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Development and Variation in Learner Language

Malcolm Johnston

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ABSTRACT

This study examines the relationship between variation in the oral interlanguage of learners of English as a second language and the propagation of these rules through the spread of fresh lexical environments for their operation. It is argued that there considerable systemacity in this process. The study then goes on to examine the feasibility of incorporating these findings into oral proficiency descriptions of language development and concludes that the same data can be used more productively and predictably from within Manfred Pienemann's Processability Theory.

DECLARATION

This thesis is the original work of the candidate with the exception of material or findings in the text specifically referred to in the bibliography as the product of either collaboration between the candidate and other persons or by other persons alone.

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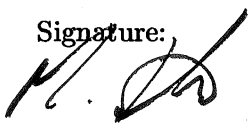

Contents

1 Introduction 6
1.1 Introductory Remarks 6

2 Theory and SLA Research: A Review 9
2.1 Theoretical Basis of the Project 9
2.1.1 Theoretical Overview 9
2.1.2 Ten Basic Questions 10
2.1.3 Historical Perspective 16
2.1.4 Contrastive Analysis 18
2.1.5 Orders of Acquisition and Morpheme Order Studies . 23
2.1.6 Variation 31
2.1.7 Implicational Scaling and Second Language Acquisition 35
2.1.8 Multi-Dimensional Variation 37
2.1.9 Lexical Levers 39
2.1.10 Where Does the Second Language Learner Begin? . . 42
2.2 Optimization of Form and Function 46
2.2.1 Learning Mechanisms—The Process of Refinement . . 49
2.2.2 Refining Processes in Adults 50
2.2.3 Decompositional Analysis—The History of a Formula 52
2.2.4 Elaboration and Discrimination—Summary 54
2.2.5 The Question of Interference/Transfer 55
2.3 Some Other Current Theoretical Approaches 58
2.3.1 Input and Interaction Studies 58
2.3.2 The Contribution of Interaction Studies 59
2.3.3 Conclusions 61
2.3.4 Government and Binding in SLA Research 61
2.3.5 Government and Binding and Universal Grammar . . 62
2.3.6 Some Preliminary Issues 64
2.3.7 The Definition of "Native Speaker" 64
2.3.8 Universal Grammar 66
2.3.9 Particular Cases and Particular Problems 69
2.3.10 Case Number One 69
2.4 To Criticize the Critic 99

2.4.1 Epilogue 106
2.5 Sequelae 107
2.5.1 Conclusions 107

3 Methodology 109
3.1 Study Design 109
3.1.1 Basic Considerations 109
3.1.2 Study Type—Longitudinal versus Cross-Sectional . . . 110
3.2 Implementation of the Study Design 112
3.2.1 Sample Size 112
3.2.2 Study Composition 114
3.2.3 Naming Conventions 116
3.2.4 Relevant Statistics 117
3.3 Data Collection 117
3.3.1 The Oral Interview 117
3.3.2 Format of the Interviews 118
3.3.3 Recording Procedures 119
3.3.4 Transcription 119
3.3.5 Computer Storage and Analysis of the Data 121
3.4 Evaluation of the Data 122
3.4.1 Parameters of the Interview Situation 122
3.4.2 Rationale for Choice of the Oral Interview 123
3.4.3 Characteristics of the Oral Interview 124
3.4.4 Possible Effects of Observation 124
3.4.5 Naturalistic Data versus Mixed Data 125
3.4.6 Possible Effects of Formal Instruction 126
3.4.7 Other Relevant Survey-Type Enterprises 129
3.4.8 Structural Studies 130
3.4.9 The Relation of Formal Language Instruction to Education 132
3.4.10 The Effect of Instruction on the Learner 133
3.4.11 Implications For the Present Study 136
3.4.12 The Effects of Instruction—Conclusions 136
3.4.13 Other Features of Possible Importance to Learning . . 136
3.4.14 Other Languages 137
3.4.15 Potential Difficulties with a Naturalistic Sample . . . 139
3.5 Limitations Imposed on the Analysis 139
3.5.1 The State of the Longitudinal Study 139
3.6 Oral Proficiency Ratings 140
3.7 Conclusions 142

4 Analysis, Results and Conclusions 149
4.1 General Statistics 149
4.1.1 Length of the Interviews 149
4.1.2 Word Counts 149
4.1.3 Fluency and Volubility 150
4.1.4 Learner Types 150
4.2 Verbal Morphology and Semantics 155
4.2.1 Limitations 155
4.2.2 Verbal Features Investigated 155
4.2.3 Non-Standard '-ing' 156
4.2.4 Irregular Past Marking 159
4.2.5 Regular Past Marking 161
4.2.6 Third Singular '-S' Marking 163
4.2.7 Aux_ing Marking—The Continuous Aspect 165
4.2.8 Aux_en Marking 173
4.3 Passives—Summary 177
4.4 Perfects 178
4.5 Verbal Nominalizations 179
4.5.1 Adverbial Tense/Aspect Marking 181
4.5.2 The Verbal System—A Tentative Order of Acquisition 181
4.6 The Verbal System—Conclusions 183
4.7 The Copula 183
4.7.1 Production of the Copula 183
4.7.2 'Is' 198
4.7.3 'I'm' and 'Am': 199
4.7.4 'Are' 200
4.7.5 'Was' and 'Were' 200
4.7.6 'Been' 201
4.7.7 'Be' 201
4.8 Summary 202
4.8.1 Note on the Tables 202
4.9 Modals 202
4.9.1 Distribution of Modals 202
4.9.2 'Can': 207
4.9.3 'Must': 208
4.9.4 'Will': 210
4.9.5 'Would': 212
4.9.6 'Could': 213
4.9.7 'Should': 214
4.9.8 'Have To': 215
4.9.9 'May': 217
4.9.10 Morphology 217
4.9.11 Summary 218
4.10 Negation 218

4.10.1 Some Methodological Problems 218
4.10.2 Categories of Negation 219
4.10.3 A Postulated Developmental Sequence 227
4.10.4 Morphology 235
4.10.5 Conclusions 236
4.11 Questions 237
4.11.1 Limitations 237
4.11.2 Collection of Question Data 237
4.11.3 Structures Analyzed 242
4.11.4 Relative Complexity 244
4.11.5 Apparent Learner Difficulty with Question Structures 247
4.11.6 The Order of Difficulty—Summary 253
4.11.7 Ill-Formed Structures 254
4.11.8 Embedded Questions 257
4.11.9 Conclusions 257
4.12 Nominal Morphology 258
4.12.1 Inflectional Morphology 258
4.12.2 Plural '-s' 258
4.12.3 Possessive "-'s" 265
4.12.4 The Developmental Status of '-s' Morphemes 267
4.12.5 An Alternative Proposition 268
4.12.6 Nominal Inflections—Summary 269
4.13 The Definite and Indefinite Articles 269
4.13.1 Limitations 269
4.13.2 Frequency of Realization 271
4.13.3 Syntactic and Semantic Environments 272
4.13.4 Semantic Environments—The Definite Article 272
4.13.5 Developmental and Variational Features 278
4.13.6 Semantics of the Indefinite Article 284
4.13.7 Summary 284
4.14 Quantifiers 287
4.14.1 Distribution of Quantifiers 287
4.14.2 Developmental Trends 288
4.14.3 'Statistical' Quantifiers 289
4.14.4 Summary—Quantifiers 296
4.14.5 Distribution of Numbers 296
4.14.6 Summary—Numbers 297
4.15 Deictics and Demonstratives 297
4.15.1 Distribution of Demonstrative Pronouns 297
4.15.2 Significant Differences—'This' and 'That' 297
4.15.3 'Here' and 'There' 303
4.15.4 'This'/'That' and 'Here'/'There'—Summary 304
4.15.5 Language-Specific Differences 305
4.15.6 Conclusions 307

4.16 Existentials 307
4.16.1 Existential 'There' 307
4.16.2 Alternative Existential Structures 311
4.16.3 Possible Explanations for Infrequent Use of 'There' . . 312
4.17 Personal Pronouns 313
4.17.1 Limitations 313
4.17.2 Distribution of Pronouns 313
4.17.3 Possessive Forms 322
4.17.4 Reflexive Forms 322
4.17.5 Conclusions 323
4.18 Prepositions 323
4.18.1 Occurrence of Prepositions 323
4.18.2 Frequency 323
4.18.3 Distribution 323
4.18.4 Acquisitional Sets 324
4.18.5 Possibilities for Further Research 324
4.18.6 Conclusions 325
4.19 Connectors and Cohesion 325
4.19.1 Limitations 325
4.19.2 Distribution of Connectors 328
4.19.3 'Because' 329
4.19.4 'If' 330
4.19.5 Other Connectors 330
4.20 Further Partial Analysis 332
4.20.1 Comparatives 332
4.20.2 Lexis 332
4.21 An Alternative Approach 334
4.22 Processability Theory Applied 334
4.22.1 Processability Theory Applied to English 334

5 Concluding Remarks **339**
5.1 Some Words in Summary 339

Chapter 1

Introduction

1.1 Introductory Remarks

This dissertation has a number of aims. They are, briefly, as follows:

1. To examine syntactic and morphological progressions in learner language and, in the first instance, determine if these progressions correlate in any satisfactory way with oral proficiency rating scales.

2. Following this it will look at the same information in the light of Processability Theory, as presented in Pienemann (1997 [143]), as well as earlier versions of this manuscript. The conclusions drawn from this investigation and the one above will be presented at the end of the study.

3. The notion of rule propagation by means of lexical spread will also be examined. This notion, as proposed by C.-J. Bailey [10] in the area of language change and variation and by Derek Bickerton in the area of creole studies [15] is one which ties in very neatly with the tenets of Processability Theory and provides some insight into what Pinker refers to as "the problem of extendibility [145]"—that is, what constitutes the "motor" of acquisition in the learner.

There are a number of points to be made concerning the limitations or boundaries of this study.

1. Although the dissertation makes reference to oral proficiency rating scales it will not make any preliminary analysis of the concepts or history of these scales. This is, first, because of the conclusions the study presents about the nature of these scales and, second, because there is a large body of literature in the field of language testing about these scales and this work is not a direct excursion into that field.

2. Nor will the dissertation examine the application of Processability Theory to the acquisition of English in the detail it deserves, since this would require an entire study on its own. It is worth noting, however, that the data which forms the basis of this study was the data which the precursors of this theory—the Multidimensional Model [130] and the Predictive Framework [144]—were developed on the basis of and that the author himself played a part in this process [96]. In one sense, then, the dissertation provides historical information relating to the changes which the original *ZISA* model underwent before it was re-conceptualized as Processability Theory. [1]

3. Although the study employs the technique of implicational scaling, the scales presented have not been optimized in the normal way for such scales, with the exception of the implicational scale generated by the application of Processability Theory to the same data. This is a deliberate consequence of ranking the informants who participated in the study in an invariant order on the basis of their oral proficiency ranking; as far as the conclusions about the relationship of language development and oral proficiency development are concerned, optimizing the scales would have confused the picture, and if any conclusion could have been salvaged from this confused picture it would have been the same.

There are a number of minor points relating to the language and presentation of the dissertation.

Pronoun use assumes the masculine form to be the unmarked one; this is principally a linguistic and stylistic consideration and is not intended to be sexist.

In the idiolect of the author the word "data" is a non-count noun and is therefore treated as singular. The author is well aware that there is a convention based on the Latin origin of the word which has been imported into English. There might be some point to the maintenance of this convention if "datum" were ever used by linguists, but this is practically almost never the case. As for precedent here, we do not treat "visa" as a plural noun. In the final analysis usage will determine the issue of the number for "data", as it has in the case of "visa". Meanwhile, for the sake of consistency, the author asks to be allowed to follow his intuitions.

Finally, the structure of the dissertation is as follows.

- After this introduction there is a chapter establishing the theoretical background of the study and discussing the general theoretical context of second language acquisition as it presently stands.

[1] References to this will be provided as appropriate in the text of the study.

- The chapter on theory is followed by a discussion of the methodology of the study and the rationale for this.
- The fourth chapter of the study presents all of the information that has been gathered for the fifteen areas of syntax and morphology investigated in the 700 plus pages of interlanguage data collected in the course of the study plus the specific conclusions arrived at for each of these areas.
- The final chapter presents a number of concluding remarks about the findings arrived at in the body of the dissertation.

Chapter 2

Theory and SLA Research: A Review

2.1 Theoretical Basis of the Project

2.1.1 Theoretical Overview

The purpose of this chapter is to provide a theoretical background for the present study and to justify the selection of the analytical tools adopted in it.

As pointed out in Chapter One, there is now a very considerable body of literature on the subject of second language acquisition. In English alone there are thousands of articles devoted to the questions raised by the study of second language acquisition, and thousands more on related subjects in adjacent disciplines. As well as articles, there are now a considerable number of whole books dealing with second language acquisition in publication, and a number of journals either exclusively or partially dedicated to this area of research. As well as all this published material there are many theses and unpublished manuscripts in circulation: some of these latter documents are among the most challenging of all. A comprehensive survey of all this material would obviously be a major enterprise in itself, and cannot be undertaken with any great degree of thoroughness here.

In any case, a project of this kind is no longer entirely necessary, given that there are now available a variety of critical overviews and summaries of work done in different areas of second language acquisition. These range from university course-oriented books to articles dealing with particular aspects of second language acquisition research. Examples of the former would be *Psycholinguistics—A Second Language Perspective* by Evelyn Hatch [78], and *Language Two* by Burt, Dulay and Krashen [51]. Amongst summary articles, notable examples would be Richards' paper on Error Analysis [150], the first chapter of Huebner's *A Longitudinal Analysis of the Acquisition of*

English [85], and the paper by Nicholas and Meisel in their joint edition of *Second Language Acquisition at the Crossroads* [62].

2.1.2 Ten Basic Questions

The last review referred to, *Second Language Acquisition: The State of the Art* provides a list of questions which offer a convenient starting point for an outline of the theoretical underpinnings of the present study. A consideration of the present enterprise in the light of these questions will also help to locate it in relation to what are probably the main issues in second language acquisition research at the present moment.

The questions, which as the authors point out are really just different ways of formulating a single basic question about the underlying mechanisms of second language acquisition, and of whether and, if so, how these mechanisms are susceptible to influence, are the following:

1. To what extent are first and second language acquisition similar or different?

2. To what extent can the similarities and differences be explained by neurobiological factors? (age, maturation, etc.)

3. What are the cognitive principles determining language acquisition? Are they language-specific or general problem solving principles? Do they change with age?

4. What is the role of individual factors such as the personality of the learner, intelligence, language learning aptitude, and so forth?

5. Does prior language learning interfere with subsequent learning? Is there (positive and/or negative) transfer from previously learned languages? Is there "creative use" of transfer? Under what circumstances do learners make use of transfer strategies?

6. What is the relationship between input and output? Does simplified input (motherese, foreigner talk) help or inhibit language acquisition?

7. Will social-psychological factors influence on [sic] the development of language acquisition and if so, in which way?

8. Does the structure of the target language itself influence the formation of hypotheses by the learner?

9. Which strategies of language acquisition and use does a learner have recourse to? (Simplification, possible transfer, etc.)

10. Will all the different factors mentioned allow or prevent variation (individual or learner-type specific)? Which areas will be affected by variation—if at all; which may never be affected?

As the authors themselves point out, at this stage "we have rather more definite questions than answers" [62].

Nevertheless, these questions serve as useful points of reference for any study in second language acquisition, and as a preliminary means of defining the orientations of the present investigation it is worthwhile looking at what it might be expected to reveal about each of these questions. Following that, a short description and history of the main theoretical currents in the field will be provided. This should help to locate the study in a historical perspective and to explain its theoretical priorities. As a final step, these priorities will be described, discussed briefly, and defended.

Similarities between L1 and L2 Acquisition

The present study is not framed in such a way as to have much to offer in empirical terms on this question since it is a study of adult second language learning. To the extent that it can help to describe and elucidate language learning processes in general, it may ultimately provide material of use for those engaged in this type of comparative study. Theoretically, the study can be located within the broad tableau of what Wode calls "an integrated perspective of language acquisition" [185]. That is, it is prepared, until the facts constrain it to do otherwise, to make the fairly strong assumption that language learning is characterized by a set of shared universal principles, which may or may not operate in their totality across different kinds of language learning. This places the study in the "nativist" tradition of linguistic research [29].

Neurobiological Factors

Once again the study is limited in the kind of direct empirical contribution it can make to this question. The informants in this project range between the ages of twenty-one and forty-five, with the majority being aged between twenty-five and thirty-five. Within this relatively limited adult span the study makes no attempt to address the question of age as a determinant of success in learning or of particular formal properties of learner language itself. From an anecdotal point of view, it should be said, there is no striking evidence that within the age-limits of the study that age *per se* (or even indirectly) is a significant variable.

Cognitive Principles Determining Language Acquisition

This is a question to which the study does address itself, and one about which it will hopefully have some contribution to make. It has to be said at this point that we are dealing here with a very complicated subject, complicated further by the fact that independent (that is, non-linguistic) descriptions of possibly relevant cognitive principles are not readily available. Indeed, one researcher, Wode, has argued that due to a paucity of adequate offerings from neurology and psychology, linguistics must develop its own terminology and formulate its own principles "on the linguistic level" [62]. Nevertheless, it is obviously a desirable goal, both from the viewpoint of theory itself and for its possible applications, to be able to formulate the mechanisms of language acquisition in the most general way possible. In addition, if we can establish the independence of cognitve processes which are crucial to language acquisition we are in a stronger position to make claims about why certain linguistic processes are universal. If we cannot actually formulate cognitive principles that appear to be independent of language itself, then the attempt to formulate so-called "linguo-cognitive" principles is still worthwhile, since these principles will hopefully attain a level of useful generality (for instance, they might at least not be language specific). And as psychology and linguistics develop, it may turn out to be possible to incorporate such principles into a more embracing general terminology.

Individual Factors

Although it considers them to be important, the study does not address the question of individual factors such as personality and intelligence in great detail. These factors are of import to the present investigation to the extent that they might affect the generalizations about language learning processes that can be made across the sample. Thus, in Chapter Three there is some discussion of the possible effects of an individual's educational history (within which factors such as intelligence may have the role of "hidden" variables) on his or her language learning.

Prior Language Learning

This has a similar status in the present study to Individual Factors. It receives some treatment in Chapter Three in the section dedicated to an evaluation of the data, but is not conceived of as an object for direct study. At this point one should probably make a distinction between different types of prior language learning. By definition, all second language learners have had at least one prior learning experience, but the question of the effect of learning a first language is arguably better dealt with in the context of the first question in the list, which is dedicated specifically to the relationship

between first and second language learning. As for other types of prior learning, we have prior learning of the target and prior learning of other languages. Then we have a further division into naturalistic and formal prior learning. In this study questions about these different kinds of prior learning are considered from the point of view of trying to validate the consistency of the data, rather than for themselves. No very definite general conclusions are reached on the effects of prior learning as a whole, although some propositions about the relative importance of formal instruction are advanced. While there seems to be a generalized belief that prior learning of other languages usually serves to facilitate and accelerate the process of further language learning, at least one study has found this not to be the case [62].

The Relationship Between Input and Output

This question is not directly investigated either, although some anecdotal observations in relation to it are made in the concluding chapter. There are serious difficulties in formulating questions about learning in behavioural terms such as input and output, since approaches which effectively disregard the learner in their research equation by default actually make all manner of assumptions about him. One such assumption is that input is transparent to the learner, when in fact what he is exposed to and what he actually absorbs may be two very different things (as in Pit Corder's distinction between input and intake) [44].

For a study working from the nativist tradition the question of input should not in principle be very important, since it is a basic assumption of this tradition that learning processes are largely self-regulating and can (indeed must) make do with input which is both imperfect and incomplete. Chomsky has forcefully argued this point in relation to the first language learner. Nor is this assumption entirely without support within second language acquisition. Felix, for instance, found error patterns of a naturalistic kind in the output of purely formal learners; these patterns could not have possibly been due to input [60].

While the relationship of input to output may not appear to be a very productive issue to become entangled in, there are nevertheless grounds for not dismissing it entirely. It should be remembered that one kind of input is characterized by the material of formal instruction. To dismiss out of hand the effects of input on output would be to dismiss out of hand any possible effects of formal instruction. Given the current investment of resources in this activity, the question of the relationship between input and output probably merits some investigation, if only in this fairly restricted application of the terms.

A further reason for considering the role of input is that in a model of acquisition in which learning can be lexically driven, as is the case with the

model which will be proposed in this chapter, specific lexical forms may interact with a developing grammar in quite dramatic ways. Possible kinds of input may differ markedly with respect to these crucial forms.

Social-Psychological Factors

Once again the study touches these only tangentially, while recognizing their importance. The comments made in 2.1.2 on individual factors are equally relevant here.

The question of the effect that socio-psychological factors have might have on the actual formal properties of learner language (as opposed to their effect on the rate or extent of learning itself) is of direct relevance to the present study. Theories which have addressed themselves to this question, such as the Pidginization Hypothesis of Schumann and the multi-dimensional theory of variation proposed by the *ZISA* group, will therefore be taken into account in the following sections.

The Structure of the Target Language

Given that the language under investigation here is English alone we cannot seek an empirical resolution of this question in the current investigation, except in so far as it might produce conclusions which could be utilized by some future comparative study.

In a trivial sense it would seem obvious that at some point in the learning process the structure of the target language must affect at least some hypotheses held by the learner. Otherwise there would never be any convergence between learner language and target language (or alternatively the hypothesis formation process would have nothing to do with learning). Therefore the above question might be better formulated in terms of whether the structure of the target language influences the learner's early or initial hypotheses, or whether it influences certain kinds of quite powerful global hypotheses that the learner may come to formulate.

In this latter connection, it is an interesting question whether what we might call certain "trends" in a language can be perceived and exaggerated by learners. To put this in another way, are learners, who have an obvious investment in trying to "regularize" the target language, particularly susceptible to what Kiparsky [137] has termed "paradigm coherence"? For instance, could the relative poverty of English verbal morphology discourage the learner from learning the little of this that remains after centuries of simplifying change, thereby registering in his personal grammar of English what may well be an actual change in progress in the language as a whole?

While such questions as the above cannot be answered definitively on the basis of the study of a single language, some suggestions as to how specific and general features of the target may affect learner's hypotheses in

non-trivial ways will be offered.

Strategies of Acquisition and Use

The term "strategy" is not used very clearly or consistently in the literature on second language acquisition. In ordinary language terms "strategy" implies plan. In second language acquisition it is not always clear how the intentional connotations of the word can be handled. If a strategy is not conscious and deliberate on the part of the learner then we need to be able to specify at least some processor in our psychological map of the learner's mind which is responsible for developing strategies. In many cases where strategies are discussed the processor is identified with the learner himself. From a psychological point of view this is rather vague. In addition, there seems to be very little agreement on what levels of linguistic operation (i.e. grammatical, pragmatic) strategies can operate, and as to when we characterize a given phenomenon as a strategy, or as an aspect of some more cohesive entity such as a rule system. An example of the protean nature of strategies can be found in Meisel's discussion of simplification—explicitly defined as a strategy. Meisel makes a distinction between restrictive and elaborative simplification. The first of these is apparently a principle of communication: it "serves the purpose of achieving an optimal result in communication while reducing the grammar in a way which makes it easy to handle". The second appears to be a principle of learning, since it "really represents an extension of the earlier system and a step toward the target variety" [126]. As Meisel himself comments elsewhere:

> It remains to be shown whether there exists a hierarchy of such strategies, i.e., whether one will win over the other cases where they conflict and also whether several strategies might merge and thus increase the probability of appearance of some linguistic features [127].

One cannot help but suspect that the term strategy is at the moment somewhat of a catchall; we apply it to processes we cannot integrate into more coherent systems.

Having said all this the author does not want to deny that there are such things as strategies, or that the issue of the processes to which they apparently refer is not an important one. Since the present study is very much concerned with the processes of second language acquisition, it will have more to say about "strategies", or at least some of the phenomena that have come under this rubric.

Variation and the Factors Affecting It

This too is a question with which the study will be directly concerned. One of the most notable features about learner language is its variability. Before the present upsurge in second language acquisition studies this variability was taken to be merely pathological confusion, a welter of misconstrued and distorted fragments of the target language. There is now no real issue as to whether or not there is system of some kind in the speech of language learners; the questions are rather just how systematic this system is, and what are the parameters which best describe it. One important development here was the application of variational models of language change to language learning (see below). These provided a much clearer picture of how an unstable or evolving system could be described, and of how change itself was implemented in such systems. Having been derived from models of language change the variational models applied to language learning operated on the assumption that non-trivial systematic variation was for the most part an adjunct of the dynamic processes (in this case characterizable as development towards the target) which were integral to learner language. More recently researchers have turned to the question of whether there might not be different kinds of variation in learner grammars, whose determinants are distinct from those which are to be found along the main developmental dimension of second language learning. In regard to this latter question the work of Meisel, Clahsen and Pienemann on the learning of German as a second language is most interesting. The present study will—albeit briefly—make an attempt to explore within the context of English the distinctions between "developmental" and "variational" features which they have developed for German [128].

Summary

The ten questions formulated by Nicholas and Meisel, though essentially interdependent, fall into two broad categories: those directly concerned with the mental processes which characterize the activities of the second language learner (questions 1,3,9 and 10), and those concerned with the impact of the learner's physical environment and own biology on these processes (questions 2,4,5,6,7 and 8). It will now be apparent that to the extent that it is possible to do so without vitiating the whole enterprise that the present study seeks to address itself to the questions of the first category.

2.1.3 Historical Perspective

Having now provided some idea of where the study stands in relation to the main research concerns of the day, we can turn to the task of providing some historical background. Once again, this can only be sketchy. Some references for historical summaries have been given above. The present

summary is indebted to these, particularly the first part of Chapter One of Thom Huebner's book, which contains a very useful discussion of contrastive analysis [85].

What follows is a brief critique of the principal theories in second language acquisition. They are dealt with in the order in which they emerged because each new theory that was proposed was largely motivated by the perceived inadequacies of its predecessor and can therefore be most easily understood in a historical context.

It should be emphasized that the present discussion restricts itself largely to those theories which attempt to account for the processes of learning itself. There have been, of course, many other kinds of projects in second language acquisition studies, especially in the past three or four years. In particular, there has been a growing interest in the interactive aspects of language learning and production: in discourse analysis, conversational analysis, and cross-cultural contact [151].

In terms of the rough typology presented above much of this work is concerned with the environment of linguistic transactions. It has already been pointed out that it is not within the scope of the study to take an active position on these questions if it can reasonably avoid doing so, and consequently no evaluation of the growing body of literature that surrounds them will be attempted.

The author is aware that he could be accused of ignoring such questions at his own risk: there has always been a position in linguistics which maintains that failure to take into account such things as the conversational dynamics, pragmatics and semantics of linguistic interactions will nullify attempts to understand what is happening at the "nuts and bolts" level of syntax, morphology and phonology. The author's response to this argument is that while there may indeed be a high level of interpenetration and interrelation between events on the various levels of generality that constitute the domain of linguistic activity, the attempt to simultaneously describe more than one or two of these is likely to end in paralysis—one simply does not know what to take up first. Taking a holistic approach to language is somewhat like trying to write a complicated computer program without using functions or subroutines: it is difficult to achieve any degree of organization and explicitness in the enterprise without defining some hierarchically related set of levels of particularity. While there might be a certain psychological arbitrariness in, say, concentrating on syntax to the temporary exclusion of meaning or discourse features, the gain in terminological explicitness and predictive power that can follow from this may in the long run result in a much clearer picture of the relative interdependence and autonomy of the elements in the whole system, and of how control is passed from level to level within it.

Debate of this kind is perhaps a reflection of the state of development of the discipline of linguistics as a whole: it lacks a unifying terminol-

ogy. Yet unifying terminologies do not necessarily come from holistic approaches. Consider the case of physics and chemistry, which freed of the influence of philosophy followed increasingly divergent courses, only to have their interdependence re-established by the discovery of the electron bond—specialization was a necessary condition of this reunification.

2.1.4 Contrastive Analysis

While the case against contrastive analysis is now well established, and generally uncontentiously accepted as proven in academic circles, this particular view of language learning is probably still the prevailing one amongst students and even many teachers. Because of the continued status of contrastive analysis in "folk linguistics" it therefore merits some discussion.

Contrastive analysis was the dominant approach of the forties and fifties to the question of language learning. The prestige accorded to contrastive analysis was not generated from within linguistics itself. Rather, contrastive analysis was the product of an era when linguistics was very much under the influence of comparative anthropology, which had had many notable successes in the field in the preceding decades, and behaviourist psychology, which, while hardly able to claim the substantial successes of anthropology, was the perfect vehicle of an ingrained and prevailing sense of determinism in American intellectual thought of the time [111]. From anthropology contrastive analysis inherited the notion of systematic structural comparison, and from psychology the notions of interference and habit formation. As Huebner points out, linguistics (American linguistics, one should say) subjected the idea of interference, which had in psychology the fairly neutral meaning of "the effect of a preceding activity upon the learning of a given task" (Osgood, quoted by Huebner), to several not very well motivated, but convenient, reductions [85]. This led to interference being negatively characterized as, for instance, the "ingrained and often misleading influence of the mother tongue", or, more colourfully, "the speaker's uncontrollable tendency to use first language habits in second language speech" [85].

In its so-called "strong" form, the contrastive analysis hypothesis asserts that the ease or difficulty with which the features of a foreign language will be learned is directly determined by their degree of similarity to equivalent features in the learner's native language. In other words, old habits are easy to transfer, but hard to break, while new ones are equally hard to learn.

Underpinned as it was by the prestige of anthropology and the appealing simplicity of behaviourist psychology, contrastive analysis simply went untested and unchallenged for some considerable period of time (formally, until 1971, in Huebner's reckoning) [85].

Despite this, not all exponents of the contrastive analysis hypothesis supported the strong form of the hypothesis. Stockwell and Bowen, whose work on German and Spanish in comparison to English probably represents the

most thorough attempt to put this theory into practice (in a series of meta-pedagogical grammars) acknowledge, for instance, that factors exist which can condition the predictions of contrastive analysis. Thus they contend that the task of an English speaker who has to learn the Spanish phoneme */i/* is an easier one than the corresponding one for a Spanish speaker who has to learn the English phonemes */iy/* and */ɪ/*: collapsing a distinction one already has is easier than learning to make a new one. Another mediating principle they suggest is that of functional load [173].

With or without modifications the contrastive analysis hypothesis is beset with defects. These are mainly the defects of a theory which never had to be thought through, and provide evidence of the degree of unquestioning acceptance which must have sustained the hypothesis. In the first place, it does not appear to have occurred to anyone that the occurrence of a particular form in a learner's speech and in his mother tongue might not be evidence for interference or transfer—to assume that it definitely is represents an error in inference akin to the logical fallacy of undistributed middle. Second, the question of whether languages are directly comparable, and if so on what organizational level (phonetic, morphological, syntactic or other) or on which grammatical level (that of surface structure, deep structure, etc.) these comparisons might be valid does not appear to have been seriously considered. Nor was the question of what level of abstraction interference or transfer might be expected to operate on: that is, there appears to have been no consideration of whether interference was a phenomenon restricted to literal similarities (for instance, plural marking with *-s* in English and Spanish) or whether it would extend to what we might call paradigmatic similarities (for example, the placement of tense markers after the verb stem in both languages), or even to some higher level of abstraction.

Then of course, there is the matter of constrastive analysis' assumption that learning is a process of habit formation and that "the use of grammatical structure depends heavily on habit" [108]. This was never empirically supported; nor was Chomsky's argument that ordinary linguistic behaviour is "innovative" in a way that cannot be accounted for by any known principles of association, reinforcement or generalization ever satisfactorily answered. Of course, habit formation also failed to account for behaviour apparently a good deal less complex than language use [28].

Lastly, contrastive analysis quite failed to stand up to empirical scrutiny. The author is not aware of precisely which data led to the abandonment of the strong form of the hypothesis; one imagines that even on an anecdotal level the evidence that its predictions were drastically wrong must have become more and more difficult to ignore. And once weakened, the contrastive analysis hypothesis became alarmingly *ad hoc*, lacking even the means to rationalize its own failures.

The history of contrastive analysis is an interesting one for what it reveals about the early methodology of language acquisition research.

One notable feature of research in this area has been its relative dependence on other disciplines, or at least other branches of linguistics, for its theoretical input. Thus, the contrastive analysis hypothesis did not turn out to be a very resilient one within the domain of linguistic theory because it was essentially the step-child of other disciplines.

It would seem that this dependence on other disciplines, while inevitable in the absence of a body of native theory, has been augmented by a tendency in some branches of language learning research to try to confer the status of theory on what are really only intuitions by clothing them in established, but not necessarily appropriate terminology. This is what happened in contrastive analysis with the concepts of interference and habit formation. And in turn the pervasiveness of habit formation as an explanation for learning in the community at large has sustained a vague form of the contrastive analysis hypothesis amongst many language learners and even teachers long after it has been dismissed from serious theoretical consideration.

The demise of the contrastive analysis hypothesis itself has some curious and instructive aspects. Deprived of its theoretical credentials by the collapse of its host theory the problem of interference has acquired a sort of orphan status in second language acquisition circles. Because of its connections with a discredited theory it has been passed from hand to hand but never given a new home. This tendency to ignore and exclude elements of a theory because the theory as a whole has been found wanting is somewhat characteristic of the history of applied linguistics in general: we have already observed the same process in pedagogical theory where the question of language content has been obscured by its association with the discredited methodology of grammar translation.

Finally, the way in which the successor to contrastive analysis, error analysis, emerged is also rather typical of the processes of theory generation in second language acquisition research. The most serious single blow to contrastive analysis came from its failure to predict learner errors [85]. The theory which supplanted it therefore duly took the production of a taxonomy of learner errors as its major task.

Error Analysis

The point at which error analysis took over from contrastive analysis really represents a major watershed in the short history of second language acquisition research. While some of the conditions that characterized the period of contrastive analysis, such as reliance on external sources for theory generation and a tendency for the theoretical pendulum to swing to extremes still prevail, subsequent enterprises have in general tried to make their theories more accountable in terms of theoretical specificity and empirical accessibility.

Once again, error analysis received a theoretical impetus from without.

As Richards says: "The applied-linguistic concept of Error Analysis was a direct outcome and application of Chomskyan linguistics and its accompanying psycholinguistic developments in language acquisition research" [150]. In research terms too work on first language acquisition and second language acquisition by children preceded studies of adults [25].

The original concern of error analysis was to document, through a consideration of typical learner errors, what seemed to be systematic and universal processes in language learning. These processes or "strategies" included such phenomena as overgeneralization of target language rules, simplification, and even language transfer itself [150].

The window on these processes were errors themselves. This may now seem a rather curious assumption. We should take into account two things at this point, however. One is that errors had a high profile at the time, since it was error data that had dealt the mortal blow to contrastive analysis. The second is that the cast of mind exemplified by contrastive analysis had by no means died with it: people still tended to think in contrastive terms. Thus, it was a natural assumption that learner language would have to examined in relation to some other object: this object was native speech, and errors were what differentiated it from learner language. The terminology of the time actually makes this contrastive assumption quite explicit. Thus learner language is referred to (in Selinker's widely used term) as "interlanguage"—a transitional system. While the destination of an interlanguage is obviously the target language it is not always clear what users of this term consider to be the point of departure (in Selinker's original paper it is implied that this is the first language) [165].

A great deal of attention was initially devoted to providing a taxonomy of error types—different errors might point to different underlying learning processes. Richards sums this up:

> A basic distinction was between "intralingual" and "interlingual" errors. Interlingual errors were accounted for by language transfer. Intralingual errors were categorized as "overgeneralizations"—that is, errors caused by extensions of target language rules to inappropriate contexts (Richards 1971); other errors were "simplifications" (errors resulting from redundancy reduction (George 1972; Richards 1975)); "developmental errors" (those reflecting built-in stages of linguistic development (Corder 1967)); "communication based errors" (errors resulting from strategies of communication (Selinker 1972)); "induced errors" (those derived from the sequencing and presentation of target language items(Stenson 1974)); "errors of avoidance" (failure to use certain types of target language features because of perceived difficulty (Schachter 1974)); "errors of overproduction" (target language features produced correctly but used too frequently (Schachter and Ruther-

ford 1979)).

He adds that the above classifications "have been used to account for errors at the levels of phonology, syntax, lexis, and speech acts" [150].

Error analysis itself suffered from various problems. The length of the preceding list of errors hints at one of these—namely, that the approach was largely taxonomic. Thus it was frequently difficult to decide which category an error should be assigned to, or to provide a reason for the choice that was made. There are various examples in the literature of conflicting category assignments [52]. In addition, error analysis was unable to place theoretical limits on the number of possible error categories that there could be, and the list was already large and growing.

Equally if not more important was the problem of the inherently contrastive nature of error analysis: it looked at learner language in target language terms. And in concentrating on learners' errors in reference to an external set of rules it failed to recognize that the system that generated those errors might be organized quite differently.

A further point to make about error analysis is that its data collection techniques left much to be desired. According to Huebner all the error analyses of adult learner language were "cross-sectional in nature and for the most part drew their informants from language classrooms" [85]. Richards, who puts a considerably wider interpretation on the term error analysis itself, notes that structured data elicitation techniques include "translation (Noss 1979), free composition (Corder 1973), elicited imitation (Ervin-Tripp 1974), picture description (Dulay and Burt 1974), sentence completion tasks, structured interviews, story telling (Selinker, Swain and Dumas 1975), and elicited intuitional data (Kellerman 1978)". He notes further that "it has been recognized that the 'system' so elicited may be an artifact of the method of data collection, since it often reflects: (a) the amount of time devoted to data collection; (b) the type of communication task used to elicit the data, and (c) the setting where the data was collected" [150]. Such data collection techniques are, of course, often more convenient to administer and tend to produce more easily analyzable data. But, as Richards points out, they are also the product of the approach itself: large quantities of specific error data are hard to come by in unstructured discourse [150].

Unlike their forerunners, practitioners of error analysis at least collected data. Unfortunately, however, in failing to devote as much care and thought to the collection of their basic materials as they did to other aspects of their work, they established a precedent which those that followed them seemed only too content to let stand. We shall see the consequences of this in the next section.

2.1.5 Orders of Acquisition and Morpheme Order Studies

Morpheme order studies represented an attempt to deal a little bit more closely with the learner's system in its own terms. Their object has been variously defined as the establishment of a fixed order of "acquisition" or "difficulty" for the morphemes of English. Once again the original impetus came from (longitudinal) child language studies, and the techniques were applied to child second language learners before they were applied to adults [46].

Morpheme order studies concentrated on such morphological aspects of English as pronoun case, pluralization, verb marking (regular, irregular and the progressive aspect), possessives, contracted forms of the copula and auxiliaries and articles. The principal studies carried out were those of Dulay and Burt (1973, 1974); Fathman (1975); Larsen-Freeman (1975); and Krashen, Sferlazza, Feldman and Fathman (1976) [106]. Ostensibly, these various studies, which involved different samples and at least two different main elicitation procedures, kept turning up morpheme orders which were both similar to each other and to the orders found in child language studies. This led to assertions that at the very least there was a common order of difficulty for these morphemes which was invariant across a number of factors such as age, first language and social background. The study by Larsen-Freeman, in which five different elicitation procedures were used, struck a slightly wrong note in relation to these claims, when the same orders were not produced with each procedure, but it was alone in this [109].

Close examination, of which there has now been a great deal, reveals, however, that morpheme order studies have a great many difficulties to contend with. Much of what follows has now been thoroughly thrashed out in the literature; nevertheless, the situation of morpheme order studies is an instructive one from the point of view of research aims, methods, conclusions and the way in which debate is handled, that it is worth going over some of the ground again [85].

We will look briefly at the research plan of morpheme order studies, the data gathering techniques employed, and the way in which this data was analyzed.

Firstly, the research plan of morpheme order studies does not appear to be particularly well conceived. The items of investigation are all markers of one kind or another, to be sure, but they are connected with very diverse parts of an overall rule system—in the target language at any rate. Morpheme order studies do not seem to have had any preliminary hypotheses about why morpheme order should be as it is; they simply took their cue from the observations which had been made by Brown [25]. Thus, even if there is an invariant morpheme order (a doubtful proposition), what would this prove? It is worth remembering in this connection that Larsen-Freeman has suggested that morpheme order in learner speech reflects nothing more

significant than frequency distributions in native speech [109]: if this is so then the whole question of morpheme order becomes quite trivial.

A deficient research plan has other consequences, apart from the danger that it may lead to one chasing one's own tail. The *ad hoc* empiricist exercise of trying to answer questions that have been inadequately formulated is not likely to have a very productive effect on future research activity. Indeed, the main effect of morpheme order studies has been to spawn other morpheme order studies. Their lack of an explicit theoretical framework also makes the problem of deciding how to treat discrepancies in the results a very difficult one to solve: in the end one can only make statistical comparisons. If these seem unduly harsh judgements to make about what are after all explorations in mostly uncharted waters, it should be remembered that we are not talking about a single study—morpheme order studies represent a significant trend in second language acquisition research; there are a multiplicity of them, and is assumed in some quarters that their findings are incontestable evidence for universal natural orders [51].

On the question of data gathering, morpheme order studies used formal elicitation procedures like the Bilingual Syntax Measure and the SLOPE test. Both the tests and the testing situation could have affected the nature of the data quite dramatically: this has been argued in some detail in regard to the BSM by Porter [148].

The situation as regards analysis and interpretation of the data is even worse. Here there are a number of distinct problems.

- Scoring procedures have not always been used consistently by studies whose results are nevertheless directly compared—this question is dealt with in some detail by Huebner [85]. Also on the question of scoring, Andersen has pointed out that quite complicated statistical procedures have to be applied to ranked data to determine whether or not such ranking is significant [2].

- The criterion for acquisition in the studies is that a feature must be produced in 80% or 90% of its obligatory contexts. This is quite arbitrary. It is clearly ridiculous to imply that someone who produces a particular feature in, say, 70% of its obligatory contexts does not control that feature any more than someone who produces it in a mere 10% of the cases, yet that is what is implied. If it is to be argued that a cutoff point had to be selected, then it is difficult to see why the native speaker level of 100% (or near enough) was not the figure chosen. Clearly, the decision about what criterion to adopt is quite crucial to the results of these studies, yet it was merely passed from one study to the next without comment.

- The notion of criterion itself is based on a further assumption that it is possible to determine what are obligatory contexts for standard

English morphemes in a language which is not standard English. This assumption is never discussed either.

- The other side of the above coin is that it is equally difficult to attribute standard English functions to forms which occur in learner language. The description of the *-ing* morpheme as the progressive morpheme is a classic example of this kind of mistake.

There are other problems that one could mention: while the studies were cross-sectional, the resulting rankings are sometimes interpreted as if they were extracted from longitudinal data; questions of difficulty are mixed in with presumed orders of acquisition; conclusions about the difference between formal instruction and natural learning are made on the basis of samples which do not clearly represent this opposition, and so on.

In morpheme order studies there has been a particularly unfortunate tendency for the technology of research to obscure the nature and value of the questions to which it is addressed. Statistical analysis and precise quantification are wasted if the underlying research questions are not properly formulated, the data collection procedures are intrusive and the interpretation of the results is vitiated by unreasoned assumptions. The history of morpheme order studies reminds one a little of Tom Wolfe's account of the test pilot who, during a power dive, became so engrossed in reading off the figures on his gauges that he neglected to look out the window at the ground rushing up beneath him [186].

The Monitor Model

In an attempt to provide morpheme order studies with some kind of theoretical environment, in 1977 Krashen (one of the morpheme order researchers) proposed the Monitor Model [105]. The word environment is used advisedly here, since the problems of morpheme order research were such that no amount of internal reworking of the morpheme order investigative framework would have overcome them. What Krashen attempted to do then was save the studies by incorporating them into a plausible theoretical framework.

The Monitor Model (initially the Monitor Hypothesis) is essentially an attempt to deal with the effect that consciousness might have on variability in language performance. Such a theory could conceivably account for the differences which had been found in morpheme orders and relate them to explicit variables like formal instruction.

Central to the Monitor Model is a bipartite division between automatic language processing and conscious linguistic processing [51]. Automatic processing is termed "acquisition" and otherwise generally referred to as "the creative construction process". While we have no influence over acquisition we can of course engage in learning—the conscious counterpart of acquisition. Children learn primarily, if not entirely, by acquisition: "Language

acquisition in children proceeds by a process which is called acquisition (henceforth a technical term)" *sic.* Krashen bases his claims about acquisition on the findings of morpheme order studies, and despite the problems with these outlined above, he maintains that "acquisition proceeds along fairly predictable stages, governed by strategies common to all acquirers approaching all languages" [104]. Adults, according to Krashen, acquire language through a mixed process of acquisition and learning: they " 'supplement' their (usually) imperfectly acquired competence by means of consciously learned linguistic knowledge" [104].

One of the strongest assertions made by the Monitor Model is the way in which learning and acquisition are related. Learning, it is argued, can only act as a filter for what has been acquired by "acquisition". Thus "Conscious linguistic knowledge acts only as a 'monitor', altering the output of the acquired system when time and conditions permit. This 'intrusion' generally takes place at some stage prior to the actual utterance" [104]. In this way Krashen accounts for differences in the performance of learners with different degrees of monitor use. Some "second language performers with highly developed monitors are thus able to out-perform their acquired competence when conditions allow this conscious knowledge to intrude" [104]. Monitoring and learning also have certain psychological correlates which help to explain differences in language learning success: attitude is related to the monitor, while aptitude is related to learning [105].

The assumptions about the relationship between conscious and automatic processes made in the Monitor Model are quite breathtaking, given the present state of knowledge in the domain of cognitive psychology and learning theory in general [71]. Theoretical cases against Krashen's theory have been presented by McLaughlin [123] and Sharwood Smith [166]. Empirical research into some of the crucial tenets of the Monitor Model, such as the assertion that children do not learn, have produced results in direct contradiction to Krashen's assertions [126].

Some of Krashen's ideas are obviously derived from Piaget. Although Krashen does not provide other sources for his model, it would seem that in assigning a role to learning he has drawn on the notion of filters employed in later versions of transformational grammar. These filters (for instance, surface structure constraints) operated on the output of the generative component to limit it in various specified ways, and were somewhat of an unwelcome but apparently necessary extension to the theory [135]. (Krashen's use of the term "creative construction" (via Dulay and Burt) is another indication of the influence of transformational grammar on his thinking). Of course, in transformational theory it was never assumed that filters were consciously applied in any way, and indeed there are numerous and explicit warnings within the literature of transformational grammar on not assuming "psychological reality" for the formal models presented [76]. Krashen's model is, on the other hand, an explicitly psychological model.

A further possible source (perhaps not a conscious one) for the Monitor Model is Freudian psychoanalytic theory. The role allotted to learning in Krashen's model is strikingly similar to the position of the superego in Freud's own model: both are shaped by instructional processes and constrained to a mode of operation whereby they can only intervene in processes or operate on material generated subconsciously. And just as we do not have direct access to the Freudian id, we have no access to the processes of acquisition and creative construction either. Without wishing to pursue this analogy much further, it might be worth pointing out an significant difference between the two theories. The Freudian model is a tripartite one (at the very least): in attempting to deal with the relationship between unconscious and conscious behaviour it posits a conscious entity, the ego, and two other entities which are not within conscious control (although one can be affected by learned behaviour). Krashen's model of conscious and automatic behaviour (even given that it is restricted to language) is a much more simplistic one.

Krashen's model does draw attention to certain apparent facts of language behaviour, such as the effect of consciousness of language production. These effects, however, had been noted and dealt with some time before in the domain of sociolinguistics by researchers such as Labov [107]. (Labov's insights have been applied to second language acquisition by Tarone [175]). Krashen's innovation has really been to treat a phenomenon previously considered to be explicable in terms of some sort of continuum model as strictly binary. The burden of proof for such a course of action must certainly lie with Krashen, given the lack of supporting evidence from within or outside linguistics [174]. This proof has not been forthcoming.

Recent modifications to the theory have weakened the strictly binary division of mental processes that constitutes its main claim to non-triviality, thereby rendering it even vaguer than it previously was. Thus, Krashen now acknowledges that "While the monitor makes conscious editing of one's own language possible, it is not the only source of self-correction. Subconsciously acquired grammar also plays such a role" [51]. In another weakening of his original position he also asserts that a constraint on the operation of the monitor is that "in most L2 learners" it "is limited to lower-level rules of the language" [51]. Lower level rules are defined as "those that are easy to conceptualize and do not require mental gymnastics" [51]. [1]

Statements like the one quoted above are typical of the degree of precision that characterizes monitor theory. In addition much of the evidence adduced in support of the theory is anecdotal—"M.J., an extremely competent free-lance editor we know, does most of her editing by feel...M.J. finds conscious rule-knowledge dispensable and even adequate *sic* for comprehensive editing" [51]. (Since M.J. is presumably a native speaker, this is

[1] How's that for formal precision?

hardly surprising, and of dubious relevance to a theory which encompasses second language learning. Krashen's selective use of evidence elsewhere in his work has been remarked upon in recent papers by Pienemann [126] and Gibbons [69]).

Monitor theory has received a good deal of promotion and attention. It is time to seriously ask if this is deserved.

Other Major Hypotheses

Since the era of morpheme order studies there have been a number of other models of the second language acquisition process advanced. These models have made more substantial and explicit proposals about both the nature of underlying linguistic competence and the processes of learning itself than morpheme order studies. While the majority of them are more tightly formulated and coherent than the Monitor Model many of these models are not associated with extensive research enterprises and therefore remain highly speculative.

The Pidginization Hypothesis

One such influential hypothesis has been the pidginization hypothesis, first formulated by John Schumann [163]. Schumann proposed his theory largely to account for the failure of one particular informant in a longitudinal study he was conducting to progress through the stages of development in certain areas of syntax that had been defined for the other speakers in the study. (The areas of syntax were negation, wh-questions and auxiliaries). Essentially, the argument proceeded in two steps. The first step involved explaining the informant's lack of progress in his learning of English in psychosociological terms: it was a product of what Schumann called "psychological distance"—the informant, Alberto, was a Spanish speaker who lived in a ethnic ghetto near Boston and had apparently little need or motivation to integrate himself into the Anglo-Saxon community. This explanation is of course quite plausible, although there is no way of knowing whether or not it was the right one in Alberto's case. In order to try and resolve this doubt, Schumann took the second step in his argument. This was to point out an analogy between formal aspects of Alberto's language and those that had been described for many pidgins. Thus, Alberto's language exhibited preverbal negation with *no*, a lack of verbal inflection, pronoun deletion and several other features. Since these features were also to be found in pidgins, and despite the fact that Alberto's speech contained features uncharacteristic of pidgins (such as use of the copula and pluralization) Schumann argued that Alberto's speech was pidginized. On this basis he was then able to formalize his social and psychological distance explanation, by arguing that the conditions which produced the simplified features of pidgins, namely the dis-

tance between pidgin speakers and speakers of the superordinate language were those which were responsible for simplified patterns in Alberto's speech as well.

The introduction of theory from the field of pidgin and creole studies did both a service and a disservice to second language acquisition studies.

Schumann's argument is essentially an argument by analogy. As such it is not an especially powerful one. In introducing the issue of pidginization into his discussion of Alberto he may have done more to complicate his argument than to clarify it. Thus, within the field of pidgin and creole studies there are basic disagreements as to the genesis of pidgins and their relations, as expressed in such concepts as "simplification", to superordinate languages [85]. Schumann is therefore basing his analogy on material that is by no means axiomatic and uncontentious. Furthermore, in describing Alberto's language as pidginized Schumann is identifying two things—namely Alberto's lack of progress and the actual formal characteristics of his language—with the one label. This is misleading, as it implies that they are somehow connected. In fact, we now know that the features which Schumann describes in Alberto's language are common to a very large number of language learners (if not all) at some point in their development. Alberto differs from some of these, as he differed from the other informants in Schumann's study, only in that he did not progress beyond a certain point. Pidgin studies may help us to understand this lack of development: they do not, however, explain it, given the impossibility of categorically identifying Alberto's language as a pidgin. (Subsequent work, such as that of Meisel and Pfaff, has advanced a strong case for not labelling such languages pidgins [138]). Had it been the case that Alberto's language was distinguishable from that of other learners by other features than its failure to evolve, then perhaps the pidginization analogy would have been more informative. As it turns out, this was not proven. And the question of why certain formal features of pidgins and learner languages are as they are cannot be resolved through the circular process of comparing one to the other.

On the positive side, the pidginization hypothesis did draw attention to the fact that natural languages such as pidgins resembled the speech of language learners, at least in certain regards. This point had also been made by Clyne at somewhat earlier stage [39]. The fact of this resemblance in turn encouraged researchers to begin thinking about the processes and principles underlying semi-evolved forms of natural language, as well as the sociological correlates of these processes.

Other Creole-Related Hypotheses

Schumann has continued to pursue his analogical approach to the psychosocial dimensions of language learning. In more recent work he has suggested that there is an analogy between second language learning and creolization

as well. (Creolization is the process whereby a pidgin becomes the first language of a generation of descendants of pidgin speakers and involves complexification of the original pidgin to the point where it can be used to carry out the full range of functions normally required of a standard natural language). Schumann's basic argument is simple: a pidgin or a restricted learner language of the type attributed to Alberto is used largely "instrumentally", that is, for the achievement of specific and restricted ends. When the second language learner "attempts to use his interlanguage for integrative and expressive purposes" it will then "complicate and expand in ways similar to creolization" [91]. There is in fact a crucial difference between creolization and second language learning which is ignored by Schumann. This is that in creolization there is no "superstrate"—no target language [85]. In addition, given that once more some of what Schumann assumes about creolization is not universally accepted, we have to ask how useful analogies of this kind really are. Reliance on analogy and comparison, is, as we have seen, deeply ingrained in the methodology of second language acquisition studies, and has sometimes led to a failure to confront the basic primary data and issues of the subject.

Difficult as this may be in practice, there is a point when hypotheses arrived at by analogy must be empirically investigated: the only way whether we can find out whether or not what Schumann proposes about the sociological dimension of second language learning is true is by investigating the behaviour of a significant number of second language learners [85].

The same general comments as have already been made on the pidginization and creolization hypotheses apply to the Decreolization Hypothesis, which was advanced by Stauble [172]. Like Schumann, Stauble attempts to correlate certain social conditions with the language learning process. Thus, she argues, there is a parallel between developmental stages in second language learning and the variety of language forms observable in a society of creole speakers where the superstrate language has become both accessible and prestigious and is gradually replacing the creole.

There are certain misconceptions in this analogy. The principal one is that a creole is a reduced language form. Another problem is that it assumes variation in learner language can inevitably be located on a developmental dimension (this will be discussed further in 2.1.8 below).

Given these problems, we need to consider how productive such analogies really are. As regards the possible parallels between second language acquisition and pidgin and creole phenomena, we now have a complete set. That is, it has been argued (by different writers) that language learner language resembles the structure of pidgins, creolizing pidgins, creoles, and decreolizing creoles [3]. While there may be some truth in all of these proposals and while one goal of linguistic investigation is to find out what is universal in language, we are surely entitled to wonder if we are not going round in circles—all the more so when much of this hypothesizing has been based on

a limited amount of data collected from six Spanish speakers.

A more productive aspect of Stauble's proposal is that she draws attention to Bickerton's principle of "lexical levers"—the principle whereby a form can enter a system with a particular function, or no function at all, and interact with the system in such a way that both the function of the form and the system change [16]. Stauble is criticized by Huebner for misinterpreting Bickerton's interest in this phenomena. It will be argued, however, in 2.1.9 above, that although the mechanics of lexical levers in decreolization and second language acquisition may differ, the underlying principle is similar and provides a means of understanding lexically driven change. This principle was also recognized as a possibly fruitful one by Bonney (1976 [20]) in the research design for the project out of which the present study grew [21]. Too literal application of externally developed theories to second language learning is obviously unproductive and hinders the process of generating native theoretical models. This does not mean that such theories are *in toto* irrelevant to an understanding of second language acquisition, however. In the next sections, we will examine how analytical tools developed for understanding *processes* in other areas of linguistic investigation are of relevance to the study of second language acquisition.

2.1.6 Variation

As mentioned above, while the relationship between second language learning and decreolization (or other aspects of the pidgin-creole situation) should perhaps be left in abeyance for the present, the analogy between the processes that have been documented in pidgin and creole studies and those which may operate in second language learning is worth considering in more detail.

One important feature of many creoles and all well documented cases of learner language is variation. Variation is, of course, not restricted to these types of language: it is a feature of all language. However, in learner languages and in creoles under certain circumstances variation is more than usually evident for a given community.

Creole Continua

This was the situation that Derek Bickerton found in Guyanese creole. His explanation for this situation is as follows.

What he calls "the pidgin-creole-decreolization" cycle begins with a second language learning situation in which the future pidgin speaker, who has his own first language (and possibly others) "is confronted with a grammar (that of the superstrate language) which is quite different from his own". (The context for this contact was generally in a colonial slave situation). Bickerton argues that "Had Euro-African contact proceeded on

non-exploitive terms, this situation would have developed along the usual foreign language-learning lines, with some Africans speaking European languages fluently, others speaking them passably, others having only a smattering, but all retaining control of their native tongues". However, "The slave trade disrupted this pattern, isolating many Africans at a very early or null stage of European language-acquisition and cutting them off from their own language communities, thereby virtually obliging them to use the imperfectly acquired European language as their only medium of communication" [16].

Bickerton continues that "After Emancipation, the language-learning process so sharply interrupted by slavery was resumed, but it was resumed with a difference". He illustrates this difference with an analogy: "The normal second-language learning situation may be compared to that of a man who already has a house building a second house. The second house will generally conform to universal ideas of what a house should be like, but it will be made of materials different to the first and may quite likely have a different design. While he is building his second house the man will not be comfortable in it but he can always go back and stay in his first house until he feels like doing some more work on the second. If and when the second house is completed he can commute between the two". The crucial difference with a pidgin speaker, however, Bickerton argues, is that he "has just begun to build his second house when some disaster destroys his first one. He has to abandon it and use what he can salvage from it to complete the second one. Naturally, the shape of the second house is constrained by the materials he has to use. It resembles neither the house he used to have nor the new one he envisaged. Still, his children grow up in it and to them it is home, a house in its own right like any other. But then, one day, generations later, an important someone comes along. This someone says that the house is only an inadequate copy of what it was meant to be. He produces what he claims were the authentic plans. He insists that the house must be remodelled to conform with them. Since there is nowhere else to go, this remodelling must take place while the family continues to live in it, regardless of the inconvenience and embarrassment that this may cause them. When he has gone, the family disagrees about what should be done. Some are for obedience, others for resistance, some are apathetic and will do what seems convenient. No agreement is reached, so finally everyone treats his own room as a separate entity. Some radically remodel theirs, others make superficial alterations. Others defiantly leave everything exactly as it is—and so the matter rests, to this day" [16].

The result is what Bickerton terms a "creole continuum", a kind of infinite set of interlocking dialects with a version of standard English at one end and the original (unremodelled) creole at the other. Figure 2.1 below shows how the sentence *I told him* might be rendered at different points of a creole continuum. It should be noted that the differences in the pronominal

ai	tould	him
ai	to:ld	him
ai	to:l	im
ai	tel	im
a	tel	im
a	tel	i
mi	tel	i
mi	tel	am

Table 2.1:

and verbal systems reflected in Figure 2.1 are quite profound, and do not involve mere surface phonological variation.

Bickerton himself was fully aware of the implications that the study of creole continua such as the Guyanese one had for an understanding of language learning and pointed out that "it is convenient in many ways to think of the Guyanese community as taking three hundred years to learn English" [16]. [2]

We have already discussed the possible problems with such analogies, and so will not pursue this point further. What is of importance, however, are the mechanisms Bickerton applied to the study of this creole continuum and the processes they revealed. Bickerton was faced with the problem of describing not one grammar with an apparently stable kernel of rules but what he terms a "dynamic" system where variability was the norm. One of the tools he adopted for this purpose was the technique of "implicational scaling", first developed by Guttman (1944) and introduced into linguistics by Elliott, Legum and Thompson (1969 [55]), and DeCamp (1971 [47]) [16].

Figure 2.2 below presents an idealized example of an implicational array. Items #1 to #8 constitute an implicational sequence. The production of item #8 guarantees the production of all other items, while the production of item #7 guarantees only the production of items #1 to #6, and so on. Within the array the eight speakers are ranked along the particular axis of change or development the array is concerned with. Such an array provides us with a means of examining both the situation of the group as a whole, and of the individuals within it. Once we have established a ranking for a group of speakers through an implicational array, we can then construct further scales on the basis of this ranking and examine whether the predictions that have been made for one rule or item hold for other aspects of the system being examined.

Closely connected with implicational scaling is the so-called "wave theory" of C.-J. Bailey [10]. This theory, which was originally developed to

[2]Bickerton is quick to point out that this is not meant in any patronizing sense.

Speaker	#1	#2	#3	#4	#5	#6	#7	#8
Item #1	1	1	1	1	1	1	1	1
Item #2	0	1	1	1	1	1	1	1
Item #3	0	0	1	1	1	1	1	1
Item #4	0	0	0	1	1	1	1	1
Item #5	0	0	0	0	1	1	1	1
Item #6	0	0	0	0	0	1	1	1
Item #7	0	0	0	0	0	0	1	1
Item #8	0	0	0	0	0	0	0	1

Table 2.2:

account for the spread of rule changes in language variation, proposed that language changes, rather than occurring across the board, began in a single linguistic environment or a small set of environments and spread in a wave-like fashion to an increasing number of surrounding environments [9]. Substituting "Environment" for "Item" in Figure II, we have a possible example of a linguistic change of the kind dealt with by the wave model. If the rule under examination operates in a particular environment, then it can be expected to operate in all the higher numbered environments. In a situation of change or variation, provided we have a statistically significant sample it should be possible to arrange speakers and environments in such a way that an implicational pattern results. The implicational pattern shows the directionality of the change in question.

There is a certain amount of disagreement as to whether implicational scales are explanatory or merely descriptive. Bailey (personal communication) has argued that in some cases it is pointless to inquire as to why a particular scale is as it is: the important point is that linguistic data has a consistent tendency to arrange itself in such ways. Others would argue that it is possible and desirable to locate the factors which shape an implicational series [34]. Bailey points out that independent of other considerations implicational scales have a good deal of psychological plausibility. Thus, he notes for an example of a 11-cell by 9-cell matrix containing implicationally arrayed data that in a state of total randomness there would be 362,880 9! combinations of environments and argues that "the human brain could hardly be able to be expected to cope with such a vast pattern of possibilities without some general principles of organization". A natural form of patterning, he maintains, is "an arrangement in which larger sets are implied by smaller ones" [9]. This is precisely the organization of an implicational array. Ultimately, the question of the explanatory adequacy of implicational scales may depend on the kind of data that is being investigated. The question is, in any case, not crucial to the present study, where description rather than prediction is the overall goal.

Bickerton's demonstration of the applicability of implicational scales to the description of variable linguistic phenomena represents one contribution of his work to second language acquisition studies. A further contribution has already been mentioned briefly. This is the principle of lexically induced change—of form before function. We will discuss this principle in more detail in a subsequent section.

2.1.7 Implicational Scaling and Second Language Acquisition

The technique of implicational scaling has been applied to second language acquisition phenomena by a number of researchers.

In phonology, Dickerson (1975) found that the accuracy with which Japanese speakers produced the target phoneme /z/ depended on its phonological environment [48]. Prevocalic environments were found to more congenial to the accurate pronunciation of /z/ than preconsonantal or pre-pausal environments. The production of /z/ in preconsonantal environments improved more for some environments than others over the period of the study. Hyltenstam (1977 [87]) found that learners of Swedish as a second language mastered the rules for placement of the negator by correctly negating auxiliaries, but not main verbs, in main clauses, and reversing this pattern in subordinate clauses. Following this came correct placement of the negator after main verbs in main clauses; the final step involved correct placement of the negator with auxiliaries in subordinate clauses. There was some evidence that correct placement of the negator also depended to some extent on the verb or auxiliary in question [87]. Felix (1977) found that English-speaking children learning German produced predicate adjectives, nouns and prepositional phrases with copular constructions with a demonstrative pronoun subject before they did so in the same constructions with a personal pronoun subject; this sequence was later repeated for lexical verbs [59]. More recently Pica (1982 [139]) found that the production of the English indefinite article was conditioned by a series of four implicationally arranged environments—her findings are discussed in more detail in 4.13 [139].

The most explicit statement of how implicational scales can be used in second language research is provided by Andersen (*An Implicational Model for Second Language Research* [2]).

Andersen notes that implicational analysis is "both a device for displaying variable linguistic data in ways which will reveal underlying systematicity in the data and a theoretical model". As a model implicational analysis provides the researcher with a framework for "dealing with systematicity, variability, groups and individual simultaneously". As a technique implicational analysis can be used for "correlating certain attributes of language use with individual speakers or groups of speakers of the particular language under study such that the presence of a particular attribute in the speech of

the individuals being studied implies the presence of certain other attributes in their speech" [2].

Some of the studies referred to above are, unfortunately, open to criticism on the basis of their data collection procedures or on the validity of their original research question. Thus Hyltenstam's data was collected by means of a highly structured instrument—a multiple choice cloze test—and it is arguable that this mode of data collection may have introduced artificial regularities into his results. Andersen's data came from written compositions, while his research was conducted from within the highly questionable framework of morpheme order studies. His paper, while providing a very thorough discussion of the issues involved in using implicational scaling and of the criteria that must be met to sustain a valid claim that one has an implicational scale for a particular series of items, also emphasizes one of the basic problems of second language acquisition research in syntax. This is the tendency for the application of elaborate and sophisticated analytical tools to obscure vital questions about the validity of the data being analyzed and even the basic research question itself. In second language acquisition research, one suspects, regrettably, that analytical techniques have sometimes become the masters, rather than the servants, of enquiry.

Nevertheless, implicational scaling provides a valuable tool for dealing with variability in both groups and individuals. Much of the data analyzed in the present study falls into quasi-implicational patterns. In so far as it is possible to make inferences about developmental sequences on the basis of a cross-sectional study, these patterns will be adduced as evidence for such sequences [75]. Apart from providing a convenient means of tabulating group and individual data, implicational scales also capture an important aspect of the learning process. This is the tendency for acquisition to spread through a series of hierarchically ranked environments.

In the use of implicational scales there are a number of questions that have to be settled. One is what criterion we use for acquisition. In Andersen's model he adopts the high-percentage (80% in his case) criterion of other morpheme acquisition studies. It has already been pointed out that is psychologically unreal. In addition, there are now very well formulated cases against this on the basis that it fails to capture (as one would have expected) the significant changes that can occur within learner systems while production rates for some given item are still well below this level [85]. An alternative approach to this is that of "quantifying all features under consideration in the same fashion, indicating the number of actual occurrences relative to the total number of occurrences" [128]. This seems considerably more satisfactory, provided that one is really in a position to determine the possible occurrences. In the present study this theoretical problem has been solved quite brutally by lack of resources: in most cases any such procedure has not been possible because of time considerations. The effect of this constraint depends very much on the type of data under consideration, at times

it is important, at times not. The approach that has been adopted in the present study is to present counts of the items or structures under consideration: this is explicit and it does away with the need for binary presentation of data, which is after all a simplification, whatever the criterion may be.

A second, related problem is whether an implicational relation can be said to exist if there are gaps in the scale. There is a test for this, the coefficient of reproducibility (Guttman, 1944 [73]) [2]. Once again due to the nature of the data presented, and its quantity, no such measures have been applied. For the present the patterns will have to stand as they are. The working assumption that has been adopted is that implicational scales, like many other things in language, are likely to "leak". Unless one is working with a huge amount of syntactic data it is very difficult to determine whether gaps are merely fortuitous or symptomatic. It will be obvious in many of the tables presented that there are tendencies: for the present it will not be possible to make strong claims about such tendencies in all cases. This will remain a matter for further research, as will the question of why implicational patterns occur.

One further point should be made about the presentation of the tables in Chapter Four. This is that no attempt has been made to optimize them by rearranging the ranking of the informants. The original ranking for these informants was derived from the application of an oral proficiency scale, and is described in Chapter Three. In many cases implicational type patterns are evident in the tables as they are arranged. Once again, it would be a valuable exercise to optimize each table and to compute mean rankings and deviations.

It will be seen from the above discussion that the present study is unlikely to suffer from an excess of research technology. Unfortunately, rather the reverse is the case. In a situation where the first priority was to obtain information on a wide variety of structures and elements, this was inevitable. In any case, the data is there, and there will be time for elaboration.

2.1.8 Multi-Dimensional Variation

We have seen how mechanisms like implicational scaling and wave theory help to deal with the variable nature of learner language and provide a mechanism for mapping the changes in rule systems.

These techniques for dealing with variability were essentially derived from theories which considered variation to be a directional phenomenon. In natural languages change and variation were seen as two sides of the same coin. Bickerton, for instance, quotes Weinreich, Labov and Herzog (1968 [112]) to the effect that "The problem of accounting for the geographical transition of dialects across a territory thus appears to be symmetrical with the problem of accounting for the transition of dialects through time in one community" and asserts himself that "linguistic variation is the syn-

chronic aspect of linguistic change, and linguistic change is the diachronic aspect of linguistic variation" [16]. He cites an example of three groups of speakers, where one group uses feature F1, another the features F1 and F2, and the third F2 as evidence that a change is in progress, running either from the first group through the second to the third or in the opposite direction, so that we are justified in referring to these features as representing "earlier" or "later" stages of the language [16].

Given this particular derivation of the mechanisms for dealing with variability, there has been a general assumption in studies working in terms of developmental stages, such as those which Hyltenstam defines for the rules of Swedish negation, that any variation or change (apart from some possible idiosyncratic features) is an indication of movement by the learner along the developmental continuum. This would certainly be the case, say, in Stauble's decreolization analogy [172].

This uni-dimensional model of language development has been challenged by work done in the *ZISA* project in Germany. Three of the workers in this project, Meisel Clahsen and Pienemann have proposed what they term a "multi-dimensional" model of language acquisition. In this alternative to a linear model of development Clahsen, Meisel and Pienemann argue that while it is not necessary to abandon the view of second language acquisition as a sequence of ordered developmental stages, it may be necessary to allow for considerable variation within each of these stages [128].

In the course of their work with a large sample of Spanish and Italian migrant workers the researchers defined what they term four developmental stages in the acquisition of German word order rules. These stages involved three rules, labelled *PARTICLE*, *INVERSION* and $VERB \rightarrow END$. The first rule involves the separation of auxiliaries and modals from the participle or main verb, which is postposed to the end of the sentence. Thus:

Ich habe ein Haus gebaut—"I have a house built"

The second rule involves an exchange of places between verb and subject, as occurs in certain constructions in English also:

Wann gehst du nach Hause?—"When go you home?"

The third involves the postposing of the finite verb in subordinate clauses:

... weil er dumm ist—"because he stupid is"

Clahsen, Meisel and Pienemann claim that there is very strong evidence in their data that these rules are implicationally ordered, in the order given (i.e. speakers who have $VERB \rightarrow END$ have the other two rules). On a consideration of the various contexts which may provide occasion for these rules they further assert that "one cannot conclude that a given rule will first

be applied for one specific context, then for the next (possibly more complex one), and so on. Learners differ greatly with respect to which context is most suitable for the application of a new rule" [128].

Within the context of the developmental stages they have defined on the basis of these word order rules they then consider another set of syntactic phenomena which have proved somewhat problematic in terms of location within developmental sequences [128]. These are deletion phenomena, involving the copula, pronouns, and elements in movement rules. They find that speakers within the same developmental stage, and even in lower stages, behave differently with regard to these rules. Thus it is possible for two speakers at stage I to exhibit different rates of deletion, and for a speaker, say, at stage I to nevertheless exhibit less deletion than one of the speakers at stage I.

In order to explain the majority of the deletion phenomena, one member of the group—Jürgen Meisel—has invoked the concept of " 'restrictive simplification', a strategy which is applied to minimize processing efforts" when using a second language, without necessarily impairing communicative effectiveness [128]. The determinants of such simplifying behaviour, they argue, are psycho-sociological ones—the speaker's "distance" from German society, whether his or her motivation is "instrumental" or "integrative", and so on. He further points out that simplifying speakers can acquire the three developmental rules they define, but may do so for a more restricted set of contexts than more "correct" speakers [128].

This, then, is their multi-dimensional model. The model has, in fact, three dimensions: two of these, the developmental and the variational, are linguistic while the third is sociological. This model is both well documented and appealing, and the analyses in Chapter Four will make reference to these distinctions. Unfortunately, at this point in time it has not been possible to work out an independently motivated set of developmental rules of the word order kind developed by the *ZISA* project. This is obviously a high priority for the near future.

2.1.9 Lexical Levers

A further contribution of Bickerton was his analysis of how lexical items served as "levers" for profound changes in the rule systems underlying creole *continua* [16]. This has important implications for an understanding of the relation of form to function in processes of language change and, in particular, language learning, and it is therefore worth examining Bickerton's proposal about lexical levers in some detail.

Due to the fact that second language acquisition is not the same as decreolization, we would not expect the mechanisms examined by Bickerton to be mirrored exactly in language learning; one general principle to be adduced from the processes described, however, would seem to be of considerable

relevance. This principle is, in essence, that a form can enter a system with a particular function, or no function at all, and interact with the system in such a way that changes occur, both in the function of the form and the system itself [17].

The clearest example of lexical levers in Guyanese Creole relates to gradual changes in the tense/aspect system initially triggered off by the adoption of negators from Standard English. The process described by Bickerton is roughly as follows:

In the basilectal end of the creole continuum (that is, the locus farthest from Standard English), the tense/aspect system is organized around principles quite different from those of English. The features which concern us here involve the marking of verbs in a narrative sequence according to a distinction between $+anterior$ and $-anterior$, as opposed to the $+past/-past$ distinction of Standard English.

Thus in basilectal Guyanese verbs which describe a sequence of events in the past in the order in which they occurred will appear in stem form: only those verbs which describe out-of-sequence events (for instance, when the narrator recalls a prior event) will be marked. This distinction is essentially the same as that which exists in English between the simple past and the past perfect. For instance, in a narrative we could say either "John searched the house, then checked the garage and looked in the woodshed" or "John checked the garage and looked in the woodshed—he had already searched the house".

In Guyanese Creole, reportage sequenced as in the first example would involve use of stem verbs only, while reportage sequenced in the second example would involve the marking of verb referring to the out-of-sequence action with the lexical prefix bin.

This means, of course, that the basilectal form of the creole has a radically different framework of temporal reference from Standard English—events are considered primarily in relation to each other rather than to the temporal position of the narrator. It should be pointed out that this orientation primarily involves active verbs (that is, verbs that describe actions). Stative verbs (verbs which describe states of being or of mind—thinking, feeling, believing, etc.) are of necessity indexed to the narrator and therefore are sensitive to a present/past distinction of the kind which characterizes Standard English. (Consider for example, the probable use of the past perfect in Standard English in the narration, "John entered the house, he had expected it to be empty": the equivalent of the stative verb expect would be marked for anteriority in a similar narration in Guyanese creole) [16].

This, then, in the situation as regards the $+anterior/-anterior$ distinction we need to focus on in the basilectal phase of the creole. As regards negation in this phase of the creole, the situation is very simple: all verbs excluding modals are negated with the preverbal negator na.

As we move along the continuum from the basilect in the direction of

Standard English we enter the mesolectal (or middle) phase of the creole. In this region of the creole certain changes in the system of negation can be observed. The first of these changes is that na disappears. It is replaced by two forms from Standard English, didn and neva. Neva functions from the moment of its appearance as a *+past* negator for active verbs. Didn, on the other hand, when it first appears, does not function in the creole as it does in Standard English. Instead, for active verbs it functions as a *+anterior* negator. (For stative verbs, due to their abovementioned speaker-indexed semantics, it functions in a manner consonant with its being a *+past* marker). The result of these changes in the system of negation is that in the basilectal-mesolectal region of the creole the tense/aspect system exhibits both the *+anterior/ − anterior* distinction of the basilect, and the *+past/ − past* distinction of Standard English.

Moving a little further into the mesolectal phase of the creole we find that didn begins to take over the function of neva—that is, in addition to its function as a negator of *+anterior* actives it becomes a *+past* negator as well (Bickerton suggests that this may in part be a consequence of its already performing this function for statives). This introduces a further formal instability into an already somewhat semantically unstable system of temporal reference, in that there are now two possible forms to negate +past active verbs: neva and didn.

This instability is resolved further into the mesolectal region by the gradual disappearance of neva. By this point the temporal reference framework for the creole has moved from *+anterior/ − anterior* to *+past/ − past*, and didn has taken over from the semantically more transparent neva as the negative representative of this new set of tense distinctions.

The significance of the above process is, as Bickerton notes, that "rather than learning new rules and categories and then acquiring the morphological fillers for them, the speaker in a creole continuum characteristically acquires new morphemes and then makes adjustments to his existing rules and categories so as to provide distinctive environments for these morphemes" [16]. This is an important principle, since it provides for lexically driven change.

One should be aware, of course, that there are certain basic differences between second language acquisition and decreolization. The principal difference that needs to be kept in mind regarding the application of Bickerton's principle is that creoles are fully-fledged natural languages, whereas learner languages are not, being significantly less rich in lexis, syntax and semantics. Decreolization, that is, involves change, while acquisition involves evolution. For it to apply to second language learning, Bickerton's principle would require some rewording. Such rewording, however, would not have to be very extensive: where the original passage has "makes adjustments to", the revised version would need "elaborates", or words to that effect.

If we are to apply the principle of lexical levers to second language acquisition, the next task that has to be undertaken is to describe more precisely

what is meant by a term such as "elaborates".

2.1.10 Where Does the Second Language Learner Begin?

Any discussion of elaboration implicitly assumes that there is something to elaborate. Defining what this "something" is and explaining how it came into being is in fact one of the major problems facing non-contrastive theories of second language acquisition. Thus, before we can make any explicit claims about elaboration it is necessary to present some sort of hypothesis about what the second language learner's point of departure is when faced with the task of learning a second language, and of how this point of departure itself was arrived at.

The Notion of Simplification

As has been noted many times, one obvious feature of learner language is its apparently "simplified" or "reduced" nature.

Many characterizations of simplification have been provided [89]. The one that follows (based largely on Meisel's description [126]) is typical, but by no means exhaustive. Simplification involves such phenomena as:

1. Deletion of elements—for instance, pronouns, the copula or other verbs, articles, prepositions, etc.
2. Generalization of the meaning of lexical elements—for instance, using the one word for car, truck and bus; extending very to mean much.
3. Reduction or elimination of morphology—for instance, verb markers, or plural and possessive markers.
4. Use of semantically less marked lexical material—for instance, must rather than should, extensive use of verbs such as do and go.
5. Use of separate lexical elements such as time adverbs rather than reliance on verb marking.
6. Tendency to avoid movement rules such as inversion or dative movement.
7. Extensive use of paraphrase to avoid complex expressions or to fill in lexical gaps.
8. Use of decomposed predicates—for instance, my son drive go school in place of causatives.
9. Avoidance of complex sentences involving embedding by recourse to co-ordinate structures or simple juxtaposition.

The principle of simplification plays a large part in many of the theories which have been proposed to account for second language learning. Thus in the seven "process models" of language learning documented in Richards' summary of research in error analysis (he notes that these models do not strictly fall within that category of research), namely "the Recreation Hypothesis (Traugott, Pit Corder), the Developmental Hypothesis (Dulay and Burt 1974), the Regression Hypothesis (McLaughlin 1975), the Complexification Hypothesis (Faerch 1978, Pit Corder 1979), the Recapitulation Hypothesis (McLaughlin 1978), the Decomposition Hypothesis (Wode 1979), and the Pidginization Hypothesis (Schumann 1978)", the concept of simplification is implicit in some form or other in at least five of these models [150].

The idea of simplification is also important in more recent theories, such as the *ZISA* group's multi-dimensional model. In an apparent attempt to accommodate the notion of simplification to this model, one of the group members, Meisel has suggested that there may be at least two types of simplification. One form of simplification is, he argues, "restrictive simplification", a sometimes conscious process whereby a grammatical system is pared down for "the purpose of achieving an optimal result in communication while reducing the grammar in a way which makes it easy to handle" [126]. Restrictive simplification characterizes the "early stages of all kinds of second language acquisition", and may persist in some learners who are psychologically and socially distanced from speakers of the target language. The other kind of simplification is elaborative simplification, which is characterized by such phenomena as overgeneralization, and represents a productive "extension of the earlier system and a step toward the target variety" [126]. Although the motivation for this distinction is clear enough, given the model of second language acquisition developed by the *ZISA* group, the notion of "elaborative simplification" seems somewhat baroque. Independently of the *ZISA* group, Stauble has suggested a third kind of simplification, "conformative simplification", which she characterizes as the elimination of nonstandard features by the integrating learner [172]. This last definition seems to be very definitely straining the ordinary language sense of simplification, if not other senses as well.

These adumbrations of the notion of simplification emphasize what were in fact problems already inherent in the concept.

First, while the notion of simplification itself may appear intuitively obvious, it is in fact quite difficult to formalize. Meisel notes that there are "a number of criteria for simplicity which apparently cannot be tied together into just one definition" [126]. He provides the following catalogue of features:

1. Simplification of surface structure, e.g. fewer elements occurring. This would include deletion of morphological information.

2. Derivational simplification, calculating the number of rules and pos-

sibly also taking into account the kind of rules applied. This covers cases of rule generalization, i.e. the scope of application may be wider, fewer elements may be mentioned in the structural description of the rule.

3. Simplification of underlying structure, e.g. fewer constituents being introduced by the Phrase Structure Grammar.

4. Psychological simplification computed on the basis of processing time, memory span, number of errors, etc.

5. Perceptual simplification, facilitating the process of decoding an utterance, e.g. by non-violation of perceptual strategies.

As Meisel notes, the relation between psychological simplification and syntactic simplification is not clear, and in syntactic simplification the production of a simplified surface structure may actually involve the application of additional rules, while alternatively additional elements in a surface structure "may result in more explicit and therefore simpler constructions" [126].

This, then, is one very serious problem with the concept of simplicity—it appears to include processes operating at quite different levels of cognition, processes which moreover may very well be at odds with each other.

One obvious point about the conglomerate of features presented above is that it does not constitute a psychologically real entity. Surface structures can probably be said to be psychologically real, but to a greater or lesser degree the psychological reality of other elements or processes in the above description is questionable.

The first major problem with the notion of simplicity can therefore be characterized in this way: we talk in terms of processes but in fact we do not have any definite model of the processor itself.

One essential component of such a processor would be an arbiter of some kind to determine which of two or more conflicting principles would gain priority in a given situation. Without such an arbiter we will not be able to formally relate different kinds of simplification.

The second problem with simplification is that it is a projected term—a term applied from the point of view of an outsider. What is simplified to a native speaker is not, as Pit Corder points out, necessarily simplified from the point of view of a non-native speaker or learner. If simplification is an intuitively obvious notion it is because we have our own simplified registers. Unfortunately, judging learner language in terms of one's own notions of simplicity may be as unfruitful as judging learner language in terms of any other aspects of native speaker competence. Later additions to the repertoire of simplifying processes, such as conformative simplification, lack the virtue of intuitive appeal without being any the less projective.

Various proposals have been made to overcome the problems outlined above. Unfortunately, all are somewhat speculative.

Pit Corder suggests that we turn the process of simplification "upside down and treat 'standard' (i.e. complex) codes as 'complicated' forms of a 'basic' simple language, and then hypothesize that there are some rather general processes of 'complication', i.e. language learning" [146]. An adjunct to this proposal is that "no approximative system developed in the learning of any language is 'obliterated' but remains available both for special communicative functions in the mother tongue and as an 'initial' hypothesis in the learning of second languages" [146].

This proposal is interesting, although vague. It was made, of course, at a time when concrete descriptions of learner language were few and far between, and the similarities between simplified or reduced registers—such as pidgins, foreigner talk, and "interlanguage"—were more obvious than their differences. While Corder's theory is primarily advanced to overcome the terminological problems associated with "simplification" and to provide a description of where it is that a second language learner starts from, it is in fact quite a convenient one from the point of view of an investigator of second language acquisition, since its assumption that earlier stages of language learning in the individual are never obliterated implies that native speaker intuitions can serve as a guide to the "basic" form of the native speaker's language. In this sense, it is less radical than it initially seems to be.

In a subsequent paper, *Language-Learner Language*, Corder, in an effort to accommodate transfer phenomena into his theory, modifies his basic idea by suggesting that the learner's starting point may vary according to his perception of the distance between the target language and his own. If this distance is perceived as great, then the learner will opt for a "recreative strategy" of the kind described above. On the other hand, if the distance is perceived as relatively minimal, then the learner may opt for a "restructuring" strategy, for which the starting point is considerably less reduced [44].

While Corder's assumptions that "simple codes" reflect in some more direct sense the underlying structure or "inner form" of languages seem highly plausible, it has to be pointed out that there are in fact many features in learner language which are quite bizarre from the point of native speaker intuitions. Or, to put it a different way, learner talk and foreigner talk differ in quite radical ways: it is unlikely that any native speaker of English has ever marked verbs with *-ing* in the way that learners regularly do, for instance. A native speaker may be able to work his way back along the learning continuum to some degree, but this is only part of the story. There is accumulating evidence that language learning involves other activities apart from what Corder terms "complication"—for example, the violation of certain constraints of speech processing [34]. Once successfully violated, these constraints are meaningless to the learner. It is unlikely, therefore, that

they can ever be reconstructed by linguistic "backtracking". In some senses, then, learning may be a one-way process. We could liken this situation of the native speaker in regard to his native language to that of a person who has solved one of those visual puzzles in which there are, say, the features of a face hidden in the foliage of a tree: once the face is perceived it cannot be unperceived. Certain aspects of the language learning process seem to have this puzzle-like nature—once solved, there simply is no puzzle.

Reductionist hypotheses of the type advanced by Corder are appealing in some senses. Their assumption that the learner's point of departure can be explained in terms of a retreat to a universal core grammar, however, is questionable. In assuming that learning can be largely understood in terms of complexification and ignoring the problem-solving aspects of the learning process reductionist theories will in all probability fail to provide an adequate characterization of the situation confronting the learner, and, consequently of his behaviour.

To provide anything like an adequate description of a starting point for the learner we need, as already observed, a specific model of a processor, and this we do not have. In its absence, we can only assume, with Corder, that the learner seeks to organize his language according to certain universal principles, and that the precise form this organization will take must be to some extent determined by the target language itself, and possibly its relationship or perceived relationship with the learner's first language. However, we will seek to supplement this attempt to characterize the learner's point of departure by assuming that he is in fact constrained in certain quite compelling ways. We will try to examine the effect of these constraints on the ways in which the learner's system can develop, and the reorganizations effected when these constraints are violated.

2.2 Optimization of Form and Function

In the previous section we enumerated some superficial examples of "simplification" and discussed some of the problems with the concept; ultimately we opted for a model similar to that proposed in so-called reductionist theories of second language learning, in which rather than "simplifying" the learner falls back on some sort of possibly universal core grammar. We have also indicated an intention to augment this perspective on the learner's point of departure by trying to take into account the fact that his grammatical organization and learning behaviour is affected by certain constraints—for instance, of a semantic or speech processing kind. That is, the learner's "basic" system, his starting point, cannot be explained only in terms of linguistic universals—these must be augmented by the introduction of constraining principles which affect the learner's way of organizing his basic grammar and processing his basic input. Equally, the learner's movement

toward the target—his learning—cannot be explained or understood simply in terms of elaboration or "complication": it needs to be interpreted also as a problem-solving process, in the course of which certain initially useful restrictions on the learner's system are discarded and become irrecoverable.

One important characteristic of "reduced" or "basic" systems is the way in which they preserve or attempt to preserve a largely invariant relation between functional (communicative) and structural (grammatical) aspects. Consider the following two descriptions:

> Invariance in form; rather than allomorphic variation; invariant relation between form and grammatical function, rather than derivational and inflectional declensional and conjugational variation; largely monomorphemic words, rather than inflected and derived words; reliance on overt word order
> (Hymes, in G. Sankoff [156]).

and

> simplified codes or languages exhibit...a simple or virtually non-existent morphological system, a more-or-less fixed word order, a simple personal pronoun system, a small number of grammatical function words and grammatical categories, little or no use of the copula, absence of an article system (less often the absence of deictic words). The semantic functions of these and other systematic systems such as tense and aspect are typically performed, when at all by lexical means, e.g. adverbs, or some 'imperial form'. The basic syntactic relations are expressed by word order (Corder [146]).

As Corder remarks, there is a "general belief that 'simple codes' are 'nearer', in some sense to the underlying structure or 'inner form' of all languages, i.e. more overtly reflect semantic categories and relations" [146]. Similarly, Slobin has suggested that "there is a tendency for Language to strive to maintain a one-to-one mapping between underlying semantic structures and surface forms" [117].

The general explanation for invariant relationships between form and function is presented in communicative terms. Thus, Hymes concludes his description of the phenomena remarked on above with the assertion that "they minimize the knowledge a speaker must have, and the speed with which he must decode, to know what in fact has grammatically happened" [156], Corder comments that the features in his description "can be seen in 'information' theory terms as being minimally redundant" [146], and Slobin argues that the tendency towards invariance exists "with the goal of making messages easily retrievable for listeners" [117].

While the communicative aspect is clearly an important one, we should remember that much of the work done on simplified registers was done on pidginization, where the orientation was sociolinguistic and communication-based rather than psycholinguistic and centred on the learning process. It is worth considering, therefore, that the principle of unitary identification of syntactic form and semantic function may in fact constitute an important organizational principle of early learner grammars. Ease of retrievability could very probably be more than a process phenomenon—a principle to facilitate encoding and decoding: it might in fact be a principle which helps to determine the architecture of the processor itself.

Thus, in a reduced system like an early learner grammar there would be very obvious economies in maintaining a system of direct addressing—one-to-one mappings—for forms and functions. For systems up to a certain point of complexity preserving an invariance between form and function may be the optimal means of organization for a processor which must be functional from the very outset. There will also be a cost, of course, in such a form of organization: expressive capacity will be minimal and reliance on extra-linguistic context will in general be greater.

There is in fact evidence that the preservation of invariance between form and function is a processor—as opposed to process—feature. This is provided by indications that the principle functions as a constraint on learnability. That is, as a consequence of maintaining one-to-one mappings it appears to be the case that learner systems exclude or reschedule apparently learnable items simply on the grounds that they are redundant. If the form-function principle were merely a principle of information transfer we would not expect this to happen. Evidence of various kinds will be presented in Chapter Four in support of this contention.

Reorganization—Systematic Features

If the form-function principle represents an optimal way of organizing the learner's system at the outset, it is clear that the economies of one-to-one mapping or direct addressing will decline as the number of forms and functions that have to be indexed grows. We would therefore expect that at some point in the learner's development a restructuring of the system would have to take place. The learner will, as it were, have to develop some more efficient filing system.

While it must be emphasized that the following proposals are very speculative, there are some possibilities to consider. One such possible system would involve an increasing tendency in the learner's system towards the hierarchical organization of information—the growth of semantic trees. In a system of this sort simple mapping or direct addressing would be replaced by indirect addressing: functions to be indexed would be addressed and located through an increasingly particular series of sub-addresses—a pro-

cess of proceeding from the general to the particular. As we will see, this postulated process of addressing and organization reflects the way in which acquisitional processes themselves appear to proceed: this is, of course, no guarantee of appropriateness but it does provide at least a processor model with some psychological reality, in that information accessing reflects the original process whereby this information was acquired and indexed.

In terms of observable phenomena, evidence of violations of the form-function constraint and the breakdown of simple mapping would consist of the appearance of previously suppressed forms and the appearance of systematic features—sets and paradigms. Sets and paradigms enable volumes of information to be accessed more efficiently, but, of course, cannot be easily supported by a processor relying on simple mapping, which has to treat them as lists. In a hierarchical system of indexing, however, paradigms are supported in quite a different way: they are accessed through semantic trees. Every item in the paradigm has allocated to it a node in the underlying tree. The hierarchical organization of these trees ensures that each item is directly accessible, defined in relation to other items, and allocated to a particular level of semantic discrimination.

The above model is clearly a speculative one, although, as mentioned, there is at least some evidence in the process of acquisition for a processor operating in the manner described. This in turn confers some psychological reality on the model.

It is also worth pointing out that the process described above, where a learner begins with an optimizing system involving simple mapping and as the system grows more unwieldy implements other principles of organization, parallels the phases of restrictive and elaborative simplification described by Meisel, or by other researchers working along similar lines, such as Schumann. The model presented is not in conflict with theirs, which are sociologically rather than psychologically oriented: its relationship to these other models is essentially complementary. In attempting to account for phenomena described under the rubric of "simplification" in cognitive terms and from a learner viewpoint it does, however, avoid some of the problems with concepts such as simplicity which were discussed above. Even if such a model should prove to be inadequate, the effort to deal with such phenomena by attempting to construct a psychologically real model of the learner system would seem to be worth persisting with.

2.2.1 Learning Mechanisms—The Process of Refinement

Having attempted to define some of the parameters of the learner's point of departure we are now in a position to consider what means he uses to move in the direction of his goal.

The most important single point to be made about the learning process is that rather than being a serial or accumulative process it is a process

involving discrimination or particularization.

This is a process which has been extensively documented in child language learning. For instance, on the phonetic level, children learn sounds according to certain fixed patterns. The first vowel normally acquired is */a/*, a low back vowel, the second is */i/*, a high front vowel. The first systematic distinction acquired by children is an optimal one. The next vowel to be acquired is generally */u/*, which is high, like */i/*, but back. At this point in his or her development the child has a phonological system which makes use of the features high and low, front and back. Having established these distinctions the child goes on to refine his phonological system by the addition of other vowels which occupy less optimal points in the system—such as */e/* and */o/*—and other features—such as roundedness or nasalization. A similar sequence of refinement operates for the acquisition of consonants. In the acquisition of lexical items, the child follows a similar process. For instance, dus may be used initially to refer only to fruit juice given in a highchair, then subsequently to refer to any liquid at all. Following the acquisition of words for, say, milk and water, dus will be constrained in meaning to refer only to fruit juice. Of particular interest here is the totally contextualized way in which the word first appears [56]. Contextualized language appears to be an important source of new material in second language acquisition as well.

2.2.2 Refining Processes in Adults

Open-Ended Elaboration—The Pronominal System

Despite the evident differences in cognitive development between children and adults the same processes can be observed in adult second language acquisition. Thus, in the acquisition of pronouns, adults appear to begin with a *+speaker/ − speaker* distinction. This gives them proforms for the first person (*I/me/I'm*) and a proform for all that is not first person, generally you. It is not long before the second category of -speaker is further broken down into the more target-like categories of second person and third person. This means that a new form enters the system—generally he and/or she. These two forms are not consistently used to mark gender, however, and this distinction may, in fact, take a very long time to become stabilized (see 4.17.2 for examples of this problem). Within the three basic person categories, other distinctions emerge gradually. Thus, within the category of first person, the distinction between subject form and object form has to emerge, as does the distinction between singular and plural. These processes are accompanied by a good deal of variability, which includes not producing proforms in obligatory contexts at all in some cases, and regressions to earlier forms of the learner's system (for instance, use of you where he is obviously meant). Nevertheless, there is a certain directionality evident in

the whole process. Certain forms, it can be said, are acquired before others: thus we would expect a speaker who uses third person pronouns to have both first and second person forms, or a speaker who uses them to use he.

All this is explicable in terms of the learner's developing a set of interacting hierarchies for pronoun use. Thus, there is the basic person hierarchy. Within this hierarchy, there are further distinctions to be made—distinctions of number, gender, and case. In the learning process, some of these distinctions are made before others. The structure of the real world and the structure of the human brain interact in such a way as to establish a series of priorities for the learning of pronouns. Some of the priorities so established are quite forcibly the way they are: thus we cannot learn number and gender distinctions in the third person if we have not developed any functional third person category. Other priorities are somewhat more pragmatically determined—they depend on the kinds of discourse situations we normally find ourselves in or create for ourselves: thus an unmarried learner with no family or group ties might find we and us largely avoidable, and a learner with minimal language is unlikely to produce many third person pronouns even if he knows these forms.

A combination of factors, therefore—some "hard" and some "soft"—determines the character of the basic pronoun hierarchy. We can think of this hierarchy as a tree. The first fork in this tree represents the basic division into me and not me. Obviously, after this initial branching the tree can grow in somewhat differing ways, but there will be a good deal of similarity between the trees for different learners. The important thing about this tree is that (a) it needs time to grow, and (b) that while there is some scope for individuality in its form there are certain ways in which it cannot grow: the smaller branches must spring from the larger ones.

The pronoun tree is a typical case of a semantic hierarchy, and the growth of the tree is representative of the way learning proceeds by refinement and discrimination. The pronouns of English or any other language can be grouped together into lists but this is not the way they are represented in the minds of speakers who use them or must learn them.

The kind of refinement that is represented by the learning of the pronominal system is referred to in the title of this section as "open-ended elaboration". This is because in theory the process of discrimination which has been described could go on more or less indefinitely: there is no principled limit to the number of pronoun categories that could exist. In practice, of course, learning will cease at the latest when there is an apparently adequately configured tree for the pronouns of English (a language which is, incidentally, not especially rich in this regard). Open-ended elaboration is typically the process we encounter when a semantic complex has to be mastered. The next type of learning we will look at also involves analysis and discrimination, but in the case the process is not one of open-ended elaboration brought to a halt by (apparent) coincidence with the target model.

2.2.3 Decompositional Analysis—The History of a Formula

There are many examples of decompositional analysis to be found in second language acquisition. Perhaps the classic case, and certainly the most well documented, concerns the acquisition of the auxiliary do through negative structures. This is in fact described in Chapter Four, but is worth recapitulating, since it raises several theoretical points which require attention.

Do's and Don't's—The Analysis of a Morpheme

1. The point of entry for do into learner grammars seems to be almost inevitably through the utterance don('t) know. This utterance is produced by all learners at a very early stage and is clearly monomorphemic. It is also worth noting that the semantics of don('t) know are non-standard, in that the phrase is used to indicate incapacity in general rather than just ignorance (some evidence for this phenomenon can be found in Huebner [85]). At the stage when don('t) know is incorporated into the learner's repetoire, the canonical method of verbal negation is with the negative particle no(t).

2. After a period of latency don('t) gradually begins to appear in other phrases besides don('t) know. Certain verbs, possibly for reasons of input frequency, appear to be congenial environments for the appearance of don't. These are like, understand, have, want, think and remember. At this stage don('t) could be classified as an alternative preverbal negator to no(t). Some learners also produce don('t) in postverbal positions for a brief period.

3. This situation in which no(t) and don('t) contend for the preverbal negator slot appears to persist for quite some time, with no(t) gradually being phased out. Even when don('t) has effectively supplanted no(t) as the standard preverbal negator, there is little evidence that it is anything other than monomorphemic.

4. The analysis of don('t) into do and not itself appears to be a very gradual process. Factors which appear to facilitate this process are the use of do in questions, the acquisition of other *-n't* negators, such as can't and haven('t), and possibly an increasingly clearer perception of the phonological shape of don't. These processes may in turn be evidence that a general reorganization of the learner's system of the type discussed in 2.2 is underway.

This process is significant for a number of reasons. First, it is an illustration of the "lexical lever" principle enunciated by Bickerton. The essence of this principle is, as indicated above, that forms can enter a system before

the functions that they ultimately come to index. Don't enters the system as a semantically vacuous item. In the rough-house of linguistic usage, as it were, it is gradually shaken free from its formulaic matrix and passed through successive functional reinterpretations. Bickerton's lexical levers led the creole speakers he studied to make adjustments to their system of tense and aspect in the direction of Standard English. Lexical levers of the kind discussed here result, in conjunction with other features, in an elaboration of the learner's grammar.

Second, it indicates that formulaic language can serve as what we might call the "seedbed" of propositional language. Here too the mechanisms used by Bickerton and Bailey in the study of creoles and language change, and by those researchers mentioned in the section on implicational scaling, provide a means of understanding and describing the propagation of rules in an evolving linguistic system. While it may still be necessary to use terms like "formula" in some kinds of linguistic discussion, the way in which a morpheme like don't is reanalyzed by application of the rules for its production in a widening range of verbal environments makes it clear that the category of formula, like many other linguistic categories is, in Ross's term, a squish [134]. There is no hard and fast division between formulas and "creative" language use, and the drawing of artificial boundaries (as in morpheme order studies) simply obscures attempts to understand the nature of learning. We will see many examples of lexically driven learning in Chapter Four; one of the most convoluted and creative uses of formulaic material, for those who are interested in this phenomenon, can be found in the emergence of the indefinite article from the isolated unit a little in the speech of two Polish informants—see 4.13.3.

'Creative' and 'Formulaic' Language

The status of formulaic language has been a matter of contention in second language acquisition, and the debate that has been generated is worth a brief comment.

The substance of this debate is how crucial to the process of acquisition are what have been termed "formulaic utterances" (Wong-Fillmore) or "prefabricated patterns and routines" (Hakuta). Wong-Fillmore argues that the segmentation of formulaic material is essential to the learning process [187]. Huebner concurs, with some riders about child/adult differences [85]. Hakuta distinguishes between "routines", which are monomorphemic, and "patterns" which contain substitution slots but comes to no conclusion as to their role in facilitating or hindering acquisition [157]. Krashen and Scarcella, on the other hand, argue that "automatic speech is neurologically different from creative language" [157], and that the two do not interact in learning, while Peters, working from within a similar framework to Krashen and Scarcella, and adducing conflicting evidence from first

language acquisition studies, argues that different types of learners may take either a *gestalt* approach or an "analytic" approach and that both kinds of language may play a part in learning [136].

Leaving aside the question of the validity of the neurological evidence cited, it should be pointed out that the participants in this debate seem to have forgotten that formal grammars cannot automatically claim to be psychologically real. Chomsky himself has pointed this out repeatedly [29]. He notes, for example, in a discussion of a phonological rule that the production of an utterance in which the rule figures does not imply that the rule has been applied in that particular case, merely that it could be applied [32]. The fact that a speaker can parse an utterance does not in fact mean that each time the utterance is produced it is so parsed: in fact, it would seem plausible that a good deal of native speaker linguistic behaviour is quite as routinized as the "formulaic" language of learners.

The crucial question from the point of view of understanding rule application is not whether a particular speech token is automatic or otherwise, but whether there is evidence that it can be accounted for by formal rules. As far as learner language is concerned, wave theory, implicational scaling and the principles of lexical levers provide powerful tools for providing formal descriptions of evolving learner grammars. The relationship between formal descriptions of language and the actual processes of production and comprehension is, of course, of vital importance, and studies of psychological phenomena such as speech processing provide some sort of nexus here, but the fact is that at this stage we have very little concrete knowledge about how the psychological aspects of production and comprehension relate to the formal features of grammars [76]. Confusing the two things in debates of the kind created by Krashen and Scarcella is not likely to add to our knowledge of such interactions.

2.2.4 Elaboration and Discrimination—Summary

We have discussed two related forms of linguistic development—semantic elaboration and syntactic discrimination. These two processes represent essentially the same principle—that of refinement—applied to two different types of material. The model that results is one that can be semantically or lexically driven. The impetus or driving force will itself vary, but can be assumed to have a strong psycho-sociological component. Under the influence of the appropriate motivational forces, learning can manifest itself in either the semantic or the syntactic component. Semantically based learning is characterized by extensions to the learner's system such as those represented by the acquisition of a pronominal system. Lexically driven learning is characterized by the acquisition of do by a process of analysis and decomposition.

It is not assumed that either process has primacy. Functions can be

acquired simply because they are needed (provided the conditions for their acquisition are met), or alternatively analytical processes can throw up forms which must then be indexed to hypothesized functions. Very often, these two processes will interact.

The above account of learning excludes many important factors. Speech processing theory obviously has a good deal to tell us about the situation of the learner that reductionist hypotheses cannot account for; Huebner's work has demonstrated that discourse notions are essential to fully understanding some, if not all, aspects of syntax [85]; systematic attempts to classify language learning strategies are evidently another important area of research [125].

The foregoing treatment will, however, hopefully serve to provide a reasonably accommodating framework for the analysis that follows, and for the conclusions that can be drawn from that analysis.

2.2.5 The Question of Interference/Transfer

Finally, we come to the question of interference of transfer. The orphaned status of this question has already been referred to. It is therefore perhaps appropriate that we leave it to last, since in this study, as elsewhere, it is not part of an integrated theory.

In his summary of error analysis Richards refers to several different recent views of language transfer. It should be emphasized that these differ from the contrastive analysis hypothesis in not seeking to attribute all errors to language transfer or explain the language learning process in contrastive terms. Richards provides the following summary [150]:

1. Specificity hypothesis: Kellerman found that learners perceive some aspects of their native language as language specific and others as universal, and resist transferring items such as idioms, which are felt to be L1 specific and non-transferable, even where the native and target languages share the same idioms.

2. Cognitive strategy hypothesis: McLaughlin (1978) suggests that interference can be considered not as evidence of transfer in the behaviorist sense, but as the result of a cognitive strategy whereby the learner makes use of L1 rules as a source of information to work out the rules of the target language.

3. Structured interference hypothesis: Wode (1979) argues that learners draw on their first language only at specific points in their second language development, under specific structural conditions, and has conducted a variety of empirical studies of phonological and grammatical development to support this hypothesis.

4. Proficiency hypothesis: Taylor (1975) proposes that learners rely on language transfer at the elementary stages of second language learning but make use of overgeneralization as they become more proficient in the target language.

5. Context hypothesis: Ervin-Tripp suggests that language transfer is greater where the target language is learned in environments such as foreign language classrooms.

The specificity hypothesis seems quite plausible, although rather trivial, in that idioms are related to lexical items [42], and learners are not given to whole-scale transfer of lexical items anyway. The question of what is universal is of course a vexed matter. There is no point in suggesting that what has apparently been transferred is seen as universal unless there is some way of defining this first. Intuitive characterizations of universality may be appealing, but lead to precisely the problems which have been discussed for intuitive notions of simplicity.

The cognitive strategy hypothesis addresses itself to the question of what level of abstraction transfer can take place on. This is an interesting question. There is some evidence that initial word order hypotheses might be formulated in this way [99]. The proficiency hypothesis seems to be formulated in very vague terms. It does not address itself to the question of whether different kinds of transfer may occur at different levels: for instance, Schachter has claimed that avoidance of relative clause structures is characteristic of learners whose languages have different rules for these (e.g. Turkish, Farsi, Japanese) [158]. Contrary to Taylor's hypothesis, relative clause formation is an activity of reasonably proficient learners. In the present study also, there is evidence of transfer phenomena in the speech of relatively proficient learners, both of the avoidance kind described by Schachter and of the direct variety.

The context hypothesis is supported to some extent by the findings of Felix, who found that learners in the school classroom would, as it were, try almost anything if forced to produce a structure they did not know [60].

More recently, Zobl has investigated some of the "formal properties that make L2 structures immune or receptive to L1 influence". One of his conclusions is that "the receiving language (i.e., the L2) must contain certain systemic biases and structural tendencies in order to render it susceptible to influence from a near-congruent L1" [188]. In addition, he concurs with Wode that transfer has a "developmental aspect"—that is, that learners must attain a certain level of development with respect to an L2 structure before transfer is activated" [188]. This approach is certainly interesting, although some phenomena discussed by Zobl could be explained by reference to other principles (compare, for instance, his discussion of differential inversion patterns for nominal and pronominal subjects in Yes/No questions for French speaking learners of English with the hypothesis concerning

transfer on the morphological level put forward below). In addition, the principles of "congruence" invoked by Zobl do not always appear to function uninhibited—an example of this is the relatively late acquisition of inversion in German by speakers of Spanish and Italian, despite structural similarities in this regard between all the languages concerned [36].

Working from another viewpoint Meisel has attempted to provide a psychological model for the conditions which favour or disfavour the use of transfer as a learning and communication strategy. This approach is part of an integrated attempt to provide a predictive model for strategies of various kinds, and represents a very necessary attempt to incorporate the notion of transfer into a wider theoretical perspective [127].

Despite a renewal of interest in transfer phenomena and a range of approaches to the question of transfer the predictive capacity of the proposals made to date is, as with many other aspects of second language acquisition theory, quite limited. This is somewhat ironic, given the strong claims made by the first transfer hypotheses—those of contrastive analysis.

The present study's contribution to the vexed question of transfer is a tentative one. As far as the author is aware, none of the theories tabled above consider the question of whether transfer might operate to different degrees on different levels of language. This seems to be an obvious question to ask if we consider the fact that there is fairly clearly a considerable degree of transfer on the phonetic level of a language. If we take languages to be built up of units of differing sizes, with the phones of a language being the smallest units, followed by morphemes, then major categories, and so on, we could then formulate the question of transfer in terms of these different levels of unit size. Given that transfer at the phonetic level clearly occurs, is the next most susceptible level the morphemic one? In previous work done by the author on the learning of English by Spanish and Turkish speakers, he found that there did indeed appear to be considerably more interference on the morphemic level than, say, the level of word order [92]. This was the case even though Turkish and Spanish differed radically both on the morphemic and the syntactic level. For instance, Turkish does not have a definite article or prepositions, and Turkish speakers exhibited a higher degree of non-production of these elements than Spanish speakers at, say, the equivalent stage of development in negation. On the other hand, while Turkish had a quite different system of negation and canonical word order to either English or Spanish, Turkish speakers exhibited no evidence of transfer whatsoever in their learning of the rules of negation, and fairly sparse and transitory evidence of Turkish word order patterns (in which the verb is almost invariably final) in their English word order. A plausible reason for this differential behaviour is that the smaller elements of a language—the phonemes and morphemes—are not (or not readily) mapped onto basic and possibly universal semantic structures: there is a higher degree of arbitrariness in them than in more complex structures. We will pursue this

hypothesis further in the forthcoming analysis, where there appears to be evidence in support of it.

Another, perhaps not unrelated possibility, is that transfer will act differentially on those items which are classified as developmental and those which are classified as variational in the *ZISA* multi-dimensional model. Thus if there are strong cognitive processing constraints on developmental features we would expect them to be relatively immune to transfer effects (indeed, Clahsen, Meisel and Pienemann draw attention to the fact that inversion is not the first of their word order rules to be learned by Spanish and Italian speakers despite its occurrence in some of the same contexts as in German in these languages) [128]. On the other hand, if variational features are not constrained in the same way as developmental ones then we might expect them to be more vulnerable to pressures such as those emanating from the first language. Once again, we shall see that there is some support for this hypothesis.

2.3 Some Other Current Theoretical Approaches

2.3.1 Input and Interaction Studies

Input and interaction studies have what could be termed an "oblique" relationship to the central themes of this thesis. This is so because the present study is concerned with processes and causes interior to the learner, while input and interaction research deals in the main with exterior phenomena and factors, with the exception of work like Givon's [168]. Therefore it would be fair to say that the two strands of research are in a complementary relation; there is no reason why this relationship should be competitive in any way.

Historically, it has been the case that a good deal of input and interaction research has treated the learner as a kind of "black box" in terms of how external phenomena and properties of language impinge on the workings of the learner's mind. In fact, there is no principled reason for why the goals of the two types of research should be mutually exclusive. The link-up point—which remains to be identified—is the following: is it the case that conversational imperatives lead to the acquisition and production of new syntactic structures? In other words, what remains to be demonstrated in detail is the precise relationship between phenomena on the sentential level and those on the super-sentential level: to date there are few extensive findings of a non-obvious nature in this domain, with exceptions like the work of Tomlin [176]. This said, Gass's [66] observation that research on the facilitating role of modified input on comprehension does not show how the latter results in *acquisition* is still a correct assessment of the state of play.

2.3.2 The Contribution of Interaction Studies

Input and interaction studies have, despite the oblique relation referred to above, contributed certain valuable findings about the learner's linguistic environment. A particularly notable case of this is the disconfirmation of Chomsky's assertion that the input learners are exposed to is "degenerate" due to hesitations, changes of course, run-on sentences, slips of the tongue and the like on the part of native speakers as providers of this input. As Larsen-Freeman and Long mention in respect of distorted input, while there is documentation of this in "foreigner talk", and elicited versions of the same, "it has become clear that deviant input is not the norm in SLA" [110, 112ff.]. This is also true of the first language counterpart of foreigner talk, namely "motherese" or "caretaker speech" [170]. As Larsen-Freeman and Long put it, the results of numerous studies conducted after Ferguson's [110] pioneering investigation of "foreigner talk" was that "The input described by these researchers was almost wholly *well formed*, although ... it constituted ... a *modified* version of the target".

"Foreigner talk" of the *grammatical* variety is more regular and restricted than the kind of speech native speakers use amongst themselves [110, 119ff.]. The main finding for grammatical input is that native speakers "employ a more restricted range of vocabulary in speech to non-natives, as measured by type-token ratio (Arthur, *et al.* 1980 [8]), with idiomatic expressions ...occurring less often (Henzl 1973, 1975, 1979 [81])". It is worth bearing in mind, however, that the devices employed by native speakers in foreigner talk also occur in discourse between native speakers, and that the the phonological, morphological, semantic and conversational adjustments to be observed in the two types of discourse are of a *quantitative* rather than a *qualitative* nature [110, p. 126].

In addition, despite the aforementioned lack of a direct nexus between grammatical operations at sentence level and interactional patterns on the levels above, input and interaction studies, in posing the question of whether the linguistic environment makes a difference [110, p. 128], have supplied a good deal of information on the context of acquisition. If we ignore vacuous assertions like Krashen's "Comprehensible Input" hypothesis, for which there is not a single shred of solid evidence, nor ever likely to be, since the term itself is undefined (general practice for this language ideologue) [71], then there are some interesting proposals about the role of conversation in developing syntax [110, 130ff.]. We can await further developments in this area.

Input and interaction studies have also investigated the question of frequency in input of various morpho-syntactic forms and their frequency of occurrence in production [110, 133–4]. The basic conclusion is that, while there is some evidence of a correlation for the two, this is not always the case—with some high-frequency items (e.g. articles) being acquired (or mas-

tered) quite late. Furthermore, the correlations discussed are ("fairly weak") Spearman rank orders. On the basis of the foregoing, there is little evidence to suggest that frequency of an item in input is a predictor of either its early acquisition or the accuracy with which it is used when it is produced.

We have mentioned one type of input and interaction theory which does also consider interior phenomena. These are interactionist theories of second language acquisition such as that of Givon [168]. To quote Larsen-Freeman and Long:

> Interactionist theories are more powerful, all other things being equal, than either nativist or environmentalist theories, because they invoke both innate and environmental factors to explain language learning.

Larsen-Freeman and Long [110, p. 266] note that:

> Greater power, it should be remembered, is a negative characteristic where theories are concerned, meaning that more factors, variables, causes, processes, etc., are needed by the researcher to handle the data of interest. Power, that is, here contrasts with a desirable attribute of theories, parsimony.

The interactionist theory Larsen-Freeman and Long discuss is Givon's 'functional-typological syntactic analysis' (FTSA) (Givon, 1979a, 1979b, 1981, 1983a, 1983b, 1984, 1985 [168]). Larsen-Freeman and Long note that:

> syntactic change is driven primarily by psycholinguistic and pragmatic principles relating to speech perception and production in face-to-face interaction.

These processes, they note, are assumed to be "derived from more basic ones underlying human perception and information processing". There follows a discussion of the difficulties involved in determining whether one is dealing with, for instance, topic-comment structure or subject-predicate structure, where the only (unusable) clue is a pause in an utterance like:

> *My family... come New York.

Sato's study of two Vietnamese learners of English is used to test Givon's propositions (cf. Larsen-Freeman and Long [110, pp. 267-268]), and the general conclusion is that his predictions are not borne out. Of particular interest is Givon's claim that there will be a higher ratio of verbs to nouns in early interlanguage. In the present data, what one finds is the reverse situation—that is, as development proceeds the percentage of verbs to other categories *increases*. In the initial stages—where it may not be appropriate

to talk in terms of syntactic categories anyway—"verbs" or "action words" are not particularly frequent at all. Sato also notes that Givon tends to assume the prior existence of phenomena—such as syntactic categories—which actually *emerge* in development (Larsen-Freeman and Long [110, p. 269]), and that he has a bias towards the written language and the "sentence", rather than towards oral production and units like "utterance".

2.3.3 Conclusions

To conclude, it has been said of input and interaction research that it frequently suffers from the problem of looking at *exterior* phenomena and causes, and neglecting to consider their *interior* counterparts. For much research in the input and interaction field the learner is simply a "black box": what may be occurring inside of that box is unknown and unquestioned. As noted, there is really no reason for this particular division of labour; it has just happened that way. So what is needed to remedy this problem is some set of proposals about how exterior and interior phenomena and causes may be related, one to another. Researchers of various persuasions *are* looking at this question—the work of Johannes Wagner is a case in point here (Wagner, J. p.c.), and that of Evelyn Hatch [78] another. Time will tell whether these approaches bear fruit.

In conclusion, I would like to emphasize that I do not take issue in any way with input and interaction research; I see it as *complementary* to the kind of research I have been conducting and will continue to pursue: the formation of some nexus between these two classes of investigation is very desirable indeed, and members of the two camps have much to learn from each other. Once again, future developments should be a matter for keen anticipation.

2.3.4 Government and Binding in SLA Research

It is not possible to give an adequate coverage of work on second language acquisition from within the framework of Government and Binding in the space available here. Nor is it my intention to deny that such studies have any value; indeed, studies of second language acquisition conducted on the basis of any kind of theory of language are of much more interest and value than those which are not. As Gregg [72] has remarked, if you want to study how a complex system like language is acquired you need an adequate description of the object in question, as well as a theory which explains its properties.

Bechtel [13] is one philosopher of science who has devoted his attention to paradigms for research into second language acquisition, and he has stipulated a number of essential components for such paradigms.

In Bechtel's terms [13] a theory requires two main components—an account of those properties which characterize the object in question, and an account of the transitions that this object may undergo. For second language acquisition, this means that you need a theory of what constitutes the target grammar (its properties), and a theory of how this grammar is acquired (the transitions it goes through).

Pursuing this line, if the research question is one concerning language acquisition, then it would also be necessary to have a description of the learner's "initial state"—that is, his or her linguistic knowledge at commencement of the learning trajectory. This, in turn, might entail questions relating to the learner's genetic endowment for language specific behaviour.

In addition to information about the learner's initial state, questions concerning language acquisition also need at the very least an "operating definition" of the mechanisms available to the learner for acquisition to take place: in short, a definition—however provisional—of what constitutes learning. One example of a definition of this kind can be found in Wexler and Culicover [179]: in this case the definition is a particularly restrictive one.

Returning to Bechtel's stipulations for an adequate theory, it seems fair to say that Government and Binding is quite strong in respect of the first component (the "property theory"), but rather less so in respect of the second (transitional) component (i.e. its account of second language acquisition is fraught with problems). To wit, Gregg [72], who is himself an adherent of Chomsky's work, claims that a theory of language involves a number of modules, and that Government and Binding only achieves adequacy in its property module.

2.3.5 Government and Binding and Universal Grammar

There are now a large body of studies of second language acquisition which have been conducted within the framework of Chomsky's theory of Government and Binding. These studies are quite diverse in terms of both theoretical assumptions and methods of data collection. (White 1989 [181]) provides an overview of the studies—at least for the 1980's. One thing which emerges from her review is very clear: there is a good deal of conflict (and even contradiction) in the findings of the studies discussed: as White comments elsewhere, (White and Genesee 1993 [182]) "...Results are mixed.". To anticipate the conclusions of the survey which follows, I believe that any reasonable assessment of Government and Binding in its present form would be obliged to conclude that it is a very difficult theory to apply to second language acquisition research, if the aim of that research is to make clear and unambiguous predictions and to obtain clear-cut findings on the basis of these predictions. As White herself says (White 1989, [181, p. 183]):

> Linguistic theory is constantly developing and being revised. As we have seen, this has led to a number of problems for researchers trying to investigate the role of UG in L2 acquisition. For example, changes in theoretical assumptions about the levels of operation of principles of UG affect predictions for the operation of these principles in L2 acquisition. Changes in parameter theory lead to problems in identifying parameters and their presumed effects.

This, to say the very least, is an understatement. Let us consider a number of the studies White discusses and their ramifications, both for Government and Binding theory itself, and for questions relating to second language acquisition research. To facilitate this overview, it is worth enumerating the possible positions a researcher can take in relation to Universal Grammar and second language acquisition (from within the framework of Government and Binding). These are, as outlined by White, (1989 [181, pp. 48–49]) the following.

1. UG is available and works exactly as it does in L1 acquisition.
2. UG is totally unavailable in L2 acquisition.
3. Access to UG is mediated via the L1. There are actually two different versions of this hypothesis:
   (a) UG is inaccessible but any aspects of it available in the L1 can be used in the L2.
   (b) L2 learners initially assume the L1 value of UG parameters, but are still able to tap UG. Hence, they can reset to L2 parameter settings.
4. UG is available but does not work in identical fashion to L1 acquisition.

As White points out, these five positions can be reduced to two main proposals:

1. That "UG in some way or other plays a role in L2 acquisition".
2. That "UG is to all intents and purposes inaccessible".

White calls these proposals the "UG hypothesis" and the "UG-is-dead hypothesis" respectively. Although I believe that this is quite a reasonable reduction—reasonable, indeed, to the point of compulsion—there remain a number of serious problems. Between them, the proposals outlined by White cover all the territory there is to be covered. This in itself is not a problem, but it becomes one when, as they are apt to do, researchers switch

in a *post hoc* way from one position to another. As the truism goes, if you can explain everything, you can explain nothing at all. This, as we will see, is what happens in some of the cases under review.

2.3.6 Some Preliminary Issues

Before we proceed any further there is another question to be addressed. Government and Binding theory is based very heavily on intuitional data from "native speakers" of a particular language. It is preferable if these are monolingual. In Chomsky's view, this is a necessary condition for the development of an adequate theory of Universal Grammar. Hence the following (Chomsky, 1986 [31, p. 17]):

> The language of the hypothesized speech community... is taken to be a 'pure' instance of UG... We exclude, for example, a speech community of uniform speakers, each of whom speaks a mixture of Russian and French... The language of such a community would not be 'pure' in the relevant sense, because it would not represent a single set of choices among the options permitted by UG but rather would include 'contradictory choices' for certain of these options.

As has been pointed out by a number of authors (Birdsong 1989 [18]; Cook 1993 [41]) this definition of native speaker rules out a probable majority of the world's population. Bilingualism (where "bi-" means more than one) is the norm for a great number of people. As Cook (Cook 1993, p. 23 [41]) notes, in the Cameroon the average person may have to speak as many of five languages (two of which are official) in the course of a single day. [3] Indeed Cook (1993 [41, p. 245]) ventures the proposition that "Taking the monolingual's knowledge of language as the basis of linguistics may be as useful as investigating cycling by looking at a man on a monocycle".

2.3.7 The Definition of "Native Speaker"

In addition to the problems of bilingualism for a monolingually based theory, there are other difficulties. For instance, as White (White 1989 [181, p. 39]) points out, citing a paper by Coppetiers (1986 [43]), there are second language learners who have been assessed as having near-native competence in their L2 and who behaved, in respect of the principles of Universal Grammar which were tested, in a very similar manner to the "native speaker" control group, showing an "internalized complex and subtle knowledge not obviously available in the input". (White 1989 [181, p. 59]). In fact, White

[3] They are well-provided with choices in this regard, with the two official languages, four lingua francas, and two-hundred and eighty-five native languages.

herself, in conjunction with Genesee, has conducted an interesting piece of research, which extends Coppetiers' program and which provides convincing evidence that there are non-native speakers of English who are, to all intents and purposes, indistinguishable from their native counterparts [182]. We shall return to this study below.

"How Native is Native?"

In view of the brief review above, we are entitled to ask: "What exactly is a native speaker?" A simple answer would be that a native speaker is a person who acquired a language (or perhaps more than one) as a child. But, in view of what has just been noted, this seems to be an excessively restrictive definition. And even if we accept such a definition, we are left with the problem of how to classify the "native speakers" who did not perform as they should have in the control groups in the many studies available in the literature, or even just the studies reported on by White [181]. [4] A more satisfactory definition might be that a native speaker of a particular language is a person capable of making the same kinds of grammaticality judgements as other "native speakers" of that language, where the primary definition involves acquisition in childhood.

If we accept a definition like the above, we clearly open the door to a wider variety of candidates than any Chomskyan position would allow for. [5] So where is the source of the data for Government and Binding? Does it exist at all? If it does, is it representative enough to constitute a valid source for information about knowledge of a language? These are not new questions; very similar objections were raised by sociolinguists like DeCamp [47] and Labov [107] in the 1970s, when points at issue were the relationship between individual competence and the competence of a community as a whole, and, in general, the necessity for variable rules in the grammars used to represent these different (yet overlapping) manifestations of competence.

There is, then, a potentially fundamental problem with the data on which transformational grammars like Government and Binding are based. Given that over 90% of language use involves spoken production and comprehension, I believe that one is entitled to question how representative of linguistic transactions grammaticality judgements are in the first place.

The objections could continue. For instance, does a poorly-educated and possibly not very intelligent person have the same intuitions as a theoretical linguist about the kinds of structures used by the latter to substantiate a property in his or her theory? Does such a person ever produce these arcane structures? If not, why not? And what does this mean in terms of

[4] This divergent behaviour is particularly evident in judgements made on "that-trace" phenomena.

[5] Given what has been said, it seems that Chomsky's "ideal speech community" is more illusion than idealization.

that person's competence? And so on and so forth.

It is the view of the present author and others (M. Pienemann, p.c.; Johannes Wagner, p.c.; K. Gregg, p.c.) that if there is a difference between native speakers and proficient non-native speakers, it is probably the case that the "native speaker" has more control over more registers in the language in question (provided, of course, that he or she is well-educated). [6] But, in terms of Government and Binding, this is a question of *performance*, and not of competence. So the problem remains. In turn, it gives rise to another question. This is the vexed issue of how to define competence and performance. This question will be addressed later. For the moment, I will say no more than that the distinction is a crucial one for the theoretical framework used by this study, but that the answer will promote the status of performance to a point far beyond anything envisaged by proponents of Government and Binding.

A Pause for Reflection

Leaving, for the moment, the problem of how to define a native speaker let us move on to another issue. It should have already become obvious from the preceding text that there is an implicit equation between Government and Binding and Universal Grammar. Indeed, the terms "UG" and "GB" are often used interchangeably in both presentations and the literature. This equivalence is, however, quite misleading. Government and Binding is not the only theory of language with something to say about Universal Grammar, and it certainly has no patent pending on the term.

2.3.8 Universal Grammar

In order to put "UG" in its proper perspective, it might be helpful to look at some alternative approaches to Universal Grammar. This, of course, calls for a preliminary definition of the object to be investigated. Let us assume, therefore, that Universal Grammar, in any of its possible manifestations is comprised of those properties, processes and entities which are irreducibly a part of natural languages, or "Language" in general.

Approaches to Universal Grammar vary greatly, and perhaps the simplest way to capture this variation is to locate the different positions that exist on a continuum which begins at an exterior or "descriptive" point and gradually becomes more rationalist or "nativist", passing, as it extends, through psychological and cognitive waystations, and terminating with the "strong innate" position of Chomsky and his followers.

The best known exponent of the "descriptive" approach to Universal Grammar is Joseph Greenberg [70]. The line of investigation followed by Greenberg, his colleagues, and his successors like Bernard Comrie [40] and

[6] This, of course, is by no means a *sufficient* definition of "native speaker".

Jack Hawkins [79] involves the collection and collation of large amounts of data from a very wide and diverse range of languages, in order to determine such things as basic declarative word order, whether adjectives precede or follow NPs, whether a particular language employs prepositions or postpositions, whether complements precede or follow heads and so on.

This class of investigation is generally referred to as typological study, and this is a well-established form of research into Universal Grammar (cf. Hawkins 1983 [80]). It is striking that language typology was once rejected by Chomsky as uninteresting [7], but now serves theory-construction in Government and Binding as the basis for—among other things—the so-called "Head-position Parameter" [181].

More formal in nature, and with "weak innate" assumptions is the functionalist approach, most prominently represented in the joint work of Brian McWhinney and Elizabeth Bates [124]. One concern of functional approaches is to examine the mapping of functions to forms, and *vice versa*: an example of this can be found in Huebner's (1980 [85]) longitudinal study of a Hmong speaker learning English in a naturalistic environment. In this study, functions assigned to forms quite closely resembling their target language counterparts appeared to be—particularly at initial stages—quite different in nature: the form isa, for instance, was arguably a topic marker rather than an instance of the copula [85]. [8]

Other examples of functionalism applied to second language acquisition can be found in the work of Scheglov [161], where the concern is to establish a taxonomy of speech acts and the formal devices that characterize them, and, more recently, in the work of Tomlin [176]. Tomlin, in particular, has turned his attention to the process whereby functions come to be expressed in specific forms—with an operationalized definition of the speaker's "attention" which can be used to predict the appearance and collateral timing of particular grammatical structures [176].

Functional explanations also figure in the work of proponents of "interactionist/environmentalist" theories, such as Givon [168], with whom we will deal in a separate section.

Briefly put, what distinguishes functionalism is its implicit assumption that pragmatic forces are what determine linguistic forms: formal operations involving "interior causes" or operating with some degree of autonomy are largely—if not wholly—excluded from the functionalist program. [9] As already stated, functionalism in second language acquisition concentrates on "exterior causes", and tends to minimize the importance of their "interior"

[7] cf. Chomsky 1965, where he writes: "Insofar as attention is restricted to surface structures, the most that can be expected is the discovery of statistical tendencies, such as those presented by Greenberg (1963) [30, p. 118]"

[8] This example is not meant to imply that Huebner is a functionalist in his overall approach to SLA.

[9] The work of Halliday is the most extreme expression of this view.

counterparts; this, however, results in a problem: why is it the case that certain functions are important and others are not? Thus, functionalist approaches tend to leave the door open to questions of a more rationalist nature, namely, why some things matter more to human beings than others. Thus, at some point or other, functionalism has to buy into the debate about how specifically linguistic the characteristic features of language behaviour are.

Moving further along our imaginary continuum, we come to the family of "unification grammars", which—with their claim of "psychological reality"—are located somewhere between the formal and the psychological bands of the "spectrum" (cf. Shieber 1986 [167]). The most prominent member of this family is Lexical-Functional Grammar (LFG), and its principal developers are Joan Bresnan and Robert Kaplan [22]; other members of the unification family include Head-Driven Phrase Structure Grammar [147].

Lexical-Functional Grammar

Given the theoretical framework—Processability Theory—which the present study partially utilizes, and which itself employs LFG in its grammatical component, it might be useful to outline the program of LFG in respect of Universal Grammar.

In essence, this program seeks to explain in formal terms how semantic relations come to be expressed in linguistic forms and structures, and to this end it postulates a formalism with particular conditions, objects, and functions which are quite different from those of Government and Binding.

Among the properties and operations which are postulated to be universal in Lexical-Functional Grammar, grammatical functions like "subject" and "object" are theoretical primitives; they are not defined in structural terms, as in Government and Binding. The notion itself that grammatical relations are primitives has been taken over in LFG from Relational Grammar, which proposed a hierarchy of such relations (beginning with subject) on the basis of studies like Keenan and Comrie's work on the "Accessibility Hierarchy" for relative clauses [100].

Relational Grammar, however, never established a real interface between semantics and its syntactic expression. And it is precisely in this area that LFG contributes to its own version of Universal Grammar. In Lexical-Functional Grammar, F-structures—where the grammatical information necessary to the semantic interpretation of the sentence is compiled—constitute a means for establishing and representing properties, features, and operations which are universal across languages. Thus, F-structures provide a cross-linguistic form for the comparison of languages which are typologically quite different (see, for instance Bresnan (1993 [23]) on locative inversion, where parallels are drawn between English and Chichewa).

Another more explicitly psychological and cognitive grammar is Kempen

and Hoenkamp's "Incremental Processing Grammar" (IPG) [90]. IPG is similar in many respects to Lexical-Functional Grammar, although it was developed independently. Levelt (1989 [113]), in his book *Speaking* makes extensive reference to the machinery of IPG, and we shall have occasion to refer to it below, in the section on Processability Theory.

Finally, we come to the "strong innate" position of Government and Binding—which is the topic to hand. Government and Binding (GB) assumes that *homo sapiens* has a "bio-program" or "virtual organ" [30] which is responsible for the facility and speed of first language acquisition. We shall examine these assumptions in detail in forthcoming sections.

2.3.9 Particular Cases and Particular Problems

What I propose to do in the following sections is to look at a number of studies of second language acquisition conducted from within the framework of Government and Binding, and, in the course of doing so, identify a series of problems which are characteristic of this class of research. This survey is not meant to be all-encompassing, but it is to be hoped that the difficulties identified will provide a reasonable picture of the obstructions which clutter GB's version of Universal Grammar.

Lydia White's book on UG and SLA has already been mentioned: let us now turn to some of the studies cited there.

2.3.10 Case Number One

Structure Dependence

Within Government and Binding, "Structure Dependence" refers to a rule or operation being sensitive to the configuration of the phrase-structure tree (as represented by $\overline{X}$ notation) involved in the application in question.

One study that looks at the question of whether or not Structure Dependence plays a role in second language acquisition is by Otsu and Naoi (White 1989 [181, pp. 63-66]). I shall discuss this study at some length, not because it is special or representative in any sense, but simply because it is the first study White reports on in detail. In other words, the choice I have made is essentially random. As we shall see, however, other choices would have yielded similar results and given rise to similar problems.

The question addressed by Otsu and Naoi was whether or not Japanese second language learners of English would, when the subject of a sentence included a relative clause, form questions derived from this sentence which were structure-dependent. This is what one would expect if second language acquisition is guided by Universal Grammar: question formation in Japanese provides no information about its English equivalent. In Japanese, no movement is involved and the WH-word remains in a sentence-internal

position, preceding the direct object; it is not, therefore, extracted [10] from any embedded clauses.

The experiment was as follows. The eleven subjects in the study were females aged between fourteen and fifteen, and had studied English for two years. They received a training session introducing them to sentence types in English which had relative clauses but were different from those used in the study. Before the test proper, the subjects were given a syntax test to determine whether they had mastered relative clause structure in declaratives, since if they had not there would, it was argued, be no point in testing them for mastery of the structures used in the study. All of the subjects passed this test. The subjects then did a question formation task in which twelve English declaratives had to be transformed into interrogatives. Four of the sentences were distractors and the remainder were similar to the example below:

(a) The girl that is smiling can jump high

to which the question counterpart is, in the default case:

(b) Can the girl that is smiling jump high?

It should be noted, however, that it is also possible to ask:

(c) Is it the girl that is smiling who can jump high?

in which case no extraction occurs. To digress for a moment, in order to make the research question here quite clear, it might be useful to provide a brief description of "extraction". Taking the sentence:

(1) The man who [t] is crossing the street is called Peter.

and converting it to:

(2) Is the man called Peter crossing the street?

involves promoting the logical subject of the WH-clause to subject of the main clause, and this promotion is accomplished by "extracting" the element represented by the trace *[t]* in the first sentence of the pair above from its position in the subordinate clause.

It has been mentioned that it is possible to to provide a well formed derivative of a sentence like (a) without resorting to extraction (i.e. example (c)). The fact that Otsu and Naoi's subjects were able to avoid this operation and still produce acceptable sentences constitutes a methodological problem for their study, in that it is possible to form grammatical questions

[10] Extraction will be described shortly.

that evade the structures they are meant to elicit. Indeed, this problem occurs, since, as we shall see, some of the subjects did produce such sentences. Thus, there was a problem for the study design—independent of potential difficulties with Structure Dependence—which prejudiced its chances of success from the outset.

The results of Otsu and Naoi were as follows. Seven subjects produced totally correct questions, as in the first example given above. Three produced grammatically correct questions, but evaded the target structure, either by questioning the main clause (as in the second example), or by transforming the sentence into a conjoined one. One subject produced five ungrammatical sentences of the following kind:

*Is the girl that smiling can jump high?

Thus this subject did not observe the postulated condition of "Structure-Dependence". White's conclusion is that, "These results... suggest that L2 learner's hypotheses about the L2 are structure-dependent" (White 1989 [181, p. 66]). But what about the learner who produced "impossible" sentences? (This is the term used by White). Does this learner not have UG? Or is it the case that she somehow "chooses" not to use it? Or did she merely get confused in the task situation? The answer is: we cannot tell. And this is really the case for all the subjects. After all, they had only been learning English for two years, and it is, I believe, beyond dispute that one cannot learn very much about a foreign language in two years in the classroom. [11] It is fair to say that there are other possible explanations for the subjects' performance. For instance, given the mode of administration of the task, they may have been using "templates" of the declaratives and transforming these into question form through some general cognitive operation. There are, indeed, other possibilities as well, but it is pointless to enumerate them. All one can say is that Otsu and Naoi's study shows that the principle of Structure-Dependence *may* be operating; it certainly does not *prove* that it is.

Further Problems

So far the objections that have been raised are those of the author. But this is not the end of the story: White has some reservations of her own (White 1989 [181, p. 66]).

First, she points out that the "subjects were quite young" and that a "number of researchers would accept the availability of UG for child L2 learners but deny it for adults". She then discusses the so-called "critical

[11] The author, who is reported to be indistinguishable from a native speaker in Spanish, remembers only a few phrases of his classroom French, although he now understands it better than he did, because in the meantime he acquired a cognate language.

period", pointing out that "Those who believe that there is a critical period for L2 acquisition are often uncertain as to the precise age at which it comes to an end" and that "it is not clear whether studies of learners who start learning the L2 at about the age of 11 or 12 can be used to determine questions of adult L2 competence", since "it depends whether or not one believes the critical period to be over by this age". Second, she observes that while "Structure-Dependence plays no role in question formation of this type in Japanese, it presumably does constrain other structures in Japanese", and that "It might be that...[the subjects]...tapped knowledge of Structure-Dependence via other structures in their L1 and were applying L1-based knowledge of this principle to new structures in the L2". She concludes that, for this study "this is not, after all, a clear case of a principle which does not operate in the L1" and that "this problem affects other studies to be discussed". And indeed it does.

The Relationship between L1 and L2 in UG

I have already discussed in some detail one study of second language acquisition from within the framework of Government and Binding. I would now like to turn to some others in order to show that the difficulties I have identified (or cited) are not isolated cases. The next study I will review involves questions of access to the principles or parameters [12] of the language being acquired on the basis that they operate or are set in the same way in the second language as in the first language.

In this connection, a relevant study reported on by White is that of Schachter (1989 [160]). Schachter assumes that the Universal Grammar hypothesis (i.e. that second language learners have access to UG) would be supported "if the learner acquires properties of the L2 which are not obvious from the L2 input and which are not derivable from the L1". Where she differs from the position of the previous researchers is that "her evidence suggests that principles of UG are only observed in L2 acquisition when they operate in a similar fashion in the L1 and the L2". That is, "that UG can only be accessed via the L1" (White 1989 [181, pp. 68–69]).

Like Otsu and Naoi, Schachter is also concerned with structure dependence, and she looks at a principle called "Subjacency"—a constraint on syntactic movement rules, which says that elements cannot move over more than one bounding node in a single operation. For English, bounding nodes are NP, S, and S'. A violation of Subjacency explains the ungrammaticality of sentences like the following:

*Who$_i$ [do you believe [the claim [that John saw t]]]?

since i, which is co-indexed with the trace [t], has been separated from it (in one operation) and has crossed the nodes $S\prime$ and NP.

[12] These will be dealt with below.

The subjects for Schachter's experiment were "native speakers" (with all the problems that the definition entails) of Indonesian, Korean and Chinese and all were learning English. Korean, apparently, does not have rules involving Subjacency. Chinese has no WH-movement, but it does have other movement rules which appear to be sensitive to Subjacency (Huang 1982 [84]). Indonesian, according to Schachter, has WH-movement of a more limited kind than English, and apparently observes Subjacency. Incorporated into the results of this experiment are those from an earlier study of speakers of Dutch (Schachter 1988 [159]). Dutch resembles English in respect of WH-movement and Subjacency. As White observes:

> Thus, Schachter can test whether UG is activatible for adults whose L1 does not show a constraint (Korean) or whether it can only be activated if it is already available in the L1 (Dutch), or whether it cannot be activated at all.

White also notes that "The Chinese and Indonesians provide an interesting intermediate case: Subjacency is activated in the L1 but not in the same range of structures as in the L2. If UG is actually dead and only accessible via the L1, such learners should not be able to apply Subjacency to new situations in the L2" (White 1989 [181, p. 69]).

Schachter's research design was (independently) quite similar to that of Otsu and Naoi. However, she used a grammaticality judgement task rather than a transliteration one. Sentences were to be marked as:

1. Clearly grammatical.
2. Probably grammatical.
3. Probably not grammatical.
4. Not grammatical.

The subjects first underwent a test designed to ensure that Subjacency should have emerged in the second language. There were twenty-four grammatical sentences with sentential subjects, relative clauses, noun phrase complements and embedded questions; there were also an equal number of ungrammatical sentences involving violations of Subjacency, such as:

*What did Susan visit the store that had in stock?

As well as the subjects mentioned, Schachter had nineteen native speaker controls. The results of her experiment were the following.

1. The speakers of Dutch behaved like the native speakers of English.

2. The speakers of Chinese, Korean, and Indonesian made correct judgements about the grammatical sentences, but often failed to reject violations of Subjacency, with the Koreans behaving more poorly than the Chinese or Indonesians.

These findings are consonant with the "UG-is-dead" hypothesis, since, to some degree or other, all groups except the Dutch and the native speakers accepted violations of Subjacency. This, however, is not necessarily the case. First, as White points out [181], there are problems with the methodology of the experiment. The rationale for the syntax test was to ensure that subjects had mastered the structures in which Subjacency was expected to operate. Schachter assumed that her sentences were an appropriate test of this because her native speaker control group accepted the grammatical sentences and rejected the ungrammatical ones, which were derived from their grammatical counterparts. But, as White notes [181], native speakers are likely to reject ungrammatical sentences regardless of their relationship to other sentences, grammatical or not. Thus, as White puts it:

> the native speaker judgments are not a guarantee that one has the right syntax test. Schachter's syntax test sentences and UG test sentences were not controlled for length and vocabulary choice, and were only partially comparable in terms of syntactic structure. The fact that the native speakers of Dutch behaved like the native speakers of English may have been due to the fact that they were more advanced in their English than the other subjects, rather than to the fact that only they had access to Subjacency.

There is more to come. It has been argued that Subjacency is subject to parametric variation (Rizzi 1982 [152]) (Sportiche 1981 [171]). If this is the case, (and, of course, it is disputable that it is) then "Some of the ungrammatical Subjacency violations used as test sentences by Schachter would not be violations in languages which have different bounding nodes from English" [181]. In addition, there are analyses of diverse Oriental languages—including those at issue here—in which there is a base-generated empty category PRO (Martohardjono and Gair 1989 [180]). This accounts for the falsely assumed grammaticality of sentences like:

*Who [do you believe [the claim [that John saw PRO]]]?

An analysis of this kind involves no movement of WH-words, and means, as White observes, that "the acceptance of Subjacency violations in the L2 is not an indication of the non-availability of a principle of UG but, rather, of an analysis of the L2 (stemming from the L1) in which Subjacency is simply irrelevant" (White 1989 [181, p. 73]).

According to Martohardjono and Gair, the occurrence of PRO is not an isolated phenomenon in the interlanguage of speakers of Oriental languages, so there is at least a possibility that it is the right one for the framework of Government and Binding, leaving aside for the moment the question of whether or not Government and Binding itself is the right framework.

So here again both reader and researcher are left in an equivocal position. What are we to make of Schachter's findings? Possible conclusions are that Universal Grammar is dead for the subjects of her study, that it is alive but constrained, that it is alive some of the time and dead for the rest, or that the study failed in various methodological and theoretical ways to identify this mysterious creature and tap its operations, or even, perhaps, that there is no such thing as Universal Grammar in the first place. [13] Clearly, this is not a very satisfactory state of affairs, yet, as any reading of the literature will demonstrate, it is probably the default case: this kind of indeterminacy is prevalent in studies based on Government and Binding.

To sum up, Schachter's results, while certainly not without content, do not definitively establish whether Subjacency is a principle which can operate without precedent in an L2. There are various other possible explanations for Schachter's findings. For instance, it could have been the case that the Dutch speakers were more advanced in their acquisition of English than their Oriental counterparts, and that this is why they rejected violations of Subjacency. Alternatively, the transliteration test may not have been definitively linguistic in nature (as in the "templates" possibility). Then again, Subjacency may not have been the principle at issue (as in the analysis presented by Martahajono and Gair, in which acceptability of the sentences was due to the category PRO being exported to English). In brief, there are no definitive answers in Schachter's study.

Is UG Really Dead?

The next study to come under scrutiny differs from the previous two, in that it asserts the inaccessibility of Universal Grammar to learners of a second language. It also takes a position with important repercussions for the theory on which this study is partially based. [14] This is Clahsen and Muysken's 1986 [38] controversial paper on the acquisition of German as a second language.

As regards Universal Grammar, Clahsen and Muysken's position in this paper (and in the discussion which has followed it [38]) is quite clear: UG operates in first language acquisition but not at all in second language acquisition. In order to do justice to Clahsen and Muysken's position, it is first necessary to outline some facts about German word order. There is

[13] In this regard, UG seems to be the linguistic equivalent of Bigfoot, the Himalayan Yeti, or the Australian Yowie—glimpsed but never captured.

[14] This is Processability Theory, and it will be described in more detail below.

a quite extensive body of literature on this topic, both in the theoretical domain and in studies of language acquisition (White 1989 [181, p. 115]), but I will, for the purposes of exposition, present the main aspects here.

In German main clauses the finite verb is found in second position. This happens even when there is topicalization or the preposing of an adverb. Examples of this are:

Die Kinder essen das Brot (SVO)

"The children eat the bread"

Das Brot essen die Kinder (OVS)

"The bread eat the children"

Nun essen die Kinder das Brot ($XVSO$)

"Now eat the children the bread"

If there is both a finite and a non-finite verb or particle in the sentence, the finite verb occupies second position and the non-finite verb is situated at the end. Thus:

Die Kinder haben das Brot gegessen ($SV^{+f}OV^{-f}$)

"The children have the bread eaten"

In subordinate clauses, however, the finite verb goes to the end:

Ich glaube, dass die Kinder das Brot essen ($ROOT - SOV^{+f}$)

"I believe that the children the bread eat"

or:

Ich glaube dass die Kinder das Brot gegessen haben
($ROOT - SOV^{-f}V^{+f}$)

"I believe that the children the bread eaten have"

Formal descriptions of German vary from theory to theory, and in Government and Binding, it is assumed that the verb-final order of subordinate clauses is the right representation for D-structure. [15] This means that German is, at an underlying level, head-final [181]. However, at S-structure, German is mixed. For syntactic categories like NP and PP, German is head-initial at all levels and in all types of clause, while VP—if it exists—is head-final. [16]

In Lexical-Functional-Grammar, on the other hand, there is only one level of structural representation—namely, constituent-structure (c-structure),

[15] Of course, the minimalist program has eliminated D-structure.

[16] This is a vexed question.

and word order is generated by a number of rewrite rules from a context-free grammar. No assumptions about buried structures are necessary [22].

Clahsen and Muysken are adherents of Government and Binding. As a consequence of this, they posit the existence of two obligatory movement rules in main clauses. One of these moves the finite verb to the COMP position. The other moves an element of some kind (be it subject, object, indirect object or adverbial) into a sentence-initial position, where it precedes the finite verb. There can be no movement in embedded clauses because the COMP node is already occupied (cf. the example given above) [38].

The implications of this for second language acquisition are, according to Clahsen and Muysken, quite crucial, since they appear to provide very clear evidence for the so-called "Fundamental Difference" hypothesis, first proposed by Robert Bley-Vroman [67], which is that first and second language acquisition are different not just in product, but in process as well.

Clahsen and Muysken arrive at their position on the basis of findings for language acquisition in German, where in first language acquisition it is the case that children at early stages of this process use SOV word order—despite its ungrammaticality in main clauses—whereas adults tend to use SVO (often referred to as "canonical word order"). This latter tendency is, they claim, even true for Turkish learners of German, although Turkish is a verb-final language. In fact, there is some dispute about the behaviour of Turkish learners [99] in regard to word order: they do sometimes produce SOV word order at early stages, as Jordens (1987 [99]) shows. The present author has some data for Turkish speakers learning English, and it is definitely the case in that data that some learners produce SOV structures. An example would be:

"People... this hamburger... eat"

So Clahsen and Muysken's claim needs to be qualified somewhat; it is certainly not the case that Turkish speakers *never* produce SOV structures in a language with a main clause order of SVO. It is, however, true that they abandon inappropriate verb final order at an early stage of the acquisition process.

According to Clahsen and Muysken, the reason for the production of SVO structures by adult learners is that they adopt a "canonical word order" *strategy*, not because they are exhibiting the effects of transfer or using a first language parameter setting. (More on this below). Exactly how and why this happens is not clear. One reason is that SVO frequently occurs in the input, but then so do other word orders, and, as White observes, an assumption of this kind "is particularly problematic in German" (White 1989 [181, p. 103]).

Another possibility is that canonical word order is semantically "transparent" in that there is an *actor*, an *action*, and a *patient* (or an *experiencer*, a *state*, and a *condition*). This is a viewpoint with which the author

has some sympathy [93]. Another reason—not entirely unrelated to the preceding one—is that this form of sentence is "neutral" or unmarked (either semantically or in terms of language typology; cf. Slobin and Bever 1982 [169]). This is also a reasonable possibility, and one with which the author feels quite comfortable himself. It must be said, however, that none of these possible explanations are easy to test, and that they do not account for alternative word orders in first language acquisition, although there is some dispute about exactly what these are for different languages (Pienemann, 1994; Clahsen 1985 [35]).

Whatever the case may be as regards canonical word order, let us proceed with our discussion of Clahsen and Muysken. The data on which they base their argument is naturalistic production data, gathered and analyzed in the course of the *ZISA* project (Meisel, J., Clahsen, H. and Pienemann, M., 1981; Clahsen, H., Meisel, J., and Pienemann, M. 1983 [36]). This involved a cross-sectional study of forty-eight speakers of Italian, Spanish or Portugese learning German without any formal instruction, and a longitudinal follow-up study with twelve of the learners. The study concentrated on the acquisition of German word order, and where it made reference to grammar—which it did not do to any great extent—it used what is now referred to as the Extended Standard Theory (that is, the version of Transformational Grammar elaborated by Chomsky and his followers in the 1970's).

The *ZISA* project found that there were five stages in the acquisition of German as a second language. These were:

1. Canonical word order. (SVO)
2. Canonical word order with optional adverb preposing. $(SVO; [X]SVO)$
3. Canonical word order with both adverb fronting and movement of non-finite elements to the end of the sentence. $(SVO; [X]SVO; SV^{+f}OV^{-f})$
4. Canonical word order with adverb fronting, movement of non-finite elements to the end of the sentence, and subject-verb inversion. $(SVO; [X][S]V^{+f}OV^{-f})$
5. Canonical word order with adverb fronting, movement of non-finite elements to the end of the sentence, subject-verb inversion, and movement of the verb to the end of the sentence in subordinate clauses $(SVO; [X][S]V^{+f}OV^{-f}; XVSO; ROOT - SV^{+f}OV^{-f})$

Examples of the sentences which correspond with each of these stages are given above, so I will not repeat them here.

These stages are not the same in first language acquisition (Pienemann 1995 [142]). One important difference is that the initial stage for children involves the production of SOV structures (although SVO also occurs). This is not necessarily the case in second language acquisition, where canonical

order appears to depend to some degree on the first language of the adult learner. Nevertheless, while the processes of first language acquisition differs from its second language counterpart, it is, I believe, possible to account for the results of both through recourse to the same mechanisms.

Notwithstanding what has just been said, it is the case, according to Clahsen and Muysken, that children and adults arrive at the same target structures via differing routes.

For children learning German as an L1 with access to UG, the rule for Stage One (actually X + 1) is an optional movement rule which puts the verb (finite or not) in second position. This, like all the rules to follow for first language acquisition in German, is a *linguistic* rule.

1. For Stage Two, there is a constraint on the optional movement rule which specifies that the verb moved to second position must be finite.

2. For Stage Three, there are preposing rules for such things as adverbs and topics, and the verb movement rule becomes obligatory.

3. By Stage Four, the acquisition process for main clauses is completed.

4. For Stage Five, there is a rule which distinguishes between main and subordinate clauses, and leaves the verb in its presupposed D-structure position at the end of the subordinate clause. If the verb is complex (that is, if it is accompanied by an auxiliary), then the auxiliary follows the verb.

   It should also be noted that the claims that Clahsen and Muysken make about the relationship between syntax and morphology in German, where Inversion and Subject-Verb Agreement appear together in both child and adult acquisition, differ for the two processes [37], and are not due to the operation of the same mechanisms.

Thus it is the case that, for adults, Clahsen and Muysken postulate something quite different in the acquisitional trajectory. Since it is assumed that adults have no access to UG, the claim is that they arrive at the stages of development described above by means of general cognitive strategies. It is also claimed that these strategies are, in some sense, "unnatural" (White 1989 [181, p. 102]; Clahsen 1984 [34]). The stages are labelled successively *PARTICLE*, *INVERSION* and *VERB* $\rightarrow$ *END*. The first rule involves the separation of auxiliaries and modals from the participle or main verb, which is postposed to the end of the sentence. Thus:

Ich habe ein Haus gebaut—"I have a house built"

The second strategy involves an exchange of places between verb and subject, as occurs in certain constructions in English also, such as:

What have we here?

The German equivalent of this is:

Wann gehst du nach Hause?—"when go you home?"

The third involves the postposing of the finite verb in subordinate clauses:

Weil er dumm ist—"because he stupid is"

There is very strong evidence, both from the *ZISA* project, and from various replications of the research done there [178], that these strategies (let us call them "rules") are implicationally ordered, just as they are given above (i.e. speakers who have $VERB \rightarrow END$ have the other two rules). Clahsen (1982) gives a partial explanation of why this is so. Basically, this is as follows. Adult learners of German are restricted by a series of speech-processing constraints on movement of constituents. These operate as follows.

Learners at Stage One can only produce linear sequences; no sentence-internal permutations are possible.

Learners at Stage Two are subject to an initialization-finalization constraint; they can move constituents to salient points of the sentence (i.e. the beginning or the end). The IFS strategy, as it is called (Clahsen, 1984 [34]) is based on findings from research into speech-processing that the end-points of a string of digits or words are easier to identify than any middle-point [34].

Learners at Stage Three are still subject to the IFS, but less so, since they can move internal constituents into salient positions.

Learners at Stage Four have shed the IFS altogether, and can permute sentence-internal constituents; hence they are capable of subject-verb inversion.

Learners at Stage Five can move the verb to the end of a subordinate clause, because they know what one is. The rule of $VERB \rightarrow END$ is based once again on findings from speech-processing research, which indicate that main clauses and subordinate clauses are processed differently (Clahsen 1984 [34]).

There is no denying that the "strategies" approach has predictive power. It has, in a considerably modified form, been applied to English by Manfred Pienemann and the author, and its predictions, as we shall see, have been fully borne out. Moreover, Clahsen's basic insight has been elaborated in considerable detail by Pienemann, and constitutes one point of departure for what is now termed "Processability Theory". [17] This theory has been applied to a number of other languages, such as Spanish (Johnston 1995 [97]), Swedish (Pienemann and Håkonsson, in progress), and Japanese (Huter, in

[17] Processability Theory will be dealt with shortly.

progress; Kawaguchi, in progress), where work is still in progress. In all cases the results have been very encouraging.

While the "strategies approach" of Clahsen and Muysken is, as has been conceded, of some interest and value, there are some serious problems with their basic position on the inaccessiblity of UG to adults. White, for instance, makes the point that "In appealing to the canonical order strategy, and in using only production data, Clahsen and Muysken have failed to distinguish between L2 *performance* and the acquisition of L2 *competence*" (White 1989 [181, p. 102]); we shall return to this later.

White also makes an important observation that "The canonical order strategy presupposes what it is supposed to explain", pointing out that even if the learner is trying to arrive at some basic word order and even if SVO is the canonical order of German, "the learner does not have control over the input", and will be confronted with a wide variety of word orders in declaratives (as the examples given above show), and will thus be in the position of having to decide which order is basic without any obvious means for doing so. Given this situation, she concludes that "It is simply begging the question to claim that SVO is the canonical order for German and that there is a processing principle which detects this" (White 1989a; p. 104; White 1989 [181]).

Further, White points out that the adoption of SVO order is not necessarily incompatible with the claim that UG is in fact operating, since "the Head-position Parameter usually operates consistently across syntactic categories" and German is an exception to this. An incorrect choice could actually *explain* why adult learners of German resort to SVO—something that the "strategies" approach cannot do, because they deal with how language is *used*, not with how it is acquired [181].

So far we have been raising possible objections to Clahsen and Muysken's proposal from within the framework of Government and Binding itself. But White's point that "strategies" pertain to the domain of performance, and do not deal with competence brings us into a wider arena. For the notion of performance to be meaningful, there has to be a body of linguistic knowledge (that is, competence) on which it can operate. For their adult learners, Clahsen and Muysken do not specify what this is. "Strategies" themselves are not sufficiently powerful to produce linguistic knowledge; they are, after all, constraints which inhibit production rather than encourage it. For strategies to work there has to be something for them to work on, and Clahsen and Muysken explicitly deny that any such thing exists. For them, competence is simply a black hole. This defect makes their position completely untenable, unless they resort to the barely defensible position that the second language learner's competence is a case of bulk transfer from the L1, which—come what will—can somehow be transmogrified into a possibly quite acceptable version of the target as the learner does nothing more than shed the shackles imposed by strategies.

It is important to realize that the essentially negative nature of strategies breaks the sustainability of any assumption that adult learners have all the competence they need in the form of first language transfer, much less that this body of knowledge is "overly powerful"—as Pienemann [142] observes. But even if the L1 bore this relation to the L2, how could "strategies" alone convert, say, Turkish into German? And in any case it is beyond dispute that interlanguages exhibit features which occur neither in the L1 nor the L2. And how would strategies produce these? It makes very little sense to assert that a language learner has no grammar, or that it is impossible for that grammar to develop independently. Yet this is what Clahsen and Muysken explicitly do. We are therefore entitled to ask what exactly is the status of the "constituents" which the "strategies" operate on. If they are not elements indexed by grammatical category, what are they? If they are merely uncategorized "words", what exactly is a word? And so on.

There is another problem for Clahsen and Muysken as well. As White and others (cf. du Plessis [50]) point out "Clahsen and Muysken describe very similar acquisition data in one way for L1 learners and in another for L2" (White 1989 [181, p. 105]). This, of course, is yet another case of begging the question. Clahsen and Muysken totally neglect the null hypothesis in not applying the same measures to both children and adults. It is obvious that if you measure the height of Building A in feet and Building B in metres that one will be so many feet tall and the other so many metres. Unsurprisingly enough, the results will be different. However, this does not prove that the buildings differ in height.

I have dwelt on the case of Clahsen and Muysken in some detail for two reasons. First, it is yet another illustration of the kinds of problems that are rampant in second language acquisition research based on Government and Binding, and I have outlined a number of objections to their Fundamental Difference proposal from within that framework. Second, as I have already noted, the essential insight contained in Clahsen's strategies proposal is a valuable one—the problem in the study just discussed is its mode of application and the assumptions that go with it.

In this dissertation I will only be able to provide a glimpse of Processability Theory in operation, and, as Pienemann himself goes to considerable length to show, the theory takes Clahsen (1982) as one of its points of departure [142]. One of the innovations of Processability Theory is the incorporation of a grammar into its framework; as we have seen this is precisely what Clahsen and Muysken fail to do. Another feature is that Clahsen's constraints become *prerequisites*, and are implicationally related, from canonical word order upwards. Very briefly, what these prerequisites do is constrain the possible hypotheses a learner can entertain about the target grammar. In this way, as Pienemann (1995 [142]) puts it "performance shapes competence", and thus the relationship between these two concepts is quite clearly defined. This was one of the many problems which beset

Clahsen and Muysken.

Other Concepts in Government and Binding: Markedness

The concept of "markedness" is another important one for advocates of Government and Binding, as well as being of considerable importance to many branches of linguistics, and, therefore, linguists in general.

"Markedness" is a concept which exists in many different forms and for many different domains. Non-generative linguists use the idea of markedness quite extensively. Examples relevant to second language acquisition research are Kellerman (1979 [103]) for meaning, Eckman (1977 [53]) for phonology, and Gass (1979 [66]) and Hyltenstam (1984 [5]) for syntax. These particular examples are cited by White (White 1989 [181, p. 117]). There are, of course, many other researchers who employ the term, and many ways of employing it. Outside language acquisition research the term is used in typological studies, of which Greenberg (1968 [70]) is a prominent exponent; Comrie (1981 [40]) and Hawkins (1987 [80]) also deserve mention in this context. Hawkins has actually extended his research to first language acquisition [80], and this is a direction which other workers in the broader field of Universal Grammar are also taking, since it is now becoming clear that theories of language in general need, in some way or other, an account of how language is acquired in order to provide an adequate description (of whatever kind) of language itself.

One of the problems with markedness, irrespective of its theoretical context, is, given some provisional definition, how to determine which is the marked value and which is the unmarked one. An example from phonology comes to mind here. This example involves Eckman's hierarchy, and comes from a second language acquisition study conducted by Fellbaum [63]. Fellbaum looks at the acquisition of aspirated and non-aspirated stops by English speaking learners of Spanish and Spanish speaking learners of English, so the study is nicely symmetrical. Following Eckman, Fellbaum assumes that unaspirated stops are the unmarked case. This assumption has important consequences for Fellbaum's study, but we will not discuss them here: what we are concerned with is how the markedness value is determined. Eckman has a principled reason for assigning the unmarked value to unaspirated stops—aspiration is an extra feature. And Eckman may well be right. However, the author, in his early study of linguistics, recalls in a lecture an account of aspiration as being a "natural" (and therefore unmarked) product of vowel onset. This would make the non-aspirated stop the marked one. It is not my intention here to state that, on the basis of the above account, that Eckman is wrong. All I wish to do is to demonstrate that there are very plausible yet contradictory reasons for assigning a marked or an unmarked value to some feature of a language, and that the kind of problem outlined above is a very common one where markedness is concerned.

For those second language acquisition researchers who conduct their work in the framework of Government and Binding the concept of markedness—however it may be determined—is an important one. In this domain, Chomsky's distinction between *core* and *periphery* is germane—for reasons I will give in a moment. A core grammar is, in White's words, "a particular instantiation of those principles and parameters that are built in" (White 1989 [181, p. 118]) for a child learner. For adults, as we shall see, this would be the case for their first language, but is not, it is claimed by some, the case for other languages they may learn. Core grammars vary from one language to another "because not all the fixed principles are instantiated in all languages, and because languages adopt different parameter settings" (White 1989 [181, p. 118]).

Parameters themselves have become part of the Chomskyan version of Universal Grammar as a means of capturing variation between languages, where this variation involves "very subtle and complex properties which are thought to be unlearnable" (White 1989 [181, p. 29]). Properties with these characteristics cannot necessarily be induced from the input because there simply is no evidence for them, either positive or negative. They are often adduced in arguments which support the so-called "Logical Problem" of language acquisition [82]. To sum up rather crudely, there are a limited number of options and principles available to the learner in the Government and Binding account of Universal Grammar, and these are referred to as *parameters*. The different values which these options can take are termed *parameter settings*. Certain clusters of grammatical phenomena, which may appear to be unrelated, come together as the result of the operation of a parameter in a particular domain. They can be invoked as an explanation of the non-occurrence of the many hypotheses about a target grammar which the learner could otherwise entertain, and, in the view of those who advocate their existence, they "give the child advance knowledge of what the possibilities will be... [since they]... limit the range of hypotheses which have to be considered" (White 1989 [181, p. 29]).

According to the proponents of parameters, "The function of input data in language acquisition is to help to fix one of the possible settings" a parameter might take. That is, the input acts as a *trigger* for knowledge that is innate to the learner (White 1989 [181, p. 29]). Another argument adduced by exponents of the "Logical Problem" is that this data is likely to be quite "degenerate" because of slips of the tongue, rephrasings, false starts, run-on sentences and so on (Fodor, J. A. 1964 [65]), and, given this kind of "noise" in the input, it would be very difficult for a learner to extrapolate rules if he or she had no "foreknowledge" of what to look for. In point of fact, it has been shown that what is termed "caretaker" speech or "motherese" is not nearly so messy as might be supposed (cf. Long and Larsen-Freeman 1991 [110, pp. 115-116]).

In this study, discussion of parameters will be rather limited. I will

simply note that, in the literature, there is a great deal of controversy about parameters: this encompasses questions as to whether they exist at all [68], or, if it assumed that they do, which ones can reliably be enumerated, and what properties "cluster" with what parameters. Other questions that arise are. What is the psychological status of parameters? Do they follow a particular order when they are triggered (cf. Felix 1984 [61])? Do they interrelate in certain other ways? Must they be binary? They are, in short, objects of quite vigorous debate.

In the context of the present discussion there is a related point to be held in mind. This is the somewhat unusual "flexibility" of Government and Binding, when it comes to reformulating important principles. Horrocks (1987 [83]) has this to say:

> Take, for example, the empty category principle ... that requires traces to be properly governed. The notion of proper government ... [has been]... adjusted several times in the course of the discussion of government theory in order to accommodate new configurations that seemed as if they ought to fall within its definition. The price of such readjustment in terms of the consequences for the theory as a whole ... [has been]... very small, other than to render the definition of proper government suspiciously broad, comprising as it did, subclauses that seemed to have little in common.

Horrocks [83] goes on to state that:

> ...it is arguably in the interests of linguistic research that linguists strive to frame universal principles of grammatical organization in such a way that their abandonment carries a price in terms of repercussions throughout the system.

The fact that some principles in Government and Binding can be adjusted to suit incoming facts without such repercussions ought, I believe, to strike a warning note with regard to their robustness and validity, and needs to be borne in mind in the discussion which follows.

I have digressed somewhat from my main concern here, namely markedness, because parameters are assumed to be inhabitants of the "unmarked" core of a grammar, while peripheral features are not. Peripheral phenomena are "idiosyncratic, language specific, and exceptional" (White 1989 [181, p. 118]). As such, they are marked.

The above might appear to constitute a relatively straightforward definition of markedness. It is, however, the case that certain parameters have both a marked setting and an unmarked one (White 1989 [181, p. 119]). So markedness can exist within the core itself. This is somewhat confusing, and

does little to advance our definition of markedness. This kind of confusion is, as far as I can see, a hallmark of theory-construction in Government and Binding.

There is another potential difficulty with markedness in Government and Binding. This is as follows. Like its competitors, Government and Binding has a lot of information stored in the lexicon—for instance, verbs must subcategorize for their objects. But lexical items themselves are probably the most idiosyncratic features a language has to offer: stripped of their inflectional and derivational morphology, non-onomatopaeic word-stems are quite arbitrary representations of their semantic content. And yet it is the lexicon of a language—which is highly marked in anyone's terms—that serves as a repository for a great deal of grammatical information. This information is acquired, then, from very heavily marked input data. So discussions of what is and what is not marked in the input have to exclude the lexicon if they are to have any meaningful consequences. Once again, we have ended up in a confounding and confusing situation. One could, of course, exclude the lexicon from consideration, as regards its status *vis-a-vis* markedness. But then lexical items can be marked in other ways from the one in consideration here, so excluding them would not really be a viable option, and would ring the death knell for sociolinguistic enterprises. Ah well... back to where we started...

Whatever the case may be as regards markedness, it is as White (White 1989 [181, p. 120])notes "A fairly prevalent assumption as far as L1 acquisition is concerned... that if the target language allows both marked and unmarked variations of some phenomenon, then acquirers will first adopt the unmarked before moving on to the marked (e.g. Hildebrand 1987; Phinney 1981; White 1982)". Hyams (1986 [86]) has a stronger version of this hypothesis, which states that a preset value of a parameter will be adopted even if there is no evidence in the target language that this value is the correct one. So there are exceptions to even the most prevalent assumptions, and it is very hard for a reviewer like myself to keep his bearings. White herself acknowledges that "on current assumptions that a marked parameter setting is one which is motivated by positive evidence" (White 1989 [181, p. 120]) both of the positions mentioned above "may be too strong", and that "if there is positive evidence in the input motivating a marked setting, it is not clear that the unmarked setting should necessarily constitute an acquisition stage before the marked one is acquired". Markedness, therefore, and however it is defined, is not of much value to a researcher who wishes to predict stages of acquisition in a language, although it has been used by at least one such person (Mazurkewich 1984 [54]) for precisely that. Needless to say, her results were somewhat equivocal, and could, in point of fact, have been due not to markedness but to transfer. Mazurkewich investigated structures in English which were the result of what was then called "pied-piping" (Ross 1967 [153]), and "preposition stranding". Examples are:

To whom did John give a book? **(Unmarked)**

Who(m) did John give a book to? **(Marked)**

The first of these examples is quite rare in English, but there are a range of arguments for it being the unmarked case (White 1989 [181, p. 123]). The results of the study in question are examined in detail by White, so I will not give them here. The following information ought to suffice for my present purpose.

1. Subjects in the study were native speakers (*sic*) of French and Inukitut (an Eskimo language).

2. The behaviour of the two language groups differed, with the French speakers generally producing the unmarked form and the speakers of Inukitut the marked one.

3. In the case of the French speakers, their preference could be accounted for by the fact that the supposedly unmarked structure is the only possibility in French.

4. In the case of the Inukitut speakers, all of their education (up to high school or college) was in English, and they may have been behaving as they did because they were at an advanced stage of acquisition of English (however it was defined) and they exhibited the same preferences as native speakers. (In fact, it seems to be the case that, if they were learning anything, it was the *unmarked* form, not the marked one, since the most "advanced" subjects produced this form more frequently than its counterpart (White 1989 [181, pp. 123–125])).

White concludes that "Although Mazurkewich argues that these results support her hypothesis that L2 learners will learn unmarked before marked, this is not the case" (White 1989 [181, p. 125]). She points out that the behaviour of the French speakers is consistent with the transfer of (possibly unmarked) structures from that language, and "cannot tell us whether L2 learners revert to core grammar" (White 1989 [181, pp. 125–126]). She also notes that the differing behaviour of the Inukitut speakers "suggests that the L1 was indeed having an influence on the French speakers" (White 1989 [181, p. 126]). Finally, she points out that preposition-stranding "is very frequent in English, even if technically marked" and that other studies of this phenomenon in both first and second language acquisition show that the marked structure is either acquired at the same time as the unmarked one (in young children—French 1985; Krause and Goodluck 1983 [132]) or *before* it (for speakers of languages other than French—Bardovi-Harlig 1986 [181]).

To sum up, markedness, however it may be defined, fails to predict the right results, and is difficult, in any case, to distinguish from plain old "transfer".

Markedness and Transfer

An alternative to Mazurkewich's hypothesis is that there is the possibility of language transfer taking place, and that this transfer is influenced by markedness. Liceras (1986 [115]; 1987 [77]; 1988 [133]) is an exponent of this position. Her work is of particular interest to me, as I have made a set of (largely verified) predictions about stages of acquisition for Spanish as a second language from within the framework of Processability Theory (Johnston 1995 [97]).

Liceras is quite explicit about the role of Transformational Grammar in her research (Liceras 1996 [115, p. 6]):

> The theory of grammar does not explain the process of acquisition. Nevertheless, as White (1980) points out, it may interact with other principles to make predictions about how acquisition proceeds in 'real time':

And further:

> the assumption that complexity causes acquisition problems and that markedness is a form of complexity leads to certain predictions from markedness theory about things that will be hard to learn or acquired late. (White 1989 [181, p. 114])

We have already considered the problems of how to define markedness from within the framework of Government and Binding. Most of the points made would also be true for its predecessor, the Extended Standard Theory—which is what Liceras uses for her description of sentence types (Liceras 1986 [115, p. 17]). In her own words, Liceras (p. 34) opts for the Extended Standard Theory for the following reasons:

> There are two main reasons for choosing EST as the theoretical framework of this study. First, it attempts to contribute to UG by distinguishing a core grammar and a marked periphery and by proposing core grammar as an idealized model for language learning purposes which can be subjected to experimental research on marked versus unmarked syntactic processes. Although this idealized model of language learning is assumed to be relevant in the case of first language acquisition, it is our intention to investigate whether the core/periphery distinction is reflected in nonnative grammars. If we assume that a model

> of second language learning should at least take into consideration UG, L1 and the structure of L2, then marked elements both in L1 and L2 should have a special status in NNGS. In other words, the structure of preferences and implicational relations among the parameters of core grammar that accounts for the differences and similarities between English and Spanish should be reflected in the Spanish NNG. In the same way the hierarchies of accessibility that Chomsky (1981) views as independent structure should also be reflected in the Spanish NNG. For example, we will examine whether marked or peripheral dimensions in L1 or L2 are systematically transferred, overgeneralized or fossilized; whether the hierarchy of accessibility proposed for the different grammatical relations is reflected in the way in which the Spanish NNG is reconstructed.

Liceras certainly lays her cards on the table in this exposition, and warrants credit for her thoroughness in doing so. There are, however, some extra problems to be considered when we come to using the concept of markedness in acquisition studies.

One such problem is that theories of markedness are largely based on research into mature language systems, not developing grammars. We have already seen that there are many different versions of markedness, and that claims about what is marked have a tendency to vary with the explanations they are based on. This problem is compounded in second language acquisition research in that the concept of markedness is being applied to a domain for which it was not developed, and therefore presupposes explanations about what is in fact trying to explain. This, of course, is begging the question. Indeed, it may not be the case that the various possible contributory causes of markedness are ranked or indeed rankable, even if they have been identified with the appropriate class of data. Having said this, I shall assume that for the exposition which follows, there is some validity in the definition of markedness which Liceras uses, despite its attendant defects.

Liceras (Liceras 1989 [115, p. 6]) states that:

> in this study it is hypothesized that markedness should be defined relative to both L1 and L2, and that it should be reflected in the degree of permeability of the nonnative grammar so that marked L1 rules will not be integrated into the interlanguage system, and lead to variability of intuitions on the part of the learner, while marked L2 rules should cause variability of intuitions.

Let us consider this. First, markedness has to be defined in relation to both L1 and L2—that is, in different terms. How, then, can conflicts

in relative weightings be reconciled? Second, Liceras uses the term "permeability"; this comes from Adjèmian (1976 [1]). Adjèmian believes that "permeability" is a characteristic of interlanguage systems which distinguish them from their mature counterparts.

Briefly, what Adjèmian claims is that interlanguage grammars are particularly susceptible to the intrusion of items from the first language, and that these intruding items may persist and lead to "fossilization" (of which more later). "Permeability", then, is really just another name for the effects of "transfer" or "interference". But is "permeability" really something unique to interlanguage? The answer is no. Consider the following: Adjèmian makes his claim about "permeability" in a language which is half French or Latin, and, in the case of the remainder, a goodly intermixture of various Scandinavian words. This is, of course, English. How then can we accept the proposition that interlanguage systems are "permeable" while mature ones are not? Where does English stand in all this? Is it not a mature language system? C.-J. Bailey, for instance, has called English a creole (p.c.). But then creoles are mature languages. In any case, English itself crops up more and more in other languages. German is a good case of this (Pienemann, p.c.). So, it seems that "permeability" is not a very useful term, or a very original concept. We should bear this in mind when we encounter it in quotes from Liceras, and just replace it with "transfer" or something of the same ilk. [18].

Leaving aside the question of "permeability", the program implied by Liceras does not take into account, even on a superficial level, the developmental aspect which has been observed to occur in transfer phenomena—such as Kellerman's [45] U curve—much less the overall role of development in the evolution of a feedback system as modelled by the operations of such developmental phenomena as "Generative Entrenchment" [184] and other accounts of processes of self-regulation. This in itself is not a fatal defect, and, as I have already said, Liceras deserves praise for the honest and straightforward way in which she lays down her foundations and reports on her findings. Nevertheless, it is a defect in her research program, and, as such, needs to be noted.

Liceras argues that an understanding of interlanguage requires the following.

- An understanding of the properties of both the L1 and the L2.
- An understanding of markedness.
- An understanding of how markedness manifests itself in interlanguage grammars.

[18]This is discussed in more detail in Johnston 1986 [96] and Johnston forthcoming [95]

In respect of this last point, Liceras takes the position that unmarked aspects of language are more likely to show up in the interlanguage than marked ones (White 1989 [181, p. 129]). Liceras considers various aspects of relative clause formation in the Spanish of speakers of English. Her conclusions about the first and second language are as follows (Liceras, 1986 [115, p. 68]).

> It has been shown that English and Spanish present the following similarities with respect to restrictive relativization:
>
> 1. Both languages have complementizer and a set of relative pronouns.
> 2. Both languages have a filter against a doubly-filled COMP.
> 3. Both languages have a rule of Deletion in COMP or alternatively both can move PRO to COMP.
> 4. Both languages can relativize SUs, DOs, IOs, GENs, Time Complements, Locatives and Adverbials.

On the other hand, the analysis has shown the following differences:

1. English and Spanish do not present the same distribution of the complementizer and wh-phrases.
2. Only Spanish has a filter against an empty COMP.
3. Preposition stranding is only possible in English.
4. Only Spanish can have oblique relativization in DO position.
5. Only Spanish has nounless constructions.
6. Only Spanish can relativize Adjective and Adverbial Phrases.
7. The rule of deletion in COMP only applies obligatorily in Spanish non-oblique relativization. Alternatively, only in Spanish the Avoid Pronoun strategy applies obligatorily".

This is a broad and comprehensive analysis, and is very typical of Liceras' thoroughness and directness. However, there is one point with which I take issue. This concerns what Liceras refers to as the "Avoid Pronoun" strategy (proposed originally, by Chomsky—see Andrews 1990 for details [6]).

As has been pointed out by, for example, Meisel [125], strategies are notoriously difficult to formalize and rank. Avery Andrews (1990) provides a formal account of the Avoid Pronoun strategy from within the framework of Lexical-Functional Grammar, and in terms of "morphological blocking"—a concept that originated with Aronoff (1976) [7]. The transformational

framework employed by Liceras is probably incapable of such a formalization. So in regard to a phenomenon which is prevalent in Spanish (as a so-called "Pro-Drop Language"), Liceras is in a somewhat disadvantaged position. This, however, is only a small part of the whole picture, and I do not wish to make too great an issue of it. In this context, however, it should be noted that Liceras discusses the Contrastive Analysis Hypothesis in terms of strategies as well (Liceras 1986 [115, pp. 4-5]).

At another point in this dissertation, I will show how a phenomenon I originally referred to as the "form-function" constraint (Johnston 1985 [94]) can be adapted to my own observations about certain kinds of avoidance of multi-functional forms, using the machinery described by Andrews. This does away with the problem of "strategies" altogether.

Let us now return to our analysis of Liceras, who makes the point (Liceras 1986 [115, p. 4]) that:

> According to Kellerman (1977) linguists are only concerned with the analysis of the material produced by the learner, and they can only deal with formal causes of errors; psycholinguists, on the other hand, are concerned with the efficient causes of interference, but—Kellerman says—'they too must initially derive their hypotheses from the observation of language data.'

Liceras further observes (Liceras, 1986 [115, p. 5]) that:

> In fact, formal and efficient causes are interrelated and it is not any easy task to differentiate what the learner knows from what he does or how he does it.

This difficulty is due in part, I believe, to the general lack of interest in the performance aspect of the competence-performance distinction within the framework of Government and Binding. As we shall see, this is an area with which Processability Theory is much more comfortable.

This class of problem surfaces again a short time later, when (Liceras, 1986 [115, pp. 10–11]) Liceras comments that, among other things, production and parsing mechanisms "do not determine WHAT the actual grammatical knowledge may be, but rather HOW it is acquired". This is somewhat of a sticking point for her. However, if one takes the view enunciated by Fodor [98] and echoed by Pienemann (1995 [142]) that "performance shapes competence", you have a ready-made heuristic for going some distance in answering the question as to how acquisition determines competence. Readers are promised that they will be provided with an example of this when we come to discuss the author's own findings.

Liceras selected forty-five adult learners of Spanish. These were divided into three groups: beginners, intermediate and advanced. All of the subjects

were speakers of English. Spanish, however, was not the only language they were learning or had learnt. All of the subjects had also studied French, and some of them knew Italian or Portuguese. As White notes (White 1989 [181, p. 129–130]), "This other language knowledge is potentially problematic, in that French, Italian and Portuguese share the prohibition against preposition stranding, so that if L2 learners of Spanish reject it, one cannot be sure that this is because of something they have noticed about Spanish, or because it is something they have learned with respect to their other non-native languages". As well as the subjects for the study, Liceras had a small control group, consisting of five native speakers of Spanish.

As mentioned, Liceras looked at (among other things) relative clause formation involving pied-piping and preposition stranding. White's summary of Liceras' results for this is as follows:

> Subjects' knowledge of piedpiping and preposition stranding was tested with a translation task and a grammaticality judgment task. Subjects were asked not only to judge sentences but also to correct and translate them, so as to ensure that the relevant properties of the sentences were being judged. There were only two preposition stranding sentences in the judgment task so results must be interpreted with caution. The beginners were much more likely to accept stranding than the intermediate and advanced groups. 43% of the beginner's responses to the stranding sentences were acceptances, in contrast to 4% for the intermediate group and 3% for the advanced group. In the translation task, the same trend was apparent, but to a lesser extent. There were six preposition stranding sentences to be translated from English to Spanish. 20% of the beginners' translations of these made use of stranding in the L2, in contrast to 1% for the intermediate group and 1% for the advanced group. (White 1989 [181, p. 130])

As White notes:

> These results suggest that contrary to Liceras' claim, the early interlanguage grammar is influenced by marked structures from the L1; however, her claim that marked L1 structures are not persistent is supported.

Thus far, then, the results are mixed. Liceras also looked in considerable detail at the complementizer system of her subjects' interlanguage grammar. As noted above, Liceras utilizes the Extended Standard Theory version of Transformational Grammar. This version postulated the existence of "filters" which operated universally to prohibit certain sequences of words (Chomsky and Lasnik 1977 [33]; Baker 1979 [12]). (As White notes, these

filters are "negative constraints" [p. 130], and in this regard are similar to the principles of Government and Binding, of which they are the precursors). Like the parameters of Government and Binding, these filters have settings: they are either on or off. Liceras is particularly interested in what is termed the "Empty Complementizer" filter, as it is off in English but on in Spanish. That is, you can say:

The man [that] I met yesterday was called Peter.

in English, which permits optional deletion of the complementizer, while in Spanish, the only possibility is:

El hombre que conoci ayer se llama Pedro.

since, the sentence:

*El hombre conoci ayer se llama Pedro.

is totally ungrammatical. In regard to this filter (sometimes represented as *[e] or [e]), Liceras predicted that native speakers of English would not transfer the marked option of an empty complementizer from English to Spanish. To borrow again from White:

> The results from her judgment task were as follows: 49% of the beginners responses, 25% of the intermediate and 9% of the advanced group were acceptances of empty COMPs, suggesting that the marked setting of the filter was operating in the early IL (White 1989 [181, p. 131])

White does acknowledge, however, that "Her hypothesis was better supported by the translation task: where the English stimulus sentence to be translated into Spanish included an empty COMP, subjects nevertheless supplied a complementizer in Spanish. Only 8% of the responses of the beginner and the intermediate groups gave sentences with empty COMPs, and only 1% of the advanced group's responses were of this type" (White 1989 [181, pp. 131–132])

As we shall see, there is a possible explanation for this discrepancy. It is also worth noting at this point that these findings are not from production data, and therefore do not tell us how the subjects would perform in a discourse situation.

Indeed, this point is raised by White herself. She notes that variability across tasks is quite common, and that some researchers (Ellis 1986; Tarone 1988 [175]) have proposed that such variability should be incorporated into a theory of linguistic competence (White 1989 [181, p. 133]). White rejects these proposals on the grounds that the way in which a speaker uses his

or her internalized knowledge of a language can be affected by many other factors, and that "these factors do not rightly belong in a theory of linguistic competence but rather in a theory of linguistic performance (see Gregg 1989, Sharwood Smith, to appear, for further discussion)" [181]. White then goes on to point out that a competence-performance distinction can account for some of the task differences in Liceras' results [181].

> In the situations... studied, the marked forms are optional; that is, a language allowing a marked structure also allows the equivalent unmarked one. Certain tasks will offer the learner the opportunity to use either the unmarked form or the marked form. It is possible that a learner's linguistic competence might include both marked and unmarked forms but that he or she might prefer the unmarked form. Some tasks give a learner a greater opportunity to exercise such preferences: a translation task, for example allows some leeway in how a particular structure is to be translated. Other tasks are intended as a means of forcing preferences to be set aside: in a judgment task, learners are asked what is in general possible, rather than what they would prefer to use on a particular occasion. Thus, certain tasks may give the impression of supporting a hypothesis more than others; the tasks where the subjects had some choice as to what structures to use suggest that there is indeed a preference for unmarked forms, but the tasks which force a decision about marked forms suggest that these are not excluded from the ILG (White 1989 [181, p. 134])

This seems to be a very reasonable position to take, even if its implications for Liceras' predictions and results are negative. Once again we can only lament the lack of spoken production data in these kinds of study. After all, the overwhelming majority of language use involves spoken production, and this implies that it is this mode of language use which most urgently requires exploration. [19]

Liceras' conclusions are as follows (Liceras 1986 [115, p. 180]):

> The initial hypotheses was that marked L1 rules would not cause permeability while L2 marked rules would. This hypothesis was based on the assumption that markedness should have psychological reality and, consequently, should be reflected in the way learners project their NNG. The hypothesis implied that markedness should be defined relative to L1 and L2. It also implied that there is a relationship between markedness, permeability and second language learning difficulty.

[19] Production data was used by Clahsen and Muysken but, in not providing a distinction between performance and competence, they effectively nullified this aspect of the data

As we have seen, these are not borne out in an unambiguous way, and Liceras would be the first to point this out. As regards the three hypotheses cited above. Liceras states that these three notions have been "used in the literature in a variety of ways" and goes on to specify her own interpretation of them (see below).

However, one very important claim made in connection with these notions, and clearly affected by their controversial status, is not addressed. This is that markedness should have psychological reality. Psychological reality, or the lack of it, is an empirical issue in speech processing and its presence or absence is amenable to relatively direct testing. Markedness, on the other hand, is a concept which has to be defined in terms of linguistic competence, and any theory which utilizes a competence-performance distinction will have to allow that competence phenomena are not necessarily realized in any directly mappable way in performance. There is nothing axiomatic about the claim that markedness—however it is defined—ought to have psychological reality: this is an empirical issue and should be investigated as such in the most direct manner possible. It might be argued that a study of interlanguage behaviour is one way of doing this, but, given the complexity of interlanguage phenomena and the lack of general agreement on many of its characteristics, this is an argument that is fraught with peril: there are just too many variables involved, some of which Liceras herself identifies, to permit this kind of study to throw any but the most diffuse kind of light on concepts such as markedness. This class of approach is just too circuitous, and, in consequence, it ends up running the risk of closing the loop altogether and becoming tautologous—in having finally to make implicit assumptions about certain phenomena when the stated aim is to investigate whether these phenomena justify such assumptions in the first place.

There are a number of concluding remarks to be made about the findings Liceras reports on.

First, her discussion of markedness (cf. [115, pp. 26–32]), in terms of different treatments of core and periphery illustrates that there is a very considerable degree of difference, and even perhaps confusion, in the definition of these notions once phenomena from particular languages are taken into account. This is a serious problem because the core is meant to constitute Universal Grammar: if it cannot be identified it can hardly be universal. Furthermore, interpretations of what is marked or unmarked vary greatly and, in addition, even straddle the core/periphery distinction—which muddies the situation even more.

In Liceras' account of her theoretical basis, there seems to be a good deal of vagueness about the relative weight of different principles and strategies, and it is difficult to see how this problem can be resolved. In accounting for the behaviour of mature languages, Chomskyan theory relies heavily on the interaction of an ensemble of sub-theoretical components to render

inevitable a particular outcome. Whatever the degree of success of the theory on the level of mature languages, it is not difficult to see that in the case of interlanguages, where the number of possible variables is greater, this approach is in serious difficulty of breaking down.

On the question of ascribing what is due to Universal Grammar, and what is due to the more mundane process of transfer, Liceras (Liceras, 1986 [115, p. 101]) writes "if by systematic variability is meant parametric variation with respect to a general abstract rule or set of features, then a model of nonnative grammar should account for that variation. Taken in that sense, systematic variability and permeability refer to the same phenomena". This is tantamount to conceding that it is impossible to distinguish between the two processes. And Liceras is not the only researcher in this situation [102]. (In addition, since the term "permeability", as employed by Liceras, is really a synonym for transfer, the whole enterprise of distinguishing between markedness and transfer becomes meaningless since it amounts to no more than a trivial change of nomenclature).

On learner-perceived distance, Liceras (Liceras 1986 [115, p. 103]) has the following to say: "Overall, the results indicate that perception of language distance does play a clear role with respect to preposition stranding and empty COMP structures on the one hand, and the use of the complementizer on the other. Perception of language distance also plays a role in the case of prepositions and verb arguments, thus affecting the results for positions involving [PP] structures. However, it is not consistently associated with the use of [NP] structures, which appear to be produced and/or rejected with no apparent logic". Learner-perceived distance is something which is very difficult to measure, and Liceras does not indicate precisely how she identifies it, much less how it might be quantified. But even if it were the case that there was some reliable instrument for capturing this variable, the variable itself behaves—at least in regard to one major grammatical category—in an apparently haphazard way.

In regard to distinctions between complementizer placement in English and Spanish, Liceras (p. 163) has this to say: "Looking at [the] results from the point of view of the language-specific/language-neutral dichotomy, the following overall distinction can be made on the basis of Baker's proposed complementizer structure":

TYPE ONE LANGUAGES	TYPE TWO LANGUAGES
[*comp*]	[*e*]
[*PP*]	[*NP*]

She then concludes that "The results of this study clearly support this distinction for [comp] and [e], but not for [PP] and [NP]. They also show that relativized positions which contain the complementizer "that/que", and are neutral in English and Spanish do not always appear at the bottom (easy end) of the implicational scale" [115].

Once again, we are looking at results which could be due to chance, since only two of the four predictions made came up with the anticipated results. This is, alas, pretty much par for the course, and—I reiterate—Liceras is unfailingly straightforward in her presentation of such findings: there are no *post-hoc* attempts at beautification in her reportage.

It would seem that some of the complexities in Liceras' findings come from a failure from within the framework she utilizes to distinguish clearly between concepts such as the initial state, subsequent development and the motor for this development, and variation. As I hope to show, there is a theory—namely, "Processability Theory"—which provides much clearer definitions of development and variation, and these have now been extended to Spanish. Ongoing work promises to clarify at least some of the issues that surround the questions of the initial state and transfer, as well as providing some insights into the motor of development and related phenomena, such as fossilization. Once again, to her credit, Liceras approaches such concepts as the latter from a primarily linguistic point of view. However, in the case of fossilization, for example, she provides no initial definition, merely accepting the given, and, as I have shown elsewhere, very muddled version of that notion (cf. Johnston 1994; Johnston, forthcoming [95]).

In conclusion, White (White 1989 [181, p. 135]) writes that "Liceras' hypotheses are to be welcomed because she attempts to integrate properties of the L1, the L2, and markedness theory, i.e. she recognizes that many different influences interact in second language acquisition. It seems, however, that there are problems in using markedness to predict when transfer will and will not occur, just as there were problems with using it to predict acquisition sequences. Despite Liceras's *sic* predictions, marked properties did transfer, especially in early stages. Unmarked properties sometimes failed to transfer, and marked properties of the L2 input were taken into account".

This is, I believe, a fair summary of Liceras' results in the 1986 study, and I have nothing to add to it here, except to say that there are well-defined stages of acquisition for Spanish as a second language in Johnston (1995 [97]). In terms of these stages, which are for oral production and were formulated on the basis of corpus data, the phenomena which Liceras deals with—for relative clause formation, at any rate—are only likely to appear in a target language form near the end of the stages described. These phenomena are quite complex, and Liceras makes a very praiseworthy attempt at investigating them. The fact that her predictions yielded mixed results *is* indicative of problems in the model she employs, but nevertheless needs to viewed in the context of the magnitude of her task, which was very great.

2.4 To Criticize the Critic

Lydia White has already been cited extensively, and we have heard what she has to say about a number of studies by her colleagues. Let us now look at some work conducted by White herself.

White (1989 [181, pp. 144 ff.]) points out that "even given the existence of principles in UG, it is still possible... for the child to arrive at incorrect hypotheses for the target language". She observes that for a target language grammar to be achieved that these incorrect hypotheses "must at some point be disconfirmed" and that, if negative evidence is not available, this can only happen on the basis of positive evidence.

To illustrate her claim, White discusses three different kinds of incorrect hypothesis and the means by which they can be disconfirmed. White notes that "These examples do not depend on UG" but that "the same issues arise in the context of parameters" (White 1989 [181, p. 142]). The author has had very little to say about "parameters" so far, but he begs the reader to bear with him for the moment, since they will be discussed shortly.

The first hypothesis discussed by White involves what she calls "under-representation" (White 1989 [181, p. 142]). What happens here is that a first language learner has an internalized grammar "X" which generates some sentences in the target language "Y", but fails to generate other sentences of "Y" because it is "less general" than the grammar of "Y". Hence the "under-representation" mentioned above. As White observes "Positive evidence can bring about change from X to Y". An example of this involves the verb "see", which can take both noun phrase complements, as in:

Mary saw John.

and sentential complements is in:

Mary saw that John was tired.

White argues that "learners do not necessarily acquire all the complements of a particular verb at once" (White 1989 [181, p. 142]), and that a learner with grammar "X" might only produce sentences where "see" has a noun phrase complement. This grammar could be extended when the learner comes across sentences with sentential complements and begins to produce them. Thus grammar "X" has evolved into grammar "Y", with the driving force being positive evidence in the input. (Actually, there is a great deal of speculation about what causes a grammar to evolve, which Steven Pinker (1984 [145]) calls the "extendibility problem". In point of fact, we do not know why a learner bothers to extend his or her grammar at all, and, while first language learners generally do comply in this regard, many second language learners do not. We shall return to this question later.)

The second kind of incorrect hypothesis discussed by White "requires that a form or rule in one grammar be replaced by something else" (White 1989 [181, p. 143]). A case of this kind could be found in a learner who overgeneralizes the English past tense marker "-ed", producing forms like "*goed*" and "*bringed*". These forms can be eliminated on the basis of positive evidence that "*went*" and "*brought*" are the correct forms for the past tense.

However, there is, as White observes (White 1989 [181, p. 143]), a problem with such a claim. This is that learners might act as if they were in the situation outlined for the first hypothesis and "think that *brought*... represents an additional past tense form", failing to consider the possibility that irregular forms are obligatory. (This is the situation for some speakers of English with the forms *fit* and *fitted*, where *fit* can be both present and past, so White's point is quite plausible). As White notes, "Many researchers therefore suggest that learners are guided by a Uniqueness Principle, which says that any particular semantic concept will have only one syntactic or morphological realization" (Berwick 1985 [14]; Pinker 1984 [145]; Wexler 1981 [54]). Such a principle would force learners to make a choice between what they get in the input and what they themselves produce, thus eliminating the non-standard forms. Or they might get negative feed-back, although this does not normally appear to be the case. [20] Negative feed-back here would produce the same effect as the Uniqueness Principle.

Whatever the case may be, there is still a problem, since neither negative feed-back nor the Uniqueness Principle have undisputable epistemological status. Indeed, in the realm of second language acquisition, this is a very long way from being the case, although we will encounter a phenomenon called "Morphological Blocking" (Aronoff 1976 [7]) which bears some resemblance to the Uniqueness Principle, and which provides a formal account of why a particular form in the lexicon is chosen in place of another, with the mechanism for this choice being a very precise account of specificity, and the structural conditions which form a framework for it [6].

To continue with our discussion of White's research, the third type of incorrect hypothesis is, in her words, "a more problematic kind of of overgeneralization" (White 1989 [181, pp. 143-144]). What happens here is that a learner with grammar "X" uses rules or forms that are not correct for grammar "Y" and therefore have to be eliminated. This differs from the previous case in that the rules concerned must be extirpated entirely: they are not replaced by anything else. An example of this situation would be the following (Baker 1979 [12]):

The child seems to be sleepy.

The child seems sleepy.

The child happens to be sleepy.

[20] That seems to be the consensus in the literature.

*The child happens sleepy.

Here we have a case where the copula can be optionally omitted after some verbs but not after others. As White says, "It appears that explicit negative evidence would be required to draw to the learner's attention to the fact that sentences... [like the final example above]... are not permitted. This is precisely the kind of evidence that is not reliably available to language learners" (White 1989 [181, p. 144]).

There are proposals which circumvent this putative need for negative evidence. Baker (1979 [12]) suggests that, in some sense or other, children are "conservative" learners, and generate hypotheses which are strictly compatible with the input. More general hypotheses must be motivated by appropriate data in the input. Since sentences like the ungrammatical one above are not produced [21], there will be no necessity for negative evidence to prohibit them. Baker's proposal runs into problems, however, since, as White notes, "children do... make generalizations that go beyond input". Indeed, one of Chomsky's arguments for Universal Grammar is that, in producing novel utterances, both children and adults do this all the time (cf. Chomsky 1967 [65]). It has been suggested that Baker's conservatism principle would have to be refined, so as to exclude certain generalizations while allowing others (Bowerman 1987 [118]; Pinker 1984 [145]). But it is unclear at this stage as to how this could be done.

Allowing that there are some problems to be overcome, we will proceed with our exposition. While conceding that versions of the Uniqueness Principle and conservatism exist in domains outside of Government and Binding—for instance, Clark (1987 [119]), O'Grady (1987 [131]), Slobin (1973 [64])—White espouses a version of the conservatism hypothesis which has been formulated within Government and Binding: this is the "Subset Principle" (Berwick 1985 [14]; Wexler and Manzini 1987 [121]; White 1989 [181, p. 145]). This, it is claimed, "can account for how a learner picks the correct value of a parameter in circumstances where the input data are ambiguous" (White 1989 [181, p. 145]).

As White puts it, "The Subset Principle is a particular attempt to guarantee that L1 acquisition can be achieved with positive evidence only". For the so-called "Subset Condition" to be met "one must consider two or more grammars which happen to yield languages which are in a subset/superset relation, i.e., the grammars generate the same subset of sentences, and one of the grammars generates additional ones".

Thus, "The grammar that generates the sentences X also generates the sentences Y. Y is a proper subset of X. The learnability problem is as follows: Y sentences are compatible with two grammars, the grammar that generates Y and the grammar that generates X" (White 1989 [181, p. 146]).

[21] except by linguists

White then outlines a scenario in which a learner is acquiring a language containing only sentences for grammar "Y" but formulates one or more rules for grammar "X". The result of this scenario is that the learner will produce overgeneralizations for which there is no positive evidence to disconfirm, since "Y" has no rules for generating these deviant constructions.

According to White, "The Subset Principle overcomes this learnability problem in the following way. It can be seen as an instruction to the learner: given input which could be accommodated by either of two grammars meeting the Subset Condition, the most restrictive grammar consistent with this input should be adopted" (White 1989 [181, p. 146]). For a learner of "Y", this is sufficient for a correct grammar of that language. In the case of an acquirer of "X", there will be "positive evidence of X sentences...[to show]...that the grammar yielding the subset is too restrictive" (White 1989 [181, p. 146]). So a learner of "X" will switch grammars and adopt the superset grammar.

White also notes "that the Subset Principle is neutral about whether an acquisition stage will be found during which the child learning an X language nevertheless exercises the Y choice", adding that in the context of positive evidence "it could be that the switch to the superset grammar will occur immediately" (White 1989 [181, p. 146]). This constitutes a potential problem for predictions about sequences of acquisition, but for the moment I will disregard its implications: they will be reviewed later.

Very importantly for White, "The Subset Condition provides another definition of markedness". In this, it "follows the tradition...that marked values require positive evidence to be set" (White 1989 [181, p. 146]). A subset language is the product of an unmarked initial assumption, while a superset language requires positive evidence for the generation of its grammar. But then, *all* language acquisition requires positive evidence of some kind, so we need a very precise definition of what constitutes a set relation if we are to continue.

White believes that parameter theory (of which more later) provides this, in that "Different values of certain parameters of UG generate languages meeting the Subset Condition" since "they yield languages which are in a subset/superset relationship" (White 1989 [181, p. 146]). Three such parameters are "Adjacency of Case Assignment", "Configurationality" and the "Governing Category Parameter". As White notes, "it is not the case that every parameter of UG yields languages meeting the Subset Condition. The Head-position Parameter, for example...does not do so; there is no way in which head-initial languages are either a subset or a superset of head-final languages". [22]

One problem with this whole analysis which I will note but not pursue is

[22]What about "mixed" languages like German? Do they belong to Bertrand Russell's elusive "set of items which are not members of any set" ?

that the Subset Principle tacitly assumes that children are born with some kind of innate concept of set theory; to say the least, this is an extraordinary and contentious assumption and no evidence whatsoever is presented to support it. The computational burden such an assumption entails is simply enormous. Since the Subset Principle does not appear to apply in any particularly useful way to second language acquisition the burden of proof that it exists at all lies wholly and squarely on White, and on this point she is not forthcoming.

Markedness is a concept which we have considered at some length already, and, as as been noted, it is not unique to Government and Binding. Indeed, some researchers—such as Mazurkewich—have used both the statistically based Greenbergian concept of markedness and the theory-dependent Chomskyan version together, despite the fact that they are, to all intents and purposes, mutually incompatible. White is certainly not guilty of this kind of theoretical confection, but, as we shall see, there are certain problems with the Subset Principle which may be sufficiently serious to disqualify it as an arbiter of markedness, at least as far as second language acquisition is concerned. For the moment, we shall leave these in abeyance and continue with the matter to hand. In this context, it is worth pointing out that, for White this alternative definition of markedness—where "unmarked and marked phenomena will always be in a subset-superset relationship"—overcomes a problem for versions of Government and Binding which use the "core-periphery" distinction to define markedness (with the "core" being unmarked) and run into difficulties in determining what belongs to which.

So far, so good; but now problems begin to crop up. As White observes, "the learner must somehow know which parameter setting results in the subset language" [181]. There are at least two ways in which this could happen:

1. It is possible in UG that parameters are indexed for markedness, and that the parameter contains the necessary information.

2. It could be that the Subset Principle "is sufficiently powerful to compute the possibilities each time it is faced with input", and that markedness does not have to be built into UG.

These proposals are mutually incompatible, and it is mandatory that one or the other be chosen. The second of these puts an enormous computational burden on the learner (Fodor and Crain 1987 [49]). However, White finds "certain attractive features" in it. This is because it allows for the possibility that UG and learning principles occupy separate "modules", and that these "no longer interact effectively in second language acquisition". Thus, differences between first and second language acquisition may be accountable for by the fact that the Subset Principle is not accessible to adults, who might not then be capable of calculating which settings of

certain parameters are marked and which are not, with the result that they sometimes make wrong choices about them, whereas this is not the case with children. This may or may not be the case, and it is clear that a number of very debatable assumptions underlie the position we have arrived at. Once again, however, let us continue.

In her discussion of the Subset Principle, White outlines a number of possible positions for second language acquisition research.

1. The Subset Principle may work for L2 learners in exactly the same way that it does for L1 learners. This she terms the *subset* hypothesis.

2. The Subset Principle is not available in second language acquisition, but where a parameter in the L1 generates a superset grammar, this will apply for the L2 as well, regardless of whether or not it is incorrect. This, for reasons which are obvious, is termed the *transfer* hypothesis.

3. Somewhere between the two positions outlined above, there is a third point of departure. As White puts it, "Some parameters have more than two values meeting the Subset Condition. L2 learners might pick a value which is neither that predicted by the Subset Principle, nor that found in the L1". This would be a possible expression of UG, even if it was incorrect for the L2.

White notes that the transfer hypothesis and the subset hypothesis "make potentially opposing claims whenever one has binary parameters meeting the Subset Condition", since it could be that the learner is choosing an unmarked subset value, or simply transferring properties of the L1. As White puts it, "It is therefore important when investigating the potential operation of the Subset Principle to look at cases where the subset and transfer hypotheses make different predictions, i.e., where the L1 value generates the superset language and the L2 requires the subset" [181].

One such case which meets these conditions is the operation of the so-called "Adjacency Condition on Case Assignment". Briefly, this requires that an NP receiving case be contiguous with its case assigner. Thus, in English, one cannot say:

*Mary does slowly her homework.

although there are a number of other positions which the adverb can occupy, such as the beginning or end of the sentence. In French, on the other hand, one can say:

Marie fait lentement ses devoirs

just as it is possible to say:

Maria hace lentemente sus deberes

in Spanish. These differences suggest that the Adjacency Condition is subject to parametric variation. In the light of this, Chomsky (1986 [31]) has proposed the settings "[+ strict adjacency]" and "[- strict adjacency]" for the parameter in question, with the first value operant in English and the second in French. The unmarked value is, according to Chomsky, the stricter setting. And this setting is only abandoned on the basis of evidence such as that given above for French and Spanish.

White investigated the behaviour of Francophone learners of English with respect to the Adjacency Condition. In this situation, the subset hypothesis predicts that the [+ strict adjacency] setting will operate, since it is the unmarked setting and there is no evidence in native speech for the alternative—marked—setting. The transfer hypothesis, however, predicts that the L1 setting will manifest itself in the L2. White favoured the latter hypothesis, and, without going into the details of her experiment (which are given at length in White 1989 [181]), this is precisely what her findings confirmed. This means that White's subjects were not acting in accordance with the Subset Principle, in respect of the Adjacency Condition.

White also enlisted the so-called "Configurationality Parameter", which either licenses or bans "scrambling" of word order in a language [181]. Here the unmarked setting is [+ configurational], since non-configurational languages permit a superset of the word orders found in configurational languages. Japanese is considered—not uncontroversially (Saito 1985 [154])—to be non-configurational. Zobl (1988 [133]) looked at Japanese learners of English. Once again, I will not provide the details of his experiment, as they are fully reported by White (1989 [181, pp. 154–156]). Zobl's findings were the same as White's, although some of his more advanced subjects appeared to be sensitive to the configurational status of English. White criticizes Zobl for not using a control group of English speakers, and notes that Configurationality and Adjacency have a "nested" relationship, in that the latter has no meaning if the former does not have a positive setting, and that it could be the case of the advanced speakers of Japanese that they have reset the former parameter but do not adopt the unmarked value for the latter, thereby producing a "possible" language but one that does not observe the restrictions of English. This observation is interesting, but of no particular relevance to the present discussion, which is focussed on the failure of Government and Binding to produce tenable predictions as to how, or indeed whether, it operates in second language acquisition.

In regard to the Subset Principle, White also reports on work done on the "Governing Category Parameter", but I will desist from reporting this, since the results of the four studies she discusses are consistent with the findings outlined above, namely, they "are inconsistent with the operation of the Subset Principle" (White 1989 [181]). Despite this, she suggests in her

conclusion to the chapter on "Learnability and the Subset Principle" that the kinds of overgeneralizations found in interlanguage grammars nevertheless represent "possible parameter settings". In the author's opinion, this looks like a case of hedging one's bets: there is always a post-hoc explanation for why the predicted results did not eventuate. As with the study of Liceras (1986 [115]), what is attributed to Universal Grammar, may well be nothing more than simple transfer. And indeed, proponents of Government and Binding have a great deal of trouble untwining their concepts of markedness and parameter setting from their much simpler and older counterparts. If they cannot find ways to do this, then they have very poor grounds for advancing the operations of complex and sometimes contradictory constructs in language acquisition when more obvious possibilities for covering the same ground have not been eliminated.

To conclude our review of work in second language acquisition done from within the framework of Government and Binding: it is clear that this theory is a difficult one to apply to second language learning, and that the studies we have looked at are fraught with problems, and frequently find what they do not predict. This, I emphasize, should not be taken as a dismissal of Government and Binding as a theory which can be applied to second language acquisition research; as I have said before, it is much better to have a theory of some kind on which to base research questions than no theory at all. In any case, as any philosopher of science will acknowledge, all observations are, in some way or other, theory-laden, and a "theory-neutral" investigation is really an impossibility: observations inevitably entail assumptions of some type, and observation itself can affect what is being looked (i.e. there is always an "Observer's Paradox" to contend with) [23]. Government and Binding is not my own theory of choice in linguistics, and I believe Lexical-Functional Grammar or some other type of "unification" grammar (such as Head-Driven Phrase Structure Grammar [147]) provides a more productive framework from which to operate. Nevertheless, work done within the Chomskyan definition of Universal Grammar is stimulating and valuable, and will perhaps eventually overcome some of the problems that beset it now: further developments should be anticipated with interest.

2.4.1 Epilogue

What was not anticipated in the last paragraph is that Chomsky, in his reformulation of a "minimalist" program, has effectively abandoned principles and parameters. This decision was not made, obviously, on the grounds of findings from second language acquisition research, but it has very considerable repercussions for the kinds of study reported on here. It was pointed out at the outset of this section that it is extremely difficult, if not impos-

[23] Formalized, in theoretical physics at any rate, by Heisenberg in the form of the Uncertainty Principle

sible, to substantiate in any satisfying empirical way, the existence of these now abandoned constructs, and the findings reported on here are consonant with such an observation. When one comes to consider the mechanisms of Government and Binding, and how they might work in second language acquisition, efforts to untwine exactly what is due to what—in terms of principles, parameters, parameter settings, markedness and transfer—lead to problems that seem almost unsurmountable, as well as predictions that either do not eventuate or which may be explained in several mutually exclusive ways: thus whatever defects the "principles and parameters" approach may have had in the theoretical domain are in no way remedied when it comes to the actual problem of language acquisition itself.

2.5 Sequelae

In a certain sense much of the criticism in this section is unnecessary, since Chomsky's minimalist program has discarded many of the principles of Government and Binding, including d-structure and Move α, and, if second language acquisition researchers are to continue to rely on Chomsky for their fundamental theory, then this turn of events has invalidated a great deal of work done in the name of a theory whose principal exponent no longer believes in it. What the researchers overtaken by this development will do remains to be seen; some seem to have continued to use the overturned formalism anyway, although it is hard to see how they can justify their actions—others are trying to incorporate the "best parts" of the two theories into one, which is a self-evidently fruitless course to pursue.

In any case, the minimalist program did not come into existence because of problems with Government and Binding revealed by studies of second language acquisition, and I believe it has been a salutary exercise to examine the kinds of difficulties that the theory has had in application to studies in second language acquisition regardless of current developments and future possibilities. I trust what I have written has its own message in its own terms and is therefore justified.

2.5.1 Conclusions

I trust that the preceding discussion has supplied a reasonable idea of the theoretical orientations of the study. A report covering the number of different aspects of English syntax that the present one has been charged with will of necessity be an exploratory one in the first instance. Hopefully though, the exploration is proceeding in the right directions. Despite the very considerable amount of research that has been done on the acquisition of English as a second language by adults, it seems that there is still a distinct need for an enterprise such as the present one.

Throughout the preceding discussion of second language acquisition research one point has consistently emerged: this is the great imbalance that exists between theory and data. In the United States, where a great deal of the theorizing and discussion that we have surveyed has been conducted it has been done so on the basis of data which is far from adequate. Thus, many studies have been based on data collected through means of formal tests and written compositions. A good deal of the data that has been collected has come from a fairly restricted section of the population—namely college students—or, in the case of unelicited data, largely from one language group - Spanish speakers. There are, of course, exceptions (Huebner is a notable one) but they are not numerous. The fact is that there appears to be no extensive corpus of spontaneous speech data from groups of adult learners of English of low proficiency, little experience of formal instruction and non Indo-European language backgrounds in existence. If it achieves nothing else, the present study will help to rectify this.

Chapter 3

Methodology

3.1 Study Design

3.1.1 Basic Considerations

As stated, the purpose of the project was to describe the processes whereby the rules governing given aspects of the syntax of English are learnt, and if possible, to determine what sorts of universal constraints govern the sequence in which these rules are learnt.

In linguistic studies there are two main approaches to the collection of data. These approaches reflect the two main notions of how to approach the question of linguistic universals.

One approach involves the collection of data from many separate sources, on the assumption that in a sufficiently sizeable and comprehensive sample universal patterns will emerge. In theoretical linguistics, this approach is typified in the typological work of Greenberg, *et al.*, and in the work of the so-called universal grammarians, such as Keenan and Comrie [101].

The other approach involves the detailed study of only a few individuals, or even a single individual, on the assumption that the regularities in language are such that a properly conceived and oriented study can always proceed from the particular to the general given that the grammar of any individual is an independent and total system whose structure must necessarily be governed by immutable linguistic universals. This philosophy is exemplified by the many different schools of transformational grammar, where the informant was often the linguist himself, and the choice of language (generally English) was considered to be largely immaterial.

A considerable amount of debate has been generated over the issue of what constitutes good linguistics and good linguistic data, with proponents of the first approach arguing that restricted data bases of the type frequently used by transformational grammarians were apt to lead to the promotion of subjective judgments into assertions of fact and to result in the confusion of the genuinely universal with the language-specific or the merely idiosyn-

cratic, while proponents of the latter approach argued that attempts to find common denominators in an overly extensive data base would preempt the deep study required to lay bare the basic structures of language and result in nothing more than statistical generalizations of a superficial kind [29].

Within the study of second language acquisition itself, these two basic positions obviously have a different status. Nevertheless they are mirrored or manifested to some degree in the form of which choice to make between the two main forms of data base available to the researcher. The alternatives here are basically either the detailed study of an individual grammar as it develops over time—that is, the longitudinal study—and the attempt to construct a picture of an evolving grammar through the study of a number of speakers at what appear to be different stages of development—as the cross-sectional study. Fortunately, since there is no real tradition of linguists studying themselves in the process of acquiring a second language, and since learners' intuitions about their own grammars seem to be considerably vaguer and more unreliable than those of native speakers, this choice has not developed into a theoretical dichotomy [157]. In fairness to Robert Bley-Vroman [19] and Dick Schmidt [162] it should be said that they have, respectively, made excursions into these areas; nevertheless, they are not the *loci* of current debate about data collection in second language acquisition, where corpus data—in some form or other—prevails.

While the longitudinal study possesses the obvious virtue of psycholinguistic coherence, in that a single system is the object of study, it is generally recognized that it is difficult within the context of such a study to sort out what is universal, what is variable in some largely predictable fashion, and what is simply unique to the individual in question. It is evident that these questions can only be decided on the basis of larger samples. Of course, these samples need not be constructed with the emulation of a longitudinal study in mind. In fact, the ideal data base for studies in second language acquisition might simply be a concurrent series of longitudinal studies, which could be cross-referenced as the need arose. Unfortunately, in practical terms the resources required to carry out a large number of longitudinal studies are simply enormous, as projects like the *ZISA* undertaking have shown [36].

With the ideal possibility of multiple longitudinal studies eliminated for practical reasons, the cross-sectional study comes back into contention. Thus the first decision for the present study was which of the two main data bases to build upon.

3.1.2 Study Type—Longitudinal versus Cross-Sectional

This question was settled largely in favour of the cross-sectional study for the following reasons:

1. The purpose of the study in terms of its informational goals was to achieve a high degree of generality, even if this imposed restrictions on the detailed study of individual grammars. The possible invalidity of a study based on only a small number of informants was a very important factor in determining sample size. Where necessary, possible or appropriate conclusions could be followed up more intensively. And, indeed, as this exposition progresses, it will become clear that such contingencies contributed to the architecture of the study.

2. The intensive nature of longitudinal studies makes them much more difficult to administer in the data-gathering phase. A well constructed longitudinal study would require an interview to be conducted with each informant every three to four weeks. This is something which is difficult to achieve if there is no sustaining social relationship between the investigator and the informant, who would otherwise quite justifiably come to feel that his or her privacy was being seriously invaded. With only a single person engaged full-time in the study the possibilities of maintaining the required level of contact with more than one or two informants would have been very low.

3. The difficulties outlined in (2) above are multiplied by a further factor. This is that at the beginning of a longitudinal study it is difficult to predict which informants will make the most rapid progress, and which informants are likely to remain accessible and co-operative over the period of time designated for the study. This is particularly so when dealing with people who are in the very unstable personal situations that are the lot of the newly arrived migrant. It becomes imperative to start any longitudinal study or group of studies with a considerably larger sample than the projected target. Thus the difficulties already mentioned are increased by a factor of two or three. To provide a concrete example: in the present study, to obtain just two interviews with each of twenty-four informants within the range prescribed by the study design it was necessary to interview some thirty-five people.

Accordingly, a decision was taken to make the foundation of the project a cross-sectional study. As already indicated, the cross-sectional study is essentially meant to be the simultaneous (synchronic) emulation of a diachronic process—that is, one which would normally unfold over time. The driving assumption of a cross-sectional study is that a suitable range of informants at various stages of development in their learning of English will produce a developmental map similar to that which would be obtained from the study of a single learner over some given time span. There are, of course, certain problems inherent in this approach, and any projections about diachronic development made on the basis of a cross-sectional study must remain tentative until confirmed by longitudinal data.

As Meisel, Clahsen and Pienemann point out, one problem in making longitudinal projections on the basis of cross-sectional evidence is that they involve the assumption that the process of acquisition is linear and uniform, when in fact longitudinal studies themselves have produced evidence in contradiction of this assumption [129]. The fact that not all systematic variation in learner language can be located on the developmental dimension is an additional constraining factor on the projective utility of cross-sectional studies. These limitations need to be kept in mind, but also put into perspective: in longitudinal studies there is the equally serious problem of sorting out the typical and the idiosyncratic.

In order to offset problems such as the problem of longitudinal projection and to generally enhance the validity of the cross-sectional study, it was also decided that a limited form of longitudinal study be implemented by means of follow-up interviews with a subset of the informants comprising the cross-sectional study.

3.2 Implementation of the Study Design

3.2.1 Sample Size

Having determined the basic architecture of the study—a cross-sectional base with a longitudinal superstructure—the question of sample numbers was the next matter that needed to be decided.

Given the present state of studies in second language acquisition, there are no real guidelines on what constitutes a valid numerical sample. In English, the present study is still the biggest of its kind, so far as the type of data gathered and the amount is concerned. (M. Long, p.c.). The limitations of the data used in many previous studies has already been discussed in the preceding chapter. Large amounts of corpus data are time-consuming to collect, transcribe and analyze. It is still the present author's opinion that one of the best data sets for English in the literature published is Thom Huebner's: this is seventeen hours of spoken speech collected in one hour interviews, but the study is a longitudinal one involving a single informant, and so cannot be directly compared to this study. Looking further afield, work done on German as a second language is represented by three major studies, the Heidelberg Project, the *ZISA* Project, and the Kiel Project [185]. The author has not been able to determine the exact size of the Kiel project, but data base of the Heidelberg Project consisted of two hours of interview material obtained from forty-eight informants, and that of the *ZISA* project a similar amount from forty-five informants, supplemented by a longitudinal study of twelve informants over a period of two years [36]. All three of the German projects had manpower resources considerably superior to the present one. The *ZISA* project, for example, consisted of a group of three principal academic researchers supported by other colleagues and students,

as well as some half dozen research assistants. Given the limited human and financial resources available to the present study, it compares very favourably in size with the German studies.

The determination of sample size was the result of various practical and theoretical considerations. As to the number of informants and the number of interviews it seemed desirable—where feasible—to work with figures comparable to the dimensions of the European projects cited above.

As to the question of specialization, there is always a trade-off between the demands of theoretical rigour and the demands of descriptive adequacy. The result in this particular case put the latter in the forefront of the design picture because in the author's opinion research into second language acquisition in the English-speaking world has suffered from a lack of corpus data in comparison with results elsewhere in Europe. That said, the quest for quantity has not smothered either potential for more intensive theoretical follow-up or obliterated the possibility of limited longitudinal confirmation of the cross-sectional results obtained. The ease with which the SAMPLE data were able to be used in extending the *ZISA* multi-dimensional model is one example of how the data collected in this study contributed to the process of theory-construction, and the existence of task-type data within the corpus offers another possibility for the resolution of some of the questions which are currently being at issue with this kind of data.

For reasons which will be discussed below and which required the data for the cross-sectional segment to be obtained in at least two separate interviewing sessions the number of informants whose data would finally be analyzed was set at twenty-four. This also allowed for the implementation of a longitudinal extension of the study. A figure of this kind enabled an allocation of two hours interviewing time per informant for the cross-sectional study, spread over two interviews.

There were various reasons for breaking the data collection in the cross-sectional study into two parts. One was that it would give informants who might have felt ill-at-ease in the first interview a chance to become accustomed to the interview situation, and in fact in the case of some of the Vietnamese speakers this proved to be an important consideration. Another was that a second interview would provide an opportunity for the interviewers to fashion questions designed to probe specific structures on the basis of their assessment of what had transpired in the first interview, and this too was a productive strategy. In fact, the final section of the second interview was eventually given over to a series of prompted tasks aimed at collecting information on structures which were not generally produced in the less structured discourse situation of the main body of the interview. This task-type data was not analyzed together with the less structured corpus data, and was only elicited when the target interview was complete, since it was important that the informants did not feel they were being given a test of any kind.

On a practical level, a figure of twenty-four informants also provided scope for the transcription of interviews collected for the longitudinal study. It was determined that this part of study should involve a final sample of eight informants, and that they should be interviewed up to five times in the following year. Some flexibility in the timing, and indeed the final tally, of interviews in this part of the study was prefigured, given the high probability that not all the informants would progress in their learning of English at the same rate, or even progress at all in some cases. In fact, this turned out to be the case.

3.2.2 Study Composition

Choice of Language Groups

The question of transfer or interference has been touched upon in the previous chapter. In order that this factor could be monitored in the study (and to increase its generality), it seemed advisable that the informants for the study be drawn from more than one language group. At the time the data was collected, there were large numbers of Polish and Vietnamese immigrants. The author had already done some work on learners of English from Hispanic and Turkish backgrounds [92], and while Turkish would have provided an interesting contrast with Vietnamese—on both the levels of word order and morphology—Polish morphology and phonetics also diverged quite dramatically from Vietnamese and promised to throw a good deal of light on issues relating to the influence of the learners first language. It was a promise that did not disappoint expectations.

Given that the languages selected were Polish and Vietnamese, the similarities in word order typology between other candidate languages—such as Arabic [1] and Russian—the synthetic (that is, morphologically rich) character of Polish seemed to be reason enough for not choosing more than two language groups from which to draw informants. Vietnamese was an obvious choice for one of the languages because it is phonetically and morphologically distinct from the others, and because of the great volume of South-East Asian immigrants entering the country at the time. Also, both languages use Roman script, which facilitates the acquisition of background linguistic information from informants.

While Vietnamese and Polish are both SVO in their canonical word order, and consequently similar in the way they handle many major grammatical features, such as subordination and relativization, on the various levels of organization beneath that of major sentential structures, they diverge quite radically.

Thus, on the micro-level of phonology, they are almost classically opposed. Polish, on the one hand, is a language rich in polysyllabic words, com-

[1] While Modern Standard Arabic is VSO, the numerous dialects are all SVO.

plex consonant clusters and fine distinctions between consonantal types—see for instance the oppositions represented by pairs such as */sz/* and */si/*.

Vietnamese, on the other, has an almost exclusively monosyllabic lexicon, no consonant clusters—*/tr/* is really an affricate—and a much smaller and widely spaced consonant set than Polish. This phonological opposition promised to generate (and indeed provided) much interesting data on the effects of phonetic difficulties on the acquisition of morphology, given the much closer affinity of Polish to English in this regard.

Moving up a step to morphology itself, there is once again a wide contrast between the two languages. Polish exhibits complex and irregular nominal and verbal morphology, while Vietnamese is classically analytic, relying, for instance, largely on adverbs and adverbial phrases for temporal reference, and prepositions and prepositional phrases for spatial and directional reference. In regard to these features, English occupies somewhat of a middle ground, which meant that some useful information about the relative difficulty of "adapting up" and "adapting down" was available.

In fact, since generally target-like word order comes fairly early in the acquisition process [92], these opposed characteristics, together with others of a more arbitrary kind, such as the restricted use of the copula in Vietnamese, and the lack of definite articles in Polish provided an opportunity to examine the effect of more persistent typological differences in the process of acquisition.

In addition to the morphological and phonological differences between the two languages, the two groups presented very different profiles in their conversational and interactional styles. It was not a part of the brief for this study to investigate such matters, but it became clear that in certain areas of syntax which were heavily discourse-dependent, such as the pronominal system, that these characteristics could not be ignored in any adequate description of the learners' language.

Balancing the Sample in Other Regards

The twenty-four places in the final sample for the cross-sectional study were equally divided amongst the Polish and Vietnamese speakers. In regard to other factors which might reasonably be expected to affect language learning, such as educational background, other languages, formal instruction in the target language, and oral proficiency, an attempt to build up a sample with an appropriate range of subjects and a reasonable degree of symmetry between the two language groups was made. As Tables 3.1 to 3.3 show, this was reasonably successful, with any differences between the two language groups being largely "representative" differences as far as the composition of the two communities in general was concerned.

The main imbalance in the symmetry of the study was the disproportionately large number of males in the Vietnamese sample. Attempts to

AJ – Andrzej J.
BB – Barbara B.
ES – Ewa S.
IS – Irena S.
JB – Jerzy B.
JR – Jan R.
KA – Krystyna A.
KB – Krystyna B.
KS – Krystof S.
LJ – Ludwiga J.
MM – Mieczyslaw M.
ZJ – Zygmunt J.

rectify this imbalance merely turned up more females at about the same level of oral proficiency as the ones selected, and were therefore unsuccessful. In any case, there is no convincing evidence that language learning in males and females is particularly different (although females do tend to be more advanced in the matter of change in their native language) [10], so this imbalance is unlikely to be significant.

3.2.3 Naming Conventions

Before examining the statistics on the informants chosen for the final samples it might be worthwhile to provide a brief explanation of the conventions employed for referring to informants and interviews.

In accordance with the standard practice in linguistic studies, the informants are referred to by their given names. In the case of the Polish speakers, an initial was added because in the preliminary sample there were several cases of duplicated christian names (there are still two Krystyna's in the final sample). For reasons of space, the names of the Polish informants had to be abbreviated in Tables 3.1 to 3.3. The following list provides a key to these abbreviations:

The convention in regard to naming interviews is quite simple. Either the name (in the case of the Vietnamese informants), or the initials (in the case of the Polish speakers) is followed by a period and a number. The number refers to the interview. Thus a reference to **aj.1** is a reference to the first interview with Andrzej J., and a reference to **canh.2** signifies the second interview with Canh. These conventions are followed throughout the study.

3.2.4 Relevant Statistics

The information given in Tables 3.1 to 3.3 is mostly self-explanatory. However, a few general comments are in order.

1. The Vietnamese system of education appears to be similar in structure to our own, as regards primary and secondary divisions, and the figures given for years of education are therefore quite straightforward. The Polish system, on the other hand, is somewhat different. Their primary period is of eight years duration. After that there are various options: a four or five year secondary period which serves as a matriculation for university, a five year technical course, a two or three year technical course, which can in turn be followed by a further three year technical course, which itself can also be reached via the four or five year secondary matriculation course. It is not surprising therefore that there was a certain amount of cross-cultural confusion during the interviews about how many years of primary and secondary schooling informants, particularly those with trade certificates, had had. We can assume that it was about twelve or thirteen for these people, with a probably high degree of technical specialization after the eight years of primary preparation.

2. All but one of the Polish informants had been obliged to learn Russian at school, and this is duly noted. The status of Russian as a second language for most of the Polish speakers requires some exploration, however. This question will be dealt with in more detail in 3.4.14 of this chapter. The discussion there will also include some comments on their experience of German.

3. In the author's opinion, the distinction between formal instruction and other forms of learning, particularly self study, is not really a clear one. Although it was not part of the present study to examine in any detail those factors which conduce to learning and those which do not (in any case consider the conclusions reached in Pienemann and Johnston 1987 [130]) some of the points raised here need further clarification. Accordingly, they will be discussed at more length, in 3.4.14 of the present chapter.

3.3 Data Collection

3.3.1 The Oral Interview

The format for data collection was regular and simple. The primary means of data-gathering was through unstructured interviews. The term "unstructured" is used, of course, in a relative sense, given that all discourse forms are structured in some way.

With one or two exceptions, the interviews were conducted by two interviewers. The interviewers were in all cases trained teachers. One of the interviewers was always the researcher: he was assisted in the first round of interviews with the Polish speakers by Geoff Brindley, and in the second by Ken Singh. For the Vietnamese speakers, Marilyn Wise was the other interviewer in both rounds of interviews. The rationale behind having two interviewers was that it would provide a somewhat more open interactional framework than the intense and often interrogatorial situation which tends to develop when a native speaker has to sustain a conversation with a learner of low proficiency. It was felt that the flexibility afforded by the presence of a third person probably outweighed the possible intimidatory effects of the informant's being figuratively outnumbered.

In the case of both groups of speakers there were circumstances that tended to soften the impact of having to talk with a tape-recorder running. The Polish informants were all attending English courses at the time of the interviews, and therefore did not find themselves in a totally unfamiliar situation. The Vietnamese speakers were all interviewed in their own homes or the home of a friend, and one of the interviewers, Marilyn Wise, was already on good terms with them. Some, indeed, appeared to enjoy the contact with an Australian, and the opportunity to practise their English.

3.3.2 Format of the Interviews

There was no predetermined format for the interviews. Before the commencement of the interview it was made clear to the informants, in their own language if necessary, that they were not undergoing a test, and they were encouraged to attempt to express themselves even if they felt or knew that what they were saying was not correct. No specific schedule of questions or tasks was ever followed. There were, of course, typical questions about how long an informant had been in the country, whether they were working, what they had done in their own country, whether they had family in Australia, and so on. These were not asked in any particular order, and allowance was made for differing cultural attitudes towards the importance and possible privacy of the information requested. Generally, at some point in the interview informants were requested to provide some narrative text—for instance, to describe the circumstances of their departure from Poland or Vietnam, or what they did on a typical working day. Informants were also encouraged to talk about their plans, to express opinions and to discuss third parties, such as spouses or children. Naturally, a great deal depended on the proficiency of the informant, and on his or her own volubility. The degree of co-operation was surprisingly high, and in general informants visibly relaxed after the first five to ten minutes, probably when they realized that they would not be required to do anything other than talk.

Naturally, there were differences in the way the two language groups han-

dled the interview situation. The Polish speakers tended to be much more assertive than the Vietnamese speakers. The latter were very punctilious about formally registering a response to every conversational move made by the interviewer(s)—a characteristic which accounts for the frequency of Yeah (cf. Vietnamese Gia) [2] in their discourse, but tended to avoid either contradicting the interlocutor or indicating failure to comprehend. The Poles, on the other hand, appeared to have few reservations about saying that they did not understand or producing negative responses to propositions they disagreed with. Extended discourse from the Vietnamese speakers often gave the impression of being somewhat in the nature of a monologue. The net result of all this for the English speaking interlocutor was a sense of more direct interaction with the Polish speakers. These characteristics, together with the fact that English and Polish paralinguistic features seem to be much more similar than those of English and Vietnamese, undoubtedly rendered the interview situation an easier one for the Polish speakers than the Vietnamese speakers.

It should be stressed that the above observations are generalizations, and that there were exceptions on both sides of the fence. The apparent distance displayed by the Vietnamese speakers was much more noticeable with the less acculturated and less well educated members of the sample. With these, the first encounter in some cases resulted in an oral proficiency assessment that was quite considerably lower than that generated by the second interview. This is a point that should be kept in mind in the assessment of oral proficiency in Vietnamese speakers.

3.3.3 Recording Procedures

Since it was not intended that the study focus on the phonological aspects of second language acquisition, no attempt was made to obtain ultra-high quality recordings. In any case, given the often crowded and noisy situations in which interviews had to be recorded this would have been quite impracticable. An ordinary cassette recorder with a detached condenser microphone proved to be quite adequate for the purposes of the study.

A further consideration here is that interviewees were very possibly less daunted by the use of such a pedestrian piece of apparatus (many had superior equipment themselves) than they would have been by something more sophisticated.

3.3.4 Transcription

The interviews were carefully transcribed according to a standard format. The aim of this format was to provide a document that provided an accurate rendition of the speech of both the informants and the interviewers

[2] A conversational marker of acknowledgement.

and which was at the same time easy to read. In the interests of readability, formats involving such things as separate columns for interviewer and informant were rejected. Likewise, no attempt was made to consistently implement even a modified phonetic script: given the very different nature of the first languages of the informants this would have been a quite herculean task, as well as an unnecessary one, since the focus of the study was on syntax. Various other possible features, such as the precise timing of pauses, and bracketting of simultaneous utterances, were not implemented either, as once again the information conveyed was not germane to the main purpose of the investigation. In addition, the incorporation of such features into the transcriptions would have greatly increased the transcription time.

Finally, since the transcripts were eventually typed into a computer, the interviews would have been ultimately constrained to a line-oriented format, and the ASCII character set of most computational devices. The superscripts and expanded character sets that phonetic or even quasi-phonetic transcription demands, and the graphics that conversational analysis employs would have considerably complicated the data entry and extraction procedures.

Within these constraints, in order to capture some of the flavour of the informants non-native pronunciation, and indeed to indicate the part this played in communication breakdowns, a kind of literary rendition of the main characteristics of the informants' speech was employed.

It should be emphasized, nevertheless, that within the particular parameters determined for the transcriptions great emphasis was placed on their accuracy. A number of people participated in the transcription phase of the work, although the bulk of the task eventually devolved upon one person. Every transcript, however, was checked against the tapes by the researcher himself. The eventual system was for the transcript to be done by hand, and then typed into a computer. At this point the researcher would take a printout, check it against the cassette recording, note down the changes and enter them himself on the computer. In this way an almost entirely consistent set of transcriptions has been produced.

A number of simple but useful conventions were employed in the transcription. There are optional line numbers: these can be suppressed. Tabs constitute the margin and are preceded by the initials of the interviewers and the informant, who is referred to throughout as I. Numbers in the margin are cassette counter units, to facilitate the location of text passages on the cassette itself. All such markers are followed by tab characters and then the body of the text itself. Within the text, there are certain consistent conventions. Major intonational breaks, and pauses, are indicated by a string of three periods, minor ones by a comma. Hesitational phenomena, such as um and er, have been faithfully reproduced. Comments within the text are enclosed in square brackets. Narrative type comments—such as **[LONG PAUSE]**—are printed in upper case, while explicatory or transla-

tion type comments are printed in lower case—for instance, this time [tense] is not good. Phonetic transcriptions are delimited with slashes, to distinguish them from explicatory comments—for instance, my name is Hoa...H [/heis/]...O...A. Non-realized phonemes or sequences of phonemes are surrounded by round brackets—for instance, becau(se) I'm very li(ke). This latter convention is particularly useful, in that it allows variant realizations of the same word to be grouped together for the purposes of analysis. Thus, (be)cause, becau(se) and (be)cau(se) can be treated as the same word for lexical purposes, rather than three separate entities. It is worth emphasizing that with computer storage changes in format (should they be necessary) are generally very easy to implement.

3.3.5 Computer Storage and Analysis of the Data

There has been considerable development in the ways in which the data can be handled since the original decision was taken.

Linguistic analysis is essentially a text processing operation, and for this purpose any version of UNIX is much better than other operating systems available. UNIX type systems have built-in utilities for character set translation, string searches and substitutions, duplicate line elimination and so on, as well as such things as elaborate sort programs. Above all they possess a highly flexible command language, and command language interpreter (the shell) which enables system utilities and/or user programs to be combined in such a way that complex tasks can frequently be accomplished without the need for much, or even any, binary software development.

The move to UNIX, and the development of the various tools necessary to facilitate the research has resulted in a very accessible data base supported by a very flexible system. Data is accessible in various ways. The basic organization of the data is quite simple, with each separate interview contained in a separate file. The flexibility of the system, however, allows data extracted from these interviews either individually or in arbitrary bundles, defined, say, by oral proficiency or first language, or indeed any other feature deemed to be relevant. Data itself can be accessed by line number, page number, or some specified characteristic pattern or patterns. It can also be passed through various filters before or after the search to accommodate it to particular uses: for instance, extraneous comments or punctuation can be output or stripped away, as the case may require. Tokens of a particular structure or element can either be output for inspection or counted and only the count recorded. If counts are done, separate totals are kept for each individual interview file, and a grand total is also recorded. Global searches can be performed very quickly; most operations take no more than a second on today's PCs. Global modifications to the data (for particular processing tasks) can also be performed very rapidly.

To this end, a set of dedicated software tools was developed by the author

and enabled the analysis of the seven hundred odd pages of transcript to be conducted rapidly and accurately, from the level of the lexicon to that of the morpheme.

3.4 Evaluation of the Data

There are a number of questions relating to the status of the data which need to be discussed. These have to do mainly with the possible constraints imposed on any conclusions by the way in which the data was collected, and by the actual composition of the sample itself.

3.4.1 Parameters of the Interview Situation

The ideal data set for a study of language acquisition would include data from all conceivable types of linguistic interaction. There are various discourse models which provide taxonomies of the main types of interactional situations. For instance, one model, the so-called Three-Level Analysis, suggests that there are three levels of generality in discourse [78]. Firstly, there is the speech situation: for example, a meeting, an interview or a classroom encounter. Secondly, within the speech situation there are speech events, which are broken down into divisions such as narrative, conversation, and service encounter. Thirdly, within the speech event itself a further division into speech acts can be made. Speech acts, which are probably the best researched elements in this model, fit, in this particular model into the following categories:

Directives	getting someone to do or not do something
Commissives	making promises or oaths
Declarations	which alter some state of affairs on being uttered
Expressives	making statements of feeling or emotion
Representatives	making statements of fact or assertions

Each of these basic acts has a set of possible forms, ranging from mitigated to aggravated, and can be hedged in various ways regarding such things as speaker responsibility or accountability. Thus directives include asking, suggesting, demanding, begging, ordering, and so forth.

The Three-Level Model is given merely as an example. Similar taxonomies have been provided by exponents of the functional/notional approach, and by systemic grammarians such as Halliday. No attempt will be made here to evaluate which of the various possible models seems the most satisfactory: the point to be made is merely the variety of possible discourse situations that exists.

Clearly, data collected from an interview situation, even if it provided examples of all the different speech act types, and some of the gradated

forms possible for each act—which is itself unlikely—would only cover a subset of possible speech events and situations. The ideal data set for a study of language learning would need to provide representative examples of discourse on all three levels, and in all stages of gradation.

The practical difficulties of achieving this would, of course, be enormous. Informants would literally have to be under twenty-four hour aural and visual surveillance, recording would have to be done by radio microphone, and the amount of data to be sifted through would be simply vast.

In the absence of the resources to carry out an operation of this kind, the unstructured interview seemed the most viable linguistic and organizational alternative. Moreover, it can be argued that in the production of syntactic structures and morphological items that conversational context will merely *conduce* to certain forms; it will not make them possible if the informant has not acquired them in the first place.

3.4.2 Rationale for Choice of the Oral Interview

One of the important tenets of sociolinguistic research and language acquisition research is that unmonitored or "spontaneous" speech should provide the most direct and reliable evidence about the nature of underlying rule systems or grammars. This tenet is based both on the intuitively appealing (though not necessarily correct) equation between spontaneity and "naturalness" and on the observations of researchers such as Labov, who found that the more attention subjects (in this case native speakers) paid to a linguistic task, the less regularity there was to be observed in what they produced [149]. It should be said here that we do not know for certain whether Labov's contention is also true for non-native speakers. In particular, the status of *attention* will be different for this latter group; how it is to be defined and measured is a problem that has yet to be solved, if it is soluble at all. Observers like Tarone [175] and Krashen [104] use the term quite extensively, but without any operationalized definition that the author has been able to find.

Whatever the status of the equation between naturalness and spontaneity (it really needs to be formulated in some testable way and with some consistent and consensual meaning), these latter findings at least encourage the hunch that the most economical descriptions of a given grammar will emerge from data which has not passed through too many (conscious or unconscious) filters. If one's aim in linguistic research is to characterize some particular system or a fragment thereof, then it is obviously a desirable goal to be able to form some notion of the most compact and coherent organization that is possible within the system or subsystem in question. Cast in these terms, then, there is very definitely a case for using unmonitored speech data to build up initial descriptions of rule systems or entire grammars: in the absence of viable alternatives it is the simplest point of

departure.

3.4.3 Characteristics of the Oral Interview

Although obviously constrained in certain ways, an interview allows the participants a good deal of freedom in what conversational moves they decide to make and gives the informant the opportunity to choose the topics of conversation. This in turn means that the informant is likely to become involved in the subject matter of the conversation and to forget the nature of the task, which increases the amount of unmonitored or "spontaneous" speech likely to be produced. The interview situation provides ample occasion for at least the three main types of speech act cited above—the Directive, Expressive and Representative. On the level of the speech event, there is occasion for the important categories of narrative and conversation. While on the level of speech situation the whole interview constitutes a not atypical cross-cultural encounter, with elements of classroom or bureaucratic encounters added. A detailed exploration of the discourse features of the oral interview has yet to be carried out [151], and would be a worthwhile extension to the present study, in that a fuller understanding of this form of discourse situation would enable the data to be used for the purposes of discourse analysis.

3.4.4 Possible Effects of Observation

Given that the elicitation of spontaneous speech is a desirable goal, one of the questions that has to be asked of data gathered in interviews in the way described, is to what extent the presence of an interviewer or interviewers, and indeed, the whole environment of the interview itself, inhibits the free production of speech and alters the nature of the data.

This is obviously a difficult question to provide a general answer for. On an anecdotal level, it can be said that with some informants from both groups it certainly appeared to be the case that they forgot the tape-recorder completely after a few minutes and became engrossed in the substance of the interview itself, while with others this was not so clear. The only means of objectively settling this question would have involved comparing material from the interviews with material obtained without the informants' cognizance, and lack of resources prevented this. Fortunately, however, just this procedure has been carried out in another piece of research.

This was a study done at the University of Passau by Pienemann, and involved German rather than English [140]. Its particular aim was to determine the effect on instruction of certain aspects of German word order in Italian children aged seven to nine. What is of relevance to the present discussion is that the study involved two types of data-gathering techniques. One technique involved what Pienemann terms "linguistic interviews". These consisted of loosely structured conversations between pairs

of informants and student interviewers, who were equipped with a list of questions and some toys and pictures to get things going; these interviews were conducted in the school environment [140]. The other data-gathering technique involved the attachment of a radio microphone to the child, who then went off into his or her play environment. The children apparently did not fully understand the function of the microphone, and did not seem either disturbed by or particularly interested in it [140], so we can assume the data gathered in this way was quite "natural" and uninfluenced by any "observational presence".

Pienemann's own expectation at the inception of his study was that there would be an obvious difference between the data gathered by the two procedures. In fact, there turned out to be no palpable difference at all. Of course, the fact that the subjects in this study were children needs to be considered. The effect of this, however, is not clear. On the one hand, it could be argued that children are less "self conscious", and therefore more likely to be consistent in their linguistic behaviour, regardless of the context. (Pienemann contests this argument on the grounds that children are amenable, under certain specified conditions, to formal instruction, and that this is evidence of their capacity to monitor [140]). On the other hand, it could be argued that since they were more malleable, the children might have been expected to conform to one set of linguistic norms in the more formal situation, and another when the were at play.

The fact remains that there was no difference in the language produced in the semi-formal and the informal situation. As Pienemann has observed, while we have reason to believe in theory that there might be a difference between data elicited through unstructured or loosely structured interviews, and data collected without the informant's cognizance the empirical evidence on this point all indicates that there is no such difference [140]. This is of course an extremely encouraging conclusion for the present study, since it implies that at least the speech act and event types which occur in the interviews are representative of their spontaneous counterparts.

3.4.5 Naturalistic Data versus Mixed Data

It is generally agreed that there are three main types of learning situations in second language acquisition. These are:

1. The purely naturalistic situation, where the learner receives no formal instruction in the target language, and learns by a process of absorption.

2. The purely formal situation, where the learner's entire exposure to the target language is within the context of formal instruction.

3. The mixed situation, in the naturalistic and the formal situations are combined.

It is worth noting for the purposes of subsequent discussion that these terms are essentially behavioural, in that they describe an observable condition, and do not take into account the learner's own mental processes.

As indicated above, there is a consensus in second language acquisition studies about the importance of obtaining data on "natural" orders of acquisition. A reason for this has already been advanced—namely that natural systems appear to be the most regular and coherent. To this end it has been generally assumed that the best kind of data for the investigation of natural systems will come from "naturalistic" learners, that is, learners who have experienced no formal instruction and have merely absorbed their language from the environment in which they live and work. This seems at first glance an obvious and uncontentious assumption. By extension, however, it is also normally assumed that data from other kinds of learner is unsuited to the purpose of determining how languages are learnt naturally. While it is reasonable to suspect that there may be differences in the patterns of development exhibited by learners in different learning situations, it is not so reasonable to simply assume that this is the case. Whether or not it actually is so is clearly an empirical question, and the safest alternative—the exclusive use of naturalistic data—may not turn out to be the only possible course for a researcher. Indeed, as we shall see, the ostensibly safest course may itself lead to difficulties.

Because the learners in the present sample are learners from a mixed acquisitional background (albeit with a fairly small admixture of formal instruction in most cases) the question of whether, and if so how naturalistic, mixed and formal learning differ is of importance to the present study.

3.4.6 Possible Effects of Formal Instruction

Tables 3.1 to 3.3 provide figures on the amount of formal instruction in English received by each informant. In the case of the Polish informants at the time of the first interview, this was either four or six weeks in all cases; three of the informants had experienced some formal instruction in English before their arrival in Australia—either at school or university, or in Austria. The figures for the Vietnamese informants are somewhat more varied, although still quite low in most cases. Once again, several of the informants had been taught some English before coming to Australia—in all cases this was at school.

Because it has always been assumed that formal instruction is efficacious, the number of studies which have addressed themselves to the question framed above is not great. In addition, as we shall see, there are some difficult problems to be overcome in achieving valid research designs for studies

of this kind. Such findings as do exist have been reviewed in recent papers by Pienemann [140], Pica [139], and Long [116]. Pienemann, whose review is the most recent and takes cognizance of the other two papers, provides the most comprehensive overview of the studies relevant to this question, and interested parties are referred to his paper. The following discussion is indebted to it.

Approaches to the Question of Formal Instruction

There are three main ways of approaching the question of whether instruction makes a difference. One approach involves an examination in hindsight of learning under different conditions and combinations of exposure and instruction. We could term this approach the survey approach. The second approach involves the comparison of language produced under conditions of formal instruction with language known to be characteristic of natural acquisition. We could call this the structural approach. The third approach involves controlled intervention in the acquisitional process itself with the aim of determining if and how it can be manipulated. This we could call the experimental approach.

Survey Type Studies

A typical review of all known studies of the first type has been conducted by Long (1983 [116]) [116]. The review involved thirteen studies. These studies used both integrative and discrete point tests (of which more later) as a means of assessing the language proficiency of their subjects. Long suggests that two distinctions help to clarify his analysis. The first is a distinction between the absolute effect of instruction, on the one hand, and its relative utility on the other. The second is a sub-classification of studies according to whether or not they take into account the learner's total opportunity to acquire the language [116]. Thus, with regard to the extent of exposure and the amount of instruction five different types of learning environments were examined. The questions addressed were the following:

1. Relative utility of exposure only and the same total amount of instruction and exposure.

2. Relative utility of differing amounts of instruction and exposure in populations with the same total amount of both.

3. Effect of amount of instruction on populations with the same amount of exposure.

4. Effect of amount of exposure on populations with the same amount of instruction.

5. Effects of amount of instruction and of amount of exposure (independently) on populations with differing amounts of both instruction and exposure.

Long's analysis of these studies is thoughtful and detailed. His conclusion was that "For SLA theory and SL educators alike, on the basis of currently available studies, an answer to the question, 'Does SL instruction make a difference?' is a not-so-tentative 'Yes"'. He notes, however, that "even if ... the data on instruction have been correctly interpreted here, they are obviously not as clearcut or as 'positive' as most TESOL professionals would like" [116].

There are grounds for belief, however, that Long's conclusions are somewhat optimistic. On a head count, seven of the studies indicated a positive effect for instruction, three indicated a negative effect and three were unclear. This is hardly an overwhelming majority. Then, in terms of methodology, as Pienemann has noted, there are some serious problems [88]:

1. The informants in almost all cases were of the "mixed" type—that is, they had also had naturalistic exposure to English. This makes it very difficult to gauge the effects of their formal learning alone.

2. In those studies in which extent of exposure was a variable, quantification of this variable in behavioural terms is not very meaningful: it would only provide the roughest of guides without the mediation of psychological and other measures. Tables 3.1 to 3.3 show how loose the correlation between assessed speaking proficiency and exposure can be.

3. Given the testing instruments used in the studies (for instance, the SLOPE test) the "difference" attributed to instruction would have to be given a very specific and restricted interpretation, namely a higher degree of correctness in certain structures. Correctness and proficiency are not by any means directly equatable, as the *ZISA* project and other enterprises (including the present one) have established.

4. In addition to the above criticisms (due to Pienemann), and following on from (3) above, it should be noted that one of the likely effects of instruction is enhanced performance in instructional type activities such as tests. It would not therefore be surprising to find that those subjects who had had relatively more instruction did relatively better in tests. Long notes that "Instruction appears to be especially useful in the early stages of SLA" [116]; this is precisely when one would expect training in the specialized activity of doing tests would be most beneficial, given the early learner's lower adaptability to unfamiliar (not to say bizarre) situations such as those posed by most testing procedures.

5. In general the studies analyzed by Long suffer from what one might term "shotgun empiricism". That is, they have no real proposal to make about the possible effects of instruction and simply apply statistical tests to differing populations in the hope that they will reveal differences. That there will be differences of some kind is very likely; their significance in the absence of any coherent theoretical proposal to predict or even account for them is another matter. This is perhaps the point where the differing viewpoints of Long and the author is most evident.

For the reasons enumerated above the author is inclined to be somewhat more cautious about Long's results. This should not be taken to mean that he believes that instruction does not make a difference—merely that studies of the kind analyzed by Long are not capable of providing decisive evidence on the question.

There have been many other studies conducted since the one discussed but they tend to share the same orientation. That is, they deal in hindsight with concepts which should be explicitly postulated at the outset.

3.4.7 Other Relevant Survey-Type Enterprises

Morpheme Order Studies

Morpheme order studies have produced claims as to the effect of formal instruction on language learning, in so far as this may or may not disturb the rank order of morpheme accuracy (see 2.1.2 for a description of these studies). These studies [110] have been critically reviewed by both Pica [139] and Pienemann [88]. Both authors make the following points:

1. Results of the various studies conducted are conflicting, with two studies [106] appearing [57] to confirm the hypothesis that formal instruction had no effect on unmonitored speech, one [120] appearing to confirm the same hypothesis in regard of both monitored and unmonitored output, and another [127] appearing to disconfirm the hypothesis for unmonitored speech.

2. The two studies which disconfirmed the effects of formal instruction were in fact not properly controlled for the formal versus informal instruction variable. Thus, the entire population for one study were in fact "mixed" type learners, not purely naturalistic or formal, as methodology would demand [106], while for the other study the "naturalistic" subjects were school children, who had probably been exposed to language arts curricula [58].

3. Rank order accuracy for morphemes is hardly coterminous with language learning as a whole. Morpheme order studies do not even reveal

the processes by which the morphemes in question are acquired, much less shed light on the many other processes which constitute language learning: in particular, the assumption implicit in morpheme order studies that language learning is a linear process involving "a gradual increase in target language correctness" is not borne out by findings from longitudinal studies [88].

4. In addition to the points made by Pica and Pienemann, it should be noted that the role played by some of the data elicitation techniques (such as the Bilingual Syntax Measure) in the data collection process is an equivocal one, and may have serious repercussions for the validity of the findings even within their own terms—see 2.1.2 for further comments.

Recent Input Studies

Pienemann also reviews a recent study by Lightbown on the influence of formal input on formal second language development [164]. The results of this study are in conflict with a previous study by Larsen-Freeman as regards rank morpheme order in teacher talk and rank order of accuracy in the output of the subjects in Larsen-Freeman's study, where it was claimed that learner orders reflected input frequencies; nor was there a direct relationship between these two orders in Lightbown's own study. Thus, this study does nothing to clarify the already confusing picture that has been outlined.

3.4.8 Structural Studies

The work of Felix is typical of this approach [60]. In a study of thirty-four German high school students learning English in a purely formal setting, Felix found that "the students' utterances showed many structural features which are also known to characterize L1 and naturalistic L2 acquisition". He concludes that "It thus appears that formal instruction cannot eliminate or suppress those processes which constitute man's natural ability to acquire language(s)" [60]. Felix did find one difference between his formal learners and naturalistic learners. This was that when forced to produce structures for which they were not developmentally ready they would sometimes randomly select a quite inappropriate structure: in other words, when pressed they said the first thing that came into their heads.

Other studies conducted from within the same framework as the Felix study described above, involving phenomena as diverse as acquisition of the indefinite article [139], through phonetic substitutions [62] to an entire longitudinal study of purely formal learning [74], have produced the same basic findings, namely that in spite of consistent differences in input, the structures and systems exhibited remain very close [88].

Experimental Studies

An example of this type of study is the one conducted by Pienemann [141]. This involved ten Italian children aged from seven to nine. In Pienemann's words, "The main idea of this experiment ...[was] ...that the children's interlanguage [was] recorded before and after a period of formal instruction so that the changes in the interlanguage ...[could] ...be investigated" [141].

The theoretical framework for Pienemann's experiment was the "multi-dimensional" theory of variation developed by the *ZISA* group, and outlined in 2.1.8. Both developmental phenomena (in this case word order rules) and variational phenomena (copula insertion) were examined in the experiment.

In the case of the word order rules investigated, it was found that a given structure could "only be learned by instruction if the learner's interlanguage is close to the point when this structure is acquired in the natural setting" [141]. This finding was termed the "teachability hypothesis".

In the case of copula insertion, however, the effect of instruction was in some cases quite dramatic, with the rate of copula insertion increasing spectacularly for some of the children. A recording of one of the children made some months after the experiment, however, showed that this increase appeared to be transient [141].

Pienemann's work, while it categorically implies that some structures are unteachable at a given stage of a learner's development is actually the most positive of the three approaches in terms of the predictions it makes about the effects of instruction. Thus, if the learner is developmentally prepared, Pienemann claims that "instruction can improve acquisition with respect to (a) the speed of acquisition, (b) the frequency of rule application and (c) the different linguistic contexts in which the rule has to be applied", while "for variable features...there is evidence that instruction has a drastic effect on L2 speech production" [88].

Pienemann points out that specific evidence for his hypothesis is "so far only related to children" and that since "the cognitive structure in adults and their memory capacity is quite different from that of children, instruction might have a different effect" [88]. While this question will have to be resolved empirically, the general similarities in adult second and child first and second language learning suggest that there is a good chance of Pienemann's findings essentially holding for adults as well.

The Question of Instruction—Summary

As far as empirical evidence is concerned, then, there is some evidence and further suggestions that instruction can, under specified conditions, have a direct effect on learning. There is to date very little or perhaps no evidence that formal language learning is a substantively different process from naturalistic learning [74]. Where instruction does appear to have an effect, this

consists in the acceleration of naturalistic processes or in possibly diverting the learner towards a less simplifying (that is, more correct) variety of learner language. As regards the effect of instruction on learner language itself it seems that there are few or no substantive differences between the language of formal and naturalistic learning. This is, of course, an encouraging conclusion from the point of view of the status of the present corpus.

3.4.9 The Relation of Formal Language Instruction to Education

As is evident from the foregoing review and discussion the status of formal language instruction as a variable in second language learning is a privileged one. This privilege derives from various assumptions, of which the two most important are (1) that instruction is efficacious, and (2) that second language learning (and, by extension, received instruction) is qualitatively different from other forms of psycho-motor learning.

The previous discussion should have made clear that (1) is somewhat of an open question. As regards (2), this is also the case. A great deal of discussion has been devoted to the question of whether and in what ways language learning differs from other kinds of learning. For example, Chomsky has postulated the existence of a "language acquisition device", an entity which is in fact a virtual organ with its own genetic program, to account for the uniformity, rapidity and completeness of first language learning despite the corrupt and incomplete data on which the child has to base his or her grammatical hypotheses [29]. [3] Even if the existence of this device could be established for child first language learners, which is not to date the case, the question of whether such a device is accessible to child and/or adult second language learners would have to be settled before a satisfactory answer to (2) could be produced. To be sure, claims about the relationship between first and second language learning have been made (compare the discussion on Monitor Theory in 2.1.5 for an example), but they are at best indecisive [62], or at worst based on demonstrably false premisses [71]. As to the claims made by Chomsky and his followers, there has been an extensive discussion in Chapter Two.

Given our present state of knowledge, and allowing that (2) remains a vexed question, it might be a worthwhile exercise to suspend for the moment the privileged status *vis-a-vis* language learning accorded to formal language instruction and to consider it in the wider context of formal education in general.

[3] Recent work in neurolinguistics puts this mode of thought in the biological dark ages.

The Effect of Formal Instruction—Some Proposals

As we have seen, a defect of many of the attempts in the published literature to deal with the question of how formal instruction might influence the learning process is their failure to formulate explicit hypotheses as to the kinds of effects that instruction might have. This *ad hoc* approach is convenient, in that it gives a researcher *carte blanche* to interpret any differences that may be found as evidence for the very general proposition that motivated the study. The cost in predictive, and even explanatory power, however, is very great. Some broad proposals as to how formal instruction might actually manifest itself in the learning process are therefore in order.

A convenient point of departure here is to examine first the basic aims of instruction itself. There would probably be a high degree of general agreement that these were (1) to increase the accuracy or target-like character of the learner's speech and its communicative effectiveness, (2) to accelerate the learning process itself, and (3) to equip the learner with a capacity for self-instruction through the development of general analytical tools which he or she could consciously apply outside of the formal learning situation.

These three aims are formulated in terms of different goals or objects. The object of the first is a product, namely language; the object of the second, is a process; while the object of the third is the producer—that is, the learner himself.

As far as the evidence permits, we have already examined the effects of instruction in respect of the first and second goals. We can therefore proceed to an examination of instruction in terms of the third goal. The terms "formal", "mixed" and "naturalistic" have already been used in this chapter, with the rider that they were essentially behavioural descriptions. In the process of examining the effects of formal instruction on the learner, we may also be able to evaluate the usefulness of terms such as these as descriptions of variables of relevance to the description of the language learner's environment.

3.4.10 The Effect of Instruction on the Learner

As stated, there would probably be a general consensus that the principal aim of formal instruction on the learner *qua* learner is to increase his or her level of "linguistic awareness". Instruction is meant to provide the learner with principles and habits that allow him to filter his own speech, as well as the input to which he is exposed, through a conscious monitor. In this way formal knowledge can affect the organization of the learner's linguistic system. The precise mechanisms whereby the conscious mind interacts with and influences automatic processes are, of course, not well understood, and the present author has no wish to add his own speculations to the mass of unsubstantiated claims already in print. (Levelt 1989 [113] is a welcome and

promising exception here, while other claims have already been discussed in connection with the Monitor Model in 2.1.5). In any case, for the purposes of the present discussion, it is not necessary to go beyond the point of registering the connection between formal instruction and increased conscious involvement in the activities of speech production and comprehension.

Given this point, we have to then ask whether the conscious activity that formal instruction is meant to encourage is different in kind from that which accompanies other forms of learning. While this is, of course, a possibility, at present there is no compelling evidence that it is the case [88].

If it is not the case, then once we separate the content of instruction—the product—from its effects on the learner, the special status of formal instruction in the target language is somewhat diminished, even before we settle the question of its effects. This is because it becomes merely a part of the learner's much more extensive educational history in general. To reiterate, this is not to say that the consciousness raising effects of formal instruction are negligible. The proposition is rather that they should not be considered in isolation from similar effects arising from other learning experiences.

The question of whether formal instruction in the target language is different from other educational experiences, and somehow privileged (questions of content apart) in regard to language learning is an empirical one. Moreover, it is one that can be considered in the light of the data from the present study.

If formal instruction is privileged in the way that is implied by its treatment as a variable in many discussions of second language learning one would expect a fairly evident correlation between some of the features documented in Tables 3.1 to 3.3, such as communicative effectiveness as measured by oral proficiency, and degree of progress. If, on the other hand, it is not essentially different from other forms of instruction in its ability to equip the learner to consciously direct his or her mental activities, then we would expect rather a correlation between the learner's whole educational profile and the features mentioned, given that formal language instruction is, for most people, likely to constitute a relatively small percentage of their total experience of formal education.

It is precisely this latter correlation that we find. Thus, five of the six most proficient Polish speakers had had university educations, as had three of the five most proficient Vietnamese speakers. (The latter all had over three years residence in the country as well, which indicates, of course, that we also need to take other factors into account, bearing in mind that at this stage of human measurement that we have barely an inkling of how to go about the task [130]).

Education, then, in promoting conscious control of the learning task, appears to carry over into all learning situations. Self-study, for instance, if conducted by an experienced learner might be almost as effective as in-

struction delivered by a third party. If we look at the case of Mieczyslaw M., who was adjudged to be the most proficient of the Polish speakers, it is clear that this informant's basically solitary study of English over a two year period while in Poland must have a good deal to do with his progress. Yet Minh, from the Vietnamese sample, with basically the same pattern of non-naturalistic learning, has progressed considerably less, despite the advantage of naturalistic exposure to English at work. The quality and effectiveness of these learners' self-study—and indeed of all their learning—appears to have been strongly influenced by their different educational levels. To put it bluntly, other things being equal, five years of engineering or chemistry may be a good deal more useful to a language learner in the long term than quite a few months of instruction in the target language.

This brings us back to the question of the usefulness of behavioural descriptions like "naturalistic" and "formal". These terms imply propositions about such things as levels of learner consciousness without really making them explicit, and yet they provide no formal recognition of the psychological dimension whatsoever. To use a distinction first formulated by Pit Corder, they assume that input—what is available to the learner—and intake—what he or she can actually process—are the same thing. We have seen that there is evidence that this is not so [44].

If educational level is important in promoting a productive state of awareness which allows the learner to learn more efficiently, whatever the circumstances of his or her exposure may be, then the distinctions between different kinds of learning can become very blurred. A classroom situation for one kind of learner who finds it difficult to compare and conceptualize, and to stand back from his or her own mental processes, may not end up being very qualitatively different from a purely naturalistic one, and may indeed be more confusing. A naturalistic situation for a learner who is equipped with the faculties mentioned above—that is, who has a kind of "inbuilt teacher"—may not be not be radically different from a classroom situation either, but for entirely the opposite reasons. An extreme case of this latter situation involves the case of the anthropological linguist living with a tribe whose language he is in the process of describing. In behavioural terms the linguist's situation is clearly naturalistic—there are no books or cassettes or qualified teachers. But since the linguist himself is his own teacher is the behavioural description a very useful one? In this case it is easy enough to provide a negative answer. In the many possible intermediate cases between that of the linguist and that of the totally untutored learner the answer is not so easy to supply.

It is the author's opinion that outside of their purely behavioural, and not very illuminating, meaning, the independent status of terms such as we have been discussing is very dubious.

3.4.11 Implications For the Present Study

In terms of effects on the learner, then, the effects of formal instruction may only really be interpretable within the wider context of a learner's educational experiences as a whole. Those learners most likely to be able to take advantage of it will probably be those who have already reaped a fair share of the consciousness-raising function of other forms of education. Of course, the preceding discussion is not meant to suggest that education is the only important parameter in determining success in language learning—rather, that is one of several. The length of the previous discussion should not be taken as an index of the importance of education as opposed to other factors. Quite the contrary, it is the result of trying to put into perspective factors which in the author's opinion have loomed too large in much recent discussion of the issues involved in second language learning.

One thing that is worth stressing at this point is that if an important effect of formal instruction of any kind is its consciousness-raising function, then those forms of formal language instruction which are likely to have the most long-range beneficial effects are just those which promote an active and exploratory role for the learner. "Brute force" methods of instruction, such as extensive drilling, obviously do not rate very well in this regard; individualized and self-directed approaches appear much more promising.

3.4.12 The Effects of Instruction—Conclusions

From the available evidence, it appears that the effects of instruction may not be qualitatively as significant as is often assumed. This is partly so because the question as it is usually formulated is somewhat misconceived, in that instruction needs to be considered in the context of other related variables. As it is formulated, as far as the formal characteristics of language produced in the behavioural environments labelled "formal", "naturalistic" and "mixed" are concerned, the question of whether instruction makes a difference must be answered largely in the negative. This means that we can be reasonably confident that the processes observed in our sample of mixed learners are not significantly different from those which a wholly naturalistic sample of similar sociological and cultural composition would reveal.

3.4.13 Other Features of Possible Importance to Learning

While it is not within the scope of the present study to engage in a detailed discussion of the factors which may influence language learning, there are certain features recorded in Tables 3.1 to 3.3 which should receive a modicum of discussion, given that they reflect on the symmetry of the sample from the point of view of language grouping. One of these is prior second language learning experience, which has the status of a constant for the Polish speakers, but is non-existent for a majority of the Vietnamese speakers.

3.4.14 Other Languages

Prior language learning experience is often mooted as a factor likely to render the task of further second language learning somewhat more amenable. Given the differing composition of the Polish and Vietnamese samples from this point of view, this point requires some discussion.

Russian for Polish Speakers

All but one of the Polish informants had been obliged to learn Russian at school. The majority, however, claimed that they could barely understand or speak it. As far as the researcher can determine this is akin to an Italian speaker saying that he can barely understand Spanish, or communicate with a Spanish speaker, or vice versa. Of course, the hostility of many Polish speakers (though less often the well-educated) to Russian is understandable. Similar professions of mutual incomprehensibility have been reported for speakers of Serbian and Croatian, despite the very minimal differences between these two languages. Given the fact that a knowledge of Polish itself automatically results in a degree of passive competence in Russian, and the resistance of many of the Polish speakers to extending this competence in any way, we can probably largely discount the effect of Russian as a learning promoting experience, at least for those Poles who were ill-disposed towards the language and culture. An analogy for the English speaker would be the effect of exposure to some non-prestige dialect of English on, say, their learning of French.

German for Polish Speakers

It is fair to assume that the Polish speakers' experience of German was somewhat more constructive as regards their learning of English. Use of German words was not uncommon amongst Polish speakers in the first round of interviews.

All of the Polish speakers had spent at least six months in a German-speaking environment prior to their arrival in Australia. However, most appear to have made no special effort to learn German during this time, and since the majority did not work it can be assumed that their contact with German speakers was quite limited. In general, the informants' own estimates of their proficiency in German were that they spoke and understood it less well than English.

Given the level of proficiency in German that the Polish speakers reached, and the probable unimportance of Russian for the reasons given, it seems that while their prior experiences of other second languages may have been of some assistance to the Polish speakers, this particular asymmetry in the sample would not appear to be particularly important, and should certainly

not be invoked as a significant factor in the more rapid progress shown by this group as compared to the Vietnamese speakers.

A Note of Caution

While there appears to be a connection of some kind between external factors and language development there is absolutely no mechanism to account for how these factors may actually impinge on the development of an interlanguage grammar, and recent surveys have shown that the more one tries to focus on a nexus of some kind, the more elusive it becomes—even to the point of conceptualization and identification, given the web such variables tend to form [130].

The Sound Barrier

It is evident from a comparison of length of exposure to English for the two language groups that the Vietnamese learners find the acquisition of English a considerably more difficult task than the Polish speakers. The reasons for this are probably multifarious, and it is not within the scope of the present project to investigate them. From an anecdotal point of view, however, it would appear that cultural distance, and its various psycho-social projections are important factors.

Another factor that ought to be noted here, since it is not evident in the tables of statistics, is the phonological distance between Vietnamese and English. Vietnamese is tonal, lexically monosyllabic and devoid of consonant clusters. English is atonal, lexically polysyllabic, and rich in consonant clusters, and it is within these phonetically difficult consonant clusters that important phonemic information is located. Thus, for the Vietnamese speaker the task of isolating lexical units, and morphemes within these, in the stream of English speech involves a major phonetic reorientation, as does the task of producing these units in a comprehensible fashion. This is not nearly so much of a problem for the Polish speakers, whose language is phonetically much more similar to English, even to the extent of having some of the same (and in English, morphemically significant) consonant clusters. (For example, Polish has */ts/*, */dz/*, and */kt/*).

For the Vietnamese speaker, all of the English morphemes with the exception of *-ing* present production and comprehension difficulties, whereas for the Polish speaker it is probably safe to say, acceptable variants are within the range of their phonological competence from the very outset. It is the author's guess that this factor is an extremely important one in explaining the overall difference in learning rates for the two groups. Some concrete evidence for this will be presented in the next chapter.

3.4.15 Potential Difficulties with a Naturalistic Sample

It was mentioned in an earlier section (3.4.5) that the safest course for the selection of data for an analysis of natural sequences in language learning was to restrict oneself to data gathered from naturalistic learners, just in case data from mixed sources turned out to be different in some way. Because this did not turn out to be possible a good deal of discussion has been devoted to the question of the status of data gathered from mixed learners. The conclusions reached were that, on the basis of the available evidence, this data was unlikely to differ very dramatically from purely naturalistic data. A further point needs to be made, however. This concerns a possible problem with naturalistic data itself.

In Australia, where most migrants have the opportunity to attend English classes, naturalistic learners are likely to be restricted to a particular band of the sociological spectrum. Thus, they are likely to be more culturally, socially and psychologically distant than other learners; it is also probable that their level of education will be lower. (Obviously, there would be exceptions to this pattern).

3.5 Limitations Imposed on the Analysis

3.5.1 The State of the Longitudinal Study

As stated in 3.2.1, the original aim of the project was to supplement the cross-sectional study with a limited form of longitudinal study.

This longitudinal study was to have a final sample size of eight learners, four from each language group. The informants for this study were to have an initial *ASLPR* rating of between 0 and 1, mainly on the assumption that less advanced learners were more likely to exhibit some progress in their learning in the ten to twelve month period over which the data was to be gathered.

The data for this longitudinal component of the study was duly collected, transcribed, and typed. One modification to the study design for the longitudinal study was made—this involved the elimination of one round of interviews for three of the Vietnamese informants, on the grounds that they did not appear to be making sufficient progress to warrant a more intensive schedule of interviews. While this was primarily the judgement of the researcher, it was reinforced by the fact that a self-perceived lack of progress amongst the Vietnamese speakers generated a certain amount of resistance to a more intensive schedule of interviews. The number of rounds of interviews, therefore, was five for the Polish speakers, and four for the Vietnamese speakers (this includes the two rounds which were part of the cross-sectional study). The fact that four rounds of interviews were concluded with this latter group is somewhat of an achievement, to judge from

some other projects which sought the long term co-operation of South East Asians.

Regrettably, however, the huge volume of work involved in extracting and analyzing the data collected in the cross-sectional part of the study has prevented, at this point in time, any detailed examination of the longitudinal data.

The rationale of the longitudinal study was that it should act to help validate conclusions formulated on the basis of the cross-sectional data: developmental projections made there would hopefully find substantiation or refutation in later interviews. It was not envisaged that the longitudinal study would have other than an ancillary function, since with a projected maximum number of interviews of five over the period of a year there would not have been sufficient detail or density of material for the study to stand on its own.

That there has so far been no real possibility to check the cross-sectional study against the longitudinal one, therefore, is unfortunate but by no means methodologically disastrous, since the cross-sectional study stands by itself and does not depend on its longitudinal extension. Follow-up work on the longitudinal data would, however, be an obvious next step in the research if further work along these lines was to be conducted.

The alternative of simply merging the longitudinal material with the cross-sectional data to produce an enlarged version of the cross-sectional study was also rejected. The cross-sectional study was carefully designed and balanced as regards its range and composition and the introduction of extra material from informants already represented in the study would have distorted its structure without increasing its representativity in any way.

3.6 Oral Proficiency Ratings

The final points regarding the organization of the data base for the study concern the application of an oral proficiency rating scale to the output of each of the informants chosen. Since one of the requirements of the brief was for the researcher to try and determine what, if any, correlation existed between speaking proficiency as assessed by an instrument like the *ASLPR* and the rule-systems of learners, and since some preliminary form of grading or sorting of the informants was necessary, the final step in readying the data base for analysis was to obtain a series of reliable oral proficiency ratings for the informants. Tables 3.5 and 3.6 show the results of these assessments.

Because the researcher himself and the most experienced raters available were considerably more familiar with the Adult Migrant Education Speaking Proficiency Descriptions (*AMES* scale) than the Australian Second Language Proficiency Ratings (*ASLPR* scale), an initial rating figure was obtained using the *AMES* scale. The figures presented in Tables 3.5

and 3.6 are thus figures from the *AMES* scale.

Given the basic similarity between the *AMES* Descriptions and the *ASLPR*, it and the fact that the *AMES* scale has more gradations than the latter, there is no real problem inherent in converting from *AMES* scale to *ASLPR*, and this is accordingly done in all subsequent references to oral proficiency. The conversion scale, which was worked out by Geoff Brindley, is presented below in Table 3.7.

In order to validate this procedure, a selection of eight interviews was given to Maggie Gray, who was familiar with both scales. The results are presented in Table 3.7, and once again show a high degree of concurrence with other figures.

Rating was not done at the time of the interview. Rather, it was done from the cassette recordings of the interviews, which were repeated several times if necessary, until the rater was satisfied with the rating. In the case of collaborative ratings (as with Ken Singh and the researcher in the second round of interviews) the procedure was for each rater to write down his rating and then for a discussion to take place if there was a discrepancy, with the tape being played again. Discrepancies, as it turned out, were never very large and it was always possible to resolve them consensually, rather than arithmetically.

As can be seen from a comparison of the figures, there was a high degree of consistency in the ratings. Ken Singh tended to rate the less proficient informants slightly higher than Geoff Brindley, who in turn tended to rate the more proficient informants slightly higher than Ken Singh. The present author tended to rate the less proficient informants lower than either of the other two raters. This is probably a reflection of our differing classroom experiences; in any case, the margin of difference was minimal.

Where there is a positive difference between a given rater's assessment of an informant in the second interview as compared with the first, this can almost certainly be attributed to the informant's either having made some progress in his or her learning of English or (in the case of some of the less proficient Vietnamese speakers) being more relaxed and forthcoming in the second interview. Where there is no difference, we have (hopefully) some useful evidence of a rater's consistency. There is one case of a slight negative difference. This was produced when Geoff Brindley rated Sang lower in the second interview than the first. All other ratings of Sang had put him at a somewhat higher position on the scale, and there is therefore some cause for doubt about this particular rating, from within the context of the oral proficiency rating process itself. We shall return to this point in a moment.

Since it was decided to use oral proficiency ratings as a means of sequencing informants in most of the tabular data to be presented in the next chapter it was necessary to make some definite choice about who was to follow who. Given that Geoff Brindley was the most experienced rater, and that the author of the study collaborated with Ken Singh on one set of rat-

ings, while Geoff's ratings were all made independently, these latter ratings were adopted.

It was mentioned that one of Geoff's ratings was contentious. In the interests of consistency the rating has been let stand. There is a further reason for doing this: if it is the case that there is some correlation between a learner's syntactic profile and his or her oral proficiency then the case of Sang in the second interview might provide an interesting test case. Consistent independent syntactic evidence that he has been wrongly ranked would support a hypothesized connection between oral proficiency and manipulation of syntax, and would demonstrate how evidence from syntax can help to decide disagreements that cannot be resolved from within the proficiency scale itself.

3.7 Conclusions

In this chapter an attempt has been made to describe and justify all the basic decisions that had to be taken regarding the implementation of the study design.

It is the author's opinion that the principal objections that might have been registered against the kind of data collected, the means of collection, and the selection of informants from whom to collect it, are not nearly so serious as theory alone would suggest they might be. Where empirical evidence can be called upon to mediate in questions concerning the possible validity of the data the outcome has been encouraging for the original study design. For the purposes of syntactic analysis, then, it is fair to say that the data can be considered to be good data. The principal limitation on it is that it comprises a cross-sectional study. Even here, however, one has the option of cross-checking hypotheses against the longitudinal extension.

It has already been remarked that many studies of second language acquisition in English are seriously flawed in either the composition of their samples or the techniques of elicitation they have employed, or in both these things. The present set of data has been compiled with the avoidance of these defects in mind. To the author's knowledge it is probably the most comprehensive and carefully compiled set of its kind so far for the English language.

Name:	IS	KS	ZJ	JB	JR	KA
Sex:	F	M	M	M	M	F
Age:	30	30	28	34	35	45
Time in Aust:	-	-	-	-	-	-
Months:	1	1	2.5	2	3	3
Profession:	Techn	Plmbr	Electn	Prntr	Fitter	Mngr
Occupation Now:	Stdnt	Stdnt	Stdnt	Stdnt	Stdnt	Stdnt
Years of Educ'n:	13	13	13	13	13	11
Other Languages:	-	-	-	-	-	-
Formal:	Russn	Russn	Russn	Russn	Russn	-
Naturalistic:	German	German	-	-	German	German
Formal English:	-	-	-	-	-	-
At School:	-	-	-	-	-	-
Hours/Week:	-	-	-	-	-	-
Years:	-	-	-	-	-	-
Elsewhere:	-	-	-	-	-	-
In Australia:	-	-	-	-	-	-
Hours/Week:	20	20	20	20	20	20
Months:	1	1	1.5	1.5	1.5	1.5
Self Study:	-	-	-	-	-	-
Hours/Week:	-	-	-	6	3	3
Months:	-	-	-	5	6	9
Informal Exposure:	-	-	-	-	-	-
to English:to English:	-	-	Shops	-	-	-

Table 3.1: Statistics: Polish Informants—Interview 1a

Name:	BB	ES	AJ	KB	LJ	MM
Sex:	F	F	M	F	F	M
Age:	34	27	33	32	33	33
Time in Aust:	-	-	-	-	-	-
Months:	2	2	2	2.5	2	2
Profession:	Engnr	Tchr	Tchr	Clerk	Tchr	Engnr
Occupation Now:	Stdnt	Stdnt	Stdnt	Stdnt	Stdnt	Stdnt
Years of Education:	16	16	16	12	16	16
Other Languages:	-	-	-	-	-	-
Formal:	Russn	Ru.Ge	Russn	Russn	Russn	Russn
Naturalistic:	German	German	German	German	German	Ru.Ge
Formal English:	-	-	-	-	-	-
At School:	-	-	-	-	-	-
Hours/Week:	-	-	-	1	-	-
Years:	-	-	-	4	-	-
Elsewhere:	-	-	-	-	-	-
Hours/Week:	-	7	2	-	-	-
In Australia:	-	-	-	-	-	-
Hours/Week:	20	20	20	20	20	20
Months:	1	1	1	1	1	1
Self Study:	-	-	-	-	-	-
Hours/Week:	6	8	8	-	8	4
Months:	6	2	1	-	1	24
Informal Exposure:	-	-	-	-	-	-
to English:	-	-	-	Friend	Friend	Friend

Table 3.2: Statistics: Polish Informants—Interview 1b

Name:	Van	My	Duc	Dung	Minh	Hoa
Sex:	M	M	M	M	M	F
Age:	21	25	30	27	25	28
Time in Aust:	-	-	-	-	-	-
Months:	1	15	24	20	24	15
Profession:	Mech	Lab'r	Artisan	Mech	Photog	H'wife
Occupation Now:	Stdnt	Worker	Cook	Worker	H'wife	Stdnt
Years of Educ'n:	5	9	7	12	12	4
Other Languages:	-	-	-	-	-	-
Formal:	-	-	-	-	-	-
Naturalistic:	-	-	-	Mandarin	-	-
Formal English:	-	-	-	-	-	-
At School:	-	-	-	-	-	-
Hours/Week:	-	-	-	-	-	-
Years:	-	-	-	-	-	-
Elsewhere:	-	-	-	-	-	-
Hours/Week:	-	-	-	-	-	-
In Australia:	-	-	-	-	-	-
Hours/Week:	12	12	12	12	6	2*
Months:	1	1	1	6	1	7
Self Study:	-	-	-	-	-	-
Hours/Week:	2	1	-	1	4	-
Months:	1	15	-	20	24	-
Informal Exposure:	-	-	-	-	-	-
to English:	-	Social	Work	Work	Work	Social

Table 3.3: Statistics: Vietnamese Informants—Interview 1a

Name:	Sang	Vinh	Tam	Canh	Long	Phuc
Sex:	M	F	M	M	M	M
Age:	21	25	30	27	25	28
Time in Aust:	-	-	-	-	-	-
Months:	3	12	42	48	48	30
Profession:	Baker	Stdnt	Sold'r	Photog	Sold'r	Stdnt
Occupation Now:	H'wife	Welder	Worker	Worker	Workert	Worker
Years of Educ'n:	7	14	12	14	10	14
Other Languages:	-	-	-	-	-	-
Formal:	-	French	-	French	-	French
Naturalistic:	-	-	-	-	-	-
Formal English:	-	-	-	-	-	-
At School:	-	-	-	-	-	-
Hours/Week:	-	6	1	2	-	6
Years:	-	2	2	3	-	2
Elsewhere:	8	-	-	-	-	-
Hours/Week:	3	-	-	-	-	-
In Australia:	-	-	-	-	-	-
Hours/Week:	20	20	12	12	20	-
Months:	2	4	4	1	0.5	-
Self Study:	-	-	-	-	-	-
Hours/Week:	8	-	1	4	-	-
Months:	3	-	30	48	-	-
Informal Exposure:	-	-	-	-	-	-
to English:	-	Social	-	Work	Work	Work

Table 3.4: Statistics:Vietnamese Informants—Interview 2b

VIETNAMESE INFORMANTS Rater:GB					POLISH INFORMANTS Rrs: GB and KS			
Range	Name	Level	Name	Level	Name	Level	Name	Level
0.0–1.0	Van	0.5	Van	0.5	IS	1.0-	IS	1.0-
1.0–2.0	My	1.0	My	1.0+	ZJ	1.0	ZJ	1.0
	Duc	1.5+	Minh	1.5	KS	1.0+	KS	1.5
	Dung	1.5+	Duc	1.5+	JB	1.5-	JB	1.5
	Minh	1.5+	Hoa	1.5+	JR	1.5	JR	1.5
	Minh	1.5+	Hoa	1.5+	JR	1.5	JR	1.5
			Dung	2.0-	KA	2.0-		
2.0–3.0	Hoa	2.0	Sang	2.0+	BB	2.0	BB	2.0
	Sang	2.0+	Vinh	2.5	ES	2.0+	KA	2.0
	Vinh	2.5	Long	2.5+	-	-	ES	2.0+
3.0–4.0	Tam	3.0	Canh	3.0	AJ	3.0+	AJ	3.0+
	Canh	3.0	Tam	3.0	KB	3.5	KB	3.0+
	Long	3.0+	Phuc	4.0	LJ	4.0-	LJ	3.5
	Phuc	4.0	-	-	MM	4.0	MM	4.0

Table 3.5: Interview 1: Oral Proficiency Ratings—GB and KS

VIETNAMESE INFORMANTS Rater: GB					POLISH INFORMANTS Rrs: KS and MJ			
Range	Name	Level	Name	Level	Name	Level	Name	Level
0.0–1.0								
1.0–2.0	Van	1.0	Van	1.0	IS	1.5+	IS	1.5
	Duc	2.0-	My	1.5	KS	1.5+	KS	1.5+
	Sang	2.0-	Duc	1.5	ZJ	2.0-	JB	2.0-
			Hoa	1.5+	JR	2.0-	KA	2.0-
			Minh	1.5+	KA	2.0-		
			Dung	2.0-				
2.0–3.0	Minh	2.0	Sang	2.0+	JB	2.0	JR	2.0
	My	2.0	Vinh	2.5	BB	2.5-	Z	J2.0
	Hoa	2.0	Long	2.5+	-	-	BB	2.5-
	Dung	2.0+	-	-	-	-	ES	2.5
	Vinh	2.5						
3.0–4.0	Tam	3.0	Canh	3.0	ES	3.0	KB	3.5
	Canh	3.0	Tam	3.0	AJ	3.0+	LJ	3.5
	Long	3.0+	Phuc	4.0	KB	3.5	AJ	4.0-
	Phuc	4.0	-	-	LJ	4.0-	MM	4.0+
					MM	4.0		

Table 3.6: Interview 2: Oral Proficiency Ratings—GB and KS

AMES	ASLPR
0.5	0
1.0	0+
1.5	1-
2.0	1
2.5	
	1+
3.0	
3.5	
	2
4.0	

Table 3.7: Conversion Scale for the AMES Scale and the ASLPR

Interview	Rating
jb.1	0+
ka.1	1-
bb.1	1
lj.1	1+
mm.1	2
duc.1	1-
sang.1	1
hoa.1	1+
tam.1	1+
long.1	2

Table 3.8: Direct ASLPR Ratings for a Selection of Informants

Chapter 4

Analysis, Results and Conclusions

4.1 General Statistics

The following tables provide some statistics regarding the interviews.

4.1.1 Length of the Interviews

Table 4.1 shows the length of the interviews in minutes. The section entitled "Qs:" refers to the elicited data, which was not used for the analyses presented in this study. The target time for the first round interviews was forty-five minutes, but for informants lower on the *ASLPR* the interviews often had to be terminated well before this. In a few cases first-round interviews ran longer than the targeted time; in one case—that of Long—the interview was double the proposed length. For the second round interviews, which also included a section of elicited data the target time was roughly thirty minutes.

4.1.2 Word Counts

Table 4.2 provides word counts for the informants' output. As is evident, these vary quite considerably, even for interviews of the same length: compare, for instance, the word counts for **es.1** or **vinh.1** with those for **mm.1** or **phuc.1**. Tables 4.3 and 4.5 and 4.6 provide information on the informants' vocabulary size and the relative percentages of the categories of noun, adjective, verb and adverb in this. The major point of significance is the increase in the percentage of verbs in the vocabularies of the more orally proficient informants; other categories seem to increase at a rate largely commensurate with the overall increase in vocabulary size. It should be stressed that Tables 4.3, 4.4 and 4.5 and 4.4 do not provide information on the relative frequency of items from each category in actual speech, where

functors such as articles, and certain verbs such as be would obviously recur with some frequency, although they only represent tiny percentages of the total vocabulary.

4.1.3 Fluency and Volubility

The variable relationship between the length of an interview and the total number of words produced prompted the compilation of some general statistics on what can loosely be called fluency, or perhaps volubility. The purpose was to see to what extent this correlated with assessed oral proficiency. It should be borne in mind that these figures for word rates are affected by various considerations.

Some informants, such as Long or Ludwiga J., have a monologuing, as opposed to an interactional, style of conversation, and therefore use most of the available time in the interview themselves. Other informants with this tendency are Canh, Hoa, Barbara B. and Jan R. On the other hand, informants such as Ewa S. throw the burden of conversation back on the interlocutor, either by the brevity of their own contributions, or through frequent requests for confirmation regarding what they have said, or through direct questions. In addition, the interviewers themselves can influence the length of conversational transactions in an interview themselves. GB and MJ, for instance, differ somewhat in this regard, with the former tending to attempt to guide the conversation more than the latter, and consequently tending to interrupt more frequently.

For reasons of conversational style, then, word rate alone is only a rough guide to volubility, and a much rougher one still to fluency. Nevertheless, there does seem to be some correlation between global word rate and oral proficiency, especially if we exclude the monologuers or penalize their word rate. From the data from the first round of interviews, a rate of under thirty words per minute would be likely to correlate with an *ASLPR* rating of less than 0+, from thirty to fifty with a rating of between 0+ and 1, fifty to sixty-five with a rating of between 1 and 1+, and sixty-five and over with a rating of 1+ upwards. The data from the second round of interviews supports this if the word rate is lifted some five words per minute for each proficiency level; perhaps the fact that the second interviews are generally shorter has some bearing on the higher word rates. Despite the fact that volubility and fluency are not the same thing, it appears likely, then, that the rate of word production is a factor which may quite strongly influence the assignment of oral proficiency ratings.

4.1.4 Learner Types

The informants' different conversational styles brings us to another question which has not been taken up in the present study, but which is of consider-

able interest. This is the question of learner types. It will be recalled that in 2.1.8 a "multi-dimensional" model of variation developed by the *ZISA* group in Germany was described. Central to the concept of this model was the notion of a "variational" dimension upon which different learners could be located. Thus, although learners might pass through the same developmental stages, their speech might involve more or less "simplification"—deletion of elements, for instance, or acquisition of developmental rules in a small subset of the contexts in which the rule could function in the target language [128].

Since independently motivated developmental sequences similar to those of the kind used as an index by the *ZISA* group have been developed for English by the author in collaboration with Manfred Pienemann and reported on in joint publications and, in addition, that framework has now been reconceptualized into Processability Theory it is not appropriate or feasible to apply the model to the present data except in a restricted form. Nevertheless, there might be some value in indicating on an anecdotal basis how the informants in the study would be located along a "standardizing-simplifying" axis. While such a procedure is partially *ad hoc* it is worth remembering that it is recognized in communicative proficiency scales that learners can be communicatively effective without being very grammatically accurate, and *vice versa*—such disparities result in so-called "unequal profiles". Naturally, assessments of the above kind are difficult to defend on empirical grounds, and constitute one of the problem areas in the validation of proficiency scales. In Chapter Five, there will be some discussion as to whether or not it is possible to provide any empirical base for these judgements. In the meantime, Tables 4.7 and 4.7 display the researcher's intuitions on this aspect of learner types. It classifies learners according to a tri-partite division of "standardizing", "neutral" and "simplifying".

As Table 4.7 and 4.8 show, it appears that those informants with a monologuing style of conversation tend also towards simplification. This is not particularly surprising, in that the monologuers seem to be less concerned with the details of their language than in communicating their message, or, in some cases, simply holding the floor. The less interactional speakers appear to monitor *all* aspects of the conversational situation less than their more interactional and "correct" counterparts—that is, their reduced monitoring is not merely a linguistic phenomenon, as in the Monitor Model [26]. The question of learner types clearly requires a great deal more research and it is not presently clear whether we are even in a position to conceptualize it adequately outside of restricted frameworks like the one employed here.

Infmt	1:	2:	Qs
aj	44	32	35
bb	42	29	23
canh	86	19	49
duc	30	27	20
dung	40	21	31
es	49	27	28
hoa	39	20	16
is	21	26	6
jb	31	30	26
jr	48	28	45
ka	45	25	31
kb	40	30	25
ks	35	32	41
lj	47	40	40
long	90	10	26
minh	28	17	31
mm	44	26	31
my	11	31	28
phuc	45	21	21
sang	32	21	29
tam	44	33	27
van	5	12	6
vinh	41	27	24
zj	41	25	34

Table 4.1: Length of Interviews in Minutes

Interview	Words	-	Interview	Words
van.1:	126	-	van.2:	551
my.1:	379	-	ka.2:	1029
is.1:	621	-	is.2:	1051
minh.1:	1022	-	long.2:	1082
zj.1:	1280	-	minh.2:	1179
jb.1:	1291	-	dung.2:	1184
dung.1:	1419	-	zj.2:	1194
ks.1:	1576	-	sang.2:	1572
sang.1:	1589	-	ks.2:	1604
ka.1:	1675	-	vinh.2:	1648
duc.1:	1865	-	jb.2:	1652
hoa.1:	2075	-	duc.2:	1692
vinh.1:	2229	-	hoa.2:	1706
es.1:	2469	-	es.2:	1887
jr.1:	2580	-	jr.2:	1891
bb.1:	2619	-	phuc.2:	2140
kb.1:	2811	-	canh.2:	2215
aj.1:	2909	-	my.2:	2279
tam.1:	2994	-	bb.2:	2346
mm.1:	3559	-	mm.2:	2421
lj.1:	4543	-	tam.2:	2484
phuc.1:	4674	-	aj.2:	2508
canh.1:	6457	-	kb.2:	2694
long.1:	9199	-	lj.2:	4491

Table 4.2: Word Counts for Informants' Text

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
% adjs:	10	10	11	10	9	12	11	15	14	15	15	13
% nouns:	47	40	40	37	38	38	46	43	33	36	43	41
% verbs:	9	10	14	18	17	15	16	13	22	22	22	21
% advbs:	4	4	4	5	4	7	4	2	5	4	5	4
# words:	171	302	289	302	463	305	392	498	407	445	567	512

Table 4.3: Percentages for Major Categories: Interview 1

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
% adjs:	14	12	9	11	9	12	15	11	12	14	14	14
% nouns:	41	42	39	44	40	35	39	36	38	35	39	37
% verbs:	15	13	18	17	17	15	18	19	19	23	21	21
% advbs:	5	5	4	6	7	4	4	5	5	4	5	7
# words:	235	313	352	245	213	325	344	368	432	425	619	413

Table 4.4: Percentages for Major Categories: Interview 2

Informant	van	my	duc	dung	minh	hoa	sang	vinh	tam	canh	long	phuc
% adjs:	0	9	9	13	6	10	9	12	10	13	13	11
% nouns:	33	42	39	38	36	35	34	34	35	44	45	39
% verbs:	14	13	19	19	21	16	19	23	24	23	20	25
% advbs:	6	9	5	6	7	7	6	7	6	4	5	7
# words:	48	116	359	304	202	274	308	334	399	828	654	655

Table 4.5: Percentages for Major Categories: Vietnamese Informants—1

Informant	van	duc	sang	minh	my	dung	hoa	vinh	tam	canh	long	phuc
% adjs:	6	8	35	7	10	11	10	10	12	11	12	12
% nouns:	49	38	37	32	44	34	32	39	33	30	26	31
% verbs:	12	17	21	21	15	23	20	20	21	25	20	29
% advbs:	5	6	6	9	4	7	8	6	7	7	10	8
# words:	115	323	336	199	370	272	290	328	393	444	223	360

Table 4.6: Percentages for Major Categories: Vietnamese Informants—2

ASLPR	Standardizing	Neutral	Simplifying
2		Mieczyslaw M.	Ludwiga J.
1+	Krystyna B. Andrzej J.		
1	Ewa S.		Barbara B.
1-	Jerzy B.	Krystyna A.	Jan R.
0+	Krystof S.		Zygmunt J.
0		Irena S.	

Table 4.7: Learner Types: The Variational Dimension—Polish Informants

ASLPR	Standardizing	Neutral	Simplifying
2		Phuc	
1+	Vinh		Long Canh Tam
1	Sang		Hoa
1-	Dung Minh		Duc
0+			My
0			Van

Table 4.8: Learner Types: The Variational Dimension—Vietnamese Informants

4.2 Verbal Morphology and Semantics

4.2.1 Limitations

This section deals with the use of all verbs, other than be and have and modals—since these forms were deemed to warrant separate treatment. The emphasis is on the syntax of the verbal system, rather than its semantics. The author recognizes that there are certain problems inherent in separating these two components when dealing with a system as complicated as the one presently under scrutiny. For instance, a discussion of, say, past tense forms is evidently restricted if figures for postulated contexts for the production of these forms are not available. In practice, due to time constraints it has not always been possible to give detailed descriptions of the semantic fields for the syntactic events under consideration. In some cases this restriction is more important than others; in any event, it has often been possible to make rough predictions about probable temporal contexts, and so forth. Nevertheless, this limitation should be noted.

4.2.2 Verbal Features Investigated

The aspects of the verbal system that were closely investigated were primarily those involving verb marking. It is just these aspects that receive

the greatest attention in the classroom. In the present study, seven basic categories were investigated. These were:

1. Non-standard -ing marking.
2. Irregular past marking.
3. Regular (that is, *-ed*) past marking.
4. Third person singular *-s* marking.
5. *Auxiliary + verb + '-ing'* marking—that is, the so-called progressive or continuative aspect, henceforth referred to as *aux_ing*.
6. *Auxiliary + verb + -ed* marking, henceforth referred to as *aux_en* marking. This surface realization covers a number of different structures, some of which are not always definitively distinguishable—*copula + verbal adjective* (for example, he is tired), agentless passive (for example, they were injured), full passive (for example, you were accepted by Australian deligation), and finally, perfect aspect forms (for example, I have just arrived in Australia).
7. Verbal nominalizations or gerunds—excluding those where no common alternative nominal form exists, such as swimming.

Tables 4.9 to 4.12 provide a breakdown of the occurrence and frequency of tokens in the above categories for the informants in the cross-sectional study. The tables are basically set out in descending distributional frequency of the form across the sample.

4.2.3 Non-Standard '-ing'

The default case for verbs in the present data is, of course, that they occur in stem form. Thus, technically, null marking is the most common form of verb marking in the corpus. With null or unmarked forms there can be a problem in providing an appropriate categorial assignment, as in the following example:

> MJ ... have you studied English?... did you I... learn English?
>
> I ... no school English
>
> - my.1: [197–200] -

(Other examples of categorially vague tokens are given in 4.10.3). Categorial blurring is a common phenomenon in the language of beginning learners and can persist beyond level 1 on the *ASLPR*. As we shall see, marking

of stem forms is in fact a means whereby learners can indicate the category of a particular item.

Amongst the non-null forms of verb marking, *-ing* marking of verbs is the most prevalent form of marking in the sample, in both gross terms and on a distributional basis. In addition, *-ing* marking is probably the first type of verbal marking to appear. This is suggested by previous studies conducted by the author, and others [92]. In the present study also, where there is only one form of verb marking present, it is *-ing* marking. In **van.1**, for instance, there is no marking of verbs—apart from one formulaic occurrence of a verbal adjective—and in **van.2** when marking appears it is *-ing* marking. Similarly, in **my.1** the only form of marking is *-ing* marking. *-ing* marking provides one of the best demonstrations of the difference between learner language and target language systems, in that a form common to both is implemented in radically different ways in the two systems. In Standard English *-ing* marking serves to indicate non-completive aspect in auxiliary + verb structures, to indicate nominalized verbs, and as a complementizer for certain classes of complement-taking verbs—notably those of perception.

In terms of application it is interesting that ESL teachers frequently interpret *-ing* marking in learner language in Standard English terms. Thus a learner who produces *-ing* marked verbs is assumed to have acquired the morphology, syntax and semantics of the present continuous. In fact, this is usually not the case.

In the learner's system *-ing* may come to assume these Standard English applications, but it also serves other quite different functions.

Tables 4.13, 4.14, 4.15, and 4.16 provide some figures for *-ing* marking across the whole cross-sectional corpus. The analysis first makes a division between syntactic context and temporal context. Since these categories (with the exception of verbal complement, where temporal reference is not borne by the complement) are not mutually exclusive a particular token may be marked down under both these headings—for instance, an *-ing* marked verb may occur in a subordinate clause and have an obviously past temporal reference. In practice, tokens which had present temporal reference and were found to occur in one of the syntactic contexts outlined in the table were not noted down again in the Temporal Context side of the table. Within the two major divisions, a series of subdivisions were made. For Syntactic Context these were Subordinate Clause, Co-ordinate Clause, Verbal Complement, Negative Verb Phrase, and Question. For Temporal Context, the divisions were Past, Present and Future. As can be seen from Tables 4.13, 4.14, 4.13, and certain syntactic contexts—namely Subordinate Clause, Co-ordinate Clause and Verbal Complement—appear to provide favourable environments for *-ing* marking. Likewise, the temporal context Past also appears to be a congenial environment. Figures for the relative frequency of these environments as compared to other environments (such as Main Clause and Present) throughout the interviews are not available, but it is reason-

able to assume the tabled environments—with the exception of Present—are not the most frequent overall. However, questions of frequency aside, it is fair to claim that on the basis of complexity the syntactic environments are "marked" in relation to other possible environments, such as Main Clause, as is the temporal context Past in relation to Present. Thus the environments which in general favour *-ing* marking are marked environments. If this is the case, then we can begin to explain the reasons for *-ing* marking in learner language.

As previously noted, *-ing* marking appears to be the first form of verbal marking produced by learners. When *-ing* marking first appears, it is most likely to occur in default or unmarked syntactic or temporal contexts, since these are by far the most prevalent in early learner language. Apart from this globally enforced regularity, there are no discernible syntactic or semantic correlatives of *-ing* marking at this stage. (There may possibly be some lexical regularities, in that certain verbs—such as work, look and go—regularly, and even invariably, take the *-ing* form). Given a lack of syntactic or semantic parameters for early *-ing* marking and also given the concurrent emergence of categorial distinctions of other kinds, the probable initial function of *-ing* marking is to indicate that the marked word is a verb, and thus help clarify sentences for processing by interlocutors.

Thus, far from being a tense or aspect marker, *-ing* begins life as a categorial marker, a verb marker whose marking function is entirely self-referential: to declare the marked object as a verb. The choice of *-ing* over other verb markers offered by Standard English (such as *-ed* or *-s*) can probably be explained by its phonological salience, since of the various bound markers available, it alone is consistently syllabic. (Lexical markers such as modals and auxiliaries are also subject to reduction and cliticization in casual speech—for instance, I will becomes I'll—which, of course, considerably reduces their salience).

In the interviews with the less proficient informants, as in **van.2**, **my.1** and **zj.1**, the syntactic environment is probably the default case, Main Clause, and the temporal environment is Present. However, as the learner begins to produce utterances of greater syntactic complexity and to complexify his or her temporal reference scheme, *-ing* marking apparently migrates from the default (unmarked) environments to the more marked environments described in Tables 4.13, 4.14, 4.15 and 4.16. A likely explanation for this is that the learner feels a need to mark verbs in some way in these marked environments and in the absence of specific hypotheses as to what these markers might be resorts to the stock verb marker *-ing*. The need to mark verbs in these marked environments is twofold for learners. In the first place, verb marking is common in natural languages in syntactic contexts such as subordinate clause and temporal contexts such as past. In the second place, marking of the verb in recently acquired complex structures enhances the processability of the utterance, as it does with simple struc-

tures produced by less proficient learners. (Although precise figures are not available, there also seems to be some tendency for *-ing* marked verbs to appear more frequently without subject noun phrases or pronouns than unmarked verbs. This may merely be a side-effect of the construction of the complex structures which favour *-ing* marking. On the other hand, since marking of the verb may render the structure in question parsable and is less complicated than selecting the appropriate anaphoric pronoun, this may actually be a contributory factor to *-ing* marking in syntactically complex environments).

As can be seen from the tables, *-ing* marking does not vary regularly with assessed oral proficiency. The question therefore remains whether it is a variable or a developmental feature. This question is further complicated by the fact that *-ing* marking is a non-standard feature that can be introduced by acquisition of new structures, which themselves represent developmental progress. Further research is required to resolve these questions.

4.2.4 Irregular Past Marking

Irregular past forms (including past participles) are the next most common form of marked verb after *-ing* marked verbs, both in gross terms and on a distributional basis. Tables 4.17 to 4.20 show the distribution of these forms in descending order of frequency across the sample. Although the total list of irregular verbs is quite long, it is worth noting that the number of verbs actually used by any single informant does not rise above ten until the 1+ oral proficiency threshold is attained, with the average number of verbs per informant at about three before that. (It rises to about eight thereafter). Further analysis of Tables 4.17 to 4.20 reveals that the five most common irregular verbs on a distributional basis are went, said, got, saw, and came—all of which occur in at least three interviews in each of the four interview groups.

The pattern of usage of irregular verbs seems to be basically the same for both language groups, with one exception. This involves got, which is used more (both frequency-wise and distributionally) by the Vietnamese speakers. In Standard English, got is either the past tense form or the past participle of the active verb get.

In this latter capacity it occurs in the idiosyncratic item have got, which is generally synonymous with have. In the speech of the learners in the study got has three functions. Two of these are the same as for Standard English. The third involves the use of got as a (frequently present tense) stative verb to signify possession—that is, in the contexts where have got would appear in Standard English. The appearance of this third function has an obvious phonological explanation—speakers either do not hear or have difficulty in producing the reduced auxiliary have. The majority of instances of got in the speech of the learners from both language groups involves either

the standard participial usage (both language groups) or the non-standard stative usage (Vietnamese speakers); in both cases to signify possession. In other words, got appears where have would also be appropriate. The wider distribution and more frequent use of got by the Vietnamese speakers is amenable to various explanations. One possibility is that this is the result of an application of the form-function constraint. (In fact, it seems that this principle can be grammatically formalized, with suitable adaptions, as a case of what Andrews refers to as "morphological blocking" within the theoretical framework of LFG [6], since what is occurring is that a more specific entry is blocking the choice of a less specific one. In order to apply the concepts of specificity and subsumption to learner interlanguage one would have to extend these notions to categorial distinctions—a step which is unnecessary with mature versions of a language—and extensions of this kind are beyond the scope of the present study, with the consequence that we will continue to use the original terminology here, with the caveat that if space permitted the "form-function principle" could be formalized in a way that would distinguish it from similar proposals in the literature, such as Andersen's [4]). Returning, then, to the present case the argument here is as follows. Have is used by the Vietnamese speakers, though not the Poles, as an existential verb. For example:

> ah... tortoise shell... where in Australia have?
>
> - duc.1: [761] -

Given the form-function principle, which would demand that different verbs preserve different (and unique) functions, Vietnamese speakers may therefore tend to use got to signify possession and reserve have for its existential function. A possible objection here is that there are a significant number of tokens (see Tables 4.19 and 4.20 for details) of got as a participle as well as a verb, which means that have appears in the construction in any case. However, an analysis of the participial tokens of got reveals that they occur overwhelmingly in the scope of negation. The upshot of this is that have appears in the bound form haven't, which appears to be a monomorphemic negator—see 4.10.3 for a detailed discussion. Thus, no direct duplication of form with have actually occurs, and the form-function principle is not violated. A more serious objection is that existential uses of have as exemplified above are not very frequent. If form-function considerations do not play a part in the selection of got as the lexical verb for possession, then it is possible that an input explanation might be the correct one. Thus, the Vietnamese speakers have had, in most cases, more exposure to native speech, where have got is the preferred form, than the Polish speakers. This point could do with further investigation.

Irregular Pasts and the Form/Function Principle

The fact that irregular past forms appear more frequently and probably earlier (certainly so in the case of the Vietnamese speakers) than regular past forms lends weight to the form-function principle, which predicts that a separate lexical form will be preferred for a separate function. The processing cost of lexical, as opposed to morphological, marking may also be lower. In fact, an investigation of the semantic principle of optimization of form and function from a speech processing viewpoint might in general be a fruitful exercise, and would supplement the formalization of this somewhat abstract principle as a form of morphological blocking.

4.2.5 Regular Past Marking

Regular verb marking (including past participles, but not verbal adjectives—such as married) is the next most frequent and widely distributed form of marking. For the purposes of this study verbs marked with non-standard syllabic *-ed* (that is, with an epenthetic vowel inserted to avoid a cluster) are credited as tokens. Such syllabic marking is more common amongst those lower down on the *ASLPR*, and may be the form in which *-ed* marking tends to first emerge. While the insertion of an epenthetic vowel before the */d/* past morpheme is explicable as a phonological simplification it may in fact be interpretable also as a spelling pronunciation: given the lack of aural salience for *-ed* the morpheme may come to the attention of learners through written material before it is perceived in speech. This would explain the behaviour of Polish speakers who produce the morpheme in its epenthesized form when it is within their native phonetic competence to produce consonant clusters like */kt/* [1]. Tables 4.21 to 4.23 show the distribution of regular marked forms in descending order of frequency across the sample.

As can be seen from the tables, production of regular past tense forms is considerably more common amongst the Polish speakers than the Vietnamese speakers. None of the latter group with a an *ASLPR* rating of less than 1- produced a single token of a regular past tense form. Given that there is no such discrepancy between the two groups in regard to production of irregular past forms, the explanation is almost certainly phonological: Vietnamese lacks final consonant clusters, and Vietnamese speakers consequently experience difficulty in detecting and/or producing such clusters in English. Polish, on the other hand, is rich in consonant clusters—including some of those that result from *-ed* marking in Standard English—and so Polish speakers are not confronted with the same phonological problem.

(Proponents of contrastive analysis type explanations for syntactic phenomena could also point out that Polish speakers would already be sensitive to verb morphology phenomena. Given the phonological considerations

[1] M. Pietukowski, p.c.

above, however, it would be very difficult to evaluate this proposal).

Amongst the Polish speakers, those verbs which occur in three or more interviews in both the first and second round of interviews are worked and lived.

Amongst the Vietnamese speakers, there are never more than two informants per round who produce regular past forms. Asked is the only verb which appears in two interviews in each round. These verbs are high frequency items at all proficiency levels, so regular marking of verbs is most likely determined by nothing more than frequency of use of the verb in question; the more it is used, the more likelihood there is of it being marked.

Tables 4.21 to 4.24 show that when regular past marking does occur, it appears to be generally contextually appropriate. In fact, for the Vietnamese speakers, all the tokens produced were in past contexts. In the case of the Polish speakers, however, the exceptions are quite interesting and help to throw some more light on the principles behind verb marking in learner language.

Non-Standard Regular Past Marking

As pointed out in the notes to Table 4.21, there are a number of tokens of past forms (flagged with asterisks in the table), produced in non-past contexts.

Irena S. uses stayed in both the first and second interviews in habitual contexts; both tokens are also subjectless. Although Jan R. uses worked seven times in past contexts in the first interview, he uses it an almost equal number of times in present or habitual contexts. The supposedly unmarked form, work, itself, however, appears only once as a finite verb: as the corrected form of worked in a past context! (Working is corrected to work in the same way in the second interview). Since work does appear eight times as a noun, worked may in fact be the unmarked verb form—or at least one of them, since working appears twice as a main verb in past contexts. Krystyna A., also in the first interview, uses her only regular past form, worked, once in a subordinate (when conditional) clause, and once in a future context (the other tokens of work are nouns). (There is also a token of worked as a noun). And Krystyna B. uses liked twice in present contexts, once after don't in a negated verb phrase.

In the second round of interviews—see Table 4.22—Andrzej J. produces founded—an interesting individual composite of both regular and irregular past marking—as the complement of will, Krystyna B. produces stopped as the complement of must, while Ludwiga J. produces observed as the complement of impossible and also provides two tokens of waited in (probably) non-counterfactual if clauses.

All of this is very similar to what has already been observed with non-standard *-ing* marking. Jan R., in the first interview, for example, pro-

vides an instance of baseline verb marking, with worked. Krystyna A. and Krystyna B. and Ludwiga J. provide instances of verb marking in subordinate clause environments, and the latter two plus Andrzej J. provide further tokens of marking in verbal complements.

Interestingly, too, in longitudinal terms, after the first interview Jan R. abandons his baseline marking of work, and Krystyna A. her subordinate clause environment and future context marking. Both informants, in fact, produce fewer (in the case of Jan R., far fewer) regular past forms as a result of these adjustments. (Compare, Huebner's Postulate of Second Language Acquisition, Number I, which states that "the acquisition of the target language function of a given interlanguage form may require the reduction of the use of that form in target language obligatory contexts" [85]).

With the Polish speakers, then, for whom *-ed* marking in Standard English is phonologically salient, we seem to have a reduplication of the situation described for non-standard *-ing*. The adoption of *-ed* marking may have more fortuitous consequences than for *-ing*, given the contexts which favour such marking. Addition of -ed to the verb in the most favourable temporal context, Past, may very frequently result in the right form being produced. And even among the syntactic environments, in counterfactual *if* clauses, there is a possibility of stumbling on the right form.

Of course, this means that, in the case of the Polish speakers at any rate, the data on regular past marking has to be interpreted with caution. The instances of regular past marking in apparently appropriate contexts may, to some undeterminable extent, represent a fortuitous coincidence of learner marking patterns and Standard English rules, and not the direct application of the Standard English rules themselves.

4.2.6 Third Singular '-S' Marking

Tables 4.9 to 4.12 show the occurrence of *-s* marking of verbs for the four interview groups. As is evident, the extent of *-s* marking varies somewhat from interview group to interview group. Variation aside, however, it is evident that *-s* marked verbs are not common at any stage, for either language group. This is superficially surprising, given the apparent simplicity of the rule; and of course failure by learners to apply it has generated a good deal of pedagogic frustration. As will become clear, however, the non-application of this particular rule is not surprising when considered from a learner viewpoint.

The first thing to note about the figures in Tables 4.9 to 4.12 is that they are simply counts of the number of tokens of *-s* marked verbs. Closer inspection of these tokens reveals a number of things.

First, not all such tokens involve third person singular verbs in present or habitual contexts. Those informants whose output is either totally or largely non-standard, person-wise, have their counts flagged with an aster-

isk. Tables 4.9 to 4.12 show that non-standard usages are common amongst informants under level 1 on the *ASLPR*. Table 4.25 provides a detailed breakdown in terms of person and time reference of *-s* marking for all interviews in which it occurred.

The two main categories for the breakdown are Person and Time Reference. While the overwhelming number of tokens produced are third person singular present tense, there are a significant number of exceptions. With these, where the rule governing person and number is violated, the violation generally involves the marking of a verb with a first person singular subject; and where the rule governing time reference is violated, it is generally by a verb whose time reference is past. There are in fact a sufficient number of these exceptions for only twelve of the twenty-eight interviews in which *-s* marked verbs are to be found to exhibit totally standard usage. In addition, only three informants (Dung, Tam and Ludwiga J.) maintain totally standard usage through both interviews. And this is with no more than two tokens per interview.

Thus, where it occurs at all, *-s* marking could not be said to be very well established. This brings us to the second point, which is that *-s* marking in all probability occurs in only a small percentage of "obligatory contexts". Definitive statistics are not presently available in defence of this point. Table 4.26 might give a very rough idea of the situation, however. It contrasts the number of occurrences of he and she in the first and second interviews with the number of *-s* marked verbs, excluding is and has, found after either pronoun. Even allowing for the variety of contexts in which these subject pronouns could appear without occasion for *-s* marking (with verbs whose tense is other than present, with modals, in questions, etc.) the number of marked verbs would seem to be very small.

Why should such an apparently straightforward rule be so incompletely applied?

There are probably a number of mutually reinforcing reasons.

From a form-function viewpoint the *-s* morpheme in English is particularly confusing. It marks noun phrases as plural, denotes the genitive case, and serves as well as a (singular) verbal marker. Morphological marking of any kind probably runs contrary to the form-function principle at its strictest, in that it does not result in the production of clearly separate forms, each with a unique function. A morphological marker itself which has one form and many functions is worse still!

From a functional viewpoint, the third singular *-s* marker is redundant, since person and number are already marked in the subject noun phrase or pronoun.

In addition, if learning is driven by principles of universal grammar, learners of English are required to master an exception to general principles of verb marking in acquiring the third person singular *-s*. This is because the observed tendency in verbal paradigms is for the third singular to be the

unmarked form (compare Spanish or Italian) [114]. In English, of course, just the reverse is the case, and so learners have to swim against the universal current, as it were.

From a speech-processing point of view the production of the plural *-s* morpheme is a discontinuous phenomenon, in that the constituent governing plurality, the noun phrase, and the plural morpheme are separated from each other by a verbal constituent. It has been shown in studies of word order that production of utterances involving discontinuous phenomena involves the violation of certain optimizing constraints, which considerably increases the computational difficulty of producing these structures [126]. Examples of structures in which the appropriate discontinuous order has not been attained are not uncommon in learner language, and attest to the process hypothesized above:

he's have

- ka.1: [624] -

she's remember me and my sister

- vinh.1: [348] -

(For a more detailed discussion see 4.12.4). All of this constitutes a powerful disincentive for the learner. The rule of plural *-s* marking may be conceptually simple, but if all, or even some, of the above holds, it should be evident that conceptual simplicity is no guarantee of linguistic "naturalness" or "simplicity". And the evidence surrounding the acquisition of third person singular *-s* suggests that the latter principles are the ones which matter to the learner.

4.2.7 Aux_ing Marking—The Continuous Aspect

This section deals with the production of verbal structures to indicate the continuous or progressive aspect. In Standard English these take the form *[BE]...[VERB]-ing*. Syntactically, the task faced by the learner acquiring these structures is thus somewhat more complicated, since more than one element has to be manipulated and an auxiliary has to be introduced. The same is true for the *aux_en* structures dealt with in the next section. This syntactic complexity reflects greater semantic complexity as well, of course. However, in the case of these structures, it appears that mastery of the syntax does not signify mastery of the underlying semantics—an obvious point, but one worth reiterating for the more complex structures.

Tables 4.9 to 4.12 show the distribution of *aux_ing* structures for the four interview groups.

As can be seen from the tables, it is only amongst informants over the 1+ level on the *ASLPR* that non-formulaic tokens of *aux_ing* structures occur. In addition, there is a distinct difference between the two language groups in the production of these structures, which are far less common in the output of the Vietnamese speakers. (In fact, valid tokens may be restricted to one or, at best, two informants).

A considerable number of the tokens produced have been labelled formulaic. The principal reasons for this are:

1. If all the tokens are repetitions of the same phrase or contain the same verb or verb phrase, or

2. The main verb is headed by monomorphemic proforms such as I'm or I am.

These reasons cover the tokens judged to be formulaic in **jb.1** (two of four), **ka.2**, **dung.1**, **dung.2**, **sang.1**, **hoa.2**, **long.2**, **phuc.1** and **phuc.2**. (Further information is provided in the notes to Tables 4.9 to 4.12).

For the apparently *bona fide* tokens of *aux_ing* there are some interesting observations to be made.

In the first round of interviews with the Polish speakers, Andrzej J., Krystyna B. and Mieczyslaw M. are the only informants to produce instances of non-formulaic tokens. For these speakers there are in fact quite a large number of tokens. What is interesting about their output is that in all cases the majority of the tokens occur in past contexts. The time reference is appropriately marked in the auxiliary, was—which, at this stage, appears to be invariant for number. This is itself interesting because copular be is realized as were in the appropriate circumstances (past reference, plural or second person singular) by the same informants. This is another instance of how contextual and categorial features need to be taken into account when describing the spread of a form. Thus, while a was/were distinction exists for these speakers in locative and equative constructions, where be is categorially a copula, in *aux_ing* constructions, where be is an auxiliary, the distinction has apparently still not emerged. It is also worth noting that while were occupies three of the five paradigm slots for past forms of be, the initial form chosen is was—probably because it is the first person singular form. A neat instance of this contrast between copular and auxiliary be is provided in **aj.1**:

> Mm, yes... from time to time... and, er... when we... when we, er... were in Austria, er..., we... er, was writing to him
>
> - aj.1: [316–317] -

To return to the main theme, however, the notable thing about these tokens is that in the first interviews (or instances) past contexts predominate. In **aj.1** the ratio of past marked tokens to present or other is ten to five. In **mm.1** the ratio is nine to four; and in **kb.1** it is a rather less decisive four to three. This predominance of past referent tokens can also be observed in the output of one of the two Polish speakers whose use of the form dates from the second interview, Ewa S., where the ratio is two to zero. Given a degree of interchangeability between past continuous and preterite forms in Standard English itself, it is difficult to definitively determine with some of the tokens whether the continuous form is appropriate in Standard English terms or not. Nevertheless, in **aj.1**, eight of the ten tokens would appear to be inappropriate, with one optionally possible and one appropriate. In **kb.1**, one token is inappropriate, one is optionally appropriate, and two are appropriate. In **mm.1**, one token is inappropriate, and the other nine are optionally appropriate. In **es.2**, one token is inappropriate, and two are appropriate.

The first thing to note here is that where there is a high ratio of past to present tokens—**aj.1** and **mm.1**—there is also a high number of inappropriate or optionally appropriate aspectual selections. Pursuing this further, by referring back to Tables 4.17 to 4.23 and 4.24, it turns out that, with one exception, the alternative forms to the *aux_ing* forms actually employed—the regular or irregular preterites—are not produced. In **aj.1**, doubtful *aux_ing* tokens occur with the verbs work, go, mind, write, study and concentrate. With the exception of worked none of these verbs occur in simple past form. In **mm.1**, optional *aux_ing* tokens occur with the verbs work (seven tokens) and think (one token), neither of which occurs in preterite form. (The same is true for Krystyna B., who uses study, walk and do).

In other words, in these speakers, where *aux_ing* tokens are both largely past and either semantically inappropriate or at best optional, for the verbs concerned they are the invariant past form. This implies that the "optional" semantic status accorded to the past tokens of *aux_ing* in **mm.1** is probably meaningless—there is no visible alternative.

Even the exception—worket in **aj.1**

is probably not a case of random coexistence of two forms. This can be seen from the following example:

> another work... and then I was... eh, I worket, er, in the rlittle private farm [firm]... er, when, er, I wor... when I worket... w... was working by, er, painter...
>
> - aj.1: [197–199] -

Here, it appears that what is happening is that the informant is attempting to correct a phonetically non-standard worket token, by replacing it with

an *aux_ing* form. An examination of the context does not suggest that this is particularly semantically well motivated (the discourse at this point is not very coherent), but at least the latter form is phonetically correct. This uncertainty on the part of the informant concerning worket probably explains the joint presence of phonetically correct preterite forms, phonetically incorrect ones, and *aux_ing* forms. Even here, then, there is no real randomness or duplication of form—the variation is produced by an attempt to achieve phonological standardness. The *aux_ing* form seems to constitute the most accessible path towards this goal. (Just as the *-ing* form does for earlier attempts at less specific verb marking). Further confirmation of the role of *aux_ing* as a marking strategy in cases where obvious doubt exists also comes from **aj.1**:

> I ...when this man, er...er...er...was, er...had a work, er...er, he, er, taught me about it, and, er...I, er...I goed...
>
> GB Oh, ho!
>
> I Scuse me, this time [tense] is not good...I was going to...to him...and I worked there
>
> - aj.1: [210–7] -

Here, when the informant becomes aware that goed is not the correct form he explicitly indicates it and then resorts to the *aux_ing* form. (Went is not produced by Andrzej J. in either of his two interviews).

Aux_ing Marking—An Interpretation

The conclusions to be drawn from the above discussion should now be fairly obvious. For some informants at least, *aux_ing* structures simply provide an invariant way of past-marking particular verbs. In this capacity they can coexist with other forms of past marking. Thus both Andrzej J. and Mieczyslaw M. produce regular and irregular past forms. It is possible that this particular form of *aux_ing* evolves out of non-standard *-ing* marking. This intuitively appealing conclusion receives some support from the data. The three verbs which receive non-standard *-ing* marking in past contexts in **aj.1** are work, study and write, and these all figure amongst his *aux_ing* verbs as well. The same is true for think in **mm.1**. However, there are no instances of the high frequency *aux_ing* verb work amongst his *-ing* marked tokens. If non-standard *aux_ing* structures do in fact represent a refinement of the hypotheses which result in non-standard *-ing* marking, then we have a very good example of how a highly non-standard feature can contribute to the eventual acquisition of standard rules. And of course this constitutes a very powerful argument against pedagogical suppression of phenomena such as non-standard *-ing* marking.

Whether the phenomenon of non-standard past *aux_ing* observed in **aj.1** and **mm.1** is a general one is not clear. Nor is it clear that past *aux_ing* tokens are the first non-formulaic instances of *aux_ing* to appear. Of the two informants who begin to produce non-formulaic *aux_ing* structures in the second interview—Jan R. and Ewa S.—the former's tokens are mostly present and the latter's all past. Although past tokens predominate in the output of Andrzej J. and Mieczyslaw M., there are a considerable number of semantically standard present tokens, and there is no way to deduce what came first.

One possibility is that non-standard *aux_ing* has to be looked at in the context of the verb marking system as a whole, where it can be understood as an overgeneralization phenomenon resulting from the spread of *aux_ing* structures to past contexts, or perhaps as a systematic attempt to mark all past context verb phrases in some way. In this connection it is worth pointing out that the most frequently chosen verbs for non-standard *-ing* marking are the same as those verbs which are legitimately marked with the morpheme, which strongly suggests that both standard and non-standard *-ing* marking are a product of the same basic operations. It is clear that this issue warrants further attention. Hopefully, the longitudinal sample should provide more specific evidence on how individual systems evolve.

In the second round of interviews with the Polish speakers, three more informants produce possible non-formulaic tokens of *aux_ing* structures. These are Jan R., Jerzy B. and Ewa S. In **jr.2**, three of the four tokens are present and one is past. One token—a present context one—is semantically non-standard. It would be interesting to check later interviews with Jan R. to see whether he subsequently passes through the phase characterized by a predominance of non-standard past tokens observed in **aj.1** and **mm.1**. In **jb.2**, the non-formulaic status of the four tokens is not so definite as for **jr.2**. One token is a repetition of a formulaic future usage which occurs in the first interview, and two of the three others are headed by the possible monomorphemic proforms he's and she's. All tokens, valid or not, are semantically standard. In **es.2**, the two tokens produced are both past and semantically inappropriate, so this speaker may possibly be passing through the non-standard past phase also. Two tokens is not much to go on, however, and it would be interesting here too to check later interviews.

Turning to the speakers who produced non-formulaic tokens in the first interviews, we find that the ratio of past to present tokens is much more even. In **aj.2**, it is five to six, and the past tokens produced are semantically standard. In **kb.2**, it is six to eight, with two of the past tokens semantically appropriate and four optional. In **mm.2**, the ratio is three to four, with one past token semantically standard, one optional, and one inappropriate. This seems to indicate an evolution in the grammars of at least Andrzej J. and Mieczyslaw M. towards a more standard use of *aux_ing* structures. (Krystyna B., it will be remembered, did not produce a marked number of

non-standard past tokens in her first interview).

A further development of interest is the appearance of a was/were distinction for past auxiliaries in the two informants highest on the *ASLPR*—Krystyna B. and Mieczyslaw M. In **aj.2**, however, in the one obligatory context for were, was is produced.

Aux_ing Marking—The Vietnamese Speakers

As already noted, the above discussion, concerns only the Polish speakers. Amongst the Vietnamese speakers, tokens, formulaic or otherwise, are less frequent. In all but two cases, where they occur at all, *aux_ing* tokens are almost certainly formulaic. The exceptions are Canh and Phuc. In the case of Phuc, four tokens are headed by the proform I am, and one is infinitival; it is largely by virtue of the remaining token that there are grounds for not considering these to be formulaic. The deciding token is the following:

> they come to... a port, you know... many people were waitin(g) for them... but all mos(t) of them are
>
> - phuc.1: [876–7] -

This is semantically appropriate (the context is past) and exhibits the singular/plural distinction in the auxiliary, made elsewhere only by the Polish speakers most advanced on the *ASLPR*. (It raises interesting questions about the possible co-presence of formulas and parsed structures, or whether it is legitimate to consider likely formulas to be parsable items once an almost certainly parsed example of the same kind appears—see 2.2.3).

The case of Canh is somewhat problematic—his use of *aux_ing* structures seems to be both irregular and idiosyncratic when compared to the behaviour of the other informants. Non-past tokens constitute the whole output. Of these, one is semantically inappropriate for aspectual reasons, three are inappropriate for temporal reasons (the marking is present, while the reference is past), three are optionally progressive, and one obligatorally so. Of the tokens which should have borne past marking, one is definitely progressive, one optionally so, and the other inappropriately marked. The rules governing production of *aux_ing* tokens in this informant are consequently not obvious. Six of the eight tokens occur in subordinate clause or quasi-relative clause environments, or questions. For example:

> You see... the mon(k)s... I think they are hiding for military service, they want to go...
>
> - canh.1: [2426–7] -

or:

> Vietname(se) come here... some people, they are... living in the... countrysi(de)...
>
> - canh.1:[1450–1] -

or:

> Ah... have... have you ever think, ah, the Vietname(se) people, come Australian, ah... they are making, ah... ah... problem(s)
>
> - sang.1: [1847–8] -

It is just these environments which favour *-ing* marking, so the explanation may lie in this direction.

For the Vietnamese speakers then, we are left with one informant whose system with regard to *aux_ing* is superficially standard (Phuc) and another whose system is difficult to characterize (Canh). This does not permit any meaningful conclusions about developmental processes to be made for this group. The lack of *aux_ing* marking in two speakers whose ratings on the *ASLPR* are comparable to those of the Polish speakers for whom numerous tokens of *aux_ing* exist points to a conflict between assessed oral proficiency and syntactic features. This conflict is also evident in the case of Ludwiga J., who produces no *aux_ing* tokens in the first interview, and only formulaic tokens in the second (it is not only in this that Ludwiga J. resembles the Vietnamese speakers).

Aux_ing Marking—A Tentative Developmental Sequence

From the evidence to hand, it is possible to suggest a (take note) tentative developmental sequence for *aux_ing*. Since the first step in any such sequence will be the formulaic tokens, it would be appropriate to make some observations on these before proceeding.

In terms of form and apparent temporal reference, the great majority of formulaic tokens are present context. The formal exceptions come from **jb.1** (two will be tokens), **jb.2** (one will be token), and **long.2** (one will be token). The exceptions in time reference come from **jr.1** (a dubious token) and **hoa.2** (two tokens), plus, of course, the formal exceptions already cited. Definitive judgements on the aspectual appropriateness of the formulaic tokens are difficult to make, and serious analysis of the semantics of formulas may not be a meaningful enterprise, in any case. Bearing this in mind, however, it appears that with the exception of **hoa.1** and **hoa.2**, where the tokens are definitely aspectually inappropriate, the remaining instances of formulaic tokens are both temporally and aspectually appropriate. While this might seem surprising at first glance, it should be remembered that formulas, especially with informants not right at the beginning of the learning

process, are likely to be produced only in the discourse situations in which they have been learnt, where they are normally correct.

The first step in the sequence, then, is characterized by the production of formulaic tokens, whose form and probable temporal reference is generally present. If the reference is not present, then it will almost certainly be future—for instance, **jb.1**, **jb.2**, **lj.2** and **long.2**.

The next step might then involve the "deformularizing" (that is, spread) of present form tokens, as in **jb.2** and **jr.2**.

Following this, there could be a stage where past forms are introduced and then come to predominate, but with the semantics very much awry, as in **aj.1** and **mm.1**. The appearance of was as an auxiliary during this stage, it is worth noting, postdates its appearance as a copula, as is the case with were, which emerges in the next stage.

There would then follow a readjustment during which appropriate constraints on the past forms were introduced. During this stage, the past auxiliary would develop a distinct plural form.

Aux_ing Marking—Conclusions

In terms of general principles, then, the developmental processes revealed in the acquisition of *aux_ing* structures reveals a number of interesting things.

1. It appears that a final target structure may be the product of different acquisitional processes, even in the same informant. Thus, present form *aux_ing* structures may evolve out of formulaic tokens, while past form structures of the same kind may primarily represent a refinement of the hypotheses responsible for non-standard *-ing* marking of verbs, rather than a straightforward generalization of present form structures to other (primarily past) contexts. In other words, learners using *aux_ing* structures in one temporal reference context do not perceive that it is the aspectual feature of non-completion that governs production of the structure, and having made this perception go on to produce the structure in other temporal contexts, when aspectually appropriate. When they do produce *aux_ing* structures in other contexts it is generally in response to some more elementary need (such as that of marking tense), and the choice of the *aux_ing* structure is determined not by aspectual semantics but by earlier patterns of verb marking in the informant's own developmental history. The consistent association of the aspectual feature of non-completion with *aux_ing* structures in fact appears to follow the spread of the structures to tense contexts other than the present, and is not absolutely established even in the most advanced informants in the study.

2. Following on from this, the fact that, at least for some learners, highly non-standard patterns of verb marking can evolve into, or contribute

to the evolution of, standard patterns (as *-ing* marking does in **aj.1** and **mm.1**) provide pedagogical grounds for tolerating these patterns in learners. Even the most apparently non-standard features of learner language may turn out to be developmental stepping-stones.

3. The fact that were is realized as a copula while not necessarily as an auxiliary reinforces the case for implicational scaling as a means of understanding learner language phenomena.

4. Finally, the fact that superficially standard phenomena, such as the *aux_ing* tokens in **mm.1**, turn out, when inspected more closely and in the context of other aspects of an informant's output, to be governed by rules quite different from the standard ones, emphasizes once again the necessity of looking at learner language in terms of its own dynamics, rather than those of Standard English, even when the latter appear to account for what is initially observed.

4.2.8 Aux_en Marking

aux_en marking, as defined here, encompasses those Standard English structures which are characterized by an auxiliary derived from be or have and a main verb in the form of a past participle. This structural description actually covers a number of distinct possible sub-structures, if lexical and semantic considerations are taken into account.

Referring to Tables 4.9 to 4.12, we can see that the main category of *aux_en* has various subdivisions.

The first of these subdivisions, and the category into which the largest number of tokens fall is that of verbal adjective. This is in fact a category with somewhat ill-defined borders. Verbal adjectives are morphologically past participles. Formally, structures containing verbal adjectives look like passive structures without a surface agent—(for instance, Coal is mined). Semantically, however, they involve states rather than processes. (This semantic distinction is recognized in some languages—such as Spanish, which employs a different auxiliary for states and processes). Some verbal adjectives, however, appear to straddle this distinction between states and processes. Common examples would be verbal adjectives like worried, interested, and even tired. While these verbs describe physical or mental states, they can appear in passive-like structures—He is worried about his work, They are interested in the offer, I'm tired of films. All of these examples have obvious active counterparts. With such structures, the agentive preposition is not the usual by, and this may be a morphological way of flagging their difference from regular passives.

One approach to verbal adjectives would have been to simply treat them as adjectives with an accidental resemblance to past participles and exclude

them from a discussion of the learner's verbal system. This seemed inadvisable, however.

In the first place, since verbal adjectives are morphologically identical to certain verb forms their use by a learner is significant, in that it indicates that the learner can at least produce these forms. This may help to decide questions such as those concerning the role of phonological difficulty in inhibiting, say, regular verb marking. If an informant produces regular look-alike verbal adjectives but not regular pasts then we would know that phonetic difficulty alone was not the reason for the absence of the latter. The present study provides some interesting material in this connection.

In the second place, the structures in which verbal adjectives appear resemble other apparently more "difficult" structures. Simple equative structures containing verbal adjectives resemble those of agentless passives, and these same structures extended into pseudo-passives by the addition of a prepositional phrase are similar to full passives. These resemblances, and the abovementioned semantic features of some verbal adjectives, may provide the learner with an entree to acquisition of full passives. As we will see, the present data provides some support for this hypothesis.

Regular Verbal Adjectives

Regular verbal adjectives, as can be seen from Tables 4.9 to 4.12, are more frequent among the Polish speakers than the Vietnamese speakers. This is a reflection of the situation *vis-a-vis* regular past marking, and it helps to confirm that it is phonetic difficulty which inhibits the production of regular past forms by the Vietnamese group.

This conclusion is strengthened when we look for verbal adjectives or past participles in the output of those informants who do not produce any regular past forms. In the fifteen interviews concerned, verbal adjective or prepositional phrase tokens with *-ed* marking are to be found in only three. Given that most speakers who produce regular pasts also produce verbal adjectives, this strongly suggests that phonetic difficulty is a factor in inhibiting the production of regular past forms.

Interestingly, in two of the three cases where the barrier to regular past marking does not appear to be phonetic (Barbara B. and Long) there are other instances of greater than normal disregard for morphological marking [94].

Amongst the verbal adjectives produced by more than one informant are married, tired, closed, and surprised.

Irregular Verbal Adjectives

Irregular verbal adjectives, other than got, occur only in the output of those informants rated most highly on the *ASLPR*—Ludwiga J., Mieczyslaw M.

and Phuc. Since irregular verbal adjectives differ from regular verbal adjectives only in being less numerous in the target lexicon, their restriction to these speakers is probably a matter of lexis. Use of irregular verbal adjectives other than got, then, may be a useful benchmark for determining the size of a learner's lexicon.

Got itself deserves special mention. The peculiar characteristics of this verb have been discussed at some length in 4.2.4, to which the reader is referred. To recap just briefly, got exhibits the following notable features:

1. Its use as a participle is far more common amongst the Vietnamese speakers than amongst the Polish speakers. In the latter group, only Mieczyslaw M. and Zygmunt J. produce tokens of participial got (three between them), while in the former, Duc, Dung, Tam, Canh, Long, and Phuc produce a total of fifty tokens. This count would in fact be much higher if tokens of the semantically identical verbal form of got were included—see Tables 4.19 and 4.20.

2. Got appears as a participle, as opposed to a non-standard verb signifying possession, mainly under the scope of negation. That is, most cases of participial got occur in the phrase haven't got This could mean that for some speakers at least got is not really a participle at all, but rather a verb. Parsing haven't got in this way eliminates the distinction between the participial form and the non-standard verbal form, which tidies this part of the grammar, but at the cost of forcing haven't to be classified as a idiosyncratic negator, which complicates the proposed system of negation.

It is not clear why got—be it participle or verb—is so much more frequently produced by the Vietnamese speakers. There are various possibilities.

At the time of the interviews almost all the Vietnamese speakers had had considerable naturalistic exposure to English, while the Poles had not. Got is probably heard more outside the classroom than within it. Sang, the one Vietnamese informant whose exposure to English at the time of our first meeting was largely formal, does not use got in the first interview, while in the second, by which time he was working with Australians, he does. (It would be worth looking at later interviews with the Polish speakers to see if they use got more frequently as their conditions of exposure to English change).

Another possibility, already canvassed, has to do with the fact that Vietnamese speakers use have as an existential verb and in seeking to preserve an identity between form and function develop another form to signify possession. While this would be a rather neat explanation, it does not receive much support in the data. There are a good many tokens of possessive have

in the output of the Vietnamese learners—and while existential have is reported to be quite common amongst Vietnamese learners [2], in the present study there are few examples to be found (three in **duc.1**, one in **vinh.2**).

Closer scrutiny of the contexts in which have and got appear would perhaps produce a semantic or discourse-based explanation of the got phenomenon. In the meantime, the input explanation seems the most probable.

Agentless Passives

These structures have the form *SUBJ + BE + PAST PARTICIPLE*, and are therefore structurally identical to verbal adjective structures. The difference between agentless passives and equative verbal adjective structures is a semantic one. Examples of agentless passives (henceforth **APs**) from the data are:

English is... used in all... all... world

- es.1: [52] -

Lignite... eh, was, er, transported to the power station

- mm.1: [242] -

before I get married, you know

- long.1: [1060] -

two of my friends were injure(d)

- phuc.1: [832] -

Some of the tokens of *APs* are clearly formulaic. With others it is difficult to judge—for instance, the token from Ewa S. Reliable judgements can only really be made if an informant produces full passives also. Nevertheless, it is quite probable that the production of agentless passives represents a step towards the development of full passives. This hinges on the learner perceiving that the same structure can express a state or a process. Once this semantic distinction has been made the learner is in a position to explore what kind of syntactic extensions to the basic structure can be used to flag the agent in the process. A discussion of the full passive structures produced may perhaps make this last point clearer.

[2] G. Brindley, p.c.

Full Passives

Full passives have the structure *SUBJ + BE + PAST PARTICIPLE + PREPOSITIONAL PHRASE.* The prepositional phrase signifies the agent (demoted subject) in the process referred to, and is normally headed with by.

Regular full passives occur only in the output of the two informants most highly placed on the *ASLPR*—Mieczyslaw M. and Phuc, and even here the number of tokens is very small. Examples are:

> Yes but, eh, I, heh, eh, I know that, eh, Polish government is, er, press(ed) by Russian government
>
> - mm.2: [357–8] -

> you were accepted by Australian deligation
>
> - phuc.2: [980] -

One other informant, Krystyna B., produces a pseudo-passive structure:

> I am worried about Poland...about my family in Poland
>
> - kb.2: [354] -

This is an interesting token, because it involves a verb of the type common amongst verbal adjectives—one referring to a state of mind or body. The evidence is obviously too scanty for any valid conclusions to be drawn, but it would be interesting to determine if the case of Krystyna B. is a representative one—that is, are the pseudo-passive structures that result from making an agentive connection with this particular class of verbs the precursors of true passives? (There is a similar example in **mm.1**).

4.3 Passives—Summary

Passives are morphologically, semantically and pragmatically complex. This complexity is reflected in the apparent lateness of their acquisition by the informants in this study. While there is insufficient evidence to provide a detailed description of how they are acquired, some suggestions have been made. Traditionally, passives are taught by demonstrations of how they are derived from active structures. If there is in fact a developmental connection between verbal adjectives, agentless passives, pseudo-passives and full passives then pedagogic practice is far removed from the paths followed by learners.

4.4 Perfects

The final structure discussed under the heading of *aux_en* is the one generated by the expression of the perfect aspect. This is characterized by the elements *HAVE + PAST PARTICIPLE*. Depending on other aspectual considerations a second participle (present or past) can follow the first; in which case the first participle is coerced to been.

The semantics of perfects are very complex. At least four distinct conditions for their production have been identified [122]. Not surprisingly, then, examples of perfects are not common in the present data. No figures for likely or obligatory contexts for perfects throughout the interviews are available. However, as a matter of deliberate policy, almost all of the second-round interviews began with a question about what the informant had been doing since the previous interview, so that there were, in most cases, occasions for the production of perfect structures. In addition, subsequent discourse would have generally produced others. Many informants were unable to understand the question about what they had been doing, and it often had to be rephrased in some other way. Of probable relevance here is the fact that the key time adverb in this question, since, is itself used by only four informants - Barbara B., Ewa S., Mieczyslaw M., and Phuc. It is quite probable, then, that many of the other informants did not even understand the adverbial component of the question; and adverbs, as we shall see, appear to be the basic elements in learners' schemes of temporal reference.

Amongst the Polish speakers, only Ewa S. (one token) and Mieczyslaw M. (three tokens) produce recognizable examples of perfects. Ewa S. and Krystyna B. also both produce formally correct structures with preterite temporal reference. In the case of Ewa S.'s acceptable token (in **es.2**) the context would appear to demand a present perfect continuous whereas a present perfect only is supplied. Amongst the Vietnamese speakers, only Sang (both interviews) and Minh produce tokens of the present perfect. Minh produces only one token, which could well be formulaic. The semantics of at least two of Sang's tokens are not entirely standard, and in all but one case (or possibly all) the participle is not marked with any realization of the *-ed* morpheme. Moreover, three of the six tokens produced occur with one of the various keywords associated with the perfect aspect, just - which raises some doubt as to their non-formularity.

In any case, whatever judgement one comes to about the uncertain cases, the conclusion remains much the same: perfects are very infrequently produced. To judge from the adverbial evidence provided by use of since, only a very few informants even indicate any productive mastery of one of the most basic semantic strands of perfectivity in English. Where perfects do appear, except in a few formulaic contexts, they are frequently defective. With a target aspectual system so intricate that the apparently opposite

notions of perfectivity and continuativity may require simultaneous expression, this is not surprising. Equally, it is not surprising that most of the learner's communicative efforts can successfully ignore the level of discrimination represented by perfects.

Developmentally, emergence of a primitive perfect may coincide with other features observable in a morphologically advanced learner above level 1+ on the *ASLPR*. Since the number of tokens is always going to be rather small, there would be considerable difficulty in practice in sorting the formulaic tokens from those displaying some degree of rule generation. As is the case with other formulaic structures of very limited generalizability, those tokens which appear may seem quite standard. Appearance of perfect structures, then, might not be a very practical developmental benchmark.

4.5 Verbal Nominalizations

The final category to be examined under the rubric of verb marking is that of gerunds. Given the importance of *-ing* marking in learner language, the gerund, as a legitimate case of this form of marking deserves some attention.

For the purposes of the present study, nominalized verbs for which no common alternative form exists—such as swimming and walking—are not classified as gerunds. In the data to hand the majority of tokens classified as *bona fide* occur in prepositional phrases. Examples would be:

after our... leaving Polish

- lj.1: [407] -

I don('t) know abou(t) speaking English

- canh.1: [503] -

I got (a)noder job for... doin(g) mysel(f)

- long.2: [30] -

There are also some tokens where the nominalization is the subject of the sentence, or the direct object:

my writing English is better than my, er... speak

- aj.1: [27] -

Makin(g) sure you can do it

- phuc.2: [260] -

I.../...try translate my thinking in Polish to English

- lj.2: [290–1] -

The production of gerunds such as those above requires a good deal of structural complexity and clear categorial definition within the learner's speech, and is a correspondingly restricted phenomenon, as is evident from Tables 4.9 to 4.20.

One informant from the higher *ASLPR* bracket, Canh, produces a number of curious nominalizations, which seem almost the result of categorial uncertainty, such as the following example:

or you mu(st) go to the hiding...from police

- canh.1: [1498–9] -

These are, however, uncharacteristic of the sample as a whole.

Lower down on the *ASLPR*, Krystof S., Jan R. and Barbara B. produce several lexically non-standard tokens of nominalized verbs. What is interesting about these is that all the tokens occur in existential sentences:

in Austria is not...er...working for...not platz working

- bb.1 [107–8] -

Sunday...mm, is big, er...running

- jr.1: [1117] -

It is also worth noting that all three informants, particularly Jan R. and Barbara B., are frequent users of non-standard marking. It is very possible, given this, that their gerunds are fortuitous products of their favoured strategy of using *-ing* when marking appears to be required. This conclusion is reinforced by the fact that the tokens produced are non-standard and not likely to have been heard.

The cases of Jan R., Barbara B., and Krystof S., are another example of how non-standard *-ing* marking can result in standard patterns.

It would require further investigation to determine whether these cases genuinely represent the beginnings of nominalization. The question of why it is existential sentences in which gerunds first make their appearance needs further attention also.

Use of gerunds may be a reasonable indicator of syntactic development. While the number of tokens in the study is not large, their pattern of use is clear cut, and there seems to be no problem with formulas.

4.5.1 Adverbial Tense/Aspect Marking

It should be clear from the preceding discussion that morphological marking of verbs while a productive phenomenon in developmental terms is nevertheless, for the majority of the informants in this study, a restricted one. Given the functional importance of being able to signify tense/aspect distinctions, learners who are in the process of acquiring verbal morphology clearly have to have other means at their disposal for indicating temporal reference. One way to do this is by using lexical markers—time adverbs.

Tables 4.93, 4.94 and 4.95 and 4.96 provide figures for the distribution of time adverbs in the present corpus. The patterns of use that emerge are, somewhat surprisingly, quite implicational in appearance. For both groups, as might have been expected, the most frequent and widely distributed time adverbs are before, after, and now. As can be seen, this basic tripartite temporal distinction is established quite early. Once this distinction emerges, "directional" adverbs of time, like ago, and sequence markers, like then, can be seen to emerge. (Before and after can also function as sequence markers, of course). During this stage "aspectual" adverbs, such as still, yet, and already also appear. Later additions to the repetoire of adverbs tend to constitute refinements of the basic categories, like yesterday and tomorrow. The implicational-type patterning that can be observed in the tables suggest that this process of refinement is gradual and predictable. In general, perhaps because of the greater difficulties posed to them by morphology, the Vietnamese speakers use time adverbs more extensively than the Polish speakers.

The role of time adverbs in temporal reference and their relationship to these lexical markers and morphological marking are topics which could be fruitfully researched in much more detail. In this connection, it would also be a worthwhile enterprise to try to establish what interrelations might exist between temporal, spatial and discourse deixis as expressed by time adverbs, and personal and deictic pronouns. While not much is known about these matters at present it is obvious that the use of time adverbs should be fostered in pedagogical practice, since they are acquired early, are functionally important, and may even assist the learner to develop a framework of temporal reference appropriate to English.

4.5.2 The Verbal System—A Tentative Order of Acquisition

Keeping in mind that any postulated order of acquisition is predicated on cross-sectional data and needs to be verified by reference to longitudinal studies, we can suggest the following order of acquisition:

1. Following categorially uncertain lexical items, such as work the first definite verbal elements are attested to by

2. *-ing* marking, which is the first form of marking to emerge. Initially purely a categorial marker, *-ing* seems to come to be produced more frequently in "marked" environments—in past contexts, and subordinate clauses, for instance. *-ing* marked verbs are followed by some irregular past forms. Initially, these are not always produced in past contexts.

3. The next form of marking to appear is regular past marking. Phonetically, the */d/* marker may be preceded by an epenthetic vowel, even when it is already within the phonological competence of the speaker to produce the relevant cluster, as it frequently is with Polish speakers. Once again, these forms may not always appear in past contexts.

4. This is followed by third person singular *-s* marking. Such marking may not always initially occur in third person singular contexts.

5. *aux_ing* forms are the next verbal forms to appear. Semantically, these forms may serve as an alternative form of simple past marking for some speakers, and be restricted to particular lexical items. A good deal of idiosyncratic behaviour can be expected with these forms.

6. At about the same stage some productive *aux_en* forms may also appear, having been prefigured by verbal adjectives and formulaic or pseudo-passives. Productive expressions of perfectivity are very rare, as are full passives.

7. Also at this point, or perhaps even later, we find some standard-looking verbal nominalizations. At an earlier stage, a very limited number of non-standard *-ing* marked verbs may appear in existential sentences in the output of some informants.

While the above sequence has been described in conventional terms, like "regular" and "irregular", it is worth keeping in mind that superficially different forms of verb marking may in fact be governed by a small set of underlying principles for quite some time in the learning process. These principles essentially entail the marking of verbs in some way in syntactic or temporal contexts which are "marked"—basically, more complicated in some sense. As a result of this, target-like patterns of marking, such as *-ing* marking in complements of verbs of perception or even *-ed* marking in past contexts may be more fortuitous than they appear to be. However, since the principle of marking in marked contexts also holds in developed natural languages, learner systems can be expected to ultimately converge with the target system.

4.6 The Verbal System—Conclusions

Verbal morphology receives a great deal of attention in language pedagogy. It is evident from the above descriptions that the development of the verbal system in learner language is a slow and complex process which bears little resemblance to the course mapped out for it in current teaching practice. Assumptions that learners have acquired target language semantics because they use some target language forms—even when these forms appear in appropriate contexts—should in particular be avoided. While a suitable approach to teaching the morphology, syntax and semantics of the verbal system needs to be worked out in detail in the classroom, it would seem that given that even the most ostensibly non-standard features can evolve productively into target forms that this is one area where learners should be permitted to proceed at their own pace and in their own style, at least until more rational pedagogic approaches to teaching verb morphology have been developed and trialled.

4.7 The Copula

4.7.1 Production of the Copula

Tables 4.27 to 4.30 display the distribution of the various forms of be—both free and bound.

At this point, several general observations are appropriate.

1. Tokens involving apparently contracted forms of be, such as I'm or what's, may in some cases be monomorphemic proforms or noun phrases. This is clear in the following examples from Hoa:

   but I'm not go school, you know

   - hoa.1: [191] -

   I'm got a jo(b), you know

   - hoa.2: [813] -

   There is a problem, however, in deciding whether, on the basis of examples like those above all tokens of the form are monomorphemic, particularly if the majority of tokens occur in utterances which are superficially standard.

2. Similarly, some orthographically independent forms of be may in fact be fragments of monomorphemic noun phrases or proforms. Thus am, in some cases, may actually be part of a proform I am rather than a particular form of be. In some cases, this is demonstrably so, as in the following examples:

Informant	is	ks	zj	jb	jr	ka	bb	es	aj	kb	lj	mm
NONSTD_ING#tokens	4	11	1	8	35	3	18	1	6	3	14	12
NONSTD_ING#verbs	2	4	1	6	14	3	11	1	4	2	9	4
IRREGS#tokens		2?	1	2	6	8	2	6	11	35	14	15
IRREGS#verbs		2?	1	1	4	2	2	5	6	11	8	10
REGS#tokens	1*			3	23	1		9	10	14	26	17
REGS#verbs	1*			3	7	1		5	7	7	11	12
3SG_S		2*		1*	1*			2	11	1	1	
AUX_ING				4*	2*				15	7		13
AUX_ENreg_vb_adj				1F	1F	1F	4		2	3	1	2
AUX_ENirr_vb_adj											1	2
AUX_ENa'lesspass								1			1	5
AUX_ENfullpass												1
perfects								2*		1*		2
AUX_ENbeen				1F				4F		3F		
GERUNDS		1*			1*		1*		3		3	4

Table 4.9: Distribution of Verb Forms: Polish Informants—Interview 1

NOTES:

1. Counts suffixed with an "F" indicate probable formulaic tokens.

2. The single regular past form occurs in a present context in the case of **is**. The perfect forms occur in preterite contexts in the cases of **es** and **kb**.

3. The tokens of third singular "-s" involve verbs with second person subjects in the cases of **ks**, **jb** and **jr**.

4. Two of the aux_ing are probably formulaic in the case of **jb**. The remaining tokens, which are questions, are dubious, as are the tokens produced by **jr**.

5. The gerunds are lexically non-standard, and appear in existential sentences in the cases of **ks**, **jr** and **bb**.

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
NONSTD_ING#tokens	7	4	6	37	1	6				3	16	6
NONSTD_ING#verbs	5	4	6	12	1	5				2	9	2
IRREGS#tokens	3	10	2	11	9	2	17	12	11	30	8	13
IRREGS#verbs	1	4	2	4	5	2	6	9	5	9	5	8
REGS#tokens	4	4		3	1	8	3	9	7	7	15	17
REGS#verbs	2	3		3	1	4	3	5	6	7	7	8
3SG_S								5	10	10	2	4
AUX_ING				4	2F	4*		2	11	14	5*	8
AUX_ENreg_vb_adj		2		2	1F		2*		1?	4	2	1*
AUX_ENirr_vb_adj			1F									2
AUX_ENa'lesspass						1F						3
AUX_ENfullpass										1*		1
AUX_ENperfects		1F						2				6
AUX_ENbeen				1F				1F	2F			
GERUNDS				1*							3	2

Table 4.10: Distribution of Verb Forms: Polish Informants—Interview 2

Informants	Van	My	Duc	Dung	Minh	Sang	Hoa	Vinh	Tam	Canh	Long	Phuc
NONSTD_ING#tokens		2	7	5	3		10	1	11	34	84	13
NONSTD_ING#verbs		2	5	4	3		5	1	5	16	17	9
IRREGS#tokens			2	8	3	14	3	12	40	47	64	58
IRREGS#verbs			2	3	2	4	2	5	8	16	12	18
REGS#tokens					1	1		3	5	7		4
REGS#verbs					1	1		2	3	5		3
3SG_S			4*	2			1*			9	3*	18
AUX_ING				2F	1F	1F			8*	1?	3*	
AUX_ENreg_vb_adj	1F			1F	1F							
AUX_ENirr_vb_adj			1F	8F					5F	20F	7F	1
AUX_ENa'lesspass			1F					1F		1?	2*	2
AUX_ENfullpass												2
AUX_ENperfects						5*		1		1		
AUX_ENbeen										6*		
GERUNDS									1F	10*		3

Table 4.11: Distribution of Verb Forms: Vietnamese Informants—1

NOTES:

1. Counts suffixed with an "F" indicate probable formulaic tokens.
2. Tokens of aux_ing for **lj** all involve "will be" and are probably formulaic. One token for **jb** is possibly not formulaic.
3. Tokens of aux_en are non-standard in the cases of **bb** ("is goed") and **aj** (intransitive verb). The passive produced by **kb** involves "worried about".
4. The gerund is lexically non-standard, and appears in an existential sentence in the case of **jr**.

Informants	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
NONSTD_ING#tokens	1	8	7	5	8	2	7	3	19	27	24	1
NONSTD_ING#verbs	1	6	5	3	6	2	4	3	7	13	8	1
IRREGS#tokens		4	10	18	4	5	18	9	37	16	5	18
IRREGS#verbs		4	7	7	3	2	9	7	9	7	3	9
REGS#tokens				3			2	2	5	6		
REGS#verbs				2			2	2	2	5		
3SG_S		2	10*	1			1		1	6		3
AUX_ING			1F			3F	1F				1*	3
AUX_ENreg_vb_adj											1	
AUX_ENirr_vb_adj							4F		2F	1F	1F	1F
AUX_ENa'lesspass								1F				3
AUX_ENfullpass												
AUX_ENperfects			2*	1								
AUX_ENbeen										1F		
GERUNDS										1	5	3

Table 4.12: Distribution of Verb Forms: Vietnamese Informants—2

NOTES:

1. Counts suffixed with an "F" indicate probable formulaic tokens.
2. Tokens of third singular "-s" involve verbs with first person subjects for **sang**.
3. Tokens of aux_en lack final "-ed" marking in the case of **sang**.
4. Tokens of aux_ing are dubious in the cases of **sang**, **hoa**, and **long**, due to monomorphemic proforms.

Int	SCL	CCL	VCP	NVP	QUE	PST	PNT	FUT	#TKS
jr.1	1	6	1	1	4	22	6		35
bb.1	1	4	3			12	2		18
lj.1	2	6		4		8			14
mm.1	4	3	2		2	1		12	
ks.1	1	1	1	1	1	8		1	11
jb.1		1	1		2	5	2		8
aj.1	1		1			3			5
is.1		2				1	1		4
ka.1						3			3
kb.1				1		1	1		3
es.1		1							1
zj.1							1		1
Total	10	21	10	9	7	67	14	1	116

Table 4.13: Non-standard "-ing" Marking: Polish Informants—1

Int	SCL	CCL	VCP	NVP	QUE	PST	PNT	FUT	#TKS
jr.2	8	4	4	1	1	4	13	1	37
lj.2	1	8	2	1		3	1	1	16
is.2			4			3	1		7
jb.2		2		1	1	3			6
mm.2	2	1	2				1		6
zj.2	2				1		2	1	6
kb.2		1	1			1			3
ks.2		1	1			1			3
ka.2							1		1
Total	14	16	14	3	4	14	19	3	85

Table 4.14: Non-standard "-ing" Marking: Polish Informants—2

NOTES:

SCL = Subordinate Clause; CCL = Co-ordinate Clause; VCP = Verbal Complement; NVP = Negative Verb Phrase; QUE = Question; PST = Past Context; PNT = Present Context; FUT = Future Context; TKS = Tokens.

Int	SCL	CCL	VCP	NVP	QUE	PST	PNT	FUT	#TKS
long.1	18	18	14	1		41	8	1	84
canh.1	8	6	10	1	1	4	2		34
phuc.1	2		8			1	1		13
tam.1	3	1	1	1	1	4			11
hoa.1		1	3	3		4	2		10
duc.1		1	2			3	1		7
dung.1	2		2			1			5
minh.1			1			2			3
my.1				1			1		2
vinh.1		1				1			1
Total	33	28	41	7	2	61	15	1	170

Table 4.15: Non-standard "-ing" Marking: Vietnamese Informants—1

Int	SCL	CCL	VCP	NVP	QUE	PST	PNT	FUT	#TKS
canh.2	8	1	7	1		4	3		27
long.2	8	3	1	1	1	7	1	4	24
tam.2	7	2	4			6			19
duc.2	1		2			3	2		8
my.2	2	2	2	1		2		1	8
dung.2	3	1			2	2		7	
sang.2	2	1	1			2	1	1	7
minh.2						4	1		5
vinh.2	1		1				1		3
hoa.2	2					1			2
phuc.2	1								1
van.2							1		1
Total	33	9	19	3	1	32	12	6	112

Table 4.16: Non-standard "-ing" Marking: Vietnamese Informants—2

> I am an(d) my wi(fe), sewin(g) now too
>
> - my.2: [703] -
>
> No...no every day...er, sometime(s)... before I am mu(s)t to work, er...to work
>
> - minh.1: [515–6] -

Sometimes, however, no decisive evidence is produced one way or the other.

3. Some of the examples of the actual form be itself occur in almost certainly monomorphemic locutions like will be (these are discussed in 4.9). It is therefore not clear what status to assign to the form be in such tokens. Informants who produce monomorphemic will be are Jerzy B., Ludwiga J., and Long. In the case of the two latter informants, the will be tokens constitute a relatively small subset of their be tokens (twenty-three from ninety-two, and three from twenty-six respectively). In the case of Jerzy B., however, in the first interview the two tokens of be both occur in will be, and in the second interview will be accounts for one of the three tokens of be. (The other examples occur with must be—this may indicate a possible route for the emergence of be in some learners).

The counts presented in Tables 4.27 to 4.30 represent the number of times a given form appeared in a particular interview. Tables 4.9 to 4.12 document those cases in which some form of be appears as an auxiliary (in

Informant	is	ks	zj	jr	jb	ka	bb	es	aj	kb	lj	mm
went			1	1			1			1	1	
got		1?							5*	2		2*
said									1	14	5	1
left				3E			1?					5
learnt								1	3			2*
heard								2	1		1	
saw										3	1	1
found		1?								1		
did				1				1				
wrote			2E						3			
told					2			1				
built								1E				1*
bought										3		1
made										1		4*
stolen										1*	1*	
sent											1	4
came						7						
spoke						1						
ate								1				
eaten								1*				
taught									1			
done									1E			
took										1		
thought										5		
bore											1	
broken											1*	
born												1*
spent												1*
understood												1

Table 4.17: Distribution of Irregular Past Forms: Polish Informants—1

NOTES:

1. Counts suffixed with an asterisk indicate that the form in question was used as a past participle or verbal adjective, unless otherwise described below. Counts suffixed with an "E" indicate that the token was produced as an echo.
2. Past forms were produced in present contexts in the cases of **ks** (all tokens) and **aj** ("got"—one token), and in an irrealis context in the case of **mm** ("got"—one token). Two tokens of "made" are past participles in the case of **mm**.

Informant	is	ks	jr	ka	zj	jb	bb	es	aj	kb	lj	mm
said			3	1			9	1	4		4	1
went		4	1		1		3		1	2		
saw				1						9	3*	1
made	3	3								3		
bought		3*						3*				3*
came						1			1	6		
got					2*							1*
told							1			8		
thought							1				1	
learnt							1	2				
gave								1		1		
found									4*		1	
took										1		1
sent											1	3*
left			5*									
wrote				4								
lost				2								
spoke				1								
born						1*						
won								3				
knew								1				
beaten								1?				
taught									1			
felt										2*		
heard												2*
done												1*

Table 4.18: Distribution of Irregular Past Forms: Polish Informants—2

NOTES:

1. Counts suffixed with an asterisk indicate that the form in question was used as a past participle or verbal adjective, unless otherwise described below. Counts suffixed with an "E" indicate that the token was produced as an echo.
2. Tokens of past forms are participials in the following cases: **ks** and **es** ("bought"—one token) and **mm** ("bought and "sent"—two tokens).
3. Past forms were produced in present contexts in the cases of **aj** ("found"—one token) and **kb** ("got"—one token), and **mm** ("heard"—one token).

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
came			1	4	1		2	1	3	8	44	2
got			1*	8*		3		2	11*	35*	8*	3
went				3			7		10	2	10	10
left			1E				2		2	1	1	1*
said						1		6	16	1	3	
lost						1E	1?				7*	1
heard								1		4*	1	1
bought			1		3					1		
took									2	1		3
told									8		1	10
thought								1				4
born								1*		4*		
met										4*		3
made										2		2
shot										3		1
sent											1	1
seen				1								
gave							3					
caught								1*				
forgotten								1*				
drove									1			
gone									1*			
fed										1*		
knew										3		
sank										2		
won										3*		
broken											1*	
found											1*	
felt												3
kept												2
saw												1
slept												1
spent												2
spoke												1

Table 4.19: Distribution of Irregular Past Forms: Vietnamese Informants—1

NOTES:

1. Counts suffixed with an asterisk indicate that the form in question was used as a past participle or verbal adjective, unless otherwise described below. Counts suffixed with an "E" indicate that the token was produced as an echo.
2. Tokens of past forms are participials in the following cases: **tam** ("got—five tokens) and **canh** ("got"—twenty-two tokens, "heard"—one token) and **long** ("got"—six tokens).

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
got			3*	1*		5*	5*	1*	5*	7*	2*	4*
told		1	2	4			1	1	11		2	3
went		1	2				5		1			1
saw				1	2		1*		2	1		
said				7			2	3	16			
did							1			2		3
came							1		1	1		
took		1?			1?							
bought				3								1
forgot							2*		1*			
lost											1	3*
caught		1										
sent			1									
broken				6*								
broke				2*								
left								1				
learnt								1				
born								1*				
rang								1				
woke									1			
heard										1*		
understood										1		
thought												1
drank												1
became												1

Table 4.20: Distribution of Irregular Past Forms: Vietnamese Informants—2

NOTES:

1. Counts suffixed with an asterisk indicate that the form in question was used as a past participle or verbal adjective, unless otherwise described below. Counts suffixed with an "E" indicate that the token was produced as an echo.
2. Tokens of "got" are participial in the following cases: **Sang**, **Minh**, **Canh**, **Vinh**, **Canh**, **Phuc** (one token), **Long** (two tokens) and **Dung** (three tokens). With the exception of the case of Phuc, verbal "got" is used in present contexts.

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
lived					2			4	1	2	3	1
worked					13*	4*			5	7		
arrived				1	3				1			1
decided									3		12	6
learned									1		1	1
started										1	1	1
stayed	1*			1								
finished				1							2	
asked					2				3			
looked									1	2		
tried											1	1
produced											1	1
writed					3							
maked		1										
watched					1							
sleeped								2				
stopped								1				
teached								1				
danced								1				
listened									3			
thinked									1			
goed									1			
readed									1			
liked										3*		
married										1		
interested											2	
suggested											2	
helped											1	
showed											1	
buyed											1	
visited											1	
moved												1
improved												1
exported												1

Table 4.21: Distribution of Regular Past Forms: Polish Informants—1

NOTES:

1. Participial forms have been excluded from this table. They were not numerous.
2. Tokens of past forms occur in non-past contexts in **is** ("stayed"), **jr** ("worked"—six tokens), **ka**, ("worked"—two tokens), and **kb** ("liked—two tokens).

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
finished		1		1					2		1	2
worked	3					3		3				
lived								1		1		3
looked									1	1	1	
stayed	1*											1*
happened				1	1E							
arrived						2					1	
learned						2		3				
talked									1	1		
called										1	1*	
studied		1										
fixed		2										
helped				1E								
composed						1						
goed							1					
meaned							1					
worried							1					
ended								1				
exercised								1				
writed									1			
knowed									1			
founded									1*			
watched										1		
stopped										1*		
decided											8*	
waited											1*	
observed											2*	
travelled												2
occupied												2
expected												1
enclosed												1

Table 4.22: Distribution of Regular Past Forms: Polish Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
asked							1		2			
studied								2				1
prayed								1				1
worked					1							
married									2			
changed									1			
happened										3		
scared										1		
failed										1		
finished										1		
contacted										1		
helped												2

Table 4.23: Distribution of Regular Past Forms: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
asked									1	1		
called					2							
looked				1								
lived							1					
started							1					
goed								1				
weighed								1				
talked									1			
enjoyed										2		
tried										1		
used										1		
produced										1		

Table 4.24: Distribution of Regular Past Forms: Vietnamese Informants—2

Interview	3SG	1PN	3PL	PNT	PST	FUT	TOTAL
phuc.1	18			16	2		18
aj.1	7	1	1	2	5		11
aj.2	10			10			10
kb.2	9	1		9		1	10
sang.2	8	2		10			10
canh.1	4	5		6	2	1	9
kb.1	5		2	2	5		7
canh.2	5		1	6			6
es.2	5			5			5
duc.1	1	3			4		4
mm.2	4			4			4
long.1	1	1	1	2	1		3
phuc.2	3			3			3
duc.2	2			2			2
dung.1	2			2			2
es.1	2			2			2
ks.1		2		2			2
lj.2	2			2			2
tam.1	2			2			2
dung.2	1			1			1
hoa.1		1		1			1
jb.1			1	1			1
jr.1		1			1		1
ks.2	1			1			1
lj.1	1				1		1
minh.2	1				1		1
mm.1	1			1			1
tam.2	1			1			1
Total	96	17	6	97	22	2	121

Table 4.25: Environments for Realization of Third Singular "-s"

Interview	"S" Marking	Pronouns
aj.1	1	7
aj.2	4	18
bb.2		6
canh.1		8
canh.2	1	17
duc.1	1	30
duc.2	1	28
dung.1	1	5
dung.2	1	22
es.1		3
es.2	3	30
hoa.1		27
hoa.2		37
is.2		10
jb.1		4
jb.2		13
jr.1		23
jr.2		16
ka.2		7
kb.1		23
kb.2	5	40
ks.1		6
ks.2	1	10
lj.1		5
lj.2		19
long.1		91
long.2		4
minh.2		19
mm.1		26
mm.2	3	19
my.1		2
my.2		23
phuc.1	5	28
phuc.2		4
sang.1		5
sang.2		17
tam.1		64
tam.2		51
vinh.1		17
vinh.2		22
zj.1		3
zj.2		6

Table 4.26: 3sg"-S" Marking Compared to Instances of "He" and "She"

aux_ing or *aux_en* structures). For convenience, these have been summed and added to the bottom of Tables 4.27 to 4.30 under the rubric of *AUX*. No attempt has been made to decompose these instances of auxiliary be into the various possible forms in the paradigm. It should be clear, however, from the figures provided that be as a copula is much more frequent and widely distributed than be as an auxiliary.

So far, no figures have yet been compiled for the non-realization of the copula in environments where it could be expected to appear, nor has any investigation of a possible hierarchy of contexts favouring the production of be been conducted. Other studies have postulated that copular be emerges first in pre-adjectival environments and subsequently appears in pre-nominal ones [129]. The research could be very usefully extended in the above directions.

Despite the limitations noted above, a number of conclusions emerge quite clearly from the analysis.

4.7.2 'Is'

Is, and its bound variants, is the most widely distributed, and by far the most frequent, form of be. Except in the case of My, where the counter-examples are probably formulaic, every informant who produces be in some form produces tokens of lexical is. While there is insufficient longitudinal data on the acquisition of is, with only two informants not producing the form in the first interview, on the basis of the cross-sectional patterning it is fairly probable that is is the first form of be to be acquired.

There is, however, a very striking discrepancy between the Polish speakers and the Vietnamese speakers in regard to the relative frequency with which is is produced. This discrepancy is evident, possibly to a lesser degree, with other forms of be as well.

Although counts for null copula have not been done, it is clear from a perusal of the transcripts together with the evidence from the relevant tables, that Vietnamese speakers very frequently fail to produce the copula in structures where the target requires it.

Within the theoretical framework of the *ZISA* Project, there is, as described in 2.1.8, a distinction drawn between developmental and variational features [36]. Developmental features are necessarily acquired in a particular order because they depend on increasingly complex and interdependent speech processing prerequisites. Variational features, however, are not constrained in the same way. The copula in German was classified in the *ZISA* project as a variational feature. Once acquired, the conditions affecting its suppliance are different from the computational and cognitive processes that determine, say, word order phenomena.

If this developmental/variational distinction is accepted there is no reason not to assume that the English copula also has the status of a variational

feature. In the absence of such a distinction, one would be left with the problem of explaining why a learner such as Phuc, who has a high *ASLPR* rating and displays considerable mastery of likely developmental features, such as modals, verb morphology and subordination, performs so poorly in this particular area.

Variational features were originally envisaged as being determined by sociolinguistic factors—exposure to native speech, social and psychological distance, etc. These factors may well have a bearing on the low rate of copula realization amongst the Vietnamese speakers.

One further factor, however, is almost certain to have a considerable influence. This is first language: Vietnamese does not require the copula in equative structures involving predicate adjectives. There are two ways of looking at the effects of first language in this case. One is to conclude that the copula, as a variational feature, and one therefore largely unconstrained by semantic or processing factors, is highly vulnerable to influence by extraneous factors, such as first language rules. A second approach, which adheres more closely to principle that variational features reflect sociolinguistic factors is to include such traits as reluctance to abandon first language features amongst the sociolinguistic factors, perhaps under some such heading as social distance. Under both approaches, variational features become likely targets for interference or transfer phenomena. The main difference is that the second approach makes more specific proposals as to when these phenomena would manifest themselves. These proposals should be testable. Amongst the present group of Vietnamese informants both Vinh and Phuc, who come from an educated middle-class family, seem to perform better in regard to copula production than the other informants. This offers tentative support to the second of the above approaches. It would be interesting to check the figures for copula production more closely, and to study this feature in a larger group of speakers to see how well the proposal is borne out.

4.7.3 'I'm' and 'Am':

As can be seen from Tables 4.27 to 4.30, amongst the informants ranked lower on the *ASLPR*, a good many tokens of I'm are demonstrably pronominal. In some cases, this is so for an informant's whole output of this particular form. In all four interview groups this phenomenon extends at least to level 1 on the *ASLPR*, and beyond it in the case of one or two informants.

With regard to am, it is frequently more difficult to find clear evidence of its use as a proform fragment. It appears, however, that the Polish speakers are more successful in manipulating this form than the Vietnamese speakers, whose output includes several cases of I am as a pronoun. This is, of course, in line with the overall pattern for copula insertion for the two language groups. In the cases of My and Duc, there are one or two cases where it

is difficult to decide if I am is a pronoun, or am is somehow being confused with have. This is because of utterances such as the following:

becau(se)—I am no money

- my.2: [844] -

4.7.4 'Are'

The discrepancy between the two language groups in the frequency with which are occurs is not so great. The probable explanation for this lies in differential discourse patterns for the two groups, for which a rough index can be found in the distribution and frequency of pronoun use. Tables 4.76 to 4.80 show that you, and to a lesser extent they, are both more widely distributed and much more frequent for the Vietnamese speakers. (The implications of this are discussed in 4.17). The much larger number of possible occasions for are in the discourse of the Vietnamese speakers probably compensates for the lower likelihood that they will produce this form of the copula.

It should also be noted that the sole tokens of are in **my.1**, **minh.1** and **ka.1** are in formulaic questions, and are therefore dubious. In **hoa.1** and **es.1** two of the three tokens produced are also in formulaic questions; and in **jb.1** the count is three out of four. Although we can assume that the form is established for these informants, this affects the status of the counts, which after adjustment sink to a single token per interview for all informants under 1+ on the *ASLPR*, with the sole exception of Dung.

4.7.5 'Was' and 'Were'

For the Polish speakers, the distribution of was is quite wide, beginning at *ASLPR* level 0+. For the informants above level 1+, there is a marked increase in the number of tokens produced. For the Vietnamese speakers, the distribution is rather more ragged (with the lower threshold still at about 0+ however) and the frequencies are considerably lower. Once again, this is presumably a reflection of the overall discrepancy for the two groups; there may, however, be a greater tendency for the copula to be inserted where a non-present tense has to be indicated, which would have the effect of decreasing the discrepancy to some extent.

For were, the distribution is narrower and more ragged. This is true for both groups, though more so for the Vietnamese. Nevertheless, there exists an implicational relationship between was and were; if the latter is produced, so is the former.

It is also worth noting that distinctions involving person and number, such as that between was and were seem to emerge at different stages for copular and auxiliary uses of be. For a more detailed discussion of this,

and other phenomena involving both be and verbs in general, the reader is referred to 4.2

4.7.6 'Been'

Been is basically found in two structures. The more widely distributed of these is in (largely formulaic) tokens of the present perfect:

> they ... er... ha(ve) been here... two year(s)
>
> - es.1: [154] -

> Yeah, my name is Dung... I have been in Australia, er, for two year(s)... nearly two years
>
> - dung.1: [8–9] -

The other use of been, which obviously results from a simplification of the present perfect structure is as a preterite form of be:

> Die bed... the rooms been... no big... no big
>
> - ka.1: [680] -

> Thailand... we been... er... in refugee camp... Songkhla
>
> - tam.1: [129] -

The distribution of been is fairly ragged, which is what one would expect for an item found almost exclusively in formulas.

A more extended discussion of perfects is to be found in the section on *aux_en* in 4.2

4.7.7 'Be'

The principal significance of the infinitival form be itself, is that since it almost always occurs as the complement of some other verb, or in conjunction with a modal, it can serve as a quite useful index of clausal complexity. Like its finite forms, however, it is subject to variable realization patterns and is both more widely distributed and more frequent amongst the Polish speakers. The variable nature of be has, of course, to be taken into account for it to produce reliable results as an index of complexity; some monitoring of the production of a form such as is would have to be carried out first.

For speakers with a high degree of copula insertion be may emerge as a form at about the same time as modals like will and must begin to be used. The reader is referred to 4.9 for more discussion of this topic.

4.8 Summary

While there are certain regularities in the production of the copula, and evidence for differential treatment of be as a copular form and an auxiliary, it appears to be the case that a good deal of variability will be manifested in the suppliance of the copula, once the processing prerequisites for its production have been met [128]. This variability seems to be partly indexable to learner type and to psycho-sociological factors, and partly a result of first language patterns.

4.8.1 Note on the Tables

The following points should be kept in mind when consulting Tables 4.27 to 4.30:

1. Tokens apparently involving contracted forms of be, such as I'm or what's, may, in some cases, very possibly be monomorphemic noun phrases or proforms. Where this is definitely the case, the counts in question are suffixed with a *P*.

2. It should also be kept in mind that in some cases apparent lexical tokens of be may be artifacts of the transcription and would be more properly described as part of monomorphemic noun phrases or proforms. For example, am might be a fragment of the proform I am, rather than a verb. These phenomena are more likely to characterize the output of the less proficient or more simplifying informants.

4.9 Modals

4.9.1 Distribution of Modals

Modals, because of their own special properties and because of the pedagogic attention they receive have been allotted a section to themselves. The basic data relating to these elements of the verbal system is summarized in Tables 4.31 to 4.34.

As with the other interview group tables, Tables 4.31 to 4.34 are organized according to*ASLPR* rating. In the case of modals, particularly with the Vietnamese speakers, this provides some encouragingly implicational patterns.

It is worth remarking at this point that in general it appears to be the case that the Vietnamese speakers pattern more regularly than the Polish speakers in their linguistic behaviour. It is conceivable that this is due to either:

Informant	is	ks	zj	jb	jr	ka	bb	es	aj	kb	lj	mm
is	11	40	12	20	74	40	156	51	43	85	85	66
it's		5			1	2	3	4	6	18	16	45
he's					1	1		1	1	3	1	
that's		1				1					15	1
NP's			1			2		1		1		
what's		2		2					1			
she's								1		2		2
I's					2							
isn't											25	
I'm	1P	7P	1?	1P	2P		6P	2	4	7	14*	4
am		1		2		7	5		6	2		
was		4	2	6	1	9	1	11	62	45	47	58
wasn't						2					5	1
are		2*		4*		1*		3*	9	3*	4	5
be				2*	1?				7	6	18*	7
were						1		2	2	2		2
weren't						1						
been				1		1*		4		3		
AUX				4*	3*			1	15	7	1	20

Table 4.27: Distribution of Copular Forms: Polish Informants—Interview 1

NOTES:

1. Tokens of "are" occur in formulaic questions in the cases of **ks** (two tokens), **jb** (three tokens), **ka**, **es** (two tokens), and **kb** (three tokens).
2. Tokens of "be" occur in the locution "will be" in the cases of **jb** and **lj** (five tokens).
3. The aux_ing tokens are formulaic in the case of **jb** and dubious in the case of **jr**.
4. With **ka** "been" is a preterite rather than a participle.
5. With **lj** some tokens of "I'm" are definitely proforms.

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
is	34	64	42	66	40	44	39	47	69	74	96	81
it's	4	2	1	9	4	3	87	13	6	30	23	22
he's	2			2		2	1	6	1	5	1	
she's				1	1	2		2		5		2
that's		1	1			1			1		6	
isn't								1			11	1
NP's									1			2
what's		3										
when's							1					
was	4	2	1	4	9	12	13	11	21	23	25	12
wasn't					1							
I'm	2?	1P	1P		1?	3*	9		10	5	6*	3
am					1	1	1	2	2	7		3
are			1	2	1	1	1	6	10	11	9	17
we're								1				
be			3	7		3*	18*	1	15	3	74*	9
been				1	3*			1	2			
were						1	1			3		1
AUX				4	2F	5*		2	11	15	5*	20

Table 4.28: Distribution of Copular Forms: Polish Informants—Interview 2

NOTES:

1. Tokens of "be" occur in the locution "will be" in the cases of **jb**, **bb** (all tokens) and **lj** (thirty-seven tokens).
2. The *aux_ing* tokens are formulaic in the case of **lj** and dubious in the case of **jb**.
3. With **ka** "been" is a preterite rather than a participle.
4. With **lj** some tokens of "I'm" are definitely proforms. In the case of **jb** all tokens of "I'm" are subsequently corrected to "I", indicating a period of transition for this informant.

Informant	Van	My	Duc	Dung	Minh	Sang	Hoa	Vinh	Tam	Canh	Long	Phuc
is			9	2	4	2	6	30	1	13	12	27
that's								4		23	63	29
he's							3	1		1	11	
what's					1		1		1			
it's								2		2		13
NP's							1					2
she's							1	1				
I's											1	
we's											1	
I'm			1P		2	2	29P	7	5	19*	9*	1
am		3?		1	3*	3		1		5		17
are		1*		5*	1*	2*	3*			40	5	31
was			1?	1		2		6		5	1	21
were			1?					1		3		13
been				1	1				1*	6	9*	
be										3	21*	3
AUX										8*	2*	7

Table 4.29: Distribution of Copular Forms: Vietnamese Informants—Interview 1

NOTES:

1. Tokens of "are" occur in formulaic questions in the cases of **My**, **Dung** (two tokens), **Minh** and **Hoa** (two tokens).
2. Tokens of "be" occur in the locution "will be" in the cases of **jb** and **lj** (five tokens).
3. The *aux_ing* and *aux_en* tokens are dubious in the case of **Canh** and **Long**.
4. With **Tam** and **Long** "been" is a preterite rather than a participle.
5. In the cases of **Hoa** (all tokens), **Canh** (two tokens), and **Long** (at least four tokens), tokens of "I'm" are definitely proforms.

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc—
is	1	1		6			1	14	1	10	1	23
it's		3		3		1		1	2	7	1	7
that's					1	1		1		6	14	19
he's				2		1				6		
NP's										2		1
what's							1					
who's							1					
where's								1				
we's									1			
I'm	4P	4P	7		1	38P		5		14*	1	4
am	3*	1*			6*		1*			1		6
be			1			2?		1			5	14
are		1?			1?		1			1		4
was			1				1	1				
been									1*	1		1
AUX											1*	6

Table 4.30: Distribution of Copular Forms: Vietnamese Informants—Interview 2

NOTES:

1. The *aux_ing* tokens are dubious in the case of **Long**.
2. With **Tam** and **Phuc** "been" is a preterite rather than a participle.
3. In the cases of **My** and **Dung** tokens of "am" are fragments of the proform "I am".

1. The fact that they have had, in nearly all cases, considerably longer periods of exposure to English than the Polish speakers, which has led to the "stabilization" of their interim grammars, and/or:

2. The fact that, unlike the Polish speakers, they were not (with the exception of three of the less proficient informants who were attending part-time courses) currently receiving instruction. If this latter consideration is a valid one, then it tends to indicate that instruction is capable of "disturbing" naturalistic learning processes. This disturbance may be explicable in Labovian terms as the result of increased linguistic "self-consciousness" [149] (see also 3.4.2). Whether or not such disturbance is a positive or a negative phenomenon must remain an open question: there is, of course, no *a priori* reason why it should be negative.

While the Vietnamese speakers pattern more regularly, the patterns for the use of modals for the two language groups are basically similar, with one exception. This concerns the relative frequency of must and can. For the Vietnamese speakers overall, can is their highest frequency item, both in gross and distributional terms. For the Polish speakers, this place is occupied by must—see Tables 4.34 to tables 4.32. This overall state of affairs is reflected in the separate counts for the use of these items for the*ASLPR* range from 0+ to 1+. For the Polish speakers above level 1+ this discrepancy in the use of can and must is considerably smaller, as can be seen from the tables.

As far as can be determined, there is no differential pattern in the semantics of these two items as they are used by the two groups which could explain this divergence. The reason for it—it is tempting to suggest—may be in fact cultural. Whorfian treatment of linguistic data, however, can be a dangerous undertaking (consider Lakoff's anecdote about the mysterious language with an extraordinarily large set of verbs for describing modes of destruction—which turned out to be English), and will not be pursued here (cf. the work of Whorf for an extreme example of the "language shapes thought" position[183]).

No very detailed analysis of the semantics of the modals which appear will be provided here. However, it is probably worth noting down some of the salient points concerning each of these modals as they are used by the informants in the study. The observations made here are based on data from the first round of interviews. Some evidence from the second round interviews will also be adduced.

4.9.2 'Can':

In general the semantics of can appear to be fairly standard. It is produced in a variety of syntactic environments from declaratives to questions. In most

cases it is followed by a main verb; occasionally it is used anaphorically:

Yes I can

- dung.1: [78] -

The principal semantic function of can is to indicate capacity. In informants higher up on the*ASLPR* scale can is also used on occasion to indicate permission (**long.1**), and probability (**canh.1**). Not surprisingly, can is sometimes used in past contexts, rather than could. It should be noted that in its function as an indicator of capacity can provides a natural means of expressing propositions about the future.

4.9.3 'Must':

In the majority of cases, must is used, in a more or less standard way, to indicate obligation. It does not seem to be used to express strong probability, however. Only one informant, Long, uses must where another modal is definitely called for:

I hope we... we must go to school one day

- long.1: [374] -

We shall return to the possible implications of this overgeneralization shortly. There are, however, a number of very puzzling tokens involving must, in the output of Long and Tam. These involve the curious locution must have to:

You must have to learn more, Tam... stop, please

- tam.1: [805] -

In all cases, (four in **tam.1**, one in **tam.2**, two in **long.1**), the final element in the structure is learn. In **tam.1**, it transpires, learn only appears in the locution have to learn, and likewise have to, so that it is very probable that have to learn is an idiosyncratic realization of learn. Similarly, in **long.1**, only two or three of the twenty-five tokens of learn are not have to learn, and the same conclusion can therefore be drawn as for Tam. If have to learn is in fact an idiosyncratic form of learn then the behaviour of must is unexceptional in these cases too.

The only notable non-standard semantic feature of must is that it is used in past contexts. Straightforward examples of this would be:

In, um, Songkhla.../... I mus(t), er, sell ci(ga)re(tt)es

- sang.1: [202] -

everybody.../...must go in Army

- long.1: [111–12] -

In their work on the acquisition of German as a second language, Klein and Dittmar found that müssen was "apparently used as a substitute for morphological tense markers of the verb", in particular as a past marker [155]. While tokens of "must" occurring in past tense contexts in the present corpus are by and large similar to the examples given above, and probably ascribable to the simple absence of an appropriate past form for must, there is some possibility that a similar process of overgeneralization may be operating in the case of English must, at least for some informants. Thus, in addition to the use of must in a sentential complement of hope (cited above) in **long.1** there are tokens such as the following:

an(d) we Army before we haven't got any money.../...but we must looking, you know, because we...we think .../...I get fisherman after that I can drive go out

after that I mus(t) thinkin(g), you know, an(d) about not be long...about two, t(h)ree week(s) I know him, you know, an(d) after that understand very well

- long.1: [188–90] & [497–9] -

These tokens (in which the verb is also marked with the past *-ing* marker), suggest that must is possibly essayed as a lexical (past) tense marker in English also. It should be emphasized that this conclusion is quite tentative, and would require further investigation. Dittmar's explanation for the use of müssen as a tense marker in German is morphological. He suggests that the phenomenon "may be explained by the fact that müss has the comfortable property of having the same form in the first and third person singular" [155]. Clearly, this explanation would not hold for English, where modals are never marked for person. An explanation for English (and an alternative explanation for German) can be found in the semantic and systemic properties of must or müssen. This is as follows. First, must is acquired early for functional reasons. Second, there is evidence that lexical morphemes are preferred over bound morphemes—this is a consequence of the learner's tendency to optimize form and function. Third, of the lexical morphemes available for overgeneralization must is the most appropriate, since will is tense-specific and can involves specific propositions about the speaker himself, unlike must, which involves more general propositions about the speaker's environment, and is arguably less marked semantically.

Whatever the status of the above proposal, Dittmar's predicated order of acquisition for modals in German (on the basis of cross-sectional data) is, as we shall see, quite similar to the projected order for English. Dittmar's basic findings are:

> müssen, wöllen, and können are the modals applied with the greatest frequency; (b) wöllen and können are applied at an earlier stage than müssen; (c) mögen and söllen are acquired very late; and (d) müssen contributes more than 50% to the applications of the rule [155].

This suggests that some possibly universal hierarchy of semantic markedness helps determine the acquisition and use of modals. Data from other languages would be interesting to examine in this connection.

Further reference to must will be made in the section on have to.

4.9.4 'Will':

Will is used largely in utterances with a future temporal reference, and in this sense could be said to be fairly standard. On occasion, will appears in utterances whose time reference is future-in-the-past, which clearly represents an overgeneralization of its use (cases of this occur in **minh.1** and **phuc.1**).

> I to learn English, er... about six month(s)... yeah... I will, er, speak Englis(h) easy
>
> - minh.1: [480–1] -

> you know... their relatives... they will be happy to see Vietnam again ... their country lan(d)
>
> - phuc.1: [874–5] -

In several of the informants higher on the ASLPR will occurs in the result clause of conditional structures, (**vinh.1** and **tam.1**), and in the first interview with Phuc (who perhaps exhibits the most development in use of modals of all the informants) will also appears, correctly, in a habitual-predictive context:

> usually they will put you into zhaol
>
> - phuc.1: [469–70] -

However, while will exhibits generally standard, if restricted, semantics, its behaviour from a developmental viewpoint requires comment. Thus, a number of informants who produce will, produce it only in the locution will be. This is particularly common amongst the Polish speakers, but it is also true of Long. The resulting cases are sometimes standard (superficially) and sometimes not, as in these two examples from **lj.1**:

I will be at home three years

- lj.1: [128] -

I will be have ten years

- lj.1: [319] -

Informants whose entire production of will involves will be tokens are Jerzy B., Barbara B. (second interview—no tokens in the first), and Ludwiga J. Besides these, a number of other informants, while producing will alone, also produce significant numbers of tokens of will be. These are Jan R. (two out of five), Andrzej J. (thirteen out of twenty-three), and Krystyna B. (four out of six in Interview One). In the case of Andrzej J., none of the will be tokens are non-standard. In other cases, will be is sometimes used with main verbs, producing non-standard structures.

This morphological peculiarity is explicable in the following terms. First, learners tend to acquire and use formulas in which be appears, probably because common utterances involving will often include be. Examples of such formulas would be:

Wi(ll)... maybe will be better... I don't know

- bb.2: [77] -

I will be looking for... for job

- jb.1: [275] -

not be long de com(p)any will be closed

- long.2: [121–2] -

Extant incorrect hypotheses about verb marking or verb versus adjective categories can then result in a further incorrect hypothesis about the phonological shape of the modal, which is analyzed as will be. Some learners (such as Ludwiga J.) persist with their hypothesis, while others (like Krystyna B.) modify it. In the case of a learner like Andrzej J., where will be occurs

only with copular adjectives, the incorrect hypothesis may have never been formulated.

The fact that unanalyzed will be is much more common amongst the Polish speakers may also be amenable to explanation. It has been noted in 4.7 that suppliance of the copula is much lower amongst Vietnamese speakers than amongst Polish speakers. Given this, and given that will be is a form of the copula, it is probable that Vietnamese-speaking learners pay less attention to the structures that appear to provide many Polish speakers with their first analyzable input for will.

4.9.5 'Would':

As indicated in the tables, for most informants who use it, would appears only in the formulaic verb phrase would like; no evidence exists for supposing that would in these tokens has independent lexical status.

There are, however, a number of exceptions. Canh, in the first interview, uses would in a manner that may not be formulaic, or at least represents a spreading application:

> would you tell me...?
>
> - canh.1: [814] -

> I would say he's.../...gooz man
>
> - canh.1: [2149] -

Andrzej J., in the first interview actually produces a future-in-the-past token of would:

> we decided, er...er, to...leave Poland and, er...if, er...it...would be possible...to come to Australia
>
> - aj.1: [261–2] -

In the second interview, on the other hand, all the tokens of would he produces appear with like, albeit with more variation in structure and discourse situation than for other informants. Ludwiga J., in the second interview, produces tokens of would be, rather than would like, as does Mieczyslaw M. This indicates that a gradual spread of environments is taking place.

Nevertheless, structures involving would in non-stereotyped environments are barely apparent. There are no full counterfactual conditionals in the data. Nor does would appear as a modal denoting past imperfectivity. Semantic complexity and relatively low functional load are sufficient to explain this. It might also be remembered that as a form with many functions, would constitutes a classic problem for the learner. As we will see, there may even be some evidence that would is avoided.

4.9.6 'Could':

This modal is restricted to a small group of informants, and is produced infrequently. Ewa S., in the first interview, provides an instance of could in a conditional structure, but as an echo of the same structure in the interviewer's question. Spontaneously, she uses could as the past form of can. It is this latter usage which characterizes the tokens produced by Andrzej J. (both interviews), and Mieczyslaw M. (second interview):

I could, er...er, to read in Poland

- aj.1: [343] -

Likewise for Ludwiga J., where the modal does not appear to be fully established as the tense carrier:

can...I could saw...see?...I could saw...I could sa(w)?

- lj.2: [671] -

Amongst the Vietnamese speakers only Long and Canh use could. The former produces only one token, which is somewhat confusing, but where the most probable interpretation is as a past form. Canh, on the other hand, seems quite prolific. The most frequent use of could is in conditional structures, both explicit and implied:

I cou(ld) say, ah, to you, ah...anything

- canh.1: [2325] -

I couldn('t) live in there...I am still young

- canh.1: [384] -

If you don't...don't, ah, don't test it you couldn't sell it

- canh.1: [756] -

Could also possibly appears as the past form of can:

I could(n't) remember a lot my history course

- canh.1: [1411] -

However, the uncertainty of the above example, as regards time reference, and of one of the conditionals cited above, as regards modality, makes it possible that for this informant can and could are allomorphs. (The second interview provides no evidence to the contrary). This would be a classic case of having the form before the function has been acquired. There is other evidence of a more than normally pronounced tendency for this situation to occur in the case of Canh [3].

In this connection, it is worth noting that Canh is distinguished by the size of his lexicon. He might not unfairly be described as a lexical magpie. This unusual sensitivity to words may mean that he is more likely than the majority of learners to remember (and use) function words for which his grammar does not have a place. Thus the categorial vagueness and consequent incoherence which characterize a good deal of this informant's output may be in some measure a product of the overdeveloped state of his lexical faculties.

If there is no real functional distinction between can and could in the output of Canh, then could, as it appears in the data, functions solely as a past form of can, and is not used in expressions of conditionality.

4.9.7 'Should':

Should is used by all those who produce it as a modal of obligation:

I...er...should.../...speak?

- es.1: [155–6] -

Em...my friend said I shoul(d) put some, ah

- vinh.2: [208] -

One informant, however—Phuc—uses should to express several other functions. These include probability, and implicit and explicit conditionality. The range of functions is displayed in the following examples:

they say that...we should work har(d)

- phuc.1: [441] -

to buil(d) a hall li(ke) that should take you a few mon(ths)

- phuc.1: [960–1] -

[3] Cf. Canh's morphology, where "-ing" and "the" function respectively as verbal and nominal morphemes

I should like to send them... Christmas cards

- phuc.1: [1032] -

I hope.../...I should have another zhob

- phuc.1: [72] -

If I have no responsibiliti(es) I thin(k) I should be very happy to go to school, you know?

- phuc.2: [333–4] -

Most of the tokens produced by Phuc involve acceptable uses of should. In some, however, such as the last example given above, would is probably the modal a native speaker would chose. But the only tokens of would produced by Phuc are formulaic. It is interesting to speculate why Phuc has chosen to develop should rather than would to express notions such as counterfactuality. The choice may have been arbitrary, or possibly motivated by some pedagogic conception of should as a prestige form. Or perhaps the complexities of would, with its dual role as modal and aspectual functor led to the selection of the more categorially straightforward should. Whatever the reason, it seems that the system of a learner as advanced even as Phuc can only support one such modal.

4.9.8 'Have To':

In contrast to its semantic near relative must, have to is neither as widespread nor as frequently produced.

Amongst the Vietnamese informants, the form is used by Sang, Tam, Canh, Long and Phuc. Of these, Sang, Canh and Phuc, who each produce a single token, use the form in a straightforward way, as in this example from Phuc:

I have to take care of my sister(s)

- phuc.1: [741–2] -

This is unquestionably a modal of obligation.

Tam and Long present problems, however. As already mentioned in 4.9.3 above, in the output of Tam all cases of have to are sub-cases of the locution have to learn, which appears to be an idiosyncratic form of learn for this informant. Thus, Tam can be excluded from consideration. The case of Long is similar, but not quite so clear cut. Like Tam, he uses have to learn (twelve tokens). He also produces locutions of a similar kind with related verbs:

but I have to read it I understand

- long.1: [1173] -

There are about four such examples. As well as these he produces tokens where have to appears to be an allomorph of have—once again, in about four cases:

they have to been here a long time

- long.1: [412] -

(These cases could also be adduced as further evidence of modals used as tense markers). There are also tokens where have to may be a modal of obligation, such as the following:

I have to send a letter for you

- long.1: [665–6] -

Nevertheless, such tokens are never unquestionable; the example just cited may be an attempt at a perfect, for example, like the one above it. The upshot is that it is not clear whether Long produces tokens of have to as a modal of obligation or not.

Amongst the Polish informants, tokens of have to are produced by Krystof S., Barbara B., Ewa S., and, possibly, Andrzej J. All of these are in the second group of interviews. These informants provide one token apiece, and, as the examples below would suggest, the form is not very spontaneously produced:

Yes, I know I must... I... had to learn English

- bb.2: [19] -

but, er... er, now I mus(t), uh... I have to, em, Canberra

- ks.2: [228] -

Presumably these learners have had their attention drawn to the more usual have to since the time of the first interview.

Compared with must, then, have to is not very frequent at all. This state of affairs would seem to be the inverse of that found in the output of native speakers, who seem to show a marked preference for have to or have got to. This apparent anomaly appears to provide further evidence for the form-function principle. The argument is as follows. Must is a form

with an unambiguous function, that of expressing obligation. Have, on the other hand, is a form with various functions. It is a lexical verb signifying possession, an auxiliary in perfect structures, and, in combination with the complementizer to, a modal of obligation. Learners, in accordance with the principle that a form should have a single function, select must to express obligation, despite its relatively lower frequency than have to in native speech itself. It should be stressed that must occurs with very high frequency in the output of informants who have had virtually no formal instruction (for example, Long), and cannot therefore be attributed to classroom input.

4.9.9 'May':

May is used by only three informants. Jerzy B. uses it to indicate possibility, and Phuc uses it, non-standardly, to indicate capacity.

> Yes, yeah after this school, eh, I... I m... I may... I may... er... le... learn, eh, at, eh... at university
>
> - jb.2: [820–1] -

> since I may helf my friends
>
> - phuc.1: [607] -

Ewa S. uses may in an idiosyncratic combination with will, to indicate possibility:

> She will may, c... come... to Australia yes... yes
>
> - es.2: [637] -

4.9.10 Morphology

From the point of view of morphology in the structures involving modals there are a number of observations worth making.

Firstly, there is some *-ing* marking of verbs after can and must, particularly the latter. Examples of verbs marked with *-ing* after must are to be found in **is.2**, **zj.2**, **jr.1**, **jr.2**, **bb.1**, **lj.2**, **mm.1**, **tam.2**, **long.1**, and **long.2**. Examples of verbs marked with can occur in **ks.2**, **kb.1**, **kb.2**, **canh.1**, and **phuc.1**. It is not really clear why must should figure so much more than can in this phenomenon. One possible explanation lies in the probability that learners get less corrective feedback on structures involving must from native speakers—who tend to use the more idiomatic have to in its place—than they do for structures with can—where there is no such

alternative. Another possibility has to do with the use of must as a non-standard tense marker, as discussed in 4.9.3 Verbs preceded by must and marked with *-ing* could perhaps be seen as constituting learner experiments with simultaneous lexical and morphological marking.

A further phenomenon which was noted was that must seemed to occur without any surface subject with some regularity. In the first round of interviews, Barbara B., Andrzej J., Ludwiga J., Tam, and Canh produce tokens of subjectless must. Since no counts exist for non-realization of subjects with other types of verbs, it is difficult to say whether this is merely a reflection of a more general state of affairs or constitutes a special case.

4.9.11 Summary

In summary, as Tables 4.31 to 4.34 show, there is a fairly strong implicational order for the modals—particularly for the Vietnamese speakers, as well as a reasonably clear correspondence between use of a particular range of modals and assessed oral proficiency. Must, will and can appear to constitute the first group of modals to be acquired. The second group is comprised of could, should and have to. A third group, which is hardly in evidence except in the most restricted set of verbal environments would be would and may. The similarity between the predicated orders of acquisition for German and English modals suggests that universal principles of semantic markedness may play a part in determining the learning of modals.

4.10 Negation

4.10.1 Some Methodological Problems

Tables 4.35 to 4.40 provide the figures for an outline of the system of negation as it appears to develop over the cross-sectional sample. The tables are divided into seven or eight basic categories.

Within these categories, separate counts for no, not, and any other relevant negator, such as don't, are maintained. It has to be said, however, that in the process of transcription it is sometimes difficult to distinguish between tokens of no and tokens of not. There are different reasons for this. The negator may occur before a word beginning with */t/* or */d/* (for instance, as in no(t) too much), which makes it difficult to decide whether it terminates with a consonant or a vowel. In the case of the Vietnamese speakers, the negator may be terminated with a glottal stop, rather than an alveolar stop. Glottal stops are a legitimate allophone of final */t/* under certain conditions in English, but of course it is difficult to know whether a glottal stop produced by a Vietnamese speaker, although it results in a word that sounds more like not than no to native speakers, is merely fortuitous or is actually evidence that the learner has acquired a viable and system-

Informant	is	ks	zj	jb	jr	ka	bb	es	aj	kb	lj	mm
must		5	3		5	5	18	8	2	8	18	12
will				2*	1	5		2	8	6	5*	8
can						1		7	3	11	12	4
can't							1*				6	
would					1F		10F		5*			
I'd					1F			5F				
could								3	1			
couldn't									1?			
should								1				

Table 4.31: Distribution of Modals: Polish Informants—Interview 1

NOTES:

1. All counts suffixed with an "F" indicate that the tokens involved are formulaic. This applies here to "would", which always appears in the phrase "would like".
2. In the case of **jb** both tokens of "will" occur in the chunk "will be".
3. In the case of **bb** the token of "can't" is echoed.
4. In the case of **aj** four of the five tokens occur with "like".
5. In the case of **lj** "will" occurs in the chunk "will be".

atic distinction between the two morphological forms of the negator. These remarks should be borne in mind when examining the counts. In any case, the acquisition of a distinction between no and not involves a morphological, rather than a syntactic rule, and is really a separate matter. Having said all this, there is nevertheless, as we will see, evidence in the data on negation that just such morphological distinctions do tend to develop along with the syntactic ones.

4.10.2 Categories of Negation

The categories themselves are the following:

Standalone or Holophrastic 'No' and 'Not'

This is the negator used by itself in answer to a question, or to negate some proposition originating from the interlocutor, or, occasionally, in correction of a previous proposition by the speaker himself.

> GB Hm, mm...and you never studied English at school? ...in Poland?
>
> I No...never

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
must	6	8	2	7	2	5	22	9	3	10	18	24
will			1	5	2	1	20*	10	14*	6	37*	5
won't									1	1	2	
we'll					1			3				
what'll								1				
can	1	3				1	3	3	14	14	15	2
can't						1		4		12	5	
I'd				1F				1F	1F			
would									9F		2	1
could									2		1	
couldn't												1
may						2		2*				
should								4				
shouldn't								4				
have_to		1?					1	1	1?			

Table 4.32: Distribution of Modals: Polish Informants—Interview 2

NOTES:

1. All counts suffixed with an "F" indicate that the tokens involved are formulaic. This applies here to "I'd", which always appears in the phrase "I'd like".
2. In the case of **bb** all twenty tokens of "will" occur in the chunk "will be"; in many cases the resulting utterances are non-standard.
3. In the case of **jr** four tokens of "will" occur in the chunk "will be"; in one case the resulting utterance is non-standard.
4. In the case of **es** the tokens of "may" both occur in the phrase "will may", which is this informant's semantic equivalent of "might".
5. In the case of **aj** eight of the fourteen tokens of "will" occur in the chunk "will be"; in all cases the resulting utterance is standard.
6. In the case of **lj** "will" always occurs in the chunk "will be". Likewise, both tokens of "won't" involve "won't be".

Informant	Van	My	Duc	Dung	Minh	Sang	Hoa	Vinh	Tam	Canh	Long	Phuc
can			5	1		6	5	18	6	7	26	17
can't				2		3	1	7	12	7	16	7
must					1	3	1	6	9	7	40	5
will					1	1		9	4	7	3*	23
would		1F				5F	2F	1F		4	3F	2F
have_to									13*	1	27*	2
could										7	1	
couldn't										8		
should										2		15*
may												1*

Table 4.33: Distribution of Modals: Vietnamese Informants—Interview 1

NOTES:

1. All counts suffixed with an "F" indicate that the tokens involved are formulaic. This applies here to "would", which always appears in the phrase "would like".
2. The asterisked counts of "have to" all involve substantial numbers of cases where the token may not be a modal of obligation, or may be functioning as a reduplicated companion to "must" to indicate very strong obligation.
3. In the case of **long** all tokens of "will" occur in the chunk "will be".
4. In the case of **phuc** "should" appears to somewhat of a catchall modal for this informant. It is used to express obligation, probability and in counter-factuals. This probably accounts for the appearance of "would" as a formula only. "May" is used to indicate capacity, rather than probability or permission.

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
can		4	2			2	1*	1	27	14	4	12
can't			10	4				1	16	3		11
cannot										1		
must			1		1	1		9	19*	3	7	1
will			1		1		1*	7	3	1	3	7
won't							1*					
have_to			1						1	1	2	1*
would							1F			1?		2F
should								2			2*	20*
could										1		
couldn't										3		

Table 4.34: Distribution of Modals: Vietnamese Informants—Interview 2

NOTES:

1. All counts suffixed with an "F" indicate that the tokens involved are formulaic. This applies here to "would", which always appears in the phrase "would like".
2. In the case of **duc** the asterisked tokens all occur in the same utterance, while the informant is searching for the right form, which is "won't" in this case.
3. The asterisked counts of "have to" all involve substantial numbers of cases where the token may not be a modal of obligation, or may be functioning as a reduplicated companion to "must" to indicate very strong obligation.
4. In the case of **long** "should" is used to express probability.
5. In the case of **phuc** "should" appears to somewhat of a catchall modal for this informant. It is used to express obligation, probability and in counter-factuals. This probably accounts for the appearance of "would" as a formula only. The token of "have to" is actually "have got to".

- jb.1: [29–32] -

...er, eleven years...eleven, no, no...(i)s two, thr...eight!...eight year(s) in, t...another factory

- jr.1: [121–2] -

At times, standalone no may have a holophrastic function, in that it condenses what would normally be a more extended negative proposition into a single word. This, obviously, is more likely to occur with speakers whose systems of negation are not very developed.

GB Mm...yes...ah, were you in Poland when Solidarity was formed?

I No.

GB You were in Austria?

I Er...no, no...er...in, er...ch...ch...I was in Poland S, Soli, Solidarity?

- jb.1: [345–53] -

It is interesting in this regard that the frequencies for standalone no tend to peak in the middle of the *ASLPR* range and to decline thereafter, despite the fact that informants higher in the range have significantly higher total word counts and could therefore be expected to produce more tokens of no as a side effect of this. Table 4.40 gives the figures for no expressed as a percentage of the total number of words used—cultural factors might have something to do with the slightly lower figures for the Vietnamese speakers. It is a little difficult to know exactly how to interpret these declining frequencies. One possibility is that standalone no does occur in a holophrastic capacity to a significant extent beyond a certain level of development. Yet another is that standalone no is not used holophrastically to such a significant degree, but that misunderstandings and communication breakdowns require a good deal of correction, reformulation, and therefore frequent negation on the part of the speaker. It would be interesting to investigate these proposals further.

Phrasal Negator 'No' or 'Not'

This category includes all those cases where the negator occurs in conjunction with an adjective, adverb, quantifier, noun phrase, prepositional phrase, etc., where these phrases occur in isolation, or from intonation and/or context are evidently not part of a verb phrase. Examples would be:

...in hostel, er, no...no longk

- zj.1: [786] -

GB Ah ha... did he speak English very well?
I Na... no very well

- zj.1: [489–91] -

Drink beer, yeah, drink beer, too much, now no, now not much... not too much

- dung.1: [127–8] -

boat not ship

- dung.1: [166] -

not in the city

- tam.1: [499] -

Such tokens can be quite acceptable products of ellipsis, of course, and are frequent in native speech. On occasion, however, they are the result of failure to insert the copula, or failure to insert both the copula and a subject. For reasons which will become clear, tokens of negated phrases within a verb phrase—that is, preceded by a verb (generally the copula)—are not counted as examples of phrasal negation.

Negative Quantifier 'No' or 'Not'

This category includes those cases where the negator functions as a quantifier. (Quantifiers in general are dealt with in more detail in 4.14.5). Also grouped with negative quantifiers is the small group of idiomatic (and formulaic) phrases which includes no good and no more, since in so far as these phrases can be parsed the no component is in fact a quantifier. Initially, these were to be eliminated from the counts for negative quantifiers, and listed separately. However, a check revealed that informants who produced such idioms also produced non-formulaic tokens of no as a negative quantifier, and that separate listings would have not altered the overall counts very greatly. Examples of no as a negative quantifier are:

my factory no job(s)

- dung.1: [589] -

No good... every day goulash

- zj.1: [770] -

Preverbal Negators

Preverbal negators include no, not and various forms of *-n't*, such as don't, can't and haven't. Some examples are:

No understand

- ka.1: [490] -

...we drink, em...vodka and, er, we, em, not eat

- mm.1: [470] -

in Vietnam I, er...no study English

- minh.1: [20] -

I don('t) have any re(la)tive(s) in Australia

- sang.1: [24] -

I didn't, er, travel before my...er, leave Poland

- aj.1: [580] -

I can't work

- bb.1: [157] -

Postverbal Negators

Postverbal negators include no, not, and some forms of *-n't*. There are several problems in classifying postverbal negators.

With the *-n't* morpheme, there is a problem in deciding whether the morpheme is really analyzable as a contracted form of not, in which case it would classify as a postverbal negator, or whether it is part of a word with no internal structure, as it obviously is in early cases of don't. The best approach seemed to be very conservative in this matter and to only count tokens of *-n't* as evidence of post verbal negation if there was evidence of actual decomposition of the word into element plus negator elsewhere in a particular informant's output, in the case of those words which themselves functioned as preverbal negators (that is, don't, can't, etc.). In the case of words which were not used as preverbal negators (for instance, isn't or wasn't), these were tentatively classed as tokens of postverbal negation, provided they did not occur in formulas such as question tags. The result was

that there are very few clear tokens of postverbal negation represented by words ending in *-n't*. As outlined above, such a decision may seem both arbitrary and unnecessarily restrictive; more detailed examination of the data, however, will show that it is probably well motivated. With the no and not morphemes, there are problems of an almost opposite kind. In the majority of cases of postverbal negation the verb concerned is the copula. Here the difficulty is in deciding whether the negator should really be grouped with the verb and assigned the status of a verbal negator, or whether it would more properly be considered as a phrasal negator. Examples of possible postverbal negation would be the following:

Europe ist... er... not safe

- es.1: [141] -

we have not enough materials

- phuc.1: [941] -

The difficulty with the analysis is to know whether to treat a structure such as this as a case of *is not* || X or *is* || X. This problem is discussed in more detail further on. In general, the approach has been to treat the negator as verbal, unless evidence from pauses or intonation suggests otherwise. More evident examples of postverbal negation than the ones cited above are:

is not problem for me

- bb.1: [574] -

she was not happy

- phuc.1: [333] -

I... will no, er... work

- ka.1: [439] -

Anaphoric 'No' or 'Not'

Anaphoric negators refer back to some proposition advanced at an earlier point in the discourse; they are relatively uncommon in the present data. Examples would be:

I'm not sure... it is true, maybe not

- lj.1: [386] -

you can get job like me now or not?

- long.1: [785–6] -

she like but she can('t)

- dung.1: [872] -

Question Tag 'No'

This is not an important category, but it seemed worth including. An example would be:

You married, no?

- ks.1: [970] -

Verbal Complement 'No'

These could possibly be treated as a subset of anaphoric negators. The examples were tabulated mainly because they provide evidence of how morphological distinctions that are well established in one area will not necessarily be made elsewhere. An example of verbal complement no is:

No, I think no

- lj.1: [443] -

4.10.3 A Postulated Developmental Sequence

Although it is necessary to exercise some caution when postulating a developmental sequence on the basis of cross-sectional data, Tables 4.35 and 4.40 suggest a sequence which is very much in line with that described in other research [27]. For the moment, the morphological distinction between no and not will be disregarded. Some points about this distinction will be made when the basic sequence has been described.

Holophrastic Negation

The first step in the acquisition of a system of negation comes with the use of standalone or holophrastic no. For early learners the semantics of no, as for many other words as well, are somewhat wider than in the target. Thus no serves not only to contradict propositions made in the discourse, but also to indicate incapacity—for example, incapacity to answer questions.

As has already been pointed out, the relative frequency of standalone no declines as oral proficiency increases and the system of negation develops. Two possible explanations for this are offered in 4.10.2 above.

At this point, the locution don't know appears. At this stage there can be very little doubt that this is not monomorphemic. As with no itself, don't know, has a wider semantic field than in Standard English, serving to indicate incapacity as well as lack of knowledge.

MJ .../...do you have a job?

I [LONG PAUSE] I...I don't know

- van.1: [30–32] -

The case of don't know is a very clear example of the acquisition of the form before the function—a phenomenon which, as we will see, is in evidence elsewhere in the acquisition of negation, and is a productive aspect of the acquisitional process.

Phrasal Negation with 'No' or 'Not'

The next stage of negation, as evidenced from the distribution patterns in Tables 4.35 and 4.37, is the preposing of a negator, either no or not, before some other element. (Don't cannot yet be classed as negator, since it still only occurs in monomorphemic don't know and don't understand). At this particular point of a learner's development, the categorial status of the negated element is often rather vague. English does not have an abundance of morphological markers to indicate categorial status, and in the elliptic utterances which are produced by learners at this stage of their development there are insufficient elements for parsing to provide unambiguous categorial labels. Examples of this categorial vagueness would be:

GB .../...did you work, at all, or...?

I In Aus(t)ria?

GB In Austria.

I No, no...no work

- jb.1: [155–161] -

MJ ...were you in the army?

I I no army

- my.1: [154–6] -

In any case, the categorial identity of the negated elements is only crucial to the analysis if some categories of words appear to be negated before others. There is some evidence from child longitudinal studies that verbs are negated by neg-preposing later than, say, noun phrases or adjectives [185]. This conclusion is neither contradicted or strongly supported by the present study. One informant, Irena S., produces tokens of phrasal negation, but not verbal negation. Other informants produce both, with phrasal negation tending to be more frequent than preverbal negation with the informants lower on the *ASLPR* scale. If there is indeed a time differential for these two types of negation for adult learners, it is probably slight.

Categorial vagueness itself, as a major phenomenon, does not persist for very long, as learners begin to incorporate words that are recognizably members of the main grammatical categories into their vocabularies. Examples of phrasal negation, involving adjectives, adverbs, quantifiers, noun phrases and prepositional phrases are given above in 4.10.2 Noun phrases and adverbs are the most frequently negated categories for the early learners.

At the stage of phrasal negation, or perhaps slightly later, no also appears as a negative quantifier. This is not surprising, since the structural description of negative quantification with no and phrasal negation is the same, although the semantics are different. Some of the tokens of no as a negative quantifier are represented by phrases like no good and no more, which are, of course, formulaic.

Preverbal negation is also in evidence at this point, constituting either a subset of cases of phrasal negation or, just possibly, a later extension of the neg-preposing rule to the category of verb. The first definite preverbal negators are no and not. Examples are given in 4.10.2 above.

Preverbal Negation with "-n't" Forms

While the first appearances of don't are as part of formulas, by about 1− on the *ASLPR*, there is some evidence that this morpheme is beginning to establish itself as an alternative preverbal negator. Table 4.39 shows that Duc uses don't in several verbal environments. Amongst the Polish informants, Jerzy B., while producing the majority of his tokens of don't with know and understand, produces one token with have. This sort of evidence shows how a new morpheme gradually assumes a definite lexical identity and function, and provides a very important insight into the learning process. Tables 4.36, refneg:453 and 4.38 provide details of the various environments for don't for all the informants in the study. While know and understand remain the dominant environments for most speakers, it can be seen that there is general increase in the range of verbal environments as one proceeds up the *ASLPR*. Regarding the final step of the acquisition of the auxiliary do as the carrier for the negator, however, there is no definite evidence in the present data, from negation itself at any rate, that don't is further

decomposed into do and not. The implications of this point for teaching will be discussed shortly.

A variant of don't, doesn't, also begins to appear in the speech of the Vietnamese informants, though not the Polish speakers, by about level 1. The majority of instances for all speakers involve the phrase doesn't matter. Where other verbs occur with doesn't, the form does not necessarily serve to mark the third person singular, as it does in Standard English (in Long's case it always occurs after they) and if it does mark this person, does not do so consistently.

There are other preverbal negators besides don't and doesn't. These are detailed in Tables 4.36, 4.37 and 4.38. Once again, these increase in number, and to some extent frequency of occurrence, across the **ASLPR**.

These preverbal negators with an -n't form present somewhat of a paradox. In native speech, such forms would not be classified as preverbal negators at all: they would in fact be examples of postverbal negation, with their forms resulting from the contraction of the postverbal negator not and its subsequent attraction to the auxiliary or modal.

As we have seen, there is overwhelming evidence that don't cannot be analyzed in this way when it first enters learner speech. Far from being analyzable into two components, it is not even a discrete element itself when it appears in the formulaic don't know. When don't becomes a discrete element, non-standard patterns of usage, such as in the example below, indicate that it is monomorphemic, and still cannot be decomposed into do and not:

> I want to make, er, to photograph again... in Australia... but, er, but don't licen(ce)
>
> - minh.1: [412–3] -

> Doesn't freedom... freedom... not freedom
>
> - dung.1: [154] -

For these reasons, it is safe to ascribe to don't the role of a preverbal negator, and not to treat its use as evidence of postverbal negation until such time as informants produce clear evidence that in their grammars it is a decomposable form—say, by producing tokens of do not, or perhaps by their use of do in questions.

The question that now has to be asked is whether the other -n't forms also have the same monomorphemic status as don't when they first begin to appear in learner speech.

Turning to Tables 4.36, 4.37 and 4.38, the first point is that in the output of the Polish speakers haven't, wasn't, isn't, and the dubious token of weren't,

while they can occur as preverbal negators as auxiliaries, do not do so in the data. They have been left in the table because of their possible significance as evidence for postverbal negation, of which more later.

The next point concerns haven't in the output of the Vietnamese speakers, and illustrates some of the problems referred to earlier in determining whether or not certain words have an internal structure. As can be seen from Table 4.38 a number of Vietnamese speakers use haven't. One of these, Vinh, uses it as a main verb, rather than as a preverbal negator, and can be excluded from the present discussion. The principal usage for the other speakers is in the locution haven't got. This covers all the tokens for Tam, Canh and Long, and three out of four tokens for Dung, leaving Sang the odd man out. Further investigation reveals that it is only Sang who uses have as an auxiliary as well as haven't, which serves to differentiate his case still further. The other informants, in declarative statements where have plus past participle would be standard—and these all involve got too—merely produce the participial form as a main verb. (This point is discussed further in 4.2). The only form of auxiliary have these informants produce, then, is haven't, and then almost always in the locution haven't got. The following examples are typical of the pattern of occurrence of got:

he got, er...er, little boat

- long.1: [174] -

we Army before we haven't got any money

- long.1: [187] -

There is an obvious parallel in this pattern of use of haven't in haven't got with that of don't in don't know.

The pattern is not exactly equivalent, in that informants when they first begin producing don't know, do not have do as a discrete element, whereas the learners who use haven't in the way described above already use have. Nevertheless, there must obviously be considerable doubt that haven't used in this way is anything but a specialized form of preverbal negator.

This doubt is strengthened when we look more closely at the patterns of use for have and haven't. Ostensibly, these words constitute an *affirmative/negative* pair. This in itself is not a guarantee that a speaker will isolate the *-n't* morpheme as a negator in his or her grammar, but it certainly makes such a move possible. The fact is, however, that two elements in this pair do not have the same categorial status. That is, the affirmative element have is used only as a main verb (except by Sang, who has already been noted as an exception), while the negative element haven't is used exclusively before main verbs, as either negator or auxiliary. The pair, then, is not really a

pair. When it functions as a main verb have is negated by don't, not the *-n't* morpheme, or uncontracted not. And as an auxiliary (in the cases under discussion) have simply does not appear at all. This makes it almost certain that for the speakers in question, haven't is monomorphemic, and therefore should be classified as a preverbal negator exactly as for early don't.

The case of haven't in the output of Dung, Tam, Canh and Long indicates how careful it is necessary to be in ascribing target structures to target-like forms. It also illustrates how a form which could ostensibly be classified primarily as an example of postverbal negation which just happens to be functioning as a preverbal negator in its matrix structure is in fact a preverbal negator pure and simple. It is possible that other forms, such as didn't, which in the target constitute examples of postverbal negation actually first enter the learner's system as formulaic preverbal negators.

Amongst the other forms of *-n't*, can't is probably the most likely candidate for a form with genuine internal structure. Nearly all of the informants use both can and can't, and, as mentioned above, the existence of such a pair should facilitate the isolation of the *-n't* morpheme as a negator, although the mere existence of the pair is not evidence that this has actually happened. Even here, however, especially in the case of the Vietnamese speakers, the picture is clouded by the non-production of the crucial final */t/*, and by the fact that the can/can't distinction is marked in the vowel in the target and by some learners, and not only by the ending.

For these reasons, most of the tokens of *-n't* forms in Tables 4.36, 4.37 and 4.38 have been treated as examples of preverbal negation, unless there is strong evidence to the contrary.

Preverbal Negation—The Spread of "-n't" Forms

To return to the description of the sequence itself. Once the *-n't* forms begin to emerge from their formulaic contexts, as detailed in Tables 4.36, 4.37 and 4.39, there is a general decline in the use of the non-standard preverbal negators no and not. This appears to take place at about level 1 on the *ASLPR* for both groups. For the Vietnamese speakers, the regularities are striking: of the last five informants only Long produces any tokens of preverbal no or not. In the case of the Polish speakers there is some persistence of not as a preverbal negator, though the percentages are low.

Postverbal Negation

The next probable stage after the inception of preverbal negation is postverbal negation. Some of the difficulties in classifying tokens as definite cases of postverbal negation have been discussed in 4.1.4 above.

For the Polish speakers postverbal negation appears somewhat earlier than for the Vietnamese speakers, and is more frequent at all stages. To

find an explanation for this we have to remember that standard, and (in this data) most non-standard, postverbal negation is restricted to a particular class of verbs. These are be, have, modals and auxiliaries. The most frequent of these verbs is the copula. As is detailed in 4.7

Vietnamese speakers have a far lower rate of copula insertion than Polish speakers, and this would obviously affect their opportunities for postverbal negation. Structures which would normally provide occasion for postverbal negation would, minus the copula, turn into examples of phrasal negation. These can in fact be found scattered throughout the data from the Vietnamese speakers.

it not much bad thing

- vinh.1: [626] -

I no happy here

- long.1: [303] -

There is another curious difference here, and that is that while Polish informants who use have as a main verb almost always produce it in negative form as haven't, the Vietnamese informants, with the exception of Vinh, choose to negate it with don't. One explanation for this could be the Vietnamese speakers' lack of a well-founded paradigm for postverbal negation with no or not—they may simply stick to preverbal negation as much as possible. Another possible explanation lies in the differential patterns of exposure to English for the two groups, with the Vietnamese speakers, having on the whole received more naturalistic exposure, using the form most frequent in native speech. If this latter explanation is valid, then it would be a case of type of input having a significant effect on the development of syntactic rules [185].

The Case of "Isn't"

In the context of postverbal negation, there is one more interesting phenomenon to be observed. This concerns the absence of a form that one would have expected to be quite common—namely isn't. Only one informant uses isn't in the first round of interviews, and more than once in the second. This is Ludwiga J. (the others are Ewa S., Mieczyslaw M., and Tam). The predicates are quite varied:

it isn't only this branch

- lj.1: [273] -

it isn't shop... what is this?

- lj.1: [329] -

It is not clear why Ludwiga J. is such a prolific user of this particular item. On the other hand, a possible explanation for its non-use by other learners can be put forward. This is as follows.

Postverbal negation of the copula follows naturally from the extension of negated phrases by the addition of a subject and copula, or the copula alone. Thus a learner who already produces structures like not every day or my house, er, no(t) in city will produce examples of postverbal negation almost as a side effect of the developmental step of using the copula. Having arrived, as it were, almost fortuitously at a satisfactory means of negating the copula, the learner has no need to search for a specifically negative form of is, was, or any other form of the copula. This is in contrast to other members (with the partial exception of have) of the class of verbs that behave like be in regard to placement of the negator, whose semantics and syntax require active decisions on the part of the learner as to how they will be negated—consider what happens if can is tacked on to an already negative verbal complement.

The case of isn't may in fact be another illustration of the form-function principle—the form is so conspicuous by its absence that one almost suspects a constraint against it, on the grounds of its redundancy.

The case of isn't—as well as the other *-n't* forms—has rather drastic implications for any teaching practice based on the assumption that contracted forms can be taught by demonstrating their derivation from uncontracted ones. From the evidence presented above, in most cases it is the contracted forms which are learnt first, and subsequently decomposed. In the event of an uncontracted structure being acquired before the contracted form, as with is not and isn't, the contracted form will probably not be learnt at all. Right throughout the data, there are very few duplications of this kind. For the numerous tokens of can't, there is only a single token of cannot (**canh.2**). For other *-n't* forms the situation is hardly better: there are six tokens of have not (four in **bb.1**, and one each in **bb.2**, **kb.2** and **phuc.1**), and two dubious tokens of do/does not (**duc.2** and **vinh.1**).

The Sequence for Negation—Summary

This completes the description of the major steps in the development of a system of negation. In the development of a target-like system, of course other rules would have to be learnt, and the rules already described would have to be consolidated. One area of importance not dealt with here concerns the rules governing the behaviour of quantifiers in the scope of negation—that is, the suppletion of words like some into any. These matters are touched upon in 4.14

The basic finding in relation to the rule of suppletion is that it is language dependent (and therefore possibly not developmental), with the Vietnamese speakers performing better in this area than the Poles, and beginning to exercise the rule after level 1 on the **ASLPR**. Regarding the features tabulated in Tables 4.35 and 4.37 and not yet discussed, namely Anaphoric Negation and Verbal Complement, little needs to be said. From the small amount of evidence available these features emerge largely after *ASLPR* levels 1 and 1+ respectively, but could be subject to individual variation.

4.10.4 Morphology

Having completed the major part of the survey on negation, we are now in a position to make some comments on the development of morphological distinctions within the system—namely, the distinction between no and not.

In the introductory part of this section, some of the problems relating to the transcription of these two items were discussed, and the limitations pointed out should be kept in mind during the subsequent discussion.

The first point to make about the negators no and not is that the phonetically simpler form no must be considered to be the prototypical form. There is evidence to support this phonological prediction. For the two usages where no is the actual target form, standalone no and negative quantifier no, there are very few tokens of not, whereas for the remaining usages, where not is the target form, there are numerous tokens of no. The hierarchy of difficulty for the two forms is therefore quite clear, and does not work in favour of the learner with the exception of the two usages cited above.

Given this, it is not surprising that no is initially more frequent than not in preverbal negation (where both forms are non-standard), postverbal negation (where no is non-standard), and phrasal negation (where no is again non-standard). Tables 4.35 and 4.36, however, show a reversal of this trend as one moves across the *ASLPR*.

Thus, in phrasal negation, for both language groups, no declines and finally disappears as not increases.

In postverbal negation amongst the Polish speakers, where the distribution of tokens is wider, no declines even more rapidly than in phrasal negation. Since the rule for postverbal negation may be easier to acquire than that for phrasal negation, this suggests that there is some interaction between syntax and morphology here. In postverbal negation for the Vietnamese speakers, the situation is not so clear because the phenomenon is more restricted in distribution, but by the stage postverbal negation appears not is only morpheme for one informant, and much the more frequent for the other.

In preverbal negation for the Vietnamese speakers, both no and not cease abruptly after Sang, with the exception of some tokens in the output of Long, whose morphological profile exhibits other inconsistencies as well. With the

Polish speakers, the basic pattern of no grading into not occurs once again. This is an interesting phenomenon, given that preverbal not is non-standard anyhow. The informants involved, Krystyna B., Ludwiga J., and Mieczyslaw M., have either abandoned no entirely (except for legitimate contexts) or almost entirely, indicating that they have formed a general hypothesis about which of the two forms is likely to be correct, even in contexts such as preverbal negation, where their rule-systems are still unstable. For Mieczyslaw M. this overgeneralization even results in a case of standalone not.

As far as the morphology of negation is concerned, then, it appears that there is a definite trend towards the acquisition of not over no, where this is appropriate, in parallel with syntactic developments. As to whether there is some interdependence between the morphology and the syntax, this is somewhat of an open question. It is possible, though by no means certain, that the process of acquiring not is slower in areas where the syntactic rules are more difficult to formulate—as in the case of phrasal versus postverbal negation in the Polish data. On the other hand, there is also evidence (though not from the same informants) that developmental processes in morphology can operate independently of syntactic ones and result in "standardizing" trends within non-standard structures—as in preverbal negation in the Polish data. Results like these suggest that we can probably expect a good deal of individual variation in this area.

Given the lack of any definite links between progress in the morphology and the syntax of negation, and the likelihood of individual variation, it would probably be best for teachers not to dwell on morphology if their aim is to inculcate syntax, particularly with learners below level 1.

4.10.5 Conclusions

The broad developmental sequence in negation begins with standalone no, and formulaic don't know (at about 0 on the *ASLPR*). There follows a stage in which phrasal and preverbal negation emerges, along with further expressions containing don't (at about *ASLPR* level 0+). Following this are the first examples of postverbal negation, involving the copula in the first instance (at about *ASLPR* level 1−). Preverbal negation is characterized further at this point, or soon after, by the emergence of don't as a separate morpheme, and the use of other *-n't* forms as preverbal negators. Some of these latter forms may also constitute examples of postverbal negation. Examples of suppleted any may begin to appear soon after (*ASLPR* level 1 onwards). The decomposition of *-n't* forms into their separate elements is still not evident, however. In the area of morphology, not begins to supplant no in the appropriate contexts, with some degree of individual variation.

There are various important implications to be drawn from the acquisition of the rules for negation.

Negation is an area of learner grammar which provides a rich fund of

examples of learning as a process of decomposition and discrimination. The way in which don't enters the system as part of a larger chunk, is isolated as a negative morpheme as it spreads to other environments, and may finally be decomposed into the separate constituents of auxiliary and negator is an excellent example of this process. The way in which don't and other *-n't* forms, such as isn't, are treated in learner grammars is also instructive of how dramatically such grammars can differ from those of native speakers in respect of the same elements, and highlights the dangers of using intuitive "simplifications" as a basis for teaching. Negation also provides pertinent examples of the interdependence of the various rule-systems that make up a total grammar: the dependence of postverbal negation on copula insertion and the interrelations between interrogation and negation through shared elements such as do and any are two obvious examples.

4.11 Questions

4.11.1 Limitations

The following analysis of questions is subject to certain limitations, as regards the data collected. This is because it is difficult to obtain all the data on questions that one might want without either compiling an enormous corpus or resorting to specific elicitation procedures. In fact, a substantial amount of data on interrogation and various other less accessible facets of learner speech actually was collected during the second round of interviews (generally at the end of Interview Two) with the help of written prompts and translations, but given the informal nature of the rest of the two interviews, a decision was taken not to mix this data with the more spontaneous conversational material which constitutes the main body of the interviews, and which has provided the basis for all of the analysis so far conducted. This elicited data still awaits analysis, and should produce some very instructive comparisons with the free speech material. As a consequence of the decision to concentrate the analysis on the informants' spontaneous production, the present analysis of question formation deals only with material obtained in the informal part of the interviews.

4.11.2 Collection of Question Data

Some of this material is totally spontaneous in origin—a by-product like any other of the conversational transactions that took place. The rest is the result of a request made at the end of the first interview that the informants ask the interviewers some questions about themselves. Only the most general suggestions as to what these questions might be were provided, and the interviewees were not pressured to proceed if they appeared reluctant to do so.

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
STANDALONE												
no	16	53	27	25	41	45	12	32	2	13	11	17
not			1									1
PHRASAL NEG												
no	1	10	5	3	5	2	4	3	3	1		
not	1	3	1	1		2	4	5	6	11	20	13
NEG QUANT												
no	1	3	3		3	1			1	3	3	2
PREVERBAL												
no		1		2	9	2						
not			1		1		6			3	4	4
-n't		6F	8F	12		1F	15F	24	15	21	41	15
POSTVERBAL												
no		1?	1?			3						
not			2		2	2	24	1	6	12	9	12
-n't				1?		3		2		9	30	7
ANAPHORIC												
not							2				1	2
-n't						1		1			2	
QUEST TAG												
no			1				1					
VERB COMP												
no									1		1	

Table 4.35: Forms of Negation: Polish Informants—Interview 1

NOTES:

1. Counts suffixed with an "F" indicate probable formulaic usages of the token(s) in question. For the purposes of these tables, formulaic usages are defined as those which involve the appearance of the morpheme in question in no more than two verbal environment. (See the text for a fuller discussion of stereotyping).

Informant	van	my	duc	dung	minh	hoa	sang	vinh	tam	canh	long	phuc
STANDALONE												
no	1	9	13	19	12	26	12	7	5	22	30	4
PHRASAL NEG												
no		3?		2	1	1	2				3	
not		1	7	2	15	2	1	9	9	11	33	8
NEG QUANT												
no			5	3	1	6	5	1	2	2	9	1
not			1	2			1?					
PREVERBAL												
no		1		1	2	5	1				17	
not		3	1	1		5*	1				5*	
-n't	2F		7	14	3F	30	7	17	33	62	105	47
POSTVERBAL												
no								2			1?	
not								9		2?	1?	10
-n't							1	2				
ANAPHORIC												
not								1			1	1
-n't				1		1	1		1			1
VERBAL COMP												
no									1			

Table 4.36: Forms of Negation: Vietnamese Informants—Interview 1

NOTES:

1. Counts suffixed with an "F" indicate probable formulaic usages of the token(s) in question. For the purposes of these tables, formulaic usages are defined as those which involve the appearance of the morpheme in question in no more than two verbal environment. (See the text for a fuller discussion of stereotyping).

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
know		3	4	6			12	18	6	14	18	15
understand		3	4	4		1	2	3	4	2		
remember								1	1			
have				1								
like									2			
want											3	
think											1	
OTHER				X				X	X	X	X	

Table 4.37: Environments for "don't": Polish Informants—Interview 1

Informant	van	my	duc	dung	minh	hoa	sang	vinh	tam	canh	long	phuc
don't	2		7	3	3	28	3	8	20	27	78	25
can't				4		1	3	5	8	8	17	7
doesn't				2		1		1	1	8	3	5
haven't				4			1	2	4	11	6	
didn't				1				2			1	10
couldn't										8		

Table 4.38: Distribution of "n't": Vietnamese Informants—Interview 1

Informant	van	my	duc	dung	minh	hoa	sang	vinh	tam	canh	long	phuc
like			1	3		3		4	5	3	6	3
know			1			16	1		4	13	20	7
have			1			2	1	3			3	
understand			3		1				5		34	
want						1				3		
think										3		1
remember										3	1	
care											9	
OTHER			1		1	3		1		5	9	10

Table 4.39: Environments for "don't": Vietnamese Informants—Interview 1

Int	%
aj.1	0.1
phuc.1	0.1
lj.1	0.2
tam.1	0.2
canh.1	0.3
long.1	0.3
vinh.1	0.3
bb.1	0.5
kb.1	0.5
mm.1	0.5
duc.1	0.8
sang.1	0.8
es.1	1.0
jb.1	1.0
jr.1	1.0
ks.1	1.0
van.1	1.0
dung.1	1.5
hoa.1	1.5
minh.1	1.5
is.1	2.0
ka.1	2.0
my.1	2.5
zj.1	4.0

Table 4.40: Standalone "No" as Percentage of Total Words

Naturally some of the learners were less inhibited than others, and the amounts of data vary somewhat. A further consequence of collecting material in this fashion is that it put the informants under no real pressure—they were not there to find out things they really needed to know, and therefore they were not obliged to stretch their linguistic resources or use what skills they had very creatively. Instead, they tended to stick to the predictable, which probably accounts for the large number of formulaic questions produced.

Furthermore, the presence of large numbers of formulaic questions constitutes a problem in itself, since in an analysis of the present kind there is insufficient data for implicational scaling and it is consequently necessary to make partly arbitrary decisions about where formulas cease and rule application begins.

Nevertheless, the results obtained suggest definite patterns, and constitute a useful set of working hypotheses for further investigations in this area. In particular, it would be most interesting to test their validity against the more extensive sets of elicited questions gathered at the end of the cross-sectional study, especially as these latter questions were modelled to some extent on the original spontaneous ones.

4.11.3 Structures Analyzed

The present analysis is principally concerned with four structures. These are WH-questions and Yes/No questions, involving either inversion or *do*-insertion. Well-formed examples of the two types of WH-questions are:

What is your hobby?

- jr.1: [1302] -

and where is the winter?

- bb.1: [494] -

how long can we... er... live here in hostel?

- ka.1: [1133] -

and:

Whas do you do now?

- sang.1: [512] -

Where do you live now?

- dung.1: [734] -

When did you get married?

- dung.1: [708] -

And well formed examples of the corresponding Yes/No set are:

Is thi(s) your car?

- minh.1: [273] -

Are you a teacher?

- es.1: [1184] -

are you married?

- jb.1: [984] -

have you ever children?

- ka.1: [1200–1] -

can you he(l)p me?

- hoa.1: [1026] -

and:

Do you understand me?

- kb.1: [257] -

did you know, um...when the Vietnamese, um, firs(t) came to Australia?

- sang.1: [575–6] -

These four structures (at times referred to for convenience as *WH/INV*, *WH/DO*, *YN/INV* and *YN/DO*) should present the learner with learning tasks of differing complexity, in terms of the reorganization of elements.

4.11.4 Relative Complexity

WH-Structures

WH-Questions with Inversion

Viewed in the context of its corresponding declarative structure, the first example cited involves a reorganization of the peripheral elements of subject and complement around the verb: *SUBJ V COMP* becomes *WH_WORD V SUB*. In transformational terms, this involves two movement rules: a preposing rule where the complement moves from final to initial position, where it is realized as some form of WH-word, and a rule of subject-verb inversion, which leaves the subject in final position. It is not clear, however, what sort of psychological reality such transformational descriptions have, especially as descriptions of trajectories of acquisition [76]. Structures of the kind which should result after the application of the first movement rule, namely *WH_WORD SUBJ V*, do occur, but are not especially frequent in the present data:

'Partontatis', what it is?

- kb.1: [652] -

What you can do for your country?

- canh.1: [2432–3] -

One problem with looking at the complexity of learning and processing tasks in transformational terms is that in learners' grammars, as is abundantly clear, a particular structure may not be analyzed into the same constituents as in the target grammar. And with fewer elements in a structure, the degree of complexity may very possibly be less.

This is a consideration to be taken into account when comparing the two types of WH-question, since it may well be the case (and there is certainly plenty of analogous evidence from areas such as pronominalization) that the form for WH-words is erroneously perceived in the first instance by the learner to be what is in fact a combination of verb and WH-word. That is, forms such as what's, where is and how are may initially be monomorphemic in the learner's grammar. The rarity of *WH_WORD SUBJ VERB* structures (of which examples were just given) when the verb is the copula can be explained as a consequence of the target pattern of *WH_WORD COPULA* being well established through early bound forms. Other support for the initial absence of a separate copula in early learner language comes from its frequent non-appearance in the declarative forms which parallel *WH/INV* questions: if these equational sentences can consist of only two elements, then related interrogative forms probably can too. Given all this, then,

only one movement rule might be required to produce *WH/INV* structures like What is this?—that is, at least until such time as decomposition of the WH-form takes place.

WH-Questions with 'Do'-Insertion

Similar considerations apply to the second WH-structure. If all of its elements are considered to be separate, then the final structure results from a rule preposing the object, and a second rule governing the insertion of do (or one of its derivations) between the WH-word and the subject. In processing and transformational terms, this structure could be presumed to be more complex than the first, in that both rearrangement and the introduction of new material have to take place. If, on the other hand, do is initially bound to the WH-element, only one preposing rule is required, and the level of complexity, in the first instance, would be similar to that of the previous structure. However, once the learner begins to register that do is not in fact bound to the WH-word, the situation as regards complexity changes. Decomposition of WH-questions with inversion into three principal components would reveal a close parallel with affirmative equational structures, and would presumably be actually associated with the stabilization of the rule system for these structures. The structure would find support in related paradigms. Decomposition of WH-questions with do, on the other hand, would leave the learner with the task of having to account for do, a morpheme whose pattern of distribution elsewhere would offer few clues at this point (remember that in the data on negation it is always bound) as to its probable function. In addition, the form-function constraint could impede any assignment of function to do, given that a lexical verb with the same form would have almost always been acquired by this time.

WH-Structures—Summary

The above considerations, then, might suggest that predictions of learning difficulty on the basis of transformational complexity need to take into account the fact that the structural descriptions for a particular unit may not be the same for native speaker and learner. At this stage, as well as processing considerations the learner has the problem of segmentation of constituents to deal with. In the process of segmentation in question structures, the learner will be faced with the problem of assigning a place in his or her grammar to elements which turn out to have separate identities, such as do.

This approach to the effect of transformational and processing complexity on learning difficulty takes into consideration both the important principle that learning involves analysis and segmentation, and could perhaps offer a more "psychologically real" account of how transformational com-

plexity might actually manifest itself in a dynamic system such as a learner grammar. Clearly, the suggestions that have been made above are tentative. The author is aware that effective predictions of difficulty for word order rules in German have been made on the basis of clearly defined processing constraints [34]. The same kind of enterprise needs to be undertaken for English. A problem with direct application of the German model is that it is designed to operate on strings with a constant number of elements. In English, phenomena like *do*-insertion which introduce lexical morphemes into the string to be processed have to be dealt with as well. It is not clear that this can be accomplished using a purely processing model—that is, processing considerations may have to supplemented by syntactic ones, since differing derivational histories for newly introduced elements may have different computational costs [128].

Taking into account that constituents in learner grammar and the target grammar may be different, for the two WH-structures it could be proposed that initially, if and while the WH-word remained bound to the verb, they are more or less equally complex, and, therefore, likely to exhibit similar, restricted patterns of use. At the point when their internal structures began to emerge more clearly it could be expected that the structure with do would present the learner with more problems than its counterpart, and would therefore spread more slowly.

Yes/No Structures

The situation is somewhat more straightforward for the first of the Yes/No structures. While binding of the verb to the subject cannot be ruled out, the wider distribution of pronouns and noun phrases renders it far less likely, and in any case it is difficult to see what would motivate such a phenomenon. Therefore, a simple inversion transformation seems the most likely path of formation for this structure.

In the case of the second Yes/No structure, a rule of *do*-insertion seems totally unavoidable, in the absence of any viable bearer for the morpheme, as is the case in WH-questions. This means that a learner who consistently produces such structures will have to accommodate do into his or her grammar in some capacity. If it does not function as an auxiliary, at the very least it would have to have the status of a question marker. The learning task presented by this structure appears to be the most complex of the four described: new material has to be inserted into the structure and in the absence of any convenient bearer, such as the WH-words provide, this new material has to allotted some definite function in the learner's grammar. In contrast to the case of *do*-insertion in WH-questions, this problem is evident at the very outset. Once again, the form-function constraint may complicate matters still further.

4.11.5 Apparent Learner Difficulty with Question Structures

Having put forward some hypotheses, it is time to turn to the data. The first point to note is that the majority of questions produced by informants at all stages of development, and presumably native speakers as well, are structurally incomplete phrases marked by intonation.

Er...er, any?

- jb.1: [39] -

Zygmunt?

- jr.1: [166] -

Wha(t)?

- duc.2: [181] -

In Poland?

- jb.2: [352] -

These forms serve to confirm and repair and generally facilitate the flow of discourse. In many cases their elliptical nature would be quite acceptable in target language terms; in some it would not. With regard to an analysis of syntactic development they do not provide much useful evidence, but it is certainly worth remembering their importance for the communicative system as a whole.

Of the structurally more complete tokens of interrogation in the present study, a great many have been discarded as formulaic. Typical examples of formulaic questions are the following:

Where do you work?

- bb.1: [1102] -

What is it?

- duc.1: [379] -

Are you marri(ed)?

- dung.1: [694] -

How ol(d) are you?

- dung.1: [712] -

would you li(ke) some cake?

- hoa.1: [527] -

do you know Civi(c) Hou(se)?

- hoa.2: [219] -

What's your name?

- jb.1: [911] -

Apart from the unequivocal cases, there is a further group of borderline cases, where, for instance, an apparently creative application of the rules in a particular informant's system turns out to be restricted to, say, a single verb. The procedure adopted in the present study is to look at these cases in relation to the non-standard forms produced by an informant. Where this latter class is extensive, and provides little supporting evidence of a system in which rules are developing, there are strong grounds for treating the borderline cases as formulas, rather than evolving forms. Tables 4.41 and 4.42 provide counts for the number of complex but non-standard structures produced by each informant.

For the remaining tokens, the patterns, for reasons which will become clear, differ somewhat for the two language groups. Tables 4.41 and 4.42 show the distribution of the four main question structures for each language group. Due to the relatively small amounts of data involved, the two interviews were treated as a single source; this is not, however, the normal practice in this study.

The notational conventions for the Tables are as follows:

1. A trailing *F* means all the tokens in a particular count were judged to be formulaic.
2. A trailing *F?* means that the tokens may be formulaic.
3. A trailing *X* indicates that the token is non-standard in some way, but still a valid example of the structure.

Polish Speakers

For the Polish speakers, the most widely distributed and frequent question type is the WH-question with inversion. The majority of these tokens were judged to be formulaic. Informants who produced almost certainly non-formulaic tokens are Krystyna A., Barbara B., and Ludwiga J.

how long can we... er... live here in hostel?

- ka.1: [1133] -

and where is the winter?

- bb.1: [494] -

what was happened?

- lj.1: [141–2] -

The token produced by this last informant, illustrates a paradoxical quirk of evaluating data for likely formularity: non-standard tokens, because they cannot have been heard and learnt as single chunks (at least from native speakers), can provide the most convincing examples of the existence and application of a set of underlying rules for a structure.

The situation for WH-questions with inversion is in accordance with the hypotheses advanced above. That is, the form, possibly at the outset with a less complex internal structure, seems relatively easy to learn and use. The production of non-formulaic tokens by learners from the middle ranges of the ASLPR onwards suggests that the transition from formula to rule based structure is comparatively straightforward.

The next most widely distributed question type is the Yes/No question with inversion. Examples are:

Are you a teacher?

- es.1: [1184] -

have you ever children?

- ka.1: [1200–1] -

can you tell me, please?

- kb.1: [1093] -

The distribution for this question type is similar to that of the *WH/INV* type. This is in accordance with the predictions made about the complexity of the two structures.

WH/DO questions are the next most widely distributed structure. Of the tokens produced, however, all but one are either formulaic or possibly formulaic. Some examples are:

when do you... come... er... next?

- es.1: [1146] -

where do you working?

- ks.1: [947] -

The one exception is somewhat unidiomatic, which as it turns out is not uncommon for this structure:

what do you think about my future..here?

- kb.1: [1083–4] -

In the case of the fourth structure, Yes/No questions with do, it is not certain that there are any non-formulaic tokens in the data. The examples from Krystof S. and Jerzy B. are:

do you married?

- ks.1: [962] -

Er, do you want, ah...?

- jb.2: [576] -

In the case of Krystof S., do could possibly be misclassified as a question marker—if so this would provide an interesting insight into how an early learner grammar attempts to deal with this form. Alternatively, given the token of do you working from the same informant cited above, this learner may have only one auxiliary. This, too, is interesting evidence, albeit too isolated for any conclusions to be drawn. The other case, lacking a complement, must remain doubtful.

Krystyna B., produces six tokens of *YN/DO* structures. However, five of these involve the likely formula do you understand...?, and the sixth the related do you know?:

do you understand me what I say?

- kb.1: [862] -

The lack of tokens of any kind for *YN/DO* structures, and the obvious difficulties in formulating a set of working hypotheses to enable the structure to be generated are exactly what was predicted. As discussed above, learners need some place in their grammar for do right from the outset to produce these structures. Both from the evidence of questions above and negation, this slot is a difficult one to provide.

Vietnamese Speakers

Turning now to Table 4.42, we find that for the Vietnamese speakers *WH/INV* is the most widely distributed question type.

What is it?

- duc.1: [390] -

How many children have you got?

- dung.1: [698] -

How o(ld) are you?

- hoa.1: [402] -

In contrast to the case of the Polish speakers, however, there are no clearly non-formulaic tokens in the data.

The reason for this appears to lie in the low rate of copula insertion amongst Vietnamese speakers. Thus, while the Vietnamese learners can acquire and produce *WH/INV* structures in which the WH-word and the copula form an unanalyzed unit, their lack of sensitivity to copular structures in general means either that they experience difficulty in parsing these structures so that they can subsequently be productively generated or that they simply produce *WH/INV* structures minus the copula. There are indeed examples of the latter in the data:

How many people in your family?

- dung.1: [720] -

what your nationality?

- minh.1: [429] -

whas your name?

- my.1: [8] -

Of course, the copula is not the only verb which can appear in *WH/INV* structures, though it is probably the predominant one in learner speech—certainly from the evidence from the Polish speakers and the formulaic tokens.

The low rate of copula insertion for the Vietnamese group no doubt affects the distribution and frequency patterns for the other question type in which inversion figures, Yes/No questions. There are in fact a number of possibly non-formulaic tokens of this structure. All of them, however, involve verbs other than be:

would you tell me

canh.1: [1814–5]

Have you got a house?

- dung.1: [773] -

can you as(k) me again?

- hoa.1: [655] -

Question formation, given that it involves ordering rules and identifiable processing constraints, is a likely developmental feature. It is therefore of some significance that such situations as this one with inversion in the speech of Vietnamese learners can occur, where the rules for a variational feature like copula insertion interact with and inhibit the rules for a developmental one.

As a result of the constraints on inverted structures for the Vietnamese learners, it is *WH/DO* structures which exhibit the widest distribution of non-formulaic tokens. Examples of these are:

Where did you come from?

- dung.1: [716] -

How much did you buy your car?

- dung.1: [800] -

how many do you have, eh... children?

- minh.1: [444] -

what do you mean 'problems'?

- phuc.1: [656] -

As in the case of the Polish speakers, some of these tokens are non-standard in other regards. A common problem is choice of the wrong WH-word:

what does, ah, Fren(ch) call 'sans b'argent'?

- canh.1: [1742] -

How do you thin(k)?

- tam.1: [1455] -

This highlights another difficulty—semantic rather than syntactic—posed by this structure but not by its *WH/INV* counterpart. Given a much wider possible range of verbs for *WH/DO* questions, the likelihood of a mismatch occurring between verb and WH-word is far greater.

For the last of the four question types, Yes/No questions with *do*-insertion, the results are much the same as for the Polish speakers. The number of informants who produce any kind of token of this structure is less than for the other three types. Only one informant, Sang, produces probably non-formulaic tokens of *YN/DO* questions. Three of the four tokens are non-standard, in that did is used when do would be the standard morpheme. The reasons for this are not clear, although it could be an attempt to mark modality (cf. would), or else a way of avoiding the duplication of lexical do.

Did you li(ke) to have a f(r)ien(d) Vietname(se)?

- sang.1: [598] -

Did you li(ke), er, to hear abou(t) my story?

- sang.1: [692–3] -

4.11.6 The Order of Difficulty—Summary

In summary, the order of difficulty for non-formulaic question types for the Vietnamese speakers is not the same as for the Polish speakers, whose results were as predicted by the hypotheses put forward at the beginning of this section. The reason for this difference lies in a different pattern of copula insertion for the Vietnamese learners, which affects the production of structures involving inversion. For the structures involving *do*-insertion, the pattern is as predicted, with the most difficult structure, *YN/DO*, once again hardly in evidence.

It should be pointed out that the pattern of distribution for formulaic or partially analyzed structures differs only slightly from that for the Polish speakers.

The case of *WH/INV* structures in the Vietnamese group is a very good example of the necessity of distinguishing formulas from rule based applications—a more cursory approach to the data would have failed to isolate an important difficulty for this group of learners.

As far as both groups of learners are concerned, and always keeping in mind the limitations of the data itself, it seems reasonable to say that the

hypothesized difficulty of the four structures appears to be reflected in the above findings. More definite confirmation will have to wait until a more extensive corpus of data on questions from a wider range of situations can be collected and analyzed. An analysis of the elicited data, where the question type is largely dictated by the form of the prompt, and avoidance patterns should therefore be obvious, would provide another means of examining the relative difficulty of these four structures, and is an evident direction for further investigation.

4.11.7 Ill-Formed Structures

To this point, the discussion has been restricted to well formed, or apparently well formed structures. The final column in Tables 4.41 and 4.42 shows the figures for structures of more than phrasal complexity which failed in some way to meet the criteria for well-formedness. As it turns out, there are numerous ways in which this can happen.

The most straightforward cases of non-standardness are those in which either inversion or *do*-insertion failed to take place. Examples of the non-application of inversion are:

where I can got, er... information?

- aj.1: [774] -

how old... he is?

- jr.1: [1322] -

They are, er, Vietname(se) maps?

- canh.1: [68] -

you can get job like me now or not?

- long.1: [785–6] -

It was difficult to find many examples of WH-questions where inversion had not been applied in the output of the Vietnamese speakers due to the strong tendency of such structures to lack a copula at all.

Examples of the non-insertion of do are:

how you ca(ll) de name?

- hoa.2: [189] -

What you want?

- long.1: [621] -

you live in Australia... yes?

- jr.1: [1286] -

You b(e)lie(ve) in da(t)?

- sang.2: [687] -

Many other types of non-standardness result from attempts to avoid the word order problems presented by questions.

One such strategy is non-insertion of the subject:

not... er... miner... er... what is?

- bb.1: [60] -

Where is?

- ka.2: [355] -

and... is good profession?

- bb.1: [1130] -

Is, er, near city?

- jr.1: [1268] -

In addition to the subject, other elements can also fail to appear:

How long... from Poland to Australia?

- ka.1: [116] -

understand me?

- bb.2: [316] -

no(t)s right?

- vinh.2: [419] -

Another strategy involves leaving out the copula, as in these examples:

whas your name?

- van.1: [119] -

what you... hobby?

- jr.1: [1338] -

This all?

- bb.1: [1049] -

Your sore? [sure]

- hoa.1: [457] -

Or an auxiliary of some kind:

How long you (d)rive car?

- minh.2: [271–2] -

How long I waiting?

- jr.1: [410] -

You think so?

- tam.1: [1357] -

Excuse me, I go to little while?

- canh.1: [2447] -

There are other possibilities as well, such as not preposing the WH-constituent:

Have you got, eh... how many children?

- duc.1: [744] -

It should also be mentioned that certain types of question are conspicuous by their total (or almost total) absence. The most notable examples are negative questions and tag questions. The range of verbs on which questions are formed is also quite restricted. The examples throughout this section provide a representative sample of these.

4.11.8 Embedded Questions

One other phenomenon for which some results exist is the production of embedded questions—that is, questions which are actually the complement of some verb like know or ask:

I don't know what I must said

- kb.1: [97] -

I don('t) care where... where I live

- phuc.2: [135] -

Embedded questions are probably a fairly good index of overall structural sophistication. One interesting characteristic of embedded questions is that the rules of inversion and *do*-insertion do not apply in them, and may therefore have to be unlearnt by learners who acquire them. This of course depends on whether they have really learnt such rules in the first place. Tables 4.42 and 4.43 provide a list of those speakers who produce embedded questions and notes whether movement or insertion rules have applied. To evaluate the significance of this latter information, it is necessary to refer back to Tables 4.41 and 4.42 for evidence of what rules a particular informant applies in direct questions. Embedded questions can, of course, take various other forms—such as infinitival or intransitive—where movement or insertion rules are not applicable, or they can be ill-formed. No attempt has been made to detail or quantify such features in the tables.

4.11.9 Conclusions

There are various points of significance to emerge from the analysis of questions.

There appears to be a definite hierarchy of difficulty amongst the main question types, with forms involving inversion less difficult than those involving *do*-insertion.

The introduction of new material into a structure—or its discovery as the structure is analyzed—seems to present a more complex processing task than movement of existing constituents. One of the difficulties with new elements is that learners must adjust their grammars to accommodate them, either as new items or as old items with new functions.

Do is a particularly difficult case of such a new element, given its prior function as a lexical verb. Yes/No questions with *do*-insertion, where there is no possible way for do to enter the system as part of another element are the most difficult of the four main question types. WH-questions with do, while easier to acquire in partially analyzed form, with do as a part of the

WH-word, present problems to the learner once he or she begins to try to analyze and generalize them. With this question type there are semantic complications as well arising from the necessity of matching the WH-forms with a wide range of verbs. The state of a learner's system with regard to copula deletion can affect structures involving inversion, as it does with the Vietnamese speakers.

The basic sequence itself, and factors such as these need to be taken into account when teaching interrogation. In general, the range of verbs used in questions is quite limited, and this too should be considered by the teacher. Some forms, such as negative questions and question tags should probably be left for a later stage. Embedded questions, on the other hand, are produced and could be taught; they represent a communicatively useful way of building up more complex structures. A fair percentage of non-standard question structures characterize the output of most informants in the sample: this should probably be tolerated by the teacher. It is worth remembering that the movement and insertion rules in questions are not usually crucial to communication, provided that there is some kind of intonational flag. Suppression of major constituents like subjects or verbs tends to reduce the communicative effectiveness of questions more than absence of inversion or *do*-insertion and corrective work might be more fruitfully focussed on these features; at the same time the teacher should bear in mind that phenomena such as subject deletion may be evidence of a change in the learner's rule system, and should be temporarily tolerated.

4.12 Nominal Morphology

4.12.1 Inflectional Morphology

This section deals only with inflectional nominal morphology, namely regular plural *-s* and genitive *-'s*.

Previous studies of the "acquisition" or order of difficulty for these morphemes, such as morpheme order studies, have assumed that the two morphemes in question were developmental features, acquired in a fixed order in relation to other morphemes. As we shall see, in the case of plural *-s* at least there is some cause for doubting this.

4.12.2 Plural '-s'

During the transcription process in any context where the plural form appeared to be obligatory and was not realized this was flagged by the addition of either (s) or (es), as appropriate, to the word in question. This practice of using round brackets to signal non-realized morphemes or phonemes is, as noted in 3.3.4, a general convention of the transcriptions. Examples of null plural marking flagged in this way would be:

Informant	WH/INV	WH/DO	YN/INV	YN/DO	NONSTD
Irena S.					
Zygmunt J.	1F				1
Krystof S.	5F	2F?	1F	1X?	5
Jerzy B.	4F	1F	1F	1F?	5
~	1F?				
Jan R.	3F				27
Krystyna A.	2F		2X		5
~	2				
Barbara B.	5F	5F	1		23
~	1				
Ewa S.	1	1F?			4
Andrzej J.			1F?		4
Krystyna B.	2F	1X	4	6F?	12
Ludwiga J.	6F	1F	2F		10
~	1X				
Mieczyslaw M.			1X		6

Table 4.41: Distribution of Question Types: Polish Informants

Informant	WH/INV	WH/DO	YN/INV	YN/DO	NONSTD
Van	2F				2
My	2F				5
Duc	3F				9
Dung	2F?	8	1F	1F	
~		1F?	3F?		2
Minh		1F	3	2F	7
~		1X			
Hoa	1F	1F?	3F?	1F	8
Sang		2	2F	4X	3
Vinh					3
Tam	1F	1X			12
Canh	1F	1F?	3	2F?	20
Long			1F		16
Phuc		1X			2

Table 4.42: Distribution of Question Types: Vietnamese Informants

Informant	Emb_Qs	Do/Inv	No_App
Krystof S.	1		
Barbara B.	8	X	
Ewa S.	2		
Andrzej J.	9	X	X
Krystyna B.	12	X	X
Ludwiga J.	13	X	X
Mieczyslaw M.	3	X	X

Table 4.43: Distribution of Embedded Questions: Polish Informants

Informant	Emb_Qs	Do/Inv	No_App
Dung	3	X	
Minh	3		
Sang	3	X	X
Vinh	2		
Tam	5		X
Canh	10	X	X
Long	12		X
Phuc	14	X	X

Table 4.44: Distribution of Embedded Questions: Vietnamese Informants

I learn two... er... job(s)

- bb.1: [44] -

Yeah.../... go to parti(es)

- dung.1: [122] -

Close attention was paid to this procedure during the checking of the transcriptions, so that the counts thus obtained should be reasonably accurate.

Methodological Problems

In checking for plural marking there are various complicating factors, and it is probably worth noting these.

1. It is not always clear that a particular semantic context is obligatorily plural. For example, consider the following exchange:

   I Em... get... Poland, er... my, er... my, er... document... Poland document mus(t) ... mus(t), er... travel?... no

   MJ Translate?

   I Translation, yes

   - ks.1: [124–9]

1. It only becomes clear further on in the interview that several documents are involved, so there is no flagging of the null plural at this point.

2. There can also be problems with the classification of generics, where it is difficult to decide if it is a determiner or the plural morpheme that has not been supplied, as in the following example:

   and their hands are on trigger... trigger ready to shoo(t)

   - phuc.1: [797–8] -

1. There are also some problems of phonetic origin. Vietnamese speakers frequently produce an /s/ phone, or something that sounds very like it to the ears of an English speaker, in place of a target alveolar stop. Thus but becomes bus, or good, gooz. This means that it sometimes difficult to know whether to credit a speaker with plural marking or not.

2. Another case, this time involving both groups, has to do with the word months, which is a frequent candidate for pluralization. Here the complication is that the singular is frequently rendered as mons (*/mans/*), and there is really no way to tell whether an informant is attempting to pluralize a token unless he or she produces both the */th/* and */s/* phonemes, which is something that even native speakers may neglect to do in casual speech. (A check on this latter phenomenon revealed that omission of tokens of months did not significantly affect any of the counts).

3. Moreover, in a more detailed analysis of plural marking, it would probably be necessary to take into account the contexts in which marking does or does not occur. There are various possible scenarios. A learner who marks plurals with some regularity in noun phrases which follow plural numbers or quantifiers, or in other contexts where plurality is evident, may not know the rules, say, for generic noun phrases. Another possibility is that an informant might mark plurals more regularly where such marking is not redundant, so that he or she may actually be less likely to mark noun phrases after plural numbers or quantifiers than elsewhere. In the present analysis only the apparently most congenial lexical environments for plural marking are investigated.

Distribution of Plural '-s'

Bearing these considerations in mind, Table 4.45 provides some figures on the number of realized and null plural morphemes for each informant.

The most obvious point in the findings is the discrepancy between the Polish speakers and the Vietnamese speakers. Amongst the Polish speakers there are only two cases of null tokens exceeding realized ones—**bb.1** and **jr.1**. Amongst the Vietnamese speakers, on the other hand, there are only four cases in which realized tokens exceed null ones—**phuc.1**, **phuc.2**, **minh.2** and **dung.2**. Unquestionably phonetic and phonological difficulties contribute heavily to the state of affairs with the Vietnamese speakers. Other factors will be discussed below.

The above result naturally calls into question the status of the plural morpheme as a developmental feature, given that it appears to exhibit extreme vulnerability to first language influence. This doubt is magnified by the results gathered from the Polish speakers, in which some of the unquestionably more developmentally advanced informants have lower ratios of realized to null tokens than the less advanced learners. (Compare the results for Irena S. and Ewa S., for instance). In addition, while there is a general improvement in the ratio of realized to null tokens in the second interview for the Polish speakers, there are some apparent cases of "backsliding", such as Krystyna A. and Krystyna B. This is not what one would

WORDS	INTERVIEWS
years	36
months	20
sometimes	18
friends	16
hours	16
weeks	16
days	15
times	14
things	13
books	12
dollars	11
problems	11

expect of a developmental feature [128].

In 4.1 a rough characterization of "standardizing" versus "simplifying" learner types was presented (Table 4.7). For the Polish informants, while the correspondences between learner type and rate of suppliance of plural *-s* is by no means regular, there seems to be some correlation between these two characteristics. This correlation might possibly improve if learner type was assessed by some less intuitive means.

In the case of the Vietnamese speakers, their phonological problems with final stops and consonant clusters complicate the issue to such a degree that there must be some doubt about the value of discussing plural *-s* for these speakers in either variational or developmental terms. Despite the gap between them and the Polish speakers, however, it should be remarked that once again in general the standardizing learners appear to have a significantly higher rate of suppliance than the simplifying ones.

Clearly, the status of the plural morpheme requires further research, with a larger corpus and a wider range of language backgrounds. On the basis of the present data, however, grave doubts about the developmental nature of this particular morpheme are unavoidable.

Lexical Candidates for Pluralization

While some 275 different words are pluralized across the whole corpus, a relatively small subset of these are pluralized repeatedly. Words that are pluralized in more than ten of the forty-eight interviews in the cross-sectional sample are the following:

As regards phonetics, the unpluralized stems of these words (as they are actually pronounced by learners) terminate predominantly in nasal consonants (six cases), vowels (four cases), and voiceless velar obstruents (two

cases). The nasal consonants may in some cases be replaced by a nasalized vowel, which would have the effect of avoiding a final cluster and preserving a vowel-consonant order. From a semantic point of view, seven of the words are time-related. While the list given above no doubt reflects the discourse parameters of the interview situation, it would be advisable to keep its composition in mind for the development of explicit materials for the practice of regular pluralization. If the phonetic observations made above are valid, they would, of course, be largely independent of particular choice of words. This topic could be fruitfully researched in more detail. Tables 4.50 and 4.51 provide figures for the distribution of the most commonly pluralized words in the first round of interviews.

Overgeneralization of Plural '-s'

As with other morphemes, plural *-s* is prone to a certain amount of overgeneralization or semantically inappropriate application. Some of this is idiosyncratic, as in the following examples:

I remember one girls they workin(g) together

- long.1: [723] -

Doctors?... yes... er... my sister is doctors

- bb.1: [624] -

Other examples, however, are more widespread, and call into question the notion of pluralization with some words:

and one years is not problem

- bb.1: [958] -

and one hours

- hoa.1: [39] -

one years... two years

- is.2: [639] -

Other examples show that there has been a failure to recognize irregular or partitive plurals:

I fishermans

- long.1: [139] -

I must to understand many peoples

- lj.1: [60] -

can I have a pants please

- kb.1: [689] -

oh, some peoples maybe

- kb.1: [834] -

The Plural Morpheme—Summary

Although further research is required, the developmental status of the plural morpheme is very dubious. This morpheme seems to be particularly susceptible to phonetic (first language) interference, and appears to display other characteristics of a variational nature. Where it does occur, certain phonetic environments seem to favour its suppliance. In addition, in the present corpus at least, there is a group of words which are likely candidates for pluralization. These factors have obvious pedagogic significance.

4.12.3 Possessive "-'s"

Possession can be lexically, syntactically, or morphologically indicated in Standard English. Lexically indicated possession involves verbs such as have and belong and involves structures in which the possessor may be grammatically a subject or an indirect object, with the choice of grammatical relations being determined by sentence pragmatic or discourse considerations. Syntactically indicated possession involves possessed-possessor structures linked by the preposition of and is subject to various semantic constraints, which restrict its usage quite powerfully. Morphologically indicated possession involves case marking of the possessor noun phrase with the ubiquitous *-'s* morpheme, and a word order in which possessor precedes possessed. These latter structures can be more generally implemented than possessed-possessor structures, though they are by no means unconstrained, and are what we generally think of as possessives in English. As a result of their dependence on the *-'s* morpheme for case marking of the possessor, they have, through the years, attracted a good deal of attention from the proponents of morpheme order acquisition studies—where the general consensus is that the possessive *-'s* morpheme is the "most difficult" or last acquired of the three *-s* morphemes [11].

Interview	Tokens
aj.2	4
phuc2	4
mm.2	2
duc.1	1
dung.1	1
is.2	1
kb.1	1
phuc.1	1
es.2	1F
vinh.2	1F
canh.1	1?
es.1	1?

Table 4.45: Distribution of Possessive "'S"

In the present study, the evidence for this is somewhat equivocal. On the one hand, the frequency and distribution of the three *-s* morphemes is in line with the predictions of morpheme order studies. However, as we shall see, closer examination of the data reveals violations of the predicted implicational order and a general lack of developmental patterning for the nominal inflections.

Distribution of Possessive "-'s"

Tables 4.45, 4.46, 4.48 and 4.48 show the distribution of possessive *-'s*.

At this point, no figures on obligatory contexts are available for comparison with those given in Tables 4.45, 4.46, 4.48 and 4.48. Figures for the realized morpheme are not high. Tokens of possessive *-'s* occur in only twelve of the forty-eight interviews in the cross-sectional study. In two interviews the tokens are formulaic, that is, they involve names—Regent's Park and Mario's Drivin(g) School. In two others they are dubious for semantic reasons. This reduces the number of certain cases to eight. The maximum number of tokens produced in any one interview is four. Excluding the dubious or formulaic tokens, the number of informants who produce tokens of possessive *-'s* is seven.

Alternative Means of Indicating Possession

Clearly, then, possessive structures do not constitute an important part of even relatively developed learner language.

For learners, alternative ways of indicating possessor-possessed relations are available. One strategy is to simply juxtapose the possessor and the

possessed in an noun phrase without producing an morphological marker. Examples are:

the gir(l) name Dung too

- hoa.1: [952] -

Possessed-possessor structures with of also occur:

I've look... looked at, er... windows of the travels agency

- kb.1: [306–7] -

Non-standard possessed-possessor structures minus the preposition also occur:

husband my, eh, shvester.../...cou(ld) found job

- ks.1: [556] -

Other more periphrastic constructions may also occur occasionally:

belong car his brother-in-law

- dung.1: [659] -

4.12.4 The Developmental Status of '-s' Morphemes

As with the plural morpheme, the data on the production of possessive *-'s* raises questions about the developmental status of this morpheme.

In the present data, while the general pattern of *-s* morpheme production coincides with the findings of morpheme order acquisition studies, closer examination of the data on the nominal *-s* morpheme and comparisons with other kinds of data does not reveal any internal structure—in the nominal *-s* morpheme data at least—which would suggest that production of the *-s* morpheme occurs in a developmentally regular or coherent way.

This lack of developmental regularity has already been discussed in some detail in respect of plurals. If we examine the behaviour of those informants who produced tokens of possessive *-'s* in relation to the other two *-'s* morphemes we find that the seven informants who produce tokens of possessive *-'s*, fall into two groups. Four—Phuc, Mieczyslaw M., Krystyna B., and Andrzej J.—rank highly on the *ASLPR*, exhibit generally advanced structural and morphological development and relatively high production figures for third singular *-s*. (Though "relatively high" is from all rough indicators likely to be "rather low"). On the other hand, these informants' control of

plural marking is somewhat variable. Thus, while Andrzej J. and Mieczyslaw M. have, respectively, relatively high and high ratios of realized to null plurals (see Tables 4.45, 4.46, 4.47 4.48 and 4.48), Krystyna B. and Phuc exhibit lower ratios. Despite this, we might reasonably expect some tokens of possessive *-'s* from the informants in this group from within a morpheme order framework.

Members of the second group—Duc, Dung, and Irena S.—rank low on the *ASLPR*, however, and generally do not exhibit the structural and morphological development of the first group of informants. Irena S. has middling ratios for realized versus null tokens of the plural but produces no tokens of third singular *-s*. Duc and Dung have lower ratios for plurals but produce a few valid tokens of third singular *-s*, although Duc in fact produces this morpheme three times out of four with first person subjects and always in past tense contexts.

If there was a clear implicational relationship of the $POSSESSIVE \supset THIRD\ SINGULAR \supset PLURAL$ type, we would not expect possessive *-'s* tokens from Irena S. or, probably, Duc.

4.12.5 An Alternative Proposition

In trying to make sense of this anomaly we should note that while the informants in the second group rate less well than those in the first on plural *-s*, what really distinguishes them is their lack of control of the third person singular morpheme. This suggests that in fact the three *-s* morphemes should perhaps not really be ordered as they are on a single implicational scale.

From a speech-processing point of view, it may be possible to make out a case for the above assertion. Roughly, this is as follows. In nominal morphology the governor for marking is within the same noun phrase complex as the marker—either in the form of a number or quantifier, or inherent in the semantics of the utterance. In verbal morphology—in the case of third person singular marking at any rate—the governor is another constituent—namely, a pronoun or noun phrase and the marker is separated from its governor by the verb [93]. Thus marking involves sentence-internal rearrangement of material. In processing terms this type of interruption leads to greater complexity. Non-standard structures in which this internal rearrangement has not occurred can indeed be found in learner speech:

> Yes... yes... he's have
>
> - ka.1: [624] -
>
> he's tell me come
>
> - long.1: [173] -

she's remember me and my sister

- vinh.1: [348] -

The upshot of the above is that it is possible that of the three *-s* morphemes, the third person singular is in fact a developmental feature, while the plural and genitive are not. This explanation fits the present data more satisfactorily than the traditional implicational ordering. Clearly, with so little data for the genitive it is hard to determine its status *vis-a-vis* the developmental/variational distinction; in the case of the plural we have seen evidence that it is very possibly not a developmental feature. From a speech-processing point of view, there seems even less reason to assume that possessive *-'s* is a developmental feature, since the marking entailed involves no reorganization of constituents.

The leaves us, of course, at loggerheads with the findings from morpheme order studies. It should be noted, however, that a critique of these studies has been offered in 2.1.5 which raises serious doubts as to the validity of their findings. The low figures for the production of genitives there and in the present data may, as has been suggested reflect nothing more than the relative frequency of this item in the typical range of discourse situations [109]. As with other varieties of the *-s* morpheme, phonetic considerations and the form-function constraint may contribute to limitations and irregularities in the suppliance of the genitive. Once again, further research will have to be conducted if these issues are to be clarified.

4.12.6 Nominal Inflections—Summary

In regard to nominal inflectional morphology, it is possible that the traditional morpheme order study order of difficulty or acquisition is erroneous, given that there exist substantial doubts about the developmental status of these markers. There is some evidence that regular plural marking may be a variational feature. Due to lack of data the status of the genitive is unclear, but, equally, it may not be developmental in nature.

Regardless of this, there is evidence of some phonetic and possibly lexical regularities in the acquisition of pluralization. These should be noted. Likewise, there is evidence that possessive *-'s* is barely used by learners in the proficiency ranges covered by the study, and consequently it should not be allocated a very high pedagogic priority.

4.13 The Definite and Indefinite Articles

4.13.1 Limitations

Tables 4.52, 4.53, 4.54 and 4.55 to 4.13.1 provide some basic information about definite and indefinite article usage in the cross-sectional sample.

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
Realized	12	20	21	18	17	27	16	27	34	47	99	55
Null	6	10	15	16	37	7	38	25	11	17	39	4

Table 4.46: Plural "-s" Morpheme: Polish Informants—1

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
Realized	21	23	30	13	10	34	21	17	52	28	82	60
Null	3	22	7	15	6	7	16	15	9	25	43	7

Table 4.47: Distribution of the Plural "-s" Morpheme: Polish Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
Realized	1	1	20	18	4	3	1	9	14	44	33	89
Null	6	6	34	18	20	32	23	41	57	121	108	29

Table 4.48: Plural "-s" Morpheme: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
Realized	-	8	7	7	2	-	19	1	16	6	8	18
Null	7	27	28	5	43	25	13	30	41	40	10	9

Table 4.49: Distribution of the Plural "-s" Morpheme: Vietnamese Informants—2

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
years		5	8	5	4	8	6	2	10	10	16	7
books	2			2	1	1		2	1		2	2
sometimes		1		1	3	1			2	1	9	1
months			4		2	3		1	1		5	
hours				1	1	1		1	3	1		
weeks			1	1					4	2	3	
friends		4						3	2		5	
times			1						2		4	2
things				1						1	2	6
days	1				1			2				
problems	1						1				6	
dollars							4	1				

Table 4.50: Most Frequently Pluralized Words: Polish Informants—1

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
years			1	4	1			6	6	5	14	8
months			1	5	2			1		1		1
days		3	3	1			1			2	4	
hours		1		4		2						3
friends						1			4		1	1
times			1							1		1
weeks					1		1					1
things								1		4		1
dollars				3							4	
sometimes						1						4
problems												2

Table 4.51: Most Frequently Pluralized Words: Vietnamese Informants—1

Table 4.13.1 gives the absolute number of tokens of the, a and an, or their phonological variants for each interview. In the case of the first-round interviews these tokens were hand checked and repetitions or erroneous tokens eliminated. This was not done for the second round of interviews, with the result that the figures given for these are undoubtedly too high. The more detailed analysis provided in subsequent tables and the comments that follow here are all based on the first round of interviews, and this should be kept in mind. Time prevented any close scrutiny of the data from the second round of interviews: in any case, it is quite unlikely that the results reported here would be significantly different had this data been included in the analysis, given the very considerable number of obligatory contexts for article usage in a single interview.

4.13.2 Frequency of Realization

The first point that has to be made in relation to both the definite and indefinite articles concerns their low rate of realization. So far, it has not been possible to identify by hand in the transcripts the obligatory or optional contexts for their production. On the evidence of Table 4.13.1 alone this looked rather low. However, in order to provide some additional approximate idea of how frequently or infrequently articles were produced, the following calculations were made. The number of nouns occurring in a given interview was tallied from the lexicon for that interview. Then the number of articles produced was expressed as a percentage of this figure. Obviously, since not all nouns would be obligatorily preceded by non-null articles (they could be legitimately preceded by null determiners, or by adjectives or quantifiers) this procedure can only provide a rough idea of the suppliance of articles in obligatory contexts. However, even assuming that half the nouns produced should have been marked with an article, the realization rate so calculated is quite low for most of the informants.

II	I
+SR +HK	-SR +HK
IV	**III**
+SR -HK	-SR -HK

This generally low rate is reduced even further in the case of an, with a total of a mere seven tokens in the whole cross-sectional corpus.

A closer examination of these tokens makes it clear that the phonological rule for an is just beginning to establish itself in these speakers. The one token for Ludwiga J. occurs in the phrase half an hour. One of the two tokens produced by Andrzej J. shows evidence of hypercorrection:

> they... will get... er... an... an good work in... their job
>
> - aj.1: [708] -

While in the case of Phuc, the one Vietnamese speaker to produce tokens of this form, three out of four come in the phrase an interpreter.

A further point of interest is that all valid tokens of an in the first round of interviews occur in front of words beginning with /*i*/. This suggests that the most favourable phonological contexts for the operation of the *n*-insertion rule for the production of an are high front vowels. From an articulatory viewpoint this makes sense, since an unmediated transition from a mid or—as is more likely in learner language—a low back vowel to a high front one would be the most difficult transition to manage.

4.13.3 Syntactic and Semantic Environments

The suppliance of articles in the present corpus seems to be conditioned fairly strongly by semantic, and to a lesser extent, syntactic factors.

4.13.4 Semantic Environments—The Definite Article

In the case of the definite article an analysis similar to that conducted by Huebner was carried out [85]. Huebner, following a model proposed by Bickerton, argues that noun phrase reference can be accounted for in terms of two binary features *+/-Specific Referent* $(+/-SR)$ and *+/-Assumed Hearer's Knowledge* $(+/-HK)$. This provides the following semantic field:

In English, noun phrases in Sector I are marked with the, while those in Sector IV—that is, generics—can be marked with the, a or pluralized and

marked with a null marker. Noun phrases in Sectors V and VI are marked with a or the null marker.

The analysis presented in Table 4.60 defines a fifth category—that of proper names or formulas. Semantically, these are a subset of the cases covered in sector II. However, given the status of formulas or monomorphemic chunks in the learning process it seemed advisable to allot them a separate classification: there is no guarantee that the article in such utterances is analyzed as such and is therefore a meaningful unit. (Huebner's solution to this problem was to exclude what he judged to be proper names and idioms from his analysis. This may be permissible if the focus of the study is primarily semantic, but from an overall developmental point of view it is clearly unsatisfactory, since formulaic language is arguably the seedbed of propositional language).

Phrases such as the city, the Communists and the hostel were classified as proper names, and utterances like the best country, the next day and the same were classified as formulas. As always there were borderline cases that had to be decided largely on intuition. A more general problem with making judgements of semantic intent on discourse based evidence is that lack of cohesion in the discourse itself can be a complicating factor. Models like the one above may appear to be applicable to all varieties of language, but in practice are a good deal easier to apply to native speech, with its well articulated discourse features, than to learner language.

As is clear from Table 4.60 the majority of the definite articles produced are as markers for proper nouns or generics. In practice, it can be quite difficult to distinguish between these two categories if the generics are, as is frequently the case, non-idiomatic. Some examples of borderline generics would be the following:

> the food here is not expensive
>
> - vinh.1: [273] -

> I... I have... I've learn... I have, um, read [/rid/] the book and, er,... er, take on paper, er, w... words and I learn words,
>
> - mm.1: [58–9] -

Given that generics do not involve specific reference, proper nouns involve it by default rather than actively (that is, the referent is effectively a member of a unique set), and the function of the definite article in a formula is empty or redundant given the semantic content of the formula (e.g. the best), this means that a preponderance of the definite articles produced by learners up to quite a high level of proficiency do not actually involve positive specific reference.

This pattern of usage is quite significant, since it means that in effect the definite article is in most cases in the present corpus not really used as a discourse marker.

When the definite article is used to indicate positive specific reference, there is evidence of confusion over the feature of *Assumed Hearer's Knowledge*. Thus, we find a number of cases where Sector III noun phrases would be acceptable to native speakers. Examples are:

> mm... firs(t) time... um... have er sik boas, er, in the, er, raiver ...the raiver... er
>
> - sang.1: [703–4] -

> In one week... er... the material... came
>
> - ka.1: [281] -

(It should be noted that the marked noun phrases in these cases have no prior referent—their possible acceptability depends on them being interpreted as idioms). In addition to these, however, there are some clear cases of unacceptable tokens, such as:

> No water... mm... every very tire(d), um... but, er... the s(h)i(p) of Thailan(d), er, ro(f)... after ro(f), er... they ta(ke) some water and ri(ce)
>
> -sang.1: [764–6] -

> I don't know yet... /... if, er... er, it will be possible, er, to work... and er..., er... not have [?] the, er, special er... electronics course
>
> - aj.1: [675–9] -

Amongst the Sector III tokens there are also some interesting cases where the speaker's confusion about which surface realization to assign to noun phrases in this category is quite patent:

> How long, er, how long, er, you work a job, er... this job
>
> - minh.1: [469] -

> people they, er, sell the something around... around the camp
>
> - vinh.1: [272–3] -

it was not possible... the one person, er, must stay in Poland

- aj.1: [590] -

and I was working in the, er, this brown coal mine

- mm.1: [189] -

Yes, it's, er, social problem because in the... a few last years

- mm.1: [479–80] -

In the mastery of the semantics of the definite article there is evident difficulty in simultaneously controlling the features of *Specific Reference* and *Assumed Hearer's Knowledge*, once the feature of *Specific Reference* becomes a factor in production of the definite article—that is, once the speaker's noun phrase marking extends beyond names, formulas and generics.

The Question of Sequencing

It is not really clear whether we would be justified in speaking of a semantic developmental sequence for the definite article.

In Huebner's analysis, which is based on the semantic quadrant presented above, his informant's progress seems to be characterized first by a series of inappropriate hypotheses—one founded on the notion of topic marking, another on global marking of noun phrases—and then by a gradual process of elimination of marking in those environments which are ungrammatical in English. This process is characterized by stops, starts and backsliding, and seems on the surface very plausible.

Such an interpretation is based, however, on the assumption that the features used in the analysis exist independently of the developing language which embodies them. That is, Huebner assumes that the learner is merely applying a set of pre-existing universal discourse features in different ways until he gets the right fit rather than actually discovering the semantic properties of the article system in English as he goes along. This effectively disallows any interplay between form and function: functions are already defined and it is merely a question of getting the forms to index them in an appropriate way.

There are problems with such an approach. Firstly, the features involved are both complicated and rather abstract, and it is difficult to envisage a psychologically real process of hypothesis testing involving them. Second, it is difficult to reconcile a developed set of oppositions of the type presented in the semantic quadrant with formulaic—semantically opaque—language. With the relevant semantic categories already established, such language is

anomalous. This leads to Huebner excluding from consideration such things as idioms, proper nouns and commonly used expressions. Yet elsewhere Huebner himself argues against the exclusion of such language forms from any analysis of developmental processes, and we have abundant evidence ourselves that formulaic material is instrumental in the evolution of propositional language [139].

An alternative approach to Huebner's which would admit all the data to hand is to characterize the learner's task in a different way. Rather than comprising an attempt to fit hypotheses to a pre-existing framework the learning task can be viewed as a process whereby this framework is actually developed. Such an interpretation is much more in line with other developmental processes we have examined—for instance, the growth of the semantic tree which characterizes the pronominal system. In addition, it allows for interplay between form and function.

In this approach, the definite article would enter the system in proper names and other forms of formulaic language where specific reference is in fact a semantically empty notion, given the one member sets involved. The feature of *Assumed Hearer's Knowledge* would come to be associated with the definite article through generics. (These are considerably more numerous in the present corpus than in the data from Huebner's informant).

As the learner's discourse became more particularized and noun phrase sets became contrastive—that is, multi-membered—the feature of *Specific Reference* would emerge. The problem for the learner would then be to decide which combinations of *Assumed Hearer's Knowledge* and *Specific Reference* were valid correlatives of the definite article in English. This is more or less the process we have commented on, and, as Table 4.60 shows such an interpretation is consonant with the data in the present study. The amount of non-formulaic data that we have on which to base any hypotheses we may wish to form is not great, and it would be rash to be over-assertive about the above conclusions. But regardless of their status, given the preponderance of proper names and formulas in the present corpus, we simply cannot afford ourselves the luxury of ignoring formulaic language, as Huebner does: too much of the data is in precisely this form.

Evidently, the process described above, or indeed the process described by Huebner is not an inevitable one. In his longitudinal study, Huebner describes a phase in which his informant simply marks all noun phrases with the definite article [85]. In the present study, this phenomenon is to be observed in two of the Vietnamese informants, Canh and Long. These informants supplied a higher percentage of articles before nouns than any other informants in the study (see Table 4.52), but analysis of their output revealed that their marking of noun phrases with the definite article was quite indiscriminate. While some sectors of the quadrant are more heavily represented than others, numerous examples of marked noun phrases in every sector are in evidence:

Sector I:

I think there is ju(s)t, er, the place they want to test weapon(s)

- canh.1: [736–7]

the father must eat before the children, yeah

- long.1: [963–4] -

SectorII

I can understan(d) the life Australian living

- canh.1: [584] -

he help take the hand-bag he come see me an(d) give back the ticket for the bus

- long.1: [170–1] -

Sector III:

If I, ah... I ha... I have the girlfriend... I want to see her, oh, ve(ry) difficul(t)

- canh.1: [1339] -

to the city an(d)... I go out to the, er... bus station, you know, I see my friend on the bus

- long.1: [167–8] -

Sector IV:

they haven't got the government, they just got a Junta

- canh.1: [781] -

they get we somewhere too... because they have the trouble before in Vietnam

- long.1: [247–8] -

Whether in their case it is a phase or the end of the road as far as developments in the system of articles are concerned is a moot point. Given the fact that both informants had been in Australia for three to four years at the time of the interviews and were amongst the more communicatively effective informants in the sample, the latter possibility—that of rule fossilization—seems the more likely of the two.

4.13.5 Developmental and Variational Features

We have discussed two possible views of the development of a rule system for target-like production of the definite article in English—a hypothesis fitting model and a developmental one. Both approaches suggest that there are regularities in the process of acquisition of the definite article. Regularities in an acquisitional process, however, do not necessarily indicate that the feature being acquired is a developmental one. We have seen that there is evidence for the existence of ordered environments for the suppliance of the copula, yet this is almost certainly a variational feature [128]. Cases like those of Canh and Long emphasize the next question to which we have to turn—that of whether the definite article is an acquisitional or variational feature.

Morpheme order studies assume that articles, like all other morphemes are developmental features. In doing so they have given no consideration to the underlying semantics of article use except in so far as this appears to coincide with target language patterns. As Huebner points out, target-like usage patterns may not actually be the product of target-like semantics [85]. The present data supports Huebner's contention. Speakers like Canh and Long have the highest rate of suppliance of definite articles of all the informants in the sample, and would therefore most closely approach morpheme order criterion for acquisition of this item. On the other hand, their underlying semantic systems appear to be among the least developed of all for those informants who use definite articles. For this reason, amongst others, we cannot adduce the evidence presented in morpheme order studies as evidence in favour of the classification of articles as developmental features.

Ultimately, the question of whether the definite article is a developmental or variational feature cannot be definitively settled until independently motivated developmental sequences have been established for English. If we make some rough assumptions about what these might be, however, the status of the definite article as a developmental feature becomes quite doubtful. Thus, in negation Canh appears to have a reached a fairly advanced stage of development, employing only standard preverbal negators in a wide range of contexts and even suppleting some into any in the scope of negation. In terms of the verbal system, he uses a wide range of irregular pasts, produces more regular pasts than other Vietnamese speakers, marks some verbs with third singular *-s*, and produces some gerunds. If the definite article were a developmental feature we could reasonably expect him to be at a more advanced stage than the global noun phrase marking stage he in fact appears to be at. For Long, the discrepancy between his pattern of usage of definite articles and other aspects of his grammar is not so great. Nevertheless, if we are talking about a developmental feature one dubious case is enough. In addition to these considerations, the most detailed description that we have of the acquisition of the definite article over time—namely Huebner's—

suggests that the acquisition process is by no means linear, or constrained in the way that one would expect the acquisition process for a developmental feature to be.

The case of the definite article *vis-a-vis* the developmental/variational distinction is an interesting one. Some of the conditions for a developmental feature are met: namely that a postulated developmental sequence can be independently motivated in that the development of a workable set of semantic distinctions must take place, in a manner analogous to the development of processing prerequisites necessary for the acquisition of word order rules. And then there are the implicational type patterns of Table 4.60. On the other hand, there are the anomalies just mentioned. Clearly, this area is one in need of further research.

The Question of Transfer

While neither Polish nor Vietnamese has an indefinite article, Vietnamese uses a postposed demonstrative adjective to indicate prior reference. An example would be:

Anh có anh không?—"You have brother not-Q?"

Anh ấy lãm gì?—"Brother that work what?"

In Chapter Two a hypothesis was advanced that interference on the morphological level was likely to be higher than say, on the clausal level. Given the existence of a definiteness marker in Vietnamese we might therefore expect that this would predispose Vietnamese speakers to be more sensitive to the possibility of such a marker in English. Tables 4.13.1 and 4.56 show that the absolute rates of suppliance and the percentage of nouns marked with the definite article is, in general, slightly higher for the Vietnamese speakers than for the Polish speakers. It is hard to tell if this difference is really significant, much less attribute it entirely to transfer. Nevertheless, the possibility cannot be discounted. Elimination of proper nouns and formulas from consideration accentuates the higher rates of suppliance for the Vietnamese speakers and therefore enhances this possibility. Nevertheless, it should be borne in mind that the differences in suppliance of the definite article are nowhere near as dramatic as those, say, for copula insertion. If there are first language transfer effects they would seem to be conditioned by functional considerations: whereas the copula is semantically vacuous and can be safely ignored, some means of indicating definiteness will have to developed at some point for communication to be effective.

In regard to the indefinite article, the low rate of suppliance for both groups may be due, amongst other factors, to its absence in the first language. In this case, since both languages are the same in this regard, a comparison is not possible: it would be interesting to see how a comparable

group of speakers of a language which does have the indefinite article fared. (See below for further discussion in relation to data from Spanish speakers).

Syntactic Environments—The Indefinite Article

As Tables 4.13.1 and 4.56 show, for most informants in both gross and percentage terms the definite article is the more commonly produced. There are probably several reasons for this. It is phonetically more salient than the indefinite article; it occurs in many frequently used idioms and proper names and can therefore enter the system in a wide variety of formulaic contexts, and in semantic terms it involves more plusses than minuses—that is, it is less vacuous than its indefinite counterpart. We have seen one consequence of the salience of the definite article above, in its choice as a global noun phrase marker by Canh and Long. If a determiner is to be overgeneralized or used indiscriminately as a nominal marker it will almost certainly be the definite article: in the present corpus there is virtually no evidence of the indefinite article being used in this way. While the acquisition of the definite article involves a process of overapplication and subsequent constraint, the acquisition of the indefinite article seems to involve a more straightforward process of emergence in which it supplants either null markers or the definite article. For these reasons a semantically based analysis of the acquisition of the indefinite article would not be very enlightening.

The procedure that has been followed with the indefinite article has been rather to look at its emergence in terms of syntactic environments. This is a process that has been hinted at in the discussion of the definite article, where it was suggested that it enters the system in formulaic language.

The analysis of the acquisition of the indefinite article is based on hypotheses put forward by Pica. In a study of eighteen Spanish speakers acquiring English under different conditions Pica proposed that the indefinite article "followed a developmental sequence characterized by its accurate suppliance in the following implicationally related environments:

1. Isolated units such as a few, a little, a lot, half an hour, once a month, twice a week.

2. The same isolated units embedded in predicate structures, e.g. have a little (NP), ate a lot (of NP), visits once a month.

3. Noun complements and direct objects in predicate structures such as is a friend, have a son, read a book.

4. Noun objects of prepositions such as in a minute, with an accent [139].

Table 4.62 presents the results of Pica's analytical framework applied to the present corpus. The results obtained are somewhat less regular than Pica's, although the picture would improve somewhat if categories (1) and

(2) were collapsed (as Pica notes, differential rates of suppliance for these two categories can be explained in terms of increasing structural complexification of the speakers' language—which is not a factor of very direct relevance to the production of articles *per se*) [139].

It is difficult to determine why the present results do not replicate Pica's. One possibility is that informants in her study were developmentally more advanced than those in the present study. Another possibility has to do with the language backgrounds of the informants in the two studies. Spanish, unlike Polish or Vietnamese, has an indefinite article. The semantics of the Spanish indefinite article are similar, though by no means identical, to those of the English one (English requires suppliance in contexts where Spanish may not [173]) and this may have led to a higher rate of suppliance in Pica's data. That first language morphological patterns can manifest themselves in a second language is a proposition which has been advanced at several points throughout this study: specific evidence for interference with articles was found by the author in the learner language of Turkish speakers, where the form one (the Turkish number and indefinite article are identical) was preferred to a [92]. (There is, of course, also the evidence relating to the definite article presented above).

Whatever the reasons for the lack of detailed concordance in the two sets of findings, the principal finding—that forms enter the system through formulas—is the same for both studies and adds to the already considerable body of evidence that formulaic material is the seedbed for propositional language: in the present study only two out of twenty informants produce articles in environments (3) or (4) without showing any evidence of isolated units.

One interesting sidelight on this principle is that through a categorial change certain isolated units can actually result in the spread of articles to environments (3) and (4). This is not commented on by Pica, so it is worth outlining.

The isolated unit concerned is a little. The phenomenon occurs in two informants—Ludwiga J. and Mieczyslaw M. In the case of the first informant, Ludwiga J., a little is used as an adverb (Pica's environment 2):

I know this problem a little too I

- lj.1: [239] -

As Table 4.62 shows, such category (2) usages constitute the majority of her article tokens. However, through the use of a little as an adjective Ludwiga J. actually extends her production of article tokens into categories (3) And (4):

I'm no a little leader

- lj.1: [519–20] -

and this standing one woman and have a little piece of bread... we buy and all shop was empty

- lj.1: [339–40] -

he sitting all day in bed... can't work and have poor family in, ah, a little country

- lj.1: [256–7] -

It is difficult to tell whether a little is still monomorphemic in these examples. In any case, this is not really crucial, since one would presume that the categorial and environmental extensions effected would lead sooner or later to a reanalysis of the unit into its constituents of article plus adjective/noun.

That this is not merely fortuitous and idiosyncratic process can be shown from the data provided by Mieczyslaw M. This informant uses a little as an isolated unit:

but, er, a bit, er, ah, um, a litt(le) more in Zgorzelec

- mm.1: [288–9] -

and as an isolated unit in a predicate structure:

and, er, and, er... in television, er, last time in Poland was a litt(le) about Australia

- mm.1: [146–7] -

The process of transition is very clear in Mieczyslaw M.'s case. The recategorization of a little from adverb to adjective may be assisted by the optional movement of the adverb to a position between verb and object. Thus, we have sentences like:

Eh, yes I think, er, I speak German a litt(le), er, a little better as English

- mm.1: [862–3] -

and:

and, er, he speak, er, a little English but not correct

- mm.1: [100] -

Placement of the adverb in this position renders it analyzable as an adjective, as in the token above, or the following one:

and many people speak a litt(le) Russian

- mm.1: [877] -

Then there are several definitely adjectival usages of a little, such as:

Yes, er, in the, er, this... er it was, eh, a lit(tle) town, er, about, er, ten thousand inhibitants

- mm.1: [220–1] -

Yes, and near, or, the lake was, er, a litt(le), um... a litt(le) houses

- mm.1: [298–9] -

These occur in category (4) environments as well:

long time ago but, er, I lived, er... in, er, a little town

- mm.1: [75] -

In Mieczyslaw M.'s case, we have evidence that adjectival a little is definitely analyzed. This is rather neatly illustrated by the following example, in which big is the adjective substituted:

don't know... is wat... not lake, only a litt(le), er ... like, it's, er... a big, er, water

- mm.1: [290–1] -

The whole of the above process is a model example of how a monomorphemic utterance is broken down through a process of repeated use in different contexts, and of the accompanying categorial adjustments that may occur during such a process.

Within the hierarchy of environments defined by Pica there appear to be further hierarchies. Thus, for category (3) environments there are certain verbs which favour the appearance of the indefinite article. Tables 4.63 and 4.64 show these—have, get and be constitute the most congenial environments for the production of the article.

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
the		5	2	6	55	22	6	17	24	34	17	35
a				4	10	3	4	6	30	7	40	32
an									2*		1	

Table 4.52: Distribution of Articles: Polish Informants—Interview 1

4.13.6 Semantics of the Indefinite Article

As mentioned in 4.13.3, the indefinite article is not subject to the semantic vagaries of its definite counterpart—it is not the chosen form for non-standard marking of noun phrases, and its emergence is slow but seems to be relatively orderly. All this could be seen as a consequence of the heavy contextual dependence—in both syntactic and verbal environments—which has been outlined above. With Phuc, the speaker whose use of the indefinite article is probably the most developed of the informants in this study, there are in fact some indications of non-standard semantics for the item. In Phuc's output there is some indication that the indefinite article is not consistently interpreted as an analogue of one. This is evident in the following examples:

he doesn'(t) make a...friends with Vietnamese

- phuc.1: [48] -

Yeah...but he tol(d) me tha(t)...I should have a one job

- phuc.1: [79] -

4.13.7 Summary

To recap, for most informants the rate of article suppliance appears to be quite low. Both the definite and indefinite articles enter the learner's system through proper names, idioms and other formulaic material. In the acquisitional process the definite article is the principal nominal marker and its acquisition is characterized by a process of semantic discrimination and hypothesis fitting that can proceed at very different rates for different learners. The acquisition of the indefinite article seems to be a somewhat more stolid and orderly process, in which null marking or non-standard usages of the definite article are gradually supplanted. There is some evidence that transfer phenomena may affect this area of learner grammar, but further research is required before more definite conclusions can be arrived at.

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
the	9	21	10	43	19	11	39	20	40	29	21	54
a	8		2	7	7	12	3	5	48	6	46	10
an									2			

Table 4.53: Distribution of Articles: Polish Informants—Interview 2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
the			7	15	4	11	20	33	36	258	261	116
a		1	16	13	5	4	21	14	14	39	40	54
an												2

Table 4.54: Distribution of Articles: Vietnamese Informants—Interview 1

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
the	8	16	27	14	28	11	6	36	50	56	25	49
a	12	23	14	6	6	3	18	26	16	27	4	29

Table 4.55: Distribution of Articles: Vietnamese Informants—Interview 2

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
the		2.1	0.8	2.9	11.7	7.7	1.2	3.7	5.6	8.4	2.0	6.0
a(n)				1.9	2.1	1.1	0.8	1.3	7.1	1.7	4.7	5.5

Table 4.56: Percentage of Articles Compared with Nouns: Polish Speakers—1

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
the	5.6	7.7	4.0	14.7	6.9	4.6	9.4	7.7	10.3	8.2	3.2	15.0
a(n)	5.0		0.8	2.4	2.6	5.0	0.7	1.9	12.9	1.7	8.1	2.8

Table 4.57: Percentage of Articles Compared with Nouns: Polish Speakers—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
the			2.1	0.4	2.6	3.8	6.7	11.6	8.1	31.6	20.7	17.8
a(n)		1.4	4.8	4.6	3.2	1.4	7.0	4.9	3.2	4.8	3.2	8.6

Table 4.58: Articles Compared with Nouns: Vietnamese Speakers—1

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
the	10.7	4.3	10.6	6.3	17.6	4.9	2.5	17.1	15.4	20.9	18.8	20.8
a(n)	16.0	6.2	5.5	2.9	3.8	1.3	7.5	12.4	4.9	10.1	3.0	12.3

Table 4.59: Articles Compared with Nouns: Vietnamese Speakers—2

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
Proper N. Formula		5	2	6	48	13	4	12	21	26	10	25
Generic -SR/+HK					2	4	2	2	1	3	2	
+SR/+HK Oblig.					3?	5		3		2	4	4
+SR/-HK Var.						5			4	2	1	6
-SR/-HK Non-Std												

Table 4.60: Functions of the Definite Article: Polish Informants—1

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
Proper N. Formula			4	15	3	3	15	17	26	*	*	85
Generic -SR/+HK			3		1	8	3	8	3	*	*	5
+SR/-HK Var							3	3	4	*	*	4
+SR/+HK Oblig								5	3	*	*	20
-SR/-HK Non-Std										*	*	

Table 4.61: Functions of the Definite Article: Vietnamese Informants—1

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
be				1			3		10	1	3	5
have					2				9	1		
do					1							
get									2			
know									1			
give									1			
speak												2
0									1	2		

Table 4.62: Environments for "a/an" in Noun Complements: Poles

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
Isolated Unit				2	1	3	1		2	3		6
I.U. in Predicate				1	3				2	1	36	16
Noun Comp/ Direct Obj.			1	4			3	4	26	2	3*	7*
Object of Preposition					2			2		1*	1*	3

Table 4.63: Environments for the Definite Article

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
Isolated Unit	1		3		2		3	3	4	7	4	1
I.U. in Predicate			2					2	2	10	6	6
Noun Comp/Direct Obj.			13	8	4	2	14	8	6	20	30	38
Object of Preposition			1	2	1?			1	2	2		13

Table 4.64: Environments for the Definite Article

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
have			4		2		6	4	1	3	10	5
get				5		1		1	1	8		6
be				1		1		1		2	1	10
send							2		1		2	1
look for			3						1	1		
give				1							2	4
take			1								1	
buy					1		1					
work					1	2						
make											1	4
OTHER			2				1	2	1	5	2	7
0			3		1	2			1	2	2	1

Table 4.65: Environments for "a/an" in Noun Complements: Vietnamese

NOTES:

1. In the asterisked cases marking of NPs was indiscriminate and voluminous and occurred in tokens in all categories.

4.14 Quantifiers

4.14.1 Distribution of Quantifiers

Tables 4.66 to 4.14.5 provide counts for the distribution of quantifiers within the two interview groups, provide counts for the occurrence of quantifiers according to *ASLPR* level and language background. Though not quantifiers themselves, words such as everything, which consist of a quantifier bound to an noun phrase, have been included, since in some cases a quantifier may first appear in this kind of bound environment.

As Tables 4.66 to 4.14.5 show, on a distributional basis the first eight ranks of the tables are occupied by the same set of quantifiers, with the exception of Table 4.67, where few displaces lot.

Moreover, the rank orders in all cases are very similar, with the only really noteworthy difference being the higher position of some in the tables based on data from the Vietnamese speakers.

As regards total numbers of occurrences of each item within each interview group, there is also considerable regularity. Thus, for the Polish speakers, the first eight most frequent items are the same for both the first and second interviews; for the first five items the order of frequency is identical. They are many, all, much, some and every, in that order—the same five most frequent items on a per informant basis. For the Vietnamese speakers, the gross counts are not quite so tidy but nevertheless of the first six most frequent items, five are the same. The five are all, every, many, much, and some, once again.

4.14.2 Developmental Trends

At first sight, with the five main quantifiers (and their bound variants), so widely distributed, and most of the others so narrowly distributed and infrequent, it appears that there may not be a great deal more to say about them, certainly in regard to any possible orders of acquisition that might be deduced from the cross-sectional data. There are, however, a number of points raised by a more careful scrutiny of the data, and several indications of possible developmental trends. The following discussion is based largely on careful scrutiny of the data from the first round of interviews. The points that will be made seem to be largely supported by the tabular data from the second round of interviews (that is, Tables 4.67 and 4.14.5).

Distributive and Global Quantifiers

First, it is worth noting that the most widely distributed quantifier is no, or some bound variant thereof. No is what might be termed an absolute quantifier. It expresses an unequivocal proposition. When non-count noun phrases are quantified by no, this proposition applies to them globally. When no quantifies count noun phrases, as in the bound form nobody, it is distributive, in that it applies to every possible member of the quantified set. In this regard, it is similar to all and every, and distinct from quantifiers like some and many, which involve degrees of applicability. Learners with a limited set of quantifiers almost always have as members of this set representatives of what could be described as the semantic poles of all and none, as can be seen from the tables. (See below for comments on early tokens of much and many). In the present data, the negative pole is realized as no, or one of its bound variants, and the positive pole as all or every, or one of its bound variants. Acquisition of these polar quantifiers, then, appears to be a precondition for the acquisition of quantifiers of degree, like some, much, and many.

The Case of 'Every'

As noted, the semantic complement of no is either every or all. One apparent anomaly in the data is the fact that both these two fairly close semantic relatives are so widely distributed and frequent. This kind of duplication runs contrary to the form-function principle. An examination of the examples in which every occurs, however, reveals that it overwhelmingly figures in adverbial phrases of time—every day, every week, and so forth. Amongst the more advanced learners there are some cases of every appearing in other contexts, and one informant, Ludwiga J., does not use it at all in adverbial phrases of time; but these are certainly the exceptions. Thus, there may be a certain degree of formularity in the use of every, even at a quite advanced stage. If this is so it might help to explain another phenomenon, namely that unlike

many other functional items every does not occur more frequently in the output of more advanced or proficient learners. The counts for every remain relatively stable throughout the sample. This is what one might expect for a formulaic item whose production continues to be stimulated by specific triggers rather than governed by principles with a gradually widening ambit. Apropos of this last observation, it is worth noting that all is produced, in most cases, more frequently by the upper-end informants.

The case of every is instructive for another reason. In the first instance, the environment for every, categorially a quantifier, is an adverbial phrase. As evidence to be adduced in regard to other quantifiers, such as some and much will demonstrate, where it is possible, quantifiers tend to emerge from a previously acquired adverbial matrix. This constitutes an important acquisitional principle.

4.14.3 'Statistical' Quantifiers

'Very'

It was mentioned in the previous section that there is evidence for the proposition that quantifiers emerge from an already acquired adverbial element. This is particularly apparent in the case of the first of the "statistical" quantifiers (that is, quantifiers which do not apply to each and every member of the quantified set). The first such quantifier is a non-standard form in its capacity as a quantifier—namely very. (The assertion that very is the first statistical quantifier to be used is based on the evidence of Irena S., who does not produce any of the other forms in this class). Examples of very as a quantifying adverb or quantifier are the following:

Mus(t) very learn

- is.2: [531] -

He very, er, help me

- jr.1: [605] -

I'm very li(ke) my country

- hoa.1: [643] -

I very li(ke)...I like very much, er, Australia

- minh.1: [324–5] -

Tokens of very as an apparently adjectival element can also be found:

car is not problem... is... have not money... is very problem

- bb.1: [779] -

It could be argued that the first set of examples are really no more than adverbs with a quantifying function. However, the early categorial overgeneralization of very does also lead to explicit occurrences of the word as a quantifier:

is very money

- bb.1: [779] -

is very... very much engineer(s)

- bb.1: [1009–10] -

In a previous study, the use of very as a quantifier was also observed [92]. In the last example from Barbara B., the self-correction provides a rather neat vignette of a probable developmental trajectory, in which very is refined into very much, or too much, which in turn, it would seem plausible, is later reduced to much alone (see below for further discussion). Some support for this argument (as well as an example of the persistence of very as a quantifier) can be found in the fact that in **lj.1** very is used as a quantifier, as in the following examples:

I very love children

- lj.1: [589] -

it was very, very supermarket(s) in Warsaw

- lj.1: [333] -

while much does not appear at all: in other words, there is a complementary relationship between the two forms. Despite the example of Ludwiga J., we could generally expect tokens of very as a quantifier to have largely disappeared by about level 1 on the *ASLPR*.

'Many'

Of the other quantifiers with wide distribution, many exhibits an increase in frequency of occurrence towards the upper end of the range. This is presumably explicable simply in terms of the increased fluency or volubility of those speakers higher on the *ASLPR*. A further point worth noting is that in the case of the lower-level Vietnamese informants, Duc, Dung and Minh, where it seems to have emerged prematurely, many occurs not as a quantifier but as the WH-word, how many, in two out of five tokens for Dung, and all tokens for the others. It is only in Hoa, and the informants ranked after her, that many appears consistently as a quantifier. The early appearance of many in a WH-word perhaps provides an indication of one way many might enter a learner's system.

Two of the Vietnamese speakers, Tam and Long, realize many as too many, in all, and nearly all, cases respectively. This lack of a distinction between the quantifier in its base form and in comparison can be more generally observed in examples with much. A possible explanation for the binding of too and much will be offered further on.

'Some'

Another quantifier which seems to exhibit a developmental pattern is some.

Amongst the Polish speakers, it is virtually absent until level 1+ on the *ASLPR* is reached. (The fact that Mieczyslaw M., the most highly rated Polish speaker produces only one token of some in two interviews once again raises the question of how well developmental features and oral proficiency correlate, and should be kept in mind).

Some appears principally as an indefinite quantifier, or plural indefinite article. On occasion, it is used with singular count nouns as well:

> we readet, er, about Australia, er, some books
>
> - aj.1: [269] -

> er... now wan... when we woos [will?] like, er... er, to... to speak about, er... some, er, very big problem
>
> - aj.1: [475–6] -

> you... you know and, er, we have only some book(s)
>
> - lj.1: [474] -

> they need some money
>
> - vinh.1: [567] -

ah, they have, ah... they make some problem(s)

- canh.1: [1866] -

Less frequently, it is used in its other sense, with a specific although unspecified referent (in Huebner's terms, *+Specific Reference, -Assumed Hearer's Knowledge* [85]):

Ah, er... er, some of them, er... er, got job(s)

- aj.1: [713] -

because some people, you know, they don't have education

- vinh.1: [400] -

Where only one of these senses is present in the output of a particular informant it is the former. This is probably a reflection of the developmental sequence in regard to these two senses of some. It is also worth noting that in all but one case speakers who produce some also produce tokens of sometimes, which may very possibly mean that the form sometimes provides the developmental matrix for some. Orthography aside, this is precisely what was observed for every, where the quantifier really existed as a bound element in an adverb, and had established itself outside of this environment in only a few cases. (Although the parallel is not exact, it would be interesting to look at all in relation to always). The final point that should be made about some is that it is both more widespread and more frequently produced amongst the Vietnamese speakers. The reasons for this are not clear: either first language patterns or differential patterns of exposure for the two groups may be an influence here.

'Any'

Any receives some discussion in 4.10 so it will only be dealt with briefly here. Amongst the Polish informants, in the first interview at least, there are no certain tokens of any. As pointed out above, this is possibly a product of language-specific features in the Polish determiner system. Likewise, bound forms of any, such as anything or anywhere are not produced either. Amongst the Vietnamese speakers, any is somewhat more in evidence, especially in those informants who rated over 1+ on the *ASLPR*. This fairly neatly reflects the pattern of occurrence of some, to which any is related. As a form any has two basic functions. In the scope of negation, and in interrogation, it is the suppleted form of some—that is, an indefinite quantifier or determiner. Its other, not unrelated function, is as a referent for any single possible member of a given set ("Think of a number, any number"). Speakers who

use any produce tokens with both functions, in roughly equal numbers. On the analogy of the pattern of acquisition for some, it would be reasonable to expect any to appear first in one of its bound forms, such as anything. However, there is no particular support for this in the data.

'Much'

While much is not an item which exhibits such an evident developmental pattern as some, the pattern whereby a quantifier appears first in an adverb or adverbial phrase, and then emerges from this context occurs at least in a subset of the data—in the output of the Vietnamese speakers. Thus, the first five Vietnamese speakers who use much use it either in an adverbial phrase, in such expressions as the following:

> Drink beer, yeah, drink beer... too much... now no
>
> - dung.1: [127] -

or as a bound element in the WH-word how much (see Table 4.67). Speakers further up the *ASLPR* in the data examined (that is, Tam, Canh, Long, and Phuc) may produce tokens of adverbial much, but they also use much, or some variant, such as too much, as a quantifier.

> Er, yes, ah... since I may helf my friends, you know ... who can'(t) speak much English
>
> - phuc.1: [607] -

The appearance of much as a quantifier, while it may mark a developmental step forward is also the occasion for further possible non-standard features to emerge in the learner's speech. This is because of the idiomatic way much is used by native speakers. Thus, in Standard English much is generally only used, in casual speech anyway, in the scope of negation, or in interrogation, or if preceded by an adverb—that is—while we say I don't have much money or He has too much ambition, or Is there much bread left?, we do not normally say things like They had much fun. Avoidance of the latter structure is normally achieved by using an alternative lexical form such as a lot of, or a great deal of. This idiosyncratic constraint on much has to be learnt by anyone who begins to use the word as a quantifier, and mastery of it could therefore be said to constitute a further developmental stage in the use of much.

A number of informants do produce tokens of a lot (of), which is a precondition to acquisition of the constraint. These are Jan R., Ewa S., Krystyna A., Ludwiga J., and Mieczyslaw M., amongst the Poles, and Tam, Canh, Long and Phuc, amongst the Vietnamese. Of these, Mieczyslaw M.,

Canh, Long and Phuc provide evidence that they are quite sensitive to this constraint. Thus Mieczyslaw M. only uses much post-adverbially or in the scope of negation:

> er, it... it was difficult for me because, er, um, not much, um, people, er, know this language
>
> - mm.1: [76–77] -

> Ah, my daughter, er, like... very much like the zoo
>
> - mm.1: [688] -

The same is true of Phuc:

> Um... don('t) thin(k) too mush... if you thin(k) too mu(ch)... you go feel bore(d) for sure, you know?
>
> - phuc.1: [151–2] -

> Yes... but not much
>
> - phuc.1: [378] -

> he kept talkin(g) and I didn't understan(d) him much
>
> - phuc.1: [582] -

Canh and Long provide examples of much in interrogatives as well as the other permissible structures mentioned above:

> them they very... suffer... ah, I worry very mu(ch)
>
> - canh.1: [1586] -

> Dictator... the people doesn('t) like him mu(ch)
>
> - canh.1: [1173] -

> And even, ah, people live in countryside, they don't know how mu(ch)
> ...h... how... mu(ch) to... er, contact
>
> - phuc.1: [1478–9] -

Oh well, less [?] they talkin(g) very much

- long.1: [443] -

if not overtime, you know, not too much money

- long.1: [356] -

I ask they, ah, repair that('s) how much money

- long.1: [652] -

Of the other informants who use a lot, Ewa S. does not provide evidence of sensitivity to the constraint on much, which she uses as an unadorned quantifier:

kommt vie... much tourist... tourist

- es.1: [427] -

Tam presents a somewhat more interesting case. All his tokens of much are bound up with too:

You know... they fut in too mu(ch) oi(l)

- tam.1: [717] -

In the event, this token and the other five he produces occur in semantically acceptable contexts, where excess actually appears to be involved. Given the fact that he does not produce tokens of much, and that his form of many is an invariant too many, this may be somewhat fortuitous; it is difficult from such evidence as there is to determine whether Tam is producing much only in pre-adverbial contexts, or whether he is simply sticking to a formula. If the latter is the case, however, it may still constitute evidence of sensitivity to the much constraint—the possible semantic non-standardness resulting from invariant use of too much is considerably less striking than the syntactic non-standardness produced by using much in the contexts forbidden by the constraint.

Use of the apparently invariant forms too much and too many is a frequently commented-on feature of learner language. It is possible that use of such forms is conditioned in some way by the much constraint. This could happen in the following way. The learner first encounters or registers much in affirmative sentences, where its lexical shape is always too much (or very much). (On the production side, this is borne out for the Vietnamese speakers, whose first tokens of much are post-adverbial). This initial hypothesis

receives no further checking against structures other than the affirmative sentences where it was first noted, and the result is that the misconstrued lexical item, too much, is used everywhere. The fact that this strategy does not result in drastically ill-formed structures when the item is imported to negatives and questions allows the learner to persist with his or her first hypothesis, which may even receive some confirmation in that post-adverbial much does actually occur, under certain conditions, in such structures.

In the case of the remaining informants who use a lot, there is possible evidence of another kind that they have some sensitivity to the constraint. These informants, Jan R., Krystyna A. and Ludwiga J., simply do not use much at all. As can be seen from Table 4.66, this results in out-of-sequence empty cells on the much row. This is particularly unexpected in the case of Ludwiga J.—that is, without an explanation like the one above.

Infrequently Produced Quantifiers

Finally, certain common quantifiers are notable for their limited distribution. These are each, both and most, which, with the exception of one case of each, seem to be restricted to informants above 1+ on the *ASLPR*, and therefore probably constitute a useful indicator as to oral proficiency, if they happen to be produced. One other quantifier, few, is somewhat more common, but is not found in the output of speakers below level 1 on the *ASLPR*. The narrow distribution and low frequency of each can probably be explained by the fact that the adverbially derived every, a close semantic relation, has wide distribution.

4.14.4 Summary—Quantifiers

To recap briefly, then. Quantifiers, where this is possible, probably emerge from adverbial phrases, or bound adverbial forms. The more distributive quantifiers, such as all and every, and no are relatively more frequently produced by speakers in the lower levels of the *ASLPR*; amongst the less distributive quantifiers, such as many, much and some, various developmental trends can be observed as one progresses up the *ASLPR*. With the article-like quantifiers any and some, language-specific patterns may be reflected, which may lend support to the differential principle of transfer/interference discussed in connection with articles in 4.13

Numbers

4.14.5 Distribution of Numbers

Given that numbers represent a superset of the quantifiers of English, we will finish the discussion of quantifiers with some observations on their production and distribution.

Basically, the tables are all fairly similar, so that the overall frequencies for the four interview groups provide an adequate description of the frequency patterns for numbers for 0+ onwards on the *ASLPR*.

Cardinal Numbers

Amongst the cardinal numbers, one to ten are consistently amongst the twenty most frequent numbers. Of the multiples of ten, hundred and thousand are consistently common, as are twenty and fifty. Zero is generally the form chosen to represent nought.

Ordinal Numbers

Of the ordinal numbers, first is by far the most common. Others seem quite rare.

4.14.6 Summary—Numbers

Numbers are frequent in the output of all informants. Even the lowest ranked informant on the *ASLPR* in the first round of interviews, Van, produces one, two, three and ten.

Clearly, numbers are easy to learn. Their semantics are unambiguous and universal, unlike, say, those of other sets, such as colours. Because of this they can presumably be taught very much as needed. All things being equal, learners would be likely to most need the numbers noted above.

In regard to numbers, one other point is worth making. It is frequently argued that in the area of lexis, there is very little predictability. The patterns described for numbers suggest that this claim may be in need of some qualification.

4.15 Deictics and Demonstratives

4.15.1 Distribution of Demonstrative Pronouns

Tables 4.70 to 4.70 provide figures for the distribution and frequencies of deictics and demonstrative pronouns. These two classes of words are grouped together because for both of them the semantic distinction between proximity and distance, be it spatial, temporal or referential, is marked.

4.15.2 Significant Differences—'This' and 'That'

One notable feature in the pattern of use in these words is that this and here are considerably more widely distributed than that and there.

Deictics and demonstratives share the function of pointing, either literally, across space (That one, over there), or metaphorically, across time (It

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
no	1	3	3		3	1			1	1	3	2
nothing		1	3			3	2	1		1		
none		3										
nil					1							
nobody												1
every		4			2	4	23	1	1	3	3	3
everything				1			1E				5	
everytimes		1										
everywhere						1E						
all	1F		9	5			13	9	4	12	22	14
many		1?			14	10		1	8	11	54	18
much			2	1			11	11	5	20		5
lot					1	2		2			1	7
some			1?		2?	3?			14	5	18	
sometimes		1		1	3	1			2	1	9	1
somebody									1	1		
someone											1	
any				1?								1?
most								1?				
each											5	
few												1

Table 4.66: Distribution of Quantifiers: Polish Informants—Interview 1

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
no	1?	7	1	2		3			1		2	2
nothing		4	6	3	2	5	1			3	2	
every	4		2*	6	2	1		5	1	6	5	
everything			6							4		
everybody		1								6		
everys												2
all	7	10		2	3	3	23	1	5	3	17	4
alls					3							
many		2		1	1	4		8	6	10	68	14
much		6		5	2		25	8	4	10	8	
some		6							5	12	13	1
sometimes		1	4				9		3	3	17	2
somebody									1	1		
somewhere									1			
someone											5	
few				1	1	1			2		1	3
any				3	2			1	1	1		
anyway											1	
lot			1		1					1		1
each									1			1

Table 4.67: Distribution of Quantifiers: Polish Informants—Interview 2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
no			5	3	1	6	5	1	2	2	9	1
nothing				2						6	8	5
nobody										5	5	
every			6	5	3		5		4	5	47	6
everything			4	2				1		2	29	1
everybody			2	1					8	1	11	1
everywhere											5	1
some					1	27	7	29	13	23	53	14
sometimes			2	11	3	5	2	7	9	12	14	10
sometime							1		1	2	1	
somebody								3	2	3	34	
somewhere									1	1	13	
someone							1				13	
all					2		1	5	10	23	16	6
many	1*		2*	5*	2*	1		4	6	21	33	12
much			3*	8*	1*	3*	1*	6	5	13	10	11
any							4			14	13	4
anything						1		1	1	11		1
anywhere									4	4	1	
anytime										1		
anybody											1	
lot									1	4	3	2
plenty				1						3		2
each			1?									3
most										1		1
both										2		
few												9

Table 4.68: Distribution of Quantifiers: Vietnamese Informants—Interview 1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
no		1	2		6	17	1	1	3	1	4	1
nothing				3	1		1	2			2	2
nobody						1			1	2		
some		1	6		1	21	2	13	7	7	5	8
sometimes			7			10	6		4	2	1	10
somebody								4			4	2
somewhere			2									2
sometime						1						
every	1	6	1		1		4	4	4		2	
everything							2			2	3	3
everywhere								1			1	
everybody								1	2			
much		3		6	21	3	1	1	6	3	1	2
all			2		2	2		1	9	9	1	4
many		1	1		1?	5	1	3	1	1		
any			2			2	3		1	2		3
anywhere			2							5		
anything						1				14		
anyway										1		1
anytime									1			
lot		1					2	6	5	9		
few							3			4		8
each							1					
plenty									1			

Table 4.69: Distribution of Quantifiers: Vietnamese Informants—Interview 2

NOTES:

1. Tokens of "many" always occur in the locution "how many" in the cases of of Van, Duc, Dung, Minh, and sometimes in the cases of Tam (one token) and Long (twenty-two).
2. Tokens of "many" are all quantifiers in the cases if Hoa, Vinh, Canh, Phuc and Tam (five tokens), where they all have the form "too many".
3. For "much" the distribution is: Duc—one adverb and one question; Dung—five adverbs and one question; Minh—one adverb; Hoa—three adverbs; Sang—one adverb; Vinh—five adverbs and one quantifier; Tam—four quantifiers; Canh—eight adverbs, three negatives and one question; Long—four adverbs, four quantifiers and two questions; Phuc—seven adverbs and one quantifier. All the tokens produced by Tam have the form "too many".
4. Tokens of "every" occur outside adverbial phrases in the cases of Dung (one token), Tam (one token) and Long (three tokens).
5. Tokens of "some" are *-Specific Referent, -Assumed Hearer's Knowledge* in the cases of Minh, Hoa (apparently most tokens), Sang, Vinh, (some tokens), Tam, Canh (some tokens), Long (some tokens), and Phuc (some tokens).
6. Tokens of "some" are *+Specific Referent, -Assumed Hearer's Knowledge* in the cases of Hoa (a few tokens) and Phuc (some tokens).

was before that) or in discourse (That's the problem). In many languages this global function is spread over various words whose purpose is to indicate the degree of propinquity between the speaker and the thing pointed at. Hence distinctions such as those between this and that in Standard English. The distinctions need not in fact be binary. In earlier dialects of English itself, the paradigm was extended with yon and yonder for more distant referents, and some current dialects exhibit finer degrees of discrimination than normal with compounds like this here and that there. Other languages frequently have a tripartite scheme of deixis; some, like Turkish, have more complicated ones still [177].

However, in early learner language, it appears that while the function of pointing has to be implemented in some way, the expression of a system of oppositions as parameters for the basic function has a lower priority. This accounts for the predominance of this and here—these words can perform the basic pointing function, and refinements can come later. This primacy of the pointing function over the locational scheme would also account for the frequent confusion of deictic pairs like this and that by learners who have acquired both members of the set quite some time before.

The development of schemes of deixis may well be related to the development of referential fields in areas such as pronominalization, in which a kind of here/there distinction has to evolve for the discourse to be able to accommodate references to third persons. This is an area which could be profitably researched in more detail.

It should be noted that when reference is made here to the emergence of a semantic distinction, such as the one underlying deictic pairs, the reference is to the realization of this distinction in the speech of the learner by whatever means are appropriate, and not to the basic cognitive distinction itself. Both Polish and Vietnamese distinguish between here and there, and this and that, so underlying cognitive capacity is not what is at issue. The problem for the learner is to choose the right word (in production), or to isolate it (in reception and processing), while his or her attention and linguistic faculties are simultaneously dealing with other matters. Depending on the developmental stage the learner is at, and the complexity of the overall communicative task, the priority allocated to items like the deictics must vary: if they are to come under conscious scrutiny a good many other processes must be automated and under control. Since this is often not the case, well established semantic distinctions, such as those underlying deixis, can fail—invariably, regularly, or occasionally, as the case may be—to achieve surface realization. While learning Spanish, the researcher himself remembers a distinct tendency to confuse the Spanish demonstratives in unmonitored speech, or speech being monitored for some other feature.

This situation, of course, holds with many other elements and structures as well. One set of distinctions that comes to mind as obviously similar are those of the pronominal system. The important point here is that cog-

nitive mastery is not a sufficient condition for linguistic mastery; for any given cognitively established set it will almost certainly the case that some elements are mastered in speech before others, due to certain overriding features of the learner's linguistic system. This is so, as the case of deixis shows, even for sets with only two elements. Other examples are abundant: amongst adjectives, for example, good is many times more frequent than bad. The underlying principle here is really quite simple—if the distinction between set members is communicatively important then one member can often be manipulated to imply the other, and if not, then a single member may represent the set well enough for the time being.

An Implication for Pedagogy

It is of interest that the implications of this simple principle for pedagogy are quite profound, however. This is because of a widespread tendency to teach members of sets together, and in fact to use set membership as a means of defining individual items—bad as the opposite of good, that as the complement of this, etc. Given that learners will initially try to acquire one member of a pair and disregard the other, insistent presentation of both members is likely to reduce the chances that at least one element will be acquired with some degree of appropriateness.

A rather neat example of this came to the researcher's attention when he was marking an English test given to Spanish-speaking secondary students. The course had apparently involved presentation and drilling of the verbs lie (to tell untruths, to become recumbent) and lay (past tense of lie, to put down). The part of the test which dealt with these items was in a cloze format, and one student produced the memorable answer He was punished because he lay the teacher.

To return to the analysis: this is used by all informants over 0+ on the *ASLPR*, in all four sets of interviews. While the distribution of this is similar for the two groups, the frequencies of occurrence are not. For the Vietnamese speakers, these hardly vary across the whole sample—the count for this does not reach double figures in any interview. For the Polish speakers, on the other hand, the frequency with which this is used tends to increase steadily as we move towards the right hand side of the table. The significance of this will be discussed further on.

4.15.3 'Here' and 'There'

The distribution of here parallels that for this, although (probably for quite arbitrary reasons) it is somewhat more ragged.

There, on the other hand, presents a different picture. Amongst the Polish informants it is raggedly distributed and infrequent. With the exception of **ks.2**, it is not found in the output of informants rated under 1 on the

ASLPR. (That is also used by Krystof S., but not by his neighbours on the scale, which tends to confirm the nexus between that and there, and also suggests that this informant should be shifted to the right—there is other evidence for this). Amongst the Vietnamese informants, the threshold, in *ASLPR* terms, for the appearance of there is about the same as for the Poles. However, for the Vietnamese speakers, once there appears it is both more frequently produced and more regular in its distribution. The reasons for this are not really clear; some possible explanations will be advanced later.

The distribution of the deictic, that, (as opposed to the complementizer) very closely parallels that for there, and tends to confirm the importance of their semantic common denominator. There are, however, a number of additional comments that should be made about the use of that.

In the first place, as a look at the notes to tables 4.70 to 4.72 will show, a large number of the tokens of that produced by the Polish speakers are sentential complementizers and not deictics at all—that is, they appear in structures like: she... said that I can go to Melbourne (**kb.1: [761–2]**). For the Polish speakers who produce significant numbers of tokens of that the ratio of complementizer that to deictic that over the two interviews is: Barbara B.—four to two; Ewa S.—thirteen to two; Andrzej J.—seven to seven; Krystyna B.—seventeen to three; Ludwiga J.—forty-eight to three; and Mieczyslaw M.—two to eight. This very considerably reduces the number of tokens of that for the Polish speakers. Given these depleted counts, the number of that tokens produced as part of the possibly formulaic locution that's becomes a significant proportion of the total output. Although the distribution of that is still more regular than for there, the adjustments to the counts lower the frequencies for that to round about the same level as for there and result in a marked contrast between the two language groups for the patterns of use of these two items.

Before looking in more detail at the ways in which this and that are used by the two groups, there is one further point to make about the Polish speakers. This concerns their use of the German das. Since all of the Polish speakers interviewed had had exposure to German it is possible that all of them may have known das. Its use, however, is restricted to those speakers who either produce very few tokens of deictic that or none at all. Das is a kind of dummy subject pronoun in German and is basically neutral as regards distance from the speaker; it is not therefore the equivalent of that. For the speakers who use it, however, das may act as a transitional form in the acquisition of that.

4.15.4 'This'/'That' and 'Here'/'There'—Summary

A brief summary of the findings might be in order at this point. This and here are more widely distributed than that and there. In *ASLPR* terms at least, acquisition of that and there appears to occur at about the same level

1 for both groups. However, with regard to actual frequencies of occurrence, the situation for the two groups is reversed. The Vietnamese speakers use this only very sparingly, whereas once they acquire that they use it with increasing frequency. The Polish speakers, on the other hand, use this with increasing frequency, but that only sparingly.

4.15.5 Language-Specific Differences

In order to attempt to understand the reasons for these inverted patterns of usage, an analysis of how each token of this and that was used was carried out on the data from the first round of interviews. Tables 4.70 and 4.75 show the results of this analysis.

The tokens were classified according to their categorial status as pronouns or adjectives, their grammatical status as subject or object, and whether the deixis they expressed was spatial, temporal or discourse-oriented. This resulted in practice in some seven different possible classifications.

1. The demonstrative functions as a subject pronoun whose referent is some object or event already present in the discourse:

   this is, er... feerst contact, er, language English

   - jr.1: [16] -

   I work(ed) with American contractor... (be)cause that really bad for me

   - phuc.1: [415–7] -

2. Alternatively, in the absence of a clearly defined previous referent, the demonstrative could be classified as a dummy subject:

   that's very difficult, you know

   - long.1: [405] -

   In many cases, it is difficult to decide whether a token is an example of a discourse subject or a dummy pronoun, so the counts for these two categories should perhaps be considered together.

3. A demonstrative functions in a discourse capacity as an object pronoun:

   I was very happy to do that

   - phuc.1: [339] -

4. Or as an adjective (no note was made of the grammatical status of the noun phrase modified):

I don't know how called this street

- kb.1: [785] -

5. Within a temporal framework, demonstratives function as object pronouns:

but before that... when I was at high school

- phuc.1: [234–5] -

6. Or adjectives:

we were lucky that ti(me)... we get a job

- phuc.1: [18] -

7. Within a spatial framework, only subject pronouns occurred:

[DRAWS] Here this... yet... this

- ka.1: [591] -

Turning first to this, it can be seen that the majority of tokens function as subject pronouns or adjectives (probably modifying subject pronouns) with a discourse-based reference. This is so for both language groups, and is what one would expect from speech data gathered in an interview situation. The Polish speakers simply utilized this to implement these functions a great deal more than the Vietnamese.

In the case of that the Polish speakers repeated their pattern of use for this, except that adjectival usage of that was hardly in evidence. One informant produced quite a number of probable dummy subject tokens of that, all in phrases beginning with that's, which suggests an element of formularity; the phrases, however, had quite a wide range of predicates. Tokens of that as an object pronoun were very rare.

With the Vietnamese speakers, there were a good many tokens of that as either a subject or adjective with discourse reference, or a dummy subject. There were also a good many tokens of that as an object pronoun, in both discourse and temporal reference capacities.

What, then, could explain the reversed patterns of use for demonstratives? The probable answer is influence of the first language.

As already stated, both languages share the binary system of oppositions that characterizes deixis in English. Their choice of which word to use under certain circumstances is not always the same, however. In the data presented in Tables 4.72 and 4.75 the principal reference frame for deictics is within discourse itself, rather than space or time. And it is in this area that Polish and Vietnamese differ as regards choice of deictic pronoun. In Polish, the

word employed is tu, which translates into English as this; in Vietnamese, it is ra, which translates as that.

Thus in the reversed frequencies we appear to have some definite evidence of first language interference or transfer.

This is interesting in itself, but of more interest still is the way first language influence operates in this case. As has already been shown, the actual distribution for the deictics, and therefore very possibly the acquisitional pattern, is the same for both groups. This suggests that deixis follows a developmental pattern. Within the broad outlines of this pattern, however, first language influence is manifested in the frequency with which items actually appear. It shows in other ways too. Polish, for instance, does not use the same words for spatial and temporal deixis, and this is reflected in the relatively low counts for temporal this and that. When new usages are acquired, such as adjectival demonstratives in the case of the Polish speakers, they tend to take the contextually preferred form—thus Polish speakers use this as a demonstrative adjective, but not that.

4.15.6 Conclusions

To summarize the above findings briefly. In terms of sequence, this and here are acquired before that and there, and this needs to be recognized in pedagogical practice, where they tend to presented together and in fact defined against each other. The plural forms these and those are barely in evidence in the present corpus, and are evidently not worth teaching before *ASLPR* level 2.

Furthermore, first language features may affect the rate of suppliance of deictic words in different contexts and as members of different grammatical categories. In this latter regard, the learning of deixis in English provides interesting and representative insights into the learning of sets, and of how a probable developmental feature is affected by the learner's first language, without, however, losing its developmental status.

4.16 Existentials

4.16.1 Existential 'There'

Existential sentences in Standard English are characterized by the structure *there + be*. In addition to be, several other verbs, such as exist, can appear after there. Obviously, there are other ways of denoting existence, but the sentences with there are the paradigm cases.

In the present study, the most notable thing about these paradigm existentials is their very low frequency of occurrence. Multiple tokens of an unambiguous kind are produced by only two informants—Andrzej J. and

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
this	3	5	11	3	15	7	8	1	44	16	79	37
dis	1	4	1		3		11	1	3			
des		2			1							
gis				1?								
zis							2					
here			2		3	18	5	5	7	18	4	19
here's						1				1		
that			1			3		6*	2*	12*	30*	4*
das	1		2			1	11	12				
that's			1			1					15	1
there								1	3*	1		
there's						1						
dort								2				
these									1			

Table 4.70: Distribution of Demonstratives: Polish Informants—1

NOTES:

1. Tokens of "that" are complementizers in the case of es (all tokens), aj (one token), kb (nine tokens), lj (twenty-nine tokens) and mm (two tokens).
2. In the case of aj two tokens of "there" are pleonastic pronouns in existentials.

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
this	13	7	8	10	8	7	31	5	41	9	39	24
dis	6	6		4							26	
here		2			11	2	21	2	6	18	3	5
that		1					6*	9*	12*	8*	18*	4
that's		1	1			1			1		6	
das		1										
dat											3*	
there		4		3?		1			11			1
there's									1			
these									1	1	1	3

Table 4.71: Distribution of Demonstratives: Polish Informants—2

NOTES:

1. Tokens of "that" or "dat" are complementizers in the case of bb (four tokens), es (seven tokens), aj (six tokens), kb (eight tokens), and lj (twenty-one tokens).

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
this: S-PN/Dis	4	10	10	3	17	4	15	1	3	3	11	3
this: ADJ/Dis			1	2?			7		40	12	50	32
this: ADJ/Temp		1	1						3		5	1
this: O-PN/Dis							2				10	1
this: S-PN/Spat						3					1	
this: O-PN/Temp									1		1	
this: Dummy-S							2					
this: ADJ/Spat								1				
that: S-PN/Dis						4				2	1	
that: Dummy-S			2								14?	1
that: O-PN/Dis									1			
that: ADJ/Dis										1		
that: O-PN/Temp												1
these: Adj/Dis									1			
das			2			1	11	12				

Table 4.72: Breakdown by Categories: Polish Informants—Interview 1

KEY:

"S": subject; "O": object; "ADJ": adjective; "Dis": discourse; "Temp": temporal; "Spat": spatial.

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
here			12	5		10	2	12	9	21	13	8
this			7	1	1	1	4		7	4	7	5
there						2	1	15	5*	28	30	16
there's						1						2
dere							1					
that						4		15	5*	38	110	104*
that's								4		23	63	29
thas							2				10	1
thats										6	3	
das							1					
dat												1*

Table 4.73: Distribution of Demonstratives: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
there		1	2	3	4	4	9	7		4		3
dere		2	8						3		2	
that			4					5	1*	19*	11	45*
that's				1		1		1		6	9	19
dat		2	1		2				2*		5	
thats			1					1		1		
thas				1								
das											4	
dats										2		
dat's											5	
here		7	3		4	1	2	1	4	2	3	5
this				2	3	1	2	3	3			9
dis		2	2			6			2	2		

Table 4.74: Distribution of Demonstratives: Vietnamese Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
this: S-PN/Dis			1		1	1?				3		5
this: ADJ/Dis			3				2		3		6	
this: ADJ/Temp			3	1			1		4			
this: O-PN/Dis										1		
this: S-PN/Spat											1	
that: S-PN/Dis						2		4		3	3	32
that: Dummy-S								4?		29?	63?	
that: O-PN/Dis								12	2	18	58	16
that: O-PN/Temp							2	3		5	25	12
that: ADJ/Dis						2	1		1	4		6
that: ADJ/Temp												27
that: ADVERB												1

Table 4.75: Breakdown by Categories: Vietnamese Informants—Interview 1

KEY:

"S": subject; "O": object; "ADJ": adjective; "Dis": discourse; "Temp": temporal; "Spat": spatial.

Vinh. Canh produces a token without any surface realization of be, which should perhaps be included:

> If there... my country no more communist, I will be back
>
> - canh.1: [654] -

A number of other informants (Krystof S., Krystyna A., Hoa, and Phuc) produce phrases with there is or there's. Careful examination of the context of these phrases, however, seems to indicate that they may be cases of locative there plus the copula. In Standard English such sentences can only occur in speech acts where there is explicit pointing, such as, There's John now. In the examples provided by the informants this is not the case. The difficulty of distinguishing between locative there and the dummy pronoun found in existential sentences is illustrated in the following example:

> mornin(g), you know... ah... one frien(d) of mi(ne)... he look t(h)rough the window... an(d) he tell us... [WHISPERS]... 'There's communist(s)!'
>
> - phuc.1: [789–91] -

4.16.2 Alternative Existential Structures

Even allowing for the doubtful tokens, there would be no more than twenty-five tokens of existential there in the whole cross-sectional corpus. This is a very small number.

Clearly, informants have to have other ways of denoting the existence of objects. One way, used on occasion by Vietnamese speakers, and once by a Polish speaker is to use have. This is how existential sentences are formed in Vietnamese, and indeed many other languages—including some English-based creoles. Thus we find sentences like the following:

> I like in Australia because, er, here have freedom
>
> - dung.1: [155] -

> ah... tortoise shell... where in Australia have?
>
> - duc.1: [761] -

> badmington... we have and... er, we play sometimes when, eh ... when... t... and isn't wind... have?... hasn't win(d)?
>
> - es.2: [586–7] -

Such sentences are not so numerous as one might expect, however. Once again, there are barely half a dozen examples in the whole cross-sectional corpus.

Probably the most common method of denoting existence is to use a copula structure—that is, one that is not specifically earmarked as an existential. (This is the procedure adopted in Polish, as it happens). Thus:

> Australia is... mm... for my occupation, is, er... many
> ... many... job(s)... er... many work
>
> - jr.1: [474–5] -

> Hm... is, er, many place(s), er, for work... free place(s)
>
> - ka.1: [1117] -

> in Poland big, er, city is big house(s)
>
> - zj.1: [653] -

Although the last example is not one of them, it is often possible to produce acceptable existential sentences in this way. More primitively, just the noun phrase by itself may get the message across:

> GB Hm... what do you think of the food in the hostel?
> I No good... every day goulash
>
> - zj.1: [768–70] -

Another common method, which generally results in acceptable structures, is to use have with a subject. Thus:

> in Bogatenia we have only... we, er, we had only, eh, one, eh, main cloub... club... er, in this club, er, we... we have library
>
> - mm.1: [301–3] -

4.16.3 Possible Explanations for Infrequent Use of 'There'

It seems curious that learners employ *there + be* so infrequently. One would think that a formula such as there's, which is generally acceptable in plural contexts, would be easy to learn. From the results of the present study, however, and from other investigations this does not appear to be the case [4].

[4]Both Geoff Brindley and myself have performed classroom experiments with parallel results.

One can only speculate as to the possible reasons. The fact that existence can be implied in various ways, such as through copular structures or even standalone noun phrases, may detract from the functional importance of explicit existential structures. In addition to this, the fact that the crucial form, there, is acquired relatively late, by which time some makeshift means of indicating existence must have already developed, may further inhibit the production of a specific existential form. Finally, the case of existential there may represent yet another example of the form-function constraint, since when there is first acquired, it is acquired as a locative pronoun. The functional duplication that results if it is used in existential structures may constitute a disincentive for the extension of the form to these structures.

Whatever the reasons for the low rate of suppliance of existential there may be, it is clear that inculcation of this item is not a valid pedagogical goal for learners at the stages of development represented in the present corpus.

4.17 Personal Pronouns

4.17.1 Limitations

The analysis of personal pronouns which follows is very limited. This is partly due to the fact that there has not been sufficient time for the transcripts to be scanned for obligatory contexts. Thus, for the time being, there is no specific information on inappropriately realized pronouns (where the non-standardness might involve person, number or case) or pronouns which were not realized at all (null tokens). Neither is there any specific information on the functioning of pronouns in discourse. Huebner has demonstrated that a proper analysis of the acquisition of a pronominal system must take discourse features into account [85], and the *ZISA* project has shown that such phenomena as pronoun deletion are systematically related to learner typologies (see 2.1.8).

It is clear, therefore, that considerable research remains to be done on the pronoun data collected. Given that the counts presented in Tables 4.76 to 4.82 and 4.83 are unprocessed, the observations which follow should be taken as suggestions rather than findings.

4.17.2 Distribution of Pronouns

Not forgetting the above caveat, there nevertheless appear to be certain discernible patterns in the pronoun data collected. The following comments will deal with these patterns as manifested in relation to person, gender, case and number.

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
I	7	34	34	51	60	35	141	135	177	104	196	180
I'm	1	1	7	1	2		6	2	7	4	14	4
I've		1						1	3			2
I'd					1			5				1
I's					2							
me	1	2	2	2	6	3	8	1	30	24	32—	13
it	1	4	3	6	4	1	1	15	39	57	97	20
it's			5		1	2	3	4	18	6	16	45
he	3	2	4	4	25	2	1	3	24	7	4	9
he's					1	1		1	3	1	1	
zhe						2						
we	1	2			3	18	1	23	23	40	113	24
you			7	9	12	10	15	3	14		37	1
you know					1						34	
she		1	5	3	3				5		2	21
she's								1	2			2
they	1				1	1		10	3	11	3	6
her		1		1							5	6
myself				1					1	1	3	3
us								3	7	4	10	
him					1				4	2	7	
them								1		1		
self					2						1	
itself								1				
himself									1			

Table 4.76: Subject and Object Pronouns: Polish Informants—1

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
I	26	58	64	87	45	79	122	104	59	154	287	129
I'm	2	1	1		1	3	9		10	5	6	3
I've				3				1	4		2	
I'd				2				1	1			
it		1		10	1	9	4	10	50	29	79	8
it's	4	2	1	9	4	3	87	14	6	30	23	22
he	9	3	5	22	4	11	5	40	18	23	30	4
he's	2			2		2	1	6	1	5	1	
she	2	12	5	6	6	14	2	11	3	31	4	21
she's				1	1	2		2		5		2
we	2	1	8	4	5	9	9	48	36	32	155	12
we'll					1			3				
we're								1				
me		4	2	4	9	4	25	1	7	26	24	12
you	1	1		2	2	2			2	36	23	1
you know		1									52	
they		1		2		2	2	6	2	20	3	22
her		2	1			2	1	1	2	9	2	5
us	1				2	1		2	3	5	14	
him								1	8	2	6	1
them						2	4		2	6		
myself						1			1			
himself						3						
hiself						1		3				
self				3								
yourself											1	

Table 4.77: Subject and Object Pronouns: Polish Informants—2

Informant	is	zj	ks	jb	jr	ka	bb	es	kb	aj	lj	mm
my	9	13	23	21	24	17	51	16	50	29	54	41
mein	2						1					
meiner	1											
his			1	4					1	6	2	1
her		1		1	1						5	6
our				2					1	10	23	
your				3	2					1	7	
their										3		

Table 4.78: Distribution of Possessive Pronouns: Polish Informants—1

Informant	is	ks	zj	jr	ka	jb	bb	es	ja	kb	lj	mm
my	12	12	9	24	11	16	42	11	7	25	35	19
her		2	1			2	1	1	2	9	2	5
our	1	1				1		6	7	2	23	2
his				4	2	4			1	3	2	1
your							1	1		1		
their									1		1	
ours											1	

Table 4.79: Distribution of Possessive Pronouns: Polish Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
I	13	33	106	33	101	111	69	116	145	320	442	214
I'm			1		2	29	2	7	5	19	9	1
I's											1	
you	3	4	24	17	9	52	14	11	46	136	170	133
you know			5	3	1	72	1	62	33	58	433	132
me		3	14	4	2	24	7	8	39	37	77	36
he		1	38	1		18	6	18	88	6	119	27
he's						3		1		1	11	
they				4	1	18	25	78	65	171	310	145
she		1	4	10		24		4	11	2	19	5
she's						1		2				
it			3	2			1	5	3	14	20	11
it's						1		2		2		13
we			1		2	22	2		14	49	145	52
we's											1	
him				3		6		1	11	4	24	6
myself					2		2	1		1	4	4
her				1				1	1	8	7	4
them								7	15	18	11	32
us								11		3		13
themselves										1		
themself												1
herself				2								
self									1			
yourself												5

Table 4.80: Subject and Object Pronouns: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
I	34	92	92	98	92	54	64	72	139	93	54	133
I'm	4	4	7		1	38		5		14	1	4
you		8	1	13	1	14	2		43	36	4	94
you know		34	1		6	28	3	12	51	45	61	84
he		42	17	41	28	51	24	25	43	11	4	3
he's				2		1				6		
me		11	14	12	14	42	5	7	28	14	12	16
they		6	6		1	26	8	29	27	40	21	16
they'll										1		
it		1	2	2	4			3	5	5	1	18
it's		3		3		1		1	2	7	1	7
them		1	1		2		3	6	5	10	1	2
she			1		1	14	14	8	32	11	1	2
him				5	4	11	1	3	14	1	1	
we						6			8	12	27	7
we's									1			
her						3	5		17	6		
us								2		1		
myself											3	4
herself										1		

Table 4.81: Subject and Object Pronouns: Vietnamese Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
my	1	4	21	24	29	62	34	36	43	56	36	36
mys						1						
your	1		1	3	3	5	2			18	4	22
his				1		1		1	1	6	23	2
her				1				1	1	8	7	4
their							2	3		3		16
yours										1		
our												5

Table 4.82: Distribution of Possessive Pronouns: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
my	14	25	23	18	83	35	15	24	23	9	10	5
mys		1										
your	1		1	11	1				1	1		6
his			4	14		1		2	4	2		
her						3	5		17	6		
their			1									
hers										1		
ours										1		

Table 4.83: Distribution of Possessive Pronouns: Vietnamese Informants—2

Person

Due to the particular nature of the discourse situation of the interview, some care has to be exercised in making generalizations about person. It would appear, however, that as regards acquisition, first person forms are the first to emerge. Thus, all informants produce tokens of these forms. The most frequent form is I, followed by my and me. As mentioned in 4.7 there are also certain non-standard first person proforms, of which I'm is the most prevalent. For example:

> becau(se) I'm very li(ke), you know
>
> - hoa.1: [183] -

> before I am mu(s)t to work
>
> - minh.1: [515] -

Person-wise, there is a discrepancy between the Polish speakers and the Vietnamese speakers as regards the next most widely distributed proform after first person forms. For the Vietnamese speakers this is you, a second person form, but for the Polish speakers it is he and/or it, both third person forms. That this discrepancy is not accidental is attested to by almost identical patterns of frequency and distribution for the items in question for both rounds of interviews. At least three factors contribute to this situation.

1. The interview situation is not specifically geared to the elicitation of second person forms, and informants were not obliged to ask questions if they were disinclined to do so. Opportunities for the production of second person pronouns therefore varied. and as a result there are gaps in the row for you in Tables 4.76 and 4.17.2—see for instance the entries for Ewa S., Barbara B., and Krystyna B.

2. It appears that the Vietnamese speakers are more culturally "other oriented" than the Polish speakers. This is evident in their constant use of yeah (compare Vietnamese gia) in dialogue. In Vietnamese culture it is a politeness imperative to register a verbal response to every conversational move made by the interlocutor, especially if the latter has high status [24]. This characteristic almost certainly also results in an increased use of you. For example:

   > Saturday...Sunday...ah...weeken(d) is the weekend ...you know, Sunday
   >
   > - duc.1: [463–4] -

Probably also as a result of the reasons cited above, Vietnamese speakers are given to frequent use of formulaic you know. For this reason a count of these tokens has been included in Tables 4.76 to 4.83.

3. Less crucially, the number of possible tokens of one third person form it is reduced for the Vietnamese speakers by the fact that they have a low rate of suppliance for the copula, which can in turn result in non-suppliance of the dummy pronoun with which it is frequently associated.

In consequence, the figures for the production of you for the Polish informants in comparison with the Vietnamese informants need to be viewed (as do other discourse-based features) in a qualified light. In fact, as regards ability to produce you there is evidence that this task was within the competence of all the Polish informants. (It is in doubt in only two cases). While in the spontaneous speech segment of the interviews Irena S. and Zygmunt J. do not use you, they produced the form without difficulty in the elicited section. Thus, while there are gaps for the production of you in Tables 4.76 and 4.17.2, these can be considered situational rather than linguistic.

Of course, the argument just put forward is a double-edged one: other gaps in the data might be explained away in a similar manner. In the crucial case of third person proforms, however, this cannot be done so readily. Thus, in the course of the interviews reasonably persistent attempts were made to extract third person discourse. Informants like Van, who does not produce third person proforms can almost certainly be assumed not to possess them. (In both interviews Van produces only first person proforms, I and my, and second person proforms, you and your. He was unable to perform the elicited tasks).

An explanation for the production frequencies for you has been offered above: if we adjust the distribution by filling in the gaps for this item for the Polish informants in view of the evidence cited above, the evidence from Van (which is consistent across two interviews) suggests that there is an implicational relationship between the three different person forms, with production of third person proforms entailing production of the other two person forms.

If this conclusion is considered unacceptable, a slightly weaker proposal can be made. This is that the use of any proform will imply that the speaker has at least a binary distinction between *+self* and *-self*. This is attested by the fact that no informant produces proforms in only one person category. In this schemata the actual form of the non-first person pronoun would not be crucial, and could, in fact, be a third person proform.

Whichever proposition is accepted, it is evident that the division of the *-self* category occurs at a relatively early stage. The fact, however, of this division being made and proforms representing all three categories being

produced does not spell the end of the evolution of the person categories. Thus, in performance, it is not uncommon for learners to make errors in person category assignment. These regularly involve the suppliance of a second person pronoun for a third person referent, often with almost instant correction to the right form:

> one is, eh, Louis, er, hyou, er... he come from Malta
>
> - is.1: [145–6] -

> wits me in Australia is my brother wis your... his... er, his... his wife
>
> - jb.1: [298–9] -

In the output of the Vietnamese speakers, there are some cases of first person and second person category confusions, generally with possessive forms:

> my daughter first... er, go to school... /... and, er, two... your two children, um... um, go to school in the public
>
> - minh.1: [357–9] -

> you happy in your, my, your teacher... mm... my teacher [LAUGHS]
>
> - sang.1: [691–2] -

This indicates that a person distinction which may be reasonably well established for one category of pronoun may be less stable in performance for a more marked category, and is a good example of the interdependence of the various semantic frames which make up the pronominal system.

While it must remain only a hypothesis, it would seem from the general pattern of usage described below that other developments in the pronominal system observe the basic person hierarchy and tend to spread from first person environments down the hierarchy. In this sense, the person hierarchy could be said to be the basic pronominal hierarchy.

Gender

Amongst those pronouns marked for gender, namely the third person forms, there are frequent confusions, particularly between the subject forms he and she:

> my husband, he got a... she get job in Merimbula

- hoa.1: [99] -

zhe was... hm... don't... zhe was... he wasn't

- ka.1: [1244] -

Errors for neuter gender referents are less common, but do occur:

I wri(te) my w... wife name... ah, he wrong

- duc.1: [185–6] -

Gender errors are frequently picked up and corrected by the speakers themselves, but are nevertheless very persistent, as the following quote from the fifth interview with Barbara B. indicates:

mother-in-law... it's in Canada... /... yeah... he... she boy!
Idots /... she's a pension, yeah, and she is... I don't know why I make that mistake...
'he' or 'she'?

- bb.5: [1334–1355] -

It is possible that this instability as regards gender marking is partly due to the fact that English provides learners with no possibilities to exercise gender distinctions further up the person hierarchy. Errors with case, where there are opportunities for development of distinctions in the first person appear, as we shall see, to be more systematic.

In the multi-dimensional framework developed by the *ZISA* project, pronouns constitute a variational feature. From evidence like that presented above and given the more "malleable" nature of variational features in general it seems highly plausible that these features are also particularly subject to performance fluctuations. This would be worth investigating further.

Case

As with gender, case is subject to a certain amount of confusion. Case errors, however, appear to be more systematic than those involving gender. Case errors mainly involve the use of object forms where the target would demand subject forms. Some examples are:

only me come here

- tam.1: [139] -

I'm about twenty peoples and me was first

- lj.1: [520] -

While first person forms figure most prominently in this kind of non-standard usage, other persons are not immune:

Her came here, er... the fifteen of December

- dung.1: [136] -

Case errors appear to have more logic to them than gender errors. Thus the use of object forms for subject ones could plausibly be interpreted as a case of overgeneralization, since the object forms occur in Standard English as citation forms and in conjuncts such as John and me arrived late. (It is possibly significant that the token from **lj.1** is preceded by and).

It might be worth observing at this point that in general, non-standard pronominal forms do not seem to occur in learner language where they are distributionally impossible in the target language. Thus, while we might find I'm or me or even my as subject pronouns, we would not expect to find I'm or my as object pronouns. Non-standard proforms are therefore interpretable as a rule as the products of faulty parsing or failure to acquire target constraints.

Number

Errors involving number are less apparent than those involving case and gender. From the present data the main generalization that can be made is that plural forms are less widely distributed and frequent than their singular counterparts, especially in the case of the Polish speakers. To what extent these findings are an artifact of the discourse situation of the interview is not clear.

4.17.3 Possessive Forms

The distributional patterns for possessive forms very closely mirror those that have been observed for subject and object forms, with the same discrepancy for the Polish and Vietnamese speakers in regard to your occurring as was found for you. This strengthens the propositions that were made above in 4.17.2 about interactional style for the two groups.

4.17.4 Reflexive Forms

Reflexive forms are quite rare, especially forms other than myself. They are generally used adverbially rather than reflexively. The following example is typical:

in Austria, er, I learn English, er... my... myself

- jb.1: [21–2] -

4.17.5 Conclusions

While considerable research remains to be done on pronouns, we can probably make the following assumptions.

The first distinction to emerge in the pronominal system is the distinction between *self* and *not self* The latter element of this disjunction is further subdivided to give the basic tripartite system of person for English. Further developments in the pronominal system will tend to spread down the person hierarchy, beginning in first person. This seems is true of the evolution of case distinctions, number distinctions, and the evolution of separate forms, such as possessives and reflexives. Gender distinctions, which perforce must develop only in third person forms are particularly hard to stabilize. Despite underlying semantic hierarchies and regularities, the pronominal system in general seems to be subject to a good deal of performance and learner type variability. It should be remembered that in the target language itself there is also considerable variability. Thus, reduced forms—'er, 'im 'em—and even some deletions—Seems OK—are in fact quite common.

4.18 Prepositions

4.18.1 Occurrence of Prepositions

Tables 4.84 to 4.84 provide figures for the distribution of the prepositions for each of the four interview groups.

4.18.2 Frequency

As regards overall frequency, the rankings are always very similar. For the four main interview groups, the eleven most frequent prepositions are the same. The three most frequent prepositions—in, to and for—are the same for both language groups, and for the various *ASLPR* level subdivisions as well.

4.18.3 Distribution

For distribution, the patterns are also regular. The eleven most widely distributed prepositions—in, to, for, about, with, of, from, at, near, on, and by, are the same as the eleven most frequent prepositions, and are the same for the four sets of interviews. In all four sets of interviews the three most widely distributed items are the same as the three most frequent ones—in, to and for. Amongst the case relations expressed by prepositions in English,

this clearly establishes the primacy of the locative, directional, benefactive and durational categories.

4.18.4 Acquisitional Sets

If we make a cutoff point at the eleventh row in Tables 4.84 to 4.18.4—that is, at the end of the most frequent and regularly distributed items—and examine the columns for individual informants, we find that there are relatively few "gaps" or empty cells in each individual list. Where there are, the out-of-sequence item is frequently near—a preposition with adjectival features—with, or about. This means that with some resequencing of informants each of Tables 4.84 to 4.18.4 could be reorganized to produce a fairly classic implicational pattern.

This would be a thoroughly satisfying result, were it not for the fact that the order of prepositions for each table is not quite the same. This is not really surprising given that production of prepositions is obviously dependent to some degree on the nature of the discourse involved. Prepositions like in, to and for are clearly common enough to emerge as high frequency items in any reasonable stretch of discourse. After these, there appears to be an intermediate group, whose occurrence is more dependent on the nature of the discourse. Rather than treat the members of this group as separate items in any implicational relationship it would seem more sensible to put them together. From the evidence of the present data, some of these—such as near, with and about are less predictable in occurrence than others.

Thus, rather than treat the tables which could be produced by resequencing the informants as displaying implicational relationships between discrete items it might therefore be more productive to specify several implicationally related sets of prepositions. Thus, we could postulate the existence of a high frequency set comprised of in, to and for. Next would come a mid-range set whose members are at, of, on, from, and with. After this there would be a further set made up of near, about, by, and possibly items such as between and under. Under most sets of discourse conditions, then, we could expect representatives of each set to appear, down to wherever a particular informant's cutoff point was. Empty cells in a list would not constitute anomalies, unless a whole set of them occurred; neither would variations in distributional frequencies within set boundaries.

4.18.5 Possibilities for Further Research

At this stage of the research, no attempt has been made to determine the appropriateness in context of the prepositions documented in Tables 4.84 to 4.18.4. Obviously, some percentage of these will be used in ways that are not standard. Furthermore, some prepositions may occur primarily in formulas of one kind or another. Obvious candidates here would be at, which tends to

Informant	is	zj	ks	jb	jr	ka	bb	es	kb	aj	lj	mm
in	7	37	33	34	98	68	78	125	46	97	129	125
to	1	13	5	6	23	3	20	8	25	60	115	37
for		2	8	7	15	10	14	10	50	32	59	32
with	1	2	15	5	3	9	8	11	8	13	20	10
from	1	1	6	2	3	9		5	2	14	5	6
at	2	1?	2	2	10				3	6	8	3
of				3	2	4		8	4	9	18	16
about		2	1					2	16	20	55	8
near		3		1	9			2		11	1	7
on					2	3		3	1	3	5	5
by		1				1			1	8		
under								1	2			
inside									1			1
between											4	1
till				1								
per										2		
outside										1		
without												2
behind												1

Table 4.84: Distribution of Prepositions: Polish Informants—1

occur in fixed phrases like at home and at three o'clock, and of, which occurs in phrases like first of May. These matters could be fruitfully investigated further. It would be particularly interesting to explore the development of the semantics of prepositions and to try to correlate prepositional semantics with other related areas, such as deixis and case relations.

4.18.6 Conclusions

Despite these limitations in the investigation, some very significant regularities can be observed in the use of prepositions. The applicability of these to teaching and assessment should be both clear and relatively straightforward.

4.19 Connectors and Cohesion

4.19.1 Limitations

Tables 4.88, 4.89 to 4.90, 4.91 provide figures for the distribution of connectors. As of yet, no analysis of this area of learner grammar has been carried out. The author has made some observations on the use of connectors and

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
in	24	37	36	32	40	34	72	62	43	50	80	48
to	3	14	4	17	4	13	15	13	44	26	138	10
for		14	9	14	25	18	25	8	35	33	50	25
on	4	8	1	1		2	4	1	7	11	4	7
at	1	1	2	3		11		2	9	3	9	2
with		4	1			10	10	16	15	6	7	17
from	2	10		4		10		2	15	9	8	9
of	2	3				1		7	19	2	17	9
by	1					5		2	2	2	1	2
near			1	4				2	3		6	3
about							20		10	15	20	1
since							1	2				1
between								1	1	4		
inside		1									2	
outside					1						1	
under									1	1		
until											1	1
into	1?						1					
without								3				
above										1		

Table 4.85: Distribution of Prepositions: Polish Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
to	1	5	24	12	19	4	35	43	63	144	148	132
in		11	33	21	24	36	37	34	37	67	76	45
for		3	7	14		6	12	2	15	63	52	48
at		1	3	1	5	1	1	13	21	10	11	19
from			2	1		1	7	1	7	15	19	12
with		1	2		1	2		15	25	22	8	21
on			3	1	4		1	1		32	26	16
of				1			5	4	2	23	19	25
by				2			10	1	2	12	2	10
about							2	4		39	62	12
near			5			10	4				3	
between				1						8		
until								3				3
under											1	1
without										4		
behind										1		
against										1		
above											1	
into												3
inside												2

Table 4.86: Distribution of Prepositions: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
in	12	34	29	10	45	13	12	10	23	11	6	14
to		26	34	12	8	7	13	25	35	53	7	34
for		7	5	4	26	18	7	11	18	20	14	13
on	1?	3	6	4	1	1		2	3	4	1	4
of	1		1?	1	2		1	1	5	10		3
at		1	3			2	11	17	8	2	1	2
from		1	4			2	3		5	8	1	4
with			3	2			3	7	12	3		8
by	1	3	4						4	2		1
about			3	4					2	32	3	11
near		4	2		1	1						
behind				1						1		
between								1		2		
under		1										
without			1									
until								2				
against										1		
since												1
below		1?										

Table 4.87: Distribution of Prepositions: Vietnamese Informants—2

on coherence in learner speech in a previous publication, in which preliminary results from a pilot language learning study were reported. Interested parties are referred to this [92]. A few general observations on the subject of connectors are offered below.

4.19.2 Distribution of Connectors

'And' and 'But'

It is evident from Tables 4.88, 4.89 and 4.90 and 4.91 that the principal overt means of discourse connection in the speech of the learners in the present corpus are and, but and because. From a cross-sectional projection it would appear that and is the first connector to be acquired for both language groups. In a previous study [92] conducted by the author, it appeared to be the case that and emerged first in intra-clausal contexts, such as the following:

beer... not li(ke)... I like tea... tea and goffee

- my.1: [314] -

I and Krystof and, er, Gladys... John, no

- is.1: [477] -

Following its emergence in intra-clausal environments it then began to appear in inter-clausal environments also:

I want to work an(d) I am sick

- my.1: [62] -

watch TV, walking and, ah, visit, eh, my family, and... reading books

- is.1: [390–1] -

This hypothesis has not been checked against the present data so far. If there is such an order of emergence it is evident that the stages of intra-clausal and inter-clausal and are fairly close together. It appears that but emerges at the same time as, or soon after, inter-clausal and. From the outset, usage of but appears to be target-like, with some possible extension of the semantic field to include exceptions which would be expressed by qualifiers like instead:

I wri(te) my name but I wri(te) my w... wife name... ah, he wrong

- duc.1: [185–6] -

It would also be worth investigating whether the emergence of but is tied in with the emergence of phrasal negation, given that tokens seem to occur quite frequently in the scope, or at least vicinity, of negation:

but no goulash

- zj.1: [779] -

no in my country... er, but in Austria

- jb.1: [21] -

4.19.3 'Because'

One of the most notable features of because in early learner language is its use as a sentence-initial answer marker. This phenomenon can be found in speakers from both language groups.

MJ .../... are you looking for a job?

I Uh, becau(se) I fini(sh)... I finish sick now... maybe nex(t) mon(th) I... I look job

- my.1: [184–7] -

MJ Do you go out at all?... or?

I No... no... not yet!

MJ Not yet.

I Becau(se)... I think... er... one wees, seven day(s) I'm very busy

- hoa.1: [290–6] -

GB I see, okay... um, and... why did you come to Australia... and not America or Canada or South Africa or...?

I Mm, because, er... Australia's... big country

- zj.1: [508–14] -

I Eh, why?

MJ Yeah.

I Oh... because, eh... because is... er, because... er... becau... because it was a little... money for... one job

- jb.1: [596–601] -

As the above examples show, in some cases the discourse that results from this strategy is well formed and in other cases it is not. The use of because as a sentence-initial answer marker in this way could be seen in functional terms as a means of assuring the interlocutor that an attempt is being made to answer the question that has been asked in a situation where the speaker is aware that his or her communicative capacities may not be equal to the task. Thus, in addition to its target usage pattern as a causal discourse connector, because becomes a more generalized causality marker, at least until such time as the learner can express causality in a more target-like and coherent way. It would be worth investigating if in fact the use of because as a sentence-initial causality marker precedes its more standard pattern of use as a discourse connector.

4.19.4 'If'

In the present data, if is produced almost exclusively by speakers above level 1 on the *ASLPR*. It seems possible, therefore, that its acquisition represents a developmental stage in the use of connectors. (When is sometimes produced by the Polish speakers as an analogue of if, but there is no clear evidence that this non-standard usage precedes standard if.) Given that on the semantic level the production of if signifies that the learner has developed an operational calculus for dealing with hypothetical situations, and on a syntactic level that he has the ability to process multi-clause syntactic structures, this is a plausible hypothesis. Further research is, however, obviously needed to confirm it.

Some observations regarding the marking and modality of verbs in if clauses are made in 4.2 The gist of these is that only simple hypotheses are expressed in the if clauses produced by learners in the present study: counterfactuals are not in evidence.

4.19.5 Other Connectors

As is evident from Tables 4.88, 4.89 and 4.90 and 4.91 other connectors are quite limited in usage and distribution. This must clearly have quite important consequences for the cohesion of learner speech, even up to a relatively advanced level of communicative proficiency. Despite limitations in the general repetoire of discourse connectors there appear to be notable differences in the cohesive qualities of the outputs of different learners. There is obviously a need for extensive future research on the question of cohesion and on how some learners manage to achieve the level of cohesion that they do with relatively limited resources.

Informant	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
and	29	8	38	10	33	41	111	44	70	79	166	125
but	1	3	14	3	2	2		28	43	18	42	45
because		3		5	14	2	13	3	12	12	43	52
or			4		1	1		3	3	6		9
then						2				5	1	1
dan						4						
if								1	1	4	16	1
than								2			4	

Table 4.88: Distribution of Connectors: Polish Informants—1

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
and	23	30	16	22	27	21	55	49	64	57	153	74
but	10	20	17	3	4	21	45	25	23	36	56	39
because	3	3	14	33	4	1	32	4	14	35	45	42
or		4				1	7	1	4	1	4	1
then			1	1	3				1		1	1
than								8	2	6	1	6
if									12	14	3	

Table 4.89: Distribution of Connectors: Polish Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
and	1	8	32	24	24	25	14	135	69	56	340	157
because		2	12	13	1	33	6	22	10	73	154	35
but			7	8	6	32	13	26	33	25	92	67
or			3	9	1	1		12	8	116	10	16
if			5			4	5	2	19	35	31	41
than				5	1				1			5
then								5		27		3
whether									1			
since												1

Table 4.90: Distribution of Connectors: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
and	10	25	20	5	57	30	10	77	71	27	29	40
because		13	15	6	31	14	12	16	17	31	12	22
but		12	8	2		21	10	28	22	16	11	40
or		1	2		1	5	1	4	7	24	2	12
if			5	2		7		2	22	1	8	31
then		1							1	18		1
than						2				4	3	1

Table 4.91: Distribution of Connectors: Vietnamese Informants—2

4.20 Further Partial Analysis

4.20.1 Comparatives

This section provides some information on another productive area of derivational noun phrase morphology, namely comparatives. From the data provided it is evident that the number of comparative adjectives used up until 1+ on the *ASLPR* is quite restricted, and the number of superlatives even more so.

Throughout the oral proficiency range represented in the present corpus, the lexical comparative, more, is a high frequency item. For the Vietnamese speakers, in fact, it is the most produced comparative at all levels, while for the Polish speakers, it is gradually supplanted by better, which is, of course, a frequent item for the Vietnamese speakers as well. Until level 1+ on the *ASLPR* regular comparatives are quite heavily outweighed by their irregular counterparts, which would lead one to suspect that the rules for productive suffixing may not become apparent for some time.

One notable characteristic in the pattern of usage of both comparatives and superlatives is the predominance of positive comparison over negative comparison in regard to value-laden items. Thus, although we find items like younger and cheaper amongst the first comparatives produced, where judgement is generally involved the comparison is much more likely to be framed in terms such as better or more than worse or less. This situation, as we shall see, is a reflection of general lexical choices.

Another evident discrepancy involves the more frequent use of superlatives by the Polish speakers. This may be due to concrete first language factors, such as a pattern of comparison in Polish which resembles the English one, or to more abstract considerations, such as the generally greater amount of derivational morphology in Polish as compared to Vietnamese.

Clearly, the whole question of derivational morphology in learner language could be productively investigated in much more detail.

4.20.2 Lexis

A great deal of primary lexical data was generated in the course of the present study. This is in the form of word-count dictionaries and lexicons. The lexicons could be made available, at least in a limited edition, as an adjunct to this study.

Basically, a lexicon consists of a dictionary which has been segmented into the following categories:

Since the assignment of words to these categories was done by a process of comparing lists and not by any parsing algorithm, there are some inaccuracies, conflicts and cases of dual membership. Nevertheless, a great deal of explicit lexical information has been made available through the lexicons. At the moment, there are individual lexicons for each interview and also

ARTICLES	DEICTICS	ADJECTIVES
COMPARATIVES	QUANTIFIERS	NUMBERS
NOUNS	PRONOUNS	INDEFINITES
WH_WORDS	STEM_VERBS	-ING_FORMS
REG_PASTS	IRREG_PASTS	COPULA
HAVE	DO_AUX	MODALS
ADVERBS	NEGATORS	PREPOSITIONS
CONNECTORS	FOREIGN_WORDS	FILLERS

lexicons for language background and oral proficiency groups (for instance, a lexicon for the level 1+ to 2 Polish speakers, the level 1 to 2 Vietnamese speakers, all level 1+ to 2 informants, and so on): additional lexicons for any arbitrary group of speakers can be prepared in a matter of minutes, however.

To date, there has been little opportunity to analyze lexis. However, certain features have come to the author's attention. Thus, there are certain regularities in lexical behaviour, such as those referred to in 4.14.5 where numbers are discussed (the finding was essentially that the first ten or so numbers are produced, and, presumably, acquired in roughly their natural order).

Another feature of considerable importance (also discussed elsewhere—for instance, in 4.15) has to do with lexical pairs, such as good/bad.

Here the apparent trend is for the learner to use only one member of the set. Thus, in the output of the informants between 0+ and 1 on the *ASLPR*, for example, good occurs fifty times, while bad is only produced once, and for informants in the 1 to 1+ range, hard is used ten times, while easy is produced only half as frequently. That this should be so is not surprising, given that a complement of a paired adjective can be produced by negation. Nevertheless, the principle is an important one because in pedagogical practice it is generally assumed that pairs should be presented together. The result of this is more often than not confusion: it seems to be the case that learners have limited memory space for lexical processing and have a greater than normal tendency to recall the wrong word if a lexical pair is vying for the one slot. (A good example of this, and one which should have been experienced by many people is the frequent confusion of names for languages in discussions about them—an error from which even native speakers are not immune).

Once again, very little is known of how lexical material is stored and referenced by monolinguals, much less second language learners, and more detailed investigation of these phenomena is obviously necessary. The tendency by learners to acquire and use one member of a two-member set might be adduced as a lexical example of the form-function principle; it would seem

that the preference for one member of a pair is more marked if the words involved are acquired early: hence the discrepancy which was observed for good and bad would not be nearly so great for, say, the pair thick/thin.

Unfortunately, these and many other interesting questions will have to be left for future research.

4.21 An Alternative Approach

What the author now proposes to do is present a brief account of a theory of language acquisition which is much simpler in form, and much more precise in terms of matching its predictions with its findings. This is what is now termed "Processability Theory". As noted in the introduction, the present dissertation is based on work which was conducted before Processability Theory was developed, and, in fact, its author had a hand in its inception, when the Multi-Dimensional Model of the *ZISA* project was applied to English (cf. Johnston 1985 [94]; Pienemann and Johnston 1987 [130]; Pienemann, Johnston and Brindley 1988 [144]). In addition, one of the main themes of this dissertation—namely, lexical rule spreading—now fits into Processability Theory, and the author has some definite proposals to make about concepts like "transfer" and "interference" and their interface with this theory in a much less complicated way than is the case with principles and parameters.

4.22 Processability Theory Applied

4.22.1 Processability Theory Applied to English

As noted at the outset, there is insufficient space to provide more than a glimpse of how Processability Theory organizes the data on which this study is based; nor is it feasible to provide an account of the reasoning which ascribes the numerous linguistic features in the table which follows to their respective stages. In any case this kind of explanation already exists in published form for the most salient of these features (cf. Johnston 1985 [93]; Pienemann, Johnston and Brindley 1988 [144] and Pienemann 1997 [143]).

What follows, then, is an implicational scale demonstrating the result of applying Processability Theory to the data previously ranked on the basis of oral proficiency; the contrasts to be noted provide an interesting contrast to this earlier ranking and demonstrate what results when extraneous factors influence what are supposed to be primarily linguistic considerations. The table itself is largely self-explanatory but a brief gloss of the linguistic features tabulated follows.

NO+X Use of **no** as a simple pre-lexical negator.
SVO Canonical word order (no grammatical relations).

Inf Inf Inf Inf	v a n	i s	d u c	m y	j r	h o a	z j	b b	j b	m i n h	k s	k a	d u n g	v i n h	e s	l o n g	s a n g	t a m	k b	c a n h	l j	m m	a j	p h u c
NO+X	-	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
SVO	?	?	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
"ING"	-	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
IRREG	0	+	+	-	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
PL"S"	-	+	+	?	+	?	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
ADVF	0	0	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
WH_+X	-	-	+	+	+	+	+	+	+	+	+	+	+	0	+	+	+	+	+	+	+	+	+	+
DON+V	?	-	?	-	-	-	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
NEG+V	-	-	-	-	-	-	-	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
RFX1	0	0	0	0	0	0	0	0	0	+	0	0	+	+	+	+	+	?	+	+	+	+	+	+
-ER	0	0	0	0	0	0	0	0	0	0	-	+	+	+	+	+	?	+	?	+	+	+	+	+
REG	-	-	-	-	-	-	-	-	+	+	?	?	-	+	+	-	?	?	+	+	+	+	+	+
PSINV	-	-	-	-	?	-	-	?	-	-	+	+	-	-	+	?	-	-	+	+	+	+	+	+
COMP	-	-	?	-	-	-	-	-	-	-	-	-	?	+	+	+	+	+	+	+	+	+	+	+
A-EN	0	0	0	0	0	0	0	?	0	0	?	?	+	?	+	-	?	+	+	+	+	+	+	+
D-FOR	0	0	0	?	?	?	0	0	0	0	0	0	+	+	+	+	+	+	+	+	+	+	+	+
D-TO	0	0	0	0	0	0	0	0	0	0	0	0	0	+	-	+	+	+	+	+	+	-	-	?
A-ING	0	0	0	0	0	0	0	0	?	0	0	0	?	-	-	+	+	-	-	+	?	+	+	+
3SG-S	0	0	0	0	0	0	0	0	0	0	0	0	0	+	+	+	0	+	0	+	+	+	+	+
AUX-2	0	0	-	-	-	-	-	-	-	-	-	?	-	+	-	+	-	-	+	-	-	-	+	+
ADVLY	0	0	0	0	0	0	0	0	0	0	0	0	0	+	0	+	0	0	0	+	+	+	+	+

Table 4.92: Developmental Stages for English as a Second Language

NOTES:

The "key" to the notation used above is as follows: "+" indicates productive occurrences of the relevant structures, features or morphemes; "0" indicates definite counter-evidence for production (i.e. non-occurrence in obligatory contexts); "-" indicates that there was no evidence of the items in question, nor any contexts or environments in which they could have been expected to appear (simple non-occurrence); "?" indicates that the evidence was unclear (because, for instance, the number of tokens was very small or the question of whether the tokens were productive or formulaic could not be settled one way or the other).

"ING"	Use of "-ing" to mark semantic action words.
IRREG	Emergence of irregular pasts (e.g. "went").
PL "S"	Productive usage of plural "-s".
ADVF	Adverb fronting (e.g. "Yesterday I go home").
WH_+X	WH-word at beginning of canonical order sentence.
DON+V	Use of "don('t)" as a monomorphemic preverbal negator.
NEG+V	Use of "no" as preverbal negator.
RFX1	Use of emphatic reflexives (e.g. "George himself").
-ER	Use of productive comparative forms of the adjective.
REG	Use of regular "-ed" past tense forms.
PSINV	Inversion with inverted element fronted (e.g. Y/N Q).
COMP	Use of complementizers and "that" complements.
A-EN	Use of auxiliaries and past participles.
D-FOR	Use of "for" as a dative/benefactive.
D-TO	Use of "to" in dative constructions.
A-ING	Emergence of auxiliaries and "-ing" participles.
3SG-S	Emergence of third singular "-s".
AUX-2	Use of auxiliary or copula in inverted second position.
ADVLY	Regular use of "-ly" as an adverbial marker.

Comments

If the reader is familiar with any of the already published literature on Processability Theory or its predecessors then the presence of features or constructions like canonical word order, fronting of various elements, the two types of inversion and subject-verb agreement in the above table will be no surprise. Given that there are published explanations (such as Johnston 1985 [93] or Pienemann, Johnston and Brindley 1988 [144]) of why these occur in the order in which they are listed (i.e. because of the development of implicationally rated speech processing prerequisites) no attempt to embark on an explanation will be embarked on here. As for the elements such as adverbial "-ly" or comparatives, the reasoning behind their inclusion is based on the fact that they are *derived* lexical categories and depend on the prior acquisition and establishment of their mother categories. In terms of processing the emergence of derivational morphology appears to be a phrasal phenomenon: what needs to be borne in mind is that, while this may be so, there is also the development of a grammar to be considered and from the latter perspective operations like feature emergence or derivation are a sign of increasing complexification. This phenomenon is perhaps in need of more extensive testing although on the basis of the present corpus it conforms very closely to predictions, with the overall fit of the implicational scale presented above being ninety-five percent. This is a good result.

Inf	is	zj	ks	jb	jr	ka	bb	es	aj	kb	lj	mm
now	1	1	7	5	4	4	12	8	9	10	13	15
after			2	7	1	1	1	1	27	2	16	1
before					6	1	2	2	3		2	2
ago		2		1					6	3	4	3
then						1			5		1	1
yet	1						1		2			
yesterday			1									2
tonight		1										
today							3					
tomorrow												2

Table 4.93: Distribution of Time Adverbs: Polish Informants—1

Informant	is	ks	zj	jr	ka	jb	bb	es	aj	kb	lj	mm
now	3	4	9	2	4	5	28	7	10	15	12	16
today	1			2		1	1		1	1	1	
then			1		3				1		1	1
after		6				16			3		8	3
ago		2			2	2			9	1		1
before	2								4	2	2	
yesterday		1		2					3	1	1	
already	1	3										
still											1	3
tomorrow									1		3	
during									3			
yet											3	

Table 4.94: Distribution of Time Adverbs: Polish Informants—2

Informant	Van	My	Duc	Dung	Minh	Hoa	Sang	Vinh	Tam	Canh	Long	Phuc
before		4	7	14	3	8	2	5	21	14	63	4
after		2	1	5		2	5	5	28	5	52	14
now		5	8	12		2	4	4	20	24	33	14
ago				4	1		2		4	5	2	1
still								1	4	7	20	6
then								5		27		2
already										6	3	2
tomorrow		1									2	
yet					1	1						
today					1					4		
tonight										1		
while										3		
overnight											1	
soon												3

Table 4.95: Distribution of Time Adverbs: Vietnamese Informants—1

Informant	Van	Duc	Sang	Minh	My	Hoa	Dung	Vinh	Tam	Canh	Long	Phuc
now		2	1	1	17	9	9	4	13	8	3	5
after	2		3		6	1	15	2	12	5	1	9
before		9		3	13	9	11	5	10	6	4	5
ago			3	2		1	14		1	2	2	
still			1					2	4	2	5	1
yet			2	2			1		7	4		
today			1					1		2	1	2
then		1							1	18		1
already				3						2	1	
tonight					2				1			
tomorrow										1		2
yesterday												1

Table 4.96: Distribution of Time Adverbs: Vietnamese Informants—2

Chapter 5

Concluding Remarks

5.1 Some Words in Summary

Besides the specific conclusions arrived at in this study there are a number of general conclusions to be drawn.

First, regarding oral proficiency scales, it would appear that while there is some correlation between syntactic and morphological development and oral proficiency it is by no means a directly applicable one. One reason for this is supplied by Processability Theory and its precursors. This is that oral proficiency scales represent language development as a *linear* concept, while, in the framework of Processability Theory there are two dimensions to be considered in interlanguage development—the developmental and the variational—and the learning trajectory is therefore not linear.

This point is dealt with at length in Pienemann and Johnston (1987 [130]), where the basic conclusion is that, for a given stage of development, it is the *neutralizing* (i.e. neither the highly standardizing nor the highly simplifying) learner who will be awarded the highest oral proficiency rating—a conclusion drawn on the basis of an analysis of the data used in this study. Hence the correlation between syntax, morphology and oral proficiency cannot be a direct one, given that a linear phenomenon and a two-dimensional one are not mappable in any predictable way from the point of view of the linear phenomenon. More could be said about this point, but since it is dealt with elsewhere it will not be pursued here.

Another consideration regarding oral proficiency scales is the extreme difficulty of conceptualizing, formalizing and measuring what would appear to be an essentially intuitive phenomenon. The preceding statement is not meant as a categorical dismissal of such scales. Intuitive appraisals of various types of ability or the difficulty of a multitude of tasks are a common aspect of human behaviour. To provide an example, the Australian rock-climbing scale is a thirty-two point one, and even when climbs differ quite radically in nature of difficulty they can be adjudged to be the same grade. Indeed,

there may be a lesson to be learnt here, given that no one has ever written a single word about the parameters of this particular grading scale. This lesson has already been prefigured above: this is that, at this stage of knowledge of human measurement, we are simply not in a position to derive a set of objective concepts from what is a complex and subjective phenomenon. The philosophy of science provides ample demonstrations of the adage that if you do not know what you are looking for you will not find it and that if you try through some such method as trait or factor analysis any findings that may emerge are likely to be either artifacts of the investigative formalism itself or blindingly obvious in the first place. [1]

In the light of the above, let us construct a five-point oral proficiency scale. First, most language professionals will arrive at a fairly respectable degree of consensus about who is a beginner, who is at some intermediate stage, and who is advanced in their ability to speak a particular language; this provides us with three points of orientation. From this, there is little difficulty or likelihood of serious dissent about who is somewhere between beginner and intermediate and who is somewhere between intermediate and advanced. So now we have our five-point scale without ever having had to take recourse to first principles: experienced intuition suffices. Nor is there anything amiss about making such judgements; indeed, they are an inevitable aspect of our behaviour and serve a quite valid purpose as they stand. The difficulty arrives when attempts to formalize and measure this behaviour begin. Once again, this point is dealt with in much more detail in Pienemann and Johnston (1987 [130]) and will not be further elaborated here. For the purposes of this dissertation all that needs to be said is that the analysis in Chapter Four clearly demonstrates that while there is some relationship between various kinds of syntactic and morphological development and oral proficiency (as one would expect), it is not possible to formulate any kind of yardstick in such terms which would, say, correlate preposition production with use of modals. This is basically the case because oral proficiency is a higher order phenomenon than the atomistic lowest common denominator that one would need, and which cannot be found in a linear approach to language development. Information about syntax and morphology may help in judgements about oral proficiency, but not in a form that is regular or predictable enough to be used as an objective measure employing the methods of Chapter Four as they were before the application of Processability Theory.

It can also be claimed, I believe, that the application of Processability Theory to the same data provides more satisfactory results. This theory does provide objective criteria—as well as an explanation of how they were arrived at—by means of which clearly demarcated stages of development can

[1] The ZISA study, using factor analysis, found that contact was an important variable in language development!

be identified in a given learner. Moreover, these stages can be determined on the basis of concrete *observations*, as opposed to subjective judgements. The only real point at which the observer is obliged to make a kind of judgement in Processability Theory is the point where it has to be determined whether or not a given utterance meets an emergence criterion, and the use of a mechanism for dealing with lexical spread in syntactic and morphological development—as presented in this dissertation—takes practically all of the uncertainty out of this, given that implicational scales are a good predictor of whether a rule is a formula or is operating in predictable productive environments. [2] (This is not meant to imply that Processability Theory does not already provide a careful outline of how to determine emergence criteria; in addition, it turns out to be the case that in practice it is not particularly difficult to determine whether something is a formula or not and simple guidelines for identifying formulae can be found in Pienemann, Johnston and Brindley 1988 [144]).

It has been stated more than once that this dissertation does not constitute an excursion into the area of language testing. For this reason the conclusions arrived at above will not be pursued further; as they stand they provide ample material for such things as studies in language testing, syllabus and curriculum design and are beyond the scope and length of the present work. Indeed, such studies could easily take on the proportions of another dissertation.

The author's purpose in this work, then, has been to examine the relationship between what could be characterized as two frameworks with an almost polar relationship (namely, oral proficiency ratings and Processability Theory) and to provide an insight into what appears to be the most promising direction for work on language development at the syntactic and morphological level. He trusts that in the course of this enterprise that the information provided and the judgements based on that information will both add to the knowledge we have in this area and stimulate further research in any area of linguistic research able to utilize this knowledge.

[2] A formula is just a rule operating in one environment.

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