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Development and Variation in Learner Language

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6 August 1997

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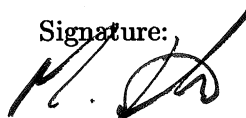
ABSTRACT

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.

Signature:

A handwritten signature in black ink, appearing to be 'M. Pienemann', written over a horizontal line.

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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.

¹References 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 cognitive 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 uncontentionally 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 metapedagogical 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 /i/: 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 contrastive 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 be 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

¹How'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

²Bickerton 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 $362,880 \cdot 9!$ 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 be 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* → *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 implicationaly ordered, in the order given (i.e. speakers who have *VERB* → *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 non-standard 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 repertoire, 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 Givón’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 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

³They 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

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

⁵Given 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

⁶This, 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 uninteresting⁷, 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”

⁷cf. 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]”

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

⁹The 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 $\bar{X}$ 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 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

¹⁰Extraction 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

¹¹The 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 parameters¹² 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'* and *NP*.

¹²These 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 activatable 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 judgments 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

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

¹⁴This 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),

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

¹⁶This 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 Portuguese 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* → *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 implicationaly ordered, just as they are given above (i.e. speakers who have *VERB* → *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* → *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 Håkansson, in progress), and Japanese (Huter, in

¹⁷Processability 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 inaccessibility 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 Inuktitut (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 Inuktitut 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 Inuktitut 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 Inuktitut 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 Adjèman (1976 [1]). Adjèman believes that “permeability” is a characteristic of interlanguage systems which distinguish them from their mature counterparts.

Briefly, what Adjèman 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: Adjèman 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 (Piene-mann, 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.

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

In respect of this last point, Licerias 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]). Licerias 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 (Licerias, 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 Licerias' thoroughness and directness. However, there is one point with which I take issue. This concerns what Licerias 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 Licerias 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"), Licerias 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 Licerias discusses the Contrastive Analysis Hypothesis in terms of strategies as well (Licerias 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 Licerias, who makes the point (Licerias 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.'

Licerias further observes (Licerias, 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 (Licerias, 1986 [115, pp. 10-11]) Licerias 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.

Licerias 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 conocí ayer se llama Pedro.

since, the sentence:

*El hombre conocí 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.

¹⁹Production 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, Licerias (Licerias, 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 Licerias is not the only researcher in this situation [102]. (In addition, since the term "permeability", as employed by Licerias, 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, Licerias (Licerias 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 Licerias 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, Licerias (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

[<i>comp</i>]	[<i>e</i>]
[<i>PP</i>]	[<i>NP</i>]

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].