of whom had spent some considerable time in London. Thus it may be the case that we need to consider the extent to which the various social groups are circumscribed to the local area, and engage in local contacts: these issues are discussed more fully in the next section, §6.3. But it is clear that the YF group did have a relatively mobile lifestyle - more so than any of the

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<sup>14</sup> That is, the precise social characteristics of each of the informants is not stipulated in Docherty *et al.* (1997); one of the areas in which the informants live is classified as working class, and the other as middle class, but no further details are provided.

other groups - and as a result may be more regularly exposed to certain variants which appear in supralocal varieties in those linguistic environments in which they (the variants) are far less frequent in the more localised variety.

Thus the gradual increase in the use of GS as a variant of (t) in TE clearly needs to be considered not just in terms of correlation with social categories, but also in terms of its distribution in certain environments, and the extent to which the new variant is being introduced by means of a lexical rule in pre-pausal position. Furthermore, in that position, it may be that we need to take account of discourse structure and turn-taking to account for some of the patterns of distribution. Again, we see a need to consider the variation in both structural and sociolinguistic terms.

As discussed in chapters 3 and 5 of this thesis, Carr (1991) attempted to explain the distribution of glottalised (that is, GR or GS variants) and weakened (that is, tapped and retroflex variants) variants of (t) in relation to lexical and postlexical foot formation: recall his claim that weakening only applies in cases in which the foot is formed postlexically (as in *fit her*), and not in cases in which the foot is formed lexically (as in *fitter*); furthermore, weakening applies only in monosyllabic or end-stressed polysyllabic verbs, and not in nouns, adjectives or prepositions. The data in both this thesis and Docherty *et al.* (1997) have suggested that Carr's claims are not descriptively adequate, for the following reasons:

- weakened variants surface where they are not predicted to do so in Carr's account. For instance, the tapped variant appears in nouns such as *butter*, and adjectives such as *later*, and the retroflex variant appears in the adjective *better*, to cite just a few examples.
- glottalised and glottal variants appear where weakened variants are predicted by Carr's account: variants of (t) in some monosyllabic verbs with a lax vowel (such as *get*, *split* and *put*) can either be [ʔ] or [ɾ], even within idiolects, while (t) in certain monosyllabic verbs with a tense vowel



(such as *bought*, *brought* and *fart*) only ever surfaced with a glottalised or glottalled variant.

The data provided by the informants for this thesis, then, would not uphold a theory which postulated glottalisation and weakening as rules which enter into an ‘elsewhere’ relation, the more specific rule of weakening blocking the general application of glottalisation (cf. Carr 1991: 49). This is not to suggest that lexical and postlexical rules are not important for describing patterns of glottalisation in TE; they clearly are, as I hope to have demonstrated above. But glottalisation and weakening phenomena in TE do not seem to be connected to foot formation in the way Carr (1991) suggests: the data for this thesis would suggest that they are primarily sociolinguistic variants, some of which may be *more likely* to appear in certain environments than others, but are not restricted in their distribution to a specific environment.

### §6.3 *Further sociolinguistic issues*

In the sections above, the discussion centred around the synthesis of sociolinguistic data with structural issues; in this final section, I want to look at a different kind of synthesis, which focuses on the relationship between gender-based variation and linguistic change. Particularly, I want to look at how the data collected for this thesis can be squared with some of the gender patterns established in chapter 4. Recall that at the end of that chapter, a quotation from Chambers (1995) was provided, which called for an analysis of gender-based variation which took into consideration the issue of mobility. It is this issue which I wish to develop here, particularly as it relates to both the phonetic, phonological, semantic and morphosyntactic variations discussed in this thesis.



The Chambers quotation mentioned above focused on the varying degrees of mobility associated with the sexes in any given community; this approach has the advantage of not ascribing particular patterns to men and women *per se*, but rather to their socialisation patterns in specific circumstances. In this regard, such an approach to gender-based variation fulfils the maxim of Eckert and McConnell-Ginet (1992), namely to 'think practically and look locally'. But then how are we to establish the community-specific gender patterns which will determine the relative mobility of the sexes? In terms of the informants for this thesis, the patterns of mobility were discernible directly from the comments made by the informants themselves, and the lifestyle they led. For instance, it was clear that of all the four groups, the younger females were the group who, as a whole, was least intent on remaining in Tyneside: they were all willing to move to other parts of the country if employment prospects were better elsewhere; and one of them had already spent some considerable time in London. Similarly, one of the older females had (in the past) had employment which had required her to travel to a number of different localities in the north of England. By contrast, many - but not all - of the male informants had much more localised work, for instance, as TV and video engineers. As a whole, these twenty Tyneside speakers were clearly relatively geographically and socially mobile; yet within the groups, it was also clear that the females were by and large more mobile than the males. In the case of the older informants, this was reinforced by the type of work they carried out: in contrast to the males, much of the work carried out by the females (such as a shop assistant in a large Newcastle department store, or as a personnel assistant in a Newcastle-based financial institution) was such that they may be more likely to be exposed to non-local (non-vernacular) variants on a regular basis; with the younger informants, it was clear that the younger women seemed to have more of a motivation to leave the area to pursue employment, in contrast to most of the

younger males: and even those males who had gone away from Tyneside briefly were clearly happy to be back, and intended to stay.

Nonetheless, one of the most striking features of the groups as a whole was their favourable attitude towards the area: even if the younger females felt that their chances of employment might be better elsewhere, this was not a reflection of how they felt about Tyneside in itself. Had there been a likelihood of them getting the kind of work they wanted in the local area, they would most likely have planned on staying. The older females were also very positive about Newcastle as a city, as were the younger males; and the older males in particular were vociferous in their defence of the north-east, particularly in relation to the economic inequality between Tyneside and the wealthier areas of England, notably in the south-east. Such a favourable attitude toward the area might well be one which would foster the continued use of local, vernacular forms: and these vernacular forms could clearly therefore function as a 'badge', symbolising local identity, as was the case in Labov's Martha's Vineyard survey (Labov 1978).

Thus there is a potential tension between social mobility on the one hand and local loyalty on the other: increased mobility should lead to an increase in supralocal forms; while a marked identification with the area should lead to an increase in local forms. And the increase in supralocal forms should lead to the establishment of levelled varieties, since increased social and geographical mobility is inherently linked to the dissolution of "the territorially based close-knit network which is needed to keep localised norms intact" (Watt and Milroy 1999: 42).

We can see this in action in the speech of the informants for this thesis, particularly in the younger generation, and most particularly with the young females: and this is true of variation in modal verb usage *and* in the patterns with the oral stops, but most clearly in the case of the oral stops. For while there was some evidence of statistically significant age and sex differences in the use of modal verbs,

clear indicators of features historically associated with TE (such as the double modal construction and the patterns of adverb placement discussed at the beginning of §6.1 above) were missing from all of the groups, even the older males. It is true to say that there were some features more associated with the local variety that seemed to characterise the older speakers generally (such as the use of *could* as a marker of past time), and that some non-local features seemed to be more characteristic of the younger groups (the use of *gotta* as a marker of root necessity, and the spread of cliticised variants of *will* and *would* to a range of clause and subject types). But these features did not show the fine gradation of social groups in the way that the phonological variables did: supralocal variants (such as GS) here were very clearly markers of the speech of the younger females in particular, while local variants (such as the vernacular glottalisation pattern) were much more characteristic of the older males. This was not true in every environment, however: where (t) appeared at a word-boundary before a liquid or a semi-vowel (§5.3.3.3), for instance, GS was clearly the favoured variant for all of the groups; there was little to differentiate the older males from the younger females here: so again, the importance of linguistic environment must be taken into account when trying to establish patterns of variation. Similarly, supralocal variants of (k) were less frequent than those of (t), even in the speech of the younger females: so while there may be a *general* move towards GS realisations of all of the oral stops (i.e. where the contrast between /p/, /t/ and /k/ would be suspended in certain positions), it is clear that GS realisations of (t) are developing more rapidly than is the case for (k)<sup>15</sup>.

#### §6.4 *Summary*

In this chapter, I have tried to illustrate how a more holistic approach to analysing variation in a speech community can aid our understanding of language

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<sup>15</sup> Recall that numbers of variants for (p) were too small to afford any useful correlations with the various social groups.

structure, using evidence from modal verb usage and oral stop variants in TE. Above all, it is the synthesis of knowledge of sociolinguistic patterns and linguistic structure that is crucial: and this clearly require a rethink of how we analyse social variation in terms of linguistic theory on the one hand, and how we incorporate the findings of theoretical linguistics into the advances made in sociolinguistics on the other. Both disciplines can learn from the other, and together can provide a principled and convincing account of (socio)linguistic competence.

## Conclusions

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In the previous chapter, I attempted to show how the sociolinguistic data collected for this thesis might best be incorporated within aspects of the structural frameworks discussed in chapters 2 and 3, as well as discussing how this related to patterns of gender-based variation discussed in chapter 4; specifically, it was suggested that these structural frameworks might require some modification to handle the variable data. In this concluding section, I want to put the previous discussion within a wider context of variation and (socio)linguistic theory, and to suggest potentials for further research.

I think it would be fair to say that the phonological and syntactic frameworks within which much of the discussion of the TE data has been couched are not normally associated with sociolinguistic variation. There are some exceptions, as noted in the introduction to this thesis: for instance, Guy (1994) considers variation within a Lexical Phonology framework, and Kroch (1994) examines syntactic change within the Principles-and-Parameters model. But generally speaking, Minimalism and gender-based variation would not generally be considered compatible bedfellows. In this thesis, I have tried to illustrate that there is potential for such a union to take place - aspects of both theories can, I think, be usefully incorporated into our knowledge of the structure of language; and the same is true for aspects of Lexical and Metrical Phonology.

But such a synthesis is not the only way in which a holistic approach to variation and formal linguistics can be achieved, and this can be illustrated by considering the approach adopted by Hudson (1986, 1997a), whose work has centred on the development of a cognitive, prototype theory of linguistic structure, Word Grammar (WG) (cf. Hudson 1990). I do not propose to provide a detailed analysis of

the WG model here; but I do want to illustrate how it may be applicable to some of the patterns of variation in TE discussed elsewhere in this thesis.

The WG approach is a theory of grammar closely allied to cognitive linguistics, in which the concept of *prototype* is paramount. A prototype is classified as “a concept which is defined by its ‘typical’ features, any of which may be overridden in exceptional cases” (Hudson 1997a: 80), and a key feature of the WG framework is “default inheritance in an inheritance, or ‘isa’, hierarchy” (Hudson 1997a: 81). Thus, as Hudson (1997a: 80-1) illustrates, if we consider the prototypical *bird*, we would assume that each member of that category has wings, can fly and lays eggs, and so on, and because a robin is a bird, it inherits by default all those properties of the prototype, as do all other birds, unless exceptionally marked, in which case the exceptional properties override the typical ones predicted by the prototype: a penguin is a bird, but is marked because, amongst other things, its primary locomotion is swimming, not flight. Furthermore, we could move from a very generalised field of knowledge (such as birds) to something much more highly specialised (such as piano sonatas), yet the same principles associated with prototypes and default hierarchies are operational: the prototypical piano sonata of the classical period has three movements, the first and third of which are generally of a faster tempo than the second; the first movement has an exposition, development and recapitulation; and so on; yet we can mark out Mozart’s Sonata in *Eb* major (K282) as exceptional as it begins with a slow movement - in fact it is the only one of Mozart’s piano sonatas to begin with a slow movement; so while K282 is clearly a sonata, it has an exceptional property: and crucially, musicians who learn about Mozart’s piano sonatas store K282 as an exception to the rule. They become accustomed to sonatas beginning with fast movements, so that this characteristic pattern becomes entrenched in their minds: if a novice music student, who has never heard a Mozart piano sonata before, listens to each one of Mozart’s piano sonatas,

they will predict the general pattern *fast-slow-fast* for the three movements, yet store K282 as an exception (*slow-fast-fast*): they will consider the ‘regular’ pattern of a sonata (in terms of the general tempo of its movements) to be *fast-slow-fast*, as that is the pattern to which they have become most accustomed.

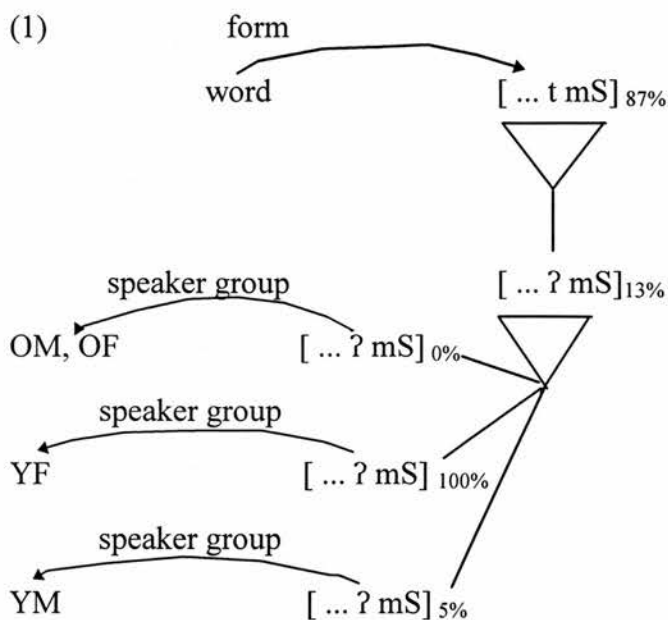
These principles can also be applied to language, and I will illustrate this with reference to two specific features of the data collected for this thesis: the first phonological and the second morphosyntactic. In what follows, I make further use of the notion of entrenchment, alluded to above and discussed in more detail in Hudson (1997a: 82-6). Entrenchment refers to the relationship between our experience<sup>1</sup> of a phenomenon and its embedding in our consciousness; in linguistic terms, we can consider the entrenchment of a variant to relate to how the speaker’s experience of variable pronunciations and constructions is connected to their linguistic competence.

In the case of (t) \_\_ C (cf. §5.3.3.8), the variants of (t) seemed to be dependent not just on linguistic environment but on the social characteristics of the speakers. Specifically, if the following consonant was the inflectional morpheme {S}, henceforth labelled ‘mS’, following Hudson (1990: 90), the variants of (t) seemed to divide the sample in the following way: the younger females always selected the glottal stop GS, the older speakers always favoured the full release variant, and the younger males generally favoured full release, but occasionally favoured the glottal stop. We can diagram this pattern using a WG network as in (1), details of which are explained below:

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<sup>1</sup> I use the (somewhat vague) term *experience* here deliberately. As Hudson (1997a: 83) notes, the mechanisms by which a given variant linguistic item becomes entrenched in a speaker’s competence is likely to involve “a great many more influences than brute frequency”. It is plausible that issues such as accommodation may be of importance here: entrenchment may be facilitated by exposure to variants when the attitudinal environment is favourable.





In (1) above, the triangles represent the 'isa' (inheritance hierarchy) relation: the base is on the more general category (in this case, the full release variants of (t)), and the apex of the triangle is connected to any concept which 'isa' that category, i.e. which enters into a hierarchy relation with that category. Thus words which are of the type [ ... t mS] (such as *cats*) have an alternative form [ ... ? mS], where the GS variant is selected. The subscript figures represent the percentage of such forms selected, based on the data collected for this thesis. In other words, of all the words in which (t) appeared before the {S} morpheme, 87% of those forms surfaced with [t] and 13% with [?] for the sample as a whole; but the young females consistently had [?] in this position, so the percentage figure for this group is 100%, while the older groups (both male and female) had [t], so the percentage figure here is 0%. Only the younger males showed variation, greatly favouring [t] above [?], reflected in the percentage of [?] forms at just 5%. Consider further in this regard the absence of *shall* in this corpus of TE. It would be feasible, following the WG discussion in Hudson (1986), to suggest the following rule in (2) below:

(2) *shall* is (a ((modal-verb)((whose actor is (not X))))))

where X is a TE speaker from this sample, and actor refers to the speaker of a word. This too might be linked to the concept of entrenchment: there is little exposure on a day to day basis to *shall* as a modal verb in TE (if we take into account the findings of McDonald (1981) which would seem to support this observation), so that *shall* is simply so rarely stored as a modal in TE that it does not surface in over 15 hours of recording; and this might also be the case for *may*.

I do not pretend to have provided anything other than an extremely selective discussion of a couple of features associated with WG; it might well be the case that a WG analysis of all of the data in this thesis would provide a profitable and convincing analysis of the data in the thesis; this would most certainly be a research project I would like to investigate further, since there are a number of important correlations between WG theory and sociolinguistic data which Hudson (1996: 256) cites.

But this does not detract from the findings of this thesis, nor to the possible correlations between sociolinguistics and the theoretical models of language structure invoked during the course of this thesis; chapter 6 discusses a number of ways in which the sociolinguistic data from the TE speakers can and should be incorporated within the frameworks. My feeling is that the actual linguistic model adopted is less crucial at this stage than engaging in a serious discussion of the possibilities of synthesising sociolinguistic and structural theories of language, since to my mind, *all* theories of language structure should be able to account for social variation: if they cannot deal with such patterns, then they require modification (along the lines presented in the previous chapter). Of course, ultimately, sociolinguistic tractability may be an important factor in selecting among theories of structure; and this will clearly become a more important issue if (or hopefully when) more research is carried out on the synthesis of sociolinguistics and formal linguistics.

Hudson (1986) provides a strong set of arguments as to why such a synthesis should take place, and discusses the need for synthesis as follows:

the present lack of interaction between the disciplines of sociolinguistics and structural linguistics does not parallel a similar lack of interaction between their subject matters, and it would be very much to the benefit of both disciplines if they could find common ground in the construction of grammars. In this enterprise structural linguists would specialize - as they do now - in structural facts, and sociolinguists would work on sociolinguistic facts, and they would have to collaborate at the numerous points where they seemed to be referring to the same grammatical constructs - lexical item X, or construction X, or phoneme X or whatever. To a very limited extent this kind of collaboration and interaction already takes place, especially as a necessary part of sociolinguistics; but it could and should be much more intensive. It seems fairly obvious that the present state of affairs in mainstream structural linguistics does little to encourage sociolinguists to contribute, and even less to encourage structural linguists to look to sociolinguistics.

(Hudson 1986: 1059-60)

I hope this thesis has gone some way towards showing that it is possible to construct, carry out and report on a piece of research into a variety of English, which tries to take on board some of the current thinking in formal linguistics and sociolinguistics, in the hope of promoting some sort of synthesis between the two disciplines.

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