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At sea in SLA: Evidence of UG in the acquisition of French and English verbs

Sheppard, Ken, Ph.D.

City University of New York, 1991

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AT SEA IN SLA:

EVIDENCE OF UG IN THE ACQUISITION

OF FRENCH AND ENGLISH VERBS

by

Ken Sheppard

A dissertation submitted to the Graduate Faculty in
Linguistics in partial fulfillment of the requirements
for the degree of Doctor of Philosophy,
The City University of New York.

1991

@ 1991

KEN SHEPPARD

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This manuscript has been read and accepted for the Graduate Faculty in Linguistics in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

5/8/91 _____

Date Chair of Examining Committee

May 7, 1991 _____

Date Executive Officer

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Supervisory Committee

The City University of New York

Abstract

AT SEA IN SLA:

EVIDENCE OF UG IN THE ACQUISITION

OF FRENCH AND ENGLISH VERBS

by

Ken Sheppard

Advisers: Professors Martin Chodorow, Richard Kayne
and Herbert Seliger (chair)

This study compares the acquisitional states of two groups of foreign language learners (a Francophone group learning English and an Anglophone group learning French) with a view to establishing the influence of Universal Grammar (see, for example, Chomsky 1981) in the process. Specifically, the two groups (48 each) were asked to render acceptability judgments about 50 sentences in each language; the sentences exemplified syntactic properties [the permissability or impermissibility of fully tensed clausal complements, Exceptional Case Marking and Control structures (PRO)] associated with three verbs in the two languages (believe/croire, promise/promettre and want/vouloir). Four overlapping hypotheses (predictions based on markedness differences between the two languages, the possibility of an underlying similarity in assumptions about the grammar of the L2, a putative preference of the French parameter setting and an alleged predisposition for L1 transfer) were addressed by means of several analyses of the data (chiefly ANOVA). The results indicate some support for the second of these hypotheses and ambiguous support for the other three. Reformulation of the study around the issues of transfer and success suggests that, while Universal Grammar may play some role in learner judgments of the L2, there is little reason to see it as the dominant factor in the generation of L2 grammars.

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Finally, I am grateful to Wolfgang Amadeus Mozart, without whose inspiration this dissertation would never have achieved even its modest level of perspicuity.

Dedication

This dissertation is dedicated to the memory of

Jeffrey Vogel

(1955 - 1985)

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Introduction

There is growing interest in the question of the extent to which an adolescent or adult foreign-language learner is able to reactivate Universal Grammar in generating the foreign language. This study looks at two types of learners: Anglophone adults who have learned French and Francophone adults who have learned English. Its purpose is to estimate the extent to which they are making common underlying assumptions about their second - or foreign - languages, specifically, assumptions about what constitutes an acceptable sentence. More precisely, the study focuses on three pairs of verbs and examines the assumptions that these learners make about their associated properties. Insofar as these assumptions are the same, of course, they provide evidence of the role of Universal Grammar; insofar as they are different, they lend support to the notion that the learner is dependent on an alternative source - or alternative sources - of information about the language. Since one likely source is the learner's native language, the study also traces the influence of that language on these assumptions. In short, the topic is the acquisition of a foreign language: what constitutes knowledge of that language and how it is derived by young adult learners.

CHAPTER ONE

The Issue

1.1 One implication of the Introduction is that languages can be characterized with respect to Universal Grammar; that is, that a speaker of a language will operate with assumptions about the grammar of the language which have not been formally acquired but stem from an innate predisposition. These assumptions are, presumably, reflected in his judgments about isolated sentences in that language. Such assumptions - or, more precisely, this knowledge of the principles governing the grammar of that language - constitute the speaker's basic competence in the language. Furthermore, it is generally assumed that these principles, though universal, vary within limits across languages: while all languages embody common properties, these properties are measurably different from language to language. That is to say, these principles have been parameterized, much the way switches attached to a single circuitry can be switched on or switched off[1]. Thus, for example, all languages contain nouns and verbs, so noun-ness and verb-ness are defining properties of all languages; on the other hand, some languages put the verb before the noun whereas other languages put it after. Although all languages string words together in some order, languages can be differentiated with reference to the order that is canonically employed. Universal Grammar is the name commonly applied to what they all have in common; variation in parameter settings provides a basis for distinguishing them. As Chomsky has written,

> UG consists of various subsystems of principles [cf. Greenberg]; it has the modular structure that we regularly discover in investigation of cognitive systems. Many of these principles are associated with parameters that must be fixed by experience. The parameters must have the property that they can be fixed by quite simple evidence, because this is what is available to the child...
>
> (1986a: 146)

---

[1] In an early version of this conception, Chomsky says that, as the open parameters are fixed by experience, "a grammar is determined, what we may call a 'core grammar'" (1981c: 39); see below.

While it is generally accepted that Universal Grammar (hereafter UG) plays a role in the acquisition of any language - indeed, it does so by definition - the critical factor in second-or foreign-language studies is the extent to which the languages are differentially parameterized. Thus, if one wants to get a fix on the extent to which a speaker of languages A/B, in making judgments about language B, is resetting his parameters to neutral or, alternatively, persisting in the setting associated with language A, one must first locate a linguistic domain which exists comparably in both languages but exemplifies some difference with respect to these settings. That is, one must be able to measure the extent to which the learner is mapping the grammar of language B onto the grammar of language A, if he is doing any such thing at all.

Furthermore, it is generally assumed in the literature (see Chomsky 1981a) that some settings are more marked than others: whereas some languages require only modest readjustment with exposure to input, others require a great deal. So languages can be distinguished not only with reference to the fact that each requires a different parameter setting, within universal limits, but also with reference to the relative complexity of these settings. The assumption in second language acquisition studies generally is of course that more marked settings - e.g. those that contain more exceptions to the rule - are harder to acquire than less marked settings. It is further assumed that UG provides an unmarked, default option (see below) which makes it more attractive than complex, language-specific alternatives. In any event, no cross-linguistic study, and certainly no foreign-language acquisition study, can confidently ignore these levels of difference if the study expects to meet the criterion of explanatory adequacy.

1.2 An example is the so-called believe-type verbs. Consider:

(1) I believe (that) John is late

(2) Je crois que Jean est en retard

(3) I believe John to be late.

(4) *Je crois Jean être en retard

As these examples show, although both languages permit a tensed clausal complement following believe, only English permits a NP + infinitive structure in this context. Accounts of these facts vary. According to Chomsky (1981a), (3) above at D-structure would resemble the following.

(5) I [VP believe [S' [S John to be late]]]

Then, through a process of S'-deletion, this structure reduces to the following at the level of S-structure.

(6) I [VP believe [S John to be late]]

Certain verbs in English are marked for S'-deletion, significantly the believe-type, making it possible for the succeeding NP to receive Case without violating the theta-criterion or the projection principle. These verbs are instances of Exceptional Case Marking[2].

An alternative analysis (Kayne 1984) suggests, by contrast, that there is no such thing as Exceptional Case Marking in English; rather, these differences between English and French flow from a difference in the way that prepositions govern in the two languages. Specifically, Kayne makes a distinction between "structural" and "inherent" Case, in part to account for the fact that Case is not always overt in English. Whereas English prepositions govern and assign Case in the same structural way that verbs do, French prepositions do so only inherently, i.e. at the level of subcategorization. Thus the matrix V governs an infinitival subject position in the subordinate clause in English but not in French. Kayne circumvents the requirement that government is never exerted across two S-type boundaries by positing a 0 complementizer for believe-type verbs, with the result that, at the D-structure level, a sentence like (3) would resemble:

(7) I [VP believe [S' 0 [S John to be late]]]

---

2 "ECM is an idiosyncratic property of the Verb believe and several others like it in English; the corresponding verbs of French, Italian, and German, for example, do not have this property..." (Chomsky 1981c: 61)

This prepositional complementizer, 0, would assign objective Case to John, thereby covering the odd facts of English. The comparable French sentence, however, would be ruled out because, in French, prepositional complementizers do not govern adjacent subjects as they do in English and no Case could be assigned; this, of course, would violate the Case filter.

1.2.1 In sum, while the exceptionality of believe-type verbs and relations across sentence boundaries have been consistent themes of Chomsky's analysis, Kayne has sought to integrate believe-type phenomena and the structure of French syntax. So, for example, following Vergnaud (1974), Chomsky posits an [+F] marker as a way of isolating this class of verbs:

> (68) b. NP is objective when governed by V
>
> In English...the verb believe with infinitival complement will be marked [+F], so that Case will be assigned to the embedded subject NP in (79) by (68b):
>
> (79) I believe [S' [S NP to be a fool]]
>
> (1980: 25, 28)

By comparison, Kayne suggests that:

> ...John in believe [0 [John to be happy]] receives its Case from 0, and not directly from believe. Now there is no reason why French should not have a prepositional 0 complementizer with the same class of verbs: croire [0 [Jean être heureux]]. However, since French prepositional complementizers do not govern the adjacent subject position, 0 will not govern Jean, with the result that it will receive no Case and will violate the Case filter.
>
> (1983: 113)

Chomsky's counter-proposal (in response to Kayne's initial study) suggests that:

> This virtually amounts to accepting the S'-deletion hypothesis for Exceptional Case-marking and raises some doubts as to whether an analysis assuming a 0-complementizer is really the correct one. Kayne's approach to unifying preposition stranding and Exceptional Case-marking is, however, sufficiently attractive so that an attempt to resolve the remaining difficulties surely seems in order.
>
> (1981: 298)

However, he persists in the view that believe-type verbs are exceptional even as he attempts to accommodate these phenomena within a post-GB framework:

> (i) I believe [a John to be the winner] (262)

(ii) * I believe [a PRO to be the winner]

(iii) * I tried [a John to be the winner]

(iv) I tried [a PRO to be the winner]

These results follow if we assume that try selects C and believe selects S, so that a = S in (i) and (ii) and a = C in (iii) and (iv). In (iii) and (iv), then, the complement of try is:

[C [COMP e] [S NP to be the winner]] (263)

The verbs believe and try govern their complements, S and C, respectively. Furthermore, believe governs the subject of S, John (the specifier of S), but try governs only C and its head, COMP. Thus, believe is able to Case-mark John in (262i), but try is unable to Case-mark John in (262iii), which therefore violates the Case filter.

(1986a: 189)

Finally, Chomsky comes full-circle with the publication of Barriers (1985):

Consider...Exceptional Case-marking constructions such as (198 a-b):

(198)

a. John expects [[stories about Bill] to be interesting]

b. John considers [[stories about Bill] interesting]

Here both brackets are maximal projections but neither is a barrier, on our assumptions. The question arises, then, whether government is possible into the embedded subject NP, as it would be if this were an ordinary object: for example, can the head and specifier of the embedded subject NP be governed from the outside? ...the answer would seem to be that the head can be governed from the outside...

(1986b: 85)

This somewhat tentative answer suggests that nothing in the general theory precludes the exceptionality of believe and the other members of this class in Chomsky's view.

Alternative analyses have been provided by Manzini (1983) and Culicover and Wilkins (1986), both of which make some use of Williams (1980). Since neither study bears directly on the issue addressed in this dissertation, they will not be dwelt on here. Suffice it to say, Manzini attempts a synthesis of control theory and binding theory, with the result that the distribution of PROs depends partially on Case properties and PROs are seen as "pure

anaphors" (421). In essence, she argues for the retention of her (optional) notion of governing category (cf. Chomsky 1981a) as the only way to account for the anaphoric relations in a sentence like the following.

(8) The boys believed [them (to be) smart]

Culicover and Wilkins make an effort to eliminate PRO (and the projection principle) altogether, preferring to relativize the theta-criterion to locality conditions. In their theory, following Williams, an antecedent is assigned to every predicate by coindexing. So, for example, in a sentence like the following believe is sub-categorized for a propositional complement and the VP is coindexed with Bill.

(9) John believed Billi [VP to be the winner]i

This has the effect of making believe regular rather than exceptional, though not without a trade-off. As Culicover and Wilkins say, in a comparison of their theory and Chomsky's (1981a), "both theories involve a certain cost. To sustain this reduction [i.e. in the categorial component], the (inviolable) PrP framework must include a system of exceptional case-marking, with (sometimes obligatory) S' deletion. The no-PRO theory must permit fairly detailed lexical entries" (149). They also add, in a footnote, that "the proper treatment of believe-type constructions in French and Spanish requires a full analysis of control in clitic languages" (149), which is not provided in this study.

Regardless of which analysis is finally accepted (and, presumably, a synthesis will ultimately be achieved), believe-type verbs clearly occupy a different status in English and French. Whether this difference derives from a difference in the classification of verbs, as in the ECM case, or from a difference in prepositional government, anyone acquiring both languages would have to set his parameters differentially. Eventually, he would develop the intuition either that S'-deletion could take place where believe-type verbs are concerned, with all the consequent adjustments in Case assignment, or that Case assignment itself was fundamentally different in the two languages - in short, that NP-to-VP structures, with the

NP receiving objective Case, are acceptable in one language but not in the other.

1.2.2 Regardless of which principle is operative (and clearly Kayne's analysis has the virtue of being less ad hoc), there can be little doubt that the licensing of NP-to-VP structures is exceptional in the languages of the world. Van Riemsdijk and Williams report that even Dutch does not conform to the pattern in English and that Swahili and Vata "appear to have no verbs of the exceptional type" (1986: 166). English certainly stands in marked contrast to Romance, and Chomsky declares unequivocally that "the subject of an infinitival is PRO in the unmarked case" (1981a: 68). In sum, believe-type verbs, in English and French, appear to constitute a linguistic domain that meets the criteria outlined above: while believe/croire structures exist in both languages, their syntax is sufficiently differentiated to require some readjustment in the setting of parameters. Furthermore, if we accept the pattern in French to be comparatively unmarked, then the acquisition of English by French speakers would seem to entail a more complex readjustment than the acquisition of French by English speakers. This is not meant to imply that ECM is already enshrined in the literature as a "parameter" - with English having a [+ ECM] setting and other languages a [- ECM] setting. Rather, it is meant to suggest that there are grounds for assuming that ECM departs from syntactic assumptions across languages and that its acquisition, therefore, may entail a parameterized departure from these universal assumptions.

1.3 One problem with this line of inquiry is that Case is rarely overt (or phonetically or lexically realized) in English: thus, it may be difficult for a native speaker to demonstrate that he knows John in (3) receives (abstract) objective Case, whether in fact it does so as a consequence of ECM or subcategorization. If, however, one also wants to understand the extent to which these theoretical differences possess psychological reality, i.e. are actualized in an informant's intuitions about the languages he knows, then of course such inquiry becomes even

more problematical[3]. If one looks at some of the effects of these differences and related distinctions involving other verbs taking infinitival complements, at least some of these levels of complexity become available for analysis. The category of effects includes (a) control structures, (b) Wh Movement and (c) V NP AP structures; other verbs include the want-type and promise-type[4].

1.3.1 Whereas croire is a control verb, believe is not, as the following examples, drawn in part from Rouveret and Vergnaud (1980), show.

(10) Jean croit aimer la musique

(11) Jean [VP croit [S' [S PRO aimer la musique]]]

(12) *John believes to like music

Thus, while most infinitives in French subordinate clauses following croire require PRO subjects[5], all such subjects in English must be specified, i.e. phonetically or lexically realized, following believe. Of course, both verbs, believe and croire, take clausal complements unproblematically, but that need not concern us for the moment. Table 1.1 makes all of these distinctions clear.

TABLE 1.1. believe/croire and Their Complements

	believe	croire
Infinitival complements		
PRO	-	+
ECM	+	-
Clausal complements	+	+

---

3 "Psychological reality" is, in Chomsky's view, "a misleading and much abused term" (1981c: 39); it is used here as a mere convenience rather than a precisely defined term of art.

4 See also Chomsky 1981a (67) for a discusssion of believe and fully tensed clausal complements.

5 Exceptions include (14) below and other structures that result from Wh Movement such as le garçon que je croyais être arrivé (Kayne 1984: 107).

If we take the French option to be unmarked, in other words, this patterning may provide the kind of domain we are looking for. Quite simply, one would expect a Francophone learner of English to have a harder time resetting his parameters than an Anglophone learner of French, who need only revert to a neutral setting, assuming of course that UG is still accessible. These differences should be reflected in differences in accuracy in the processing of written input for the purpose of making an acceptability judgment.

1.3.2 Other distinctions emerge when we look at Wh Movement in believe-type verbs; Kayne (1984) adduces the following examples.

(13) *Je crois Jean être le plus intelligent

(14) Quel garçon crois-tu être le plus intelligent?

(15) Which boy do you believe to be the most intelligent?

In short, both languages permit Wh Movement of lexical NPs, but only English permits sentences in which the NP remains in place. This suggests (on the basis of relatively little evidence) that French NPs behave strangely: as Kayne says, with reference to patterns other than V NP VP, "it is never the case, as far as we know, that the postverbal NP must be moved by Wh Movement to be licit" (1984: 111). Kayne goes on from this point to argue for successive cyclicity, with quel garçon perching in COMP only long enough to receive Case from croire before movement to the front of the sentence at the level of S-structure and, as we have seen, Case conferral by means of an abstract prepositional complementizer in English. The relevant point here is that these strings present still one more contrast between English and French, in this case, with French appearing to be more marked.

Gross (1968) also found some curious exceptions to the prevailing rule (quoted in Rouveret and Vergnaud 1980):

(16) On le croit être capable de faire cela

(17) Qui crois-tu être à Paris en ce moment?

Apparently, if these sentences are in fact acceptable, clitic-fronting confers acceptability since,

as we have seen, (4) is clearly unacceptable. (17) is of course similar to (14) in most respects. The status of these sentences is still indeterminate though they have been widely discussed, most importantly by Pollock (1989).

Rouveret and Vergnaud (1980) also cite the following examples, which do not contain an infinitive, as unproblematical.

(18) On le croit capable de faire cela

(19) Qui crois-tu à Paris en ce moment?

(20) On croit Pierre capable de faire cela

(21) On croit Pierre à Paris en ce moment

Of these, (18) is only trivially different from (16); (20) is similar to (22) below. In any case, neither of these examples contains the critical infinitive.

1.3.3 Another interesting contrast, one provided by Ruwet (1979), involves the V NP AP pattern in both languages:

(22) Je crois Jean intelligent

(23) *I believe John intelligent

(24) Quel garçon crois-tu intelligent?

Although these examples do not exemplify infinitival complements, they do at least suggest that Case assignment to postverbal lexical NPs is not easily generalizable, i.e. some mechanism must be found whereby the integrity of complements and the assignment of Case are both accounted for. Some informants have suggested that, in (22) and (24) above, there seems to be a selectional restriction on croire which makes it an unlikely choice for this environment, considérer or trouver being less controversial possibilities, as indeed their English equivalents (consider and find) are. Be that as it may, the principal sticking point, overall, is the co-occurrence in statements of subject NPs and infinitives, which is licensed in

English and blocked in French[6]. Apart from that restriction, if we accept Kayne's analysis, French seems at least as permissive as English, which is paradoxically more marked both in what it restricts and in what it permits.

1.4 Some of these issues become clearer when we look at other verbs that exhibit Exceptional Case Marking (hereafter ECM) and control effects. This information is summarized in Table 1.2.

TABLE 1.2. Control and ECM: believe, want and promise

	PRO	ECM	Clausal complements
believe	-	+	+
want	+	+	-
promise	+	-	+

Whereas two of these verbs require ECM, only want and promise are control verbs, as the following examples show.

(25) I want to be on time

(26) I [VP want [S' (0) [S PRO to be on time]]]

(27) I promise to be on time

(28) I [VP promise [NP [S' [S PRO to be on time]]]]

Want of course does not instantiate obligatory control, since ECM is possible, but promise clearly does, as the following shows.

(29) I [VP promise Johni [NP [S' [S PROj to be on time]]]]

---

6 In French, such verbs as laisser and voir permit NP VPinf structures; thus, sentences like Jean a laissé Marie partir and Louis voyait quelqu'un venir vers lui (Rouveret and Vergnaud 1980: 127) are possible. Similarly, there are many verbs in English that fall outside the believe-type category, promise, of course, among them. Alexander and Kunz (1964) include dozens of verbs inside the believe-type category (i.e. as acceptable before an NP VPinf structure), among them claim (hesitantly) and presume, though obviously these two differ from believe in also licensing PRO structures. In any case, this native speaker is doubtful of the acceptability of sentences like she claimed him to be her only hope.

Co-indexing between John and PRO is presumably ruled out by some version of the projection principle; in any case, promise does not contrast with its French equivalent, promettre, in the distribution of any of these properties (see below). Only believe and promise take clausal complements; want permits for-clauses, of course, at least with the insertion of adverbial phrases, but that issue has been conscientiously avoided in this discussion.

Comparable verbs in French, however, pattern differently and suggest another basis for comparison of the two systems. In the first place, the problem of Case assignment, as we have seen, does not arise except possibly where Wh Movement is concerned, so ECM is at best only a peripheral issue. The facts appear in Table 1.3.

TABLE 1.3. Control and ECM: croire, vouloir and promettre

	PRO	ECM	Clausal complements
croire	+	-	+
vouloir	+	-	+
promettre	+	-	+

It is clear from this table that vouloir exhibits properties that differ from its English equivalent's:

(30) *Je veux Jean être à l'heure

(31) Je veux que Jean soit à l'heure

By contrast, it is also clear that promettre patterns like English:

(32) Je promets d'être à l'heure

(33) J'ai promis à Jean d'être à l'heure

(34) Je promets que Jean sera là

(35) Je [VP promets Jeani [NP [S' [S PROj être à l'heure]]]]

Thus, with reference to these pairs of verbs in French and English, we have something like an interlingual hierarchy, with croire/believe being the most dissimilar, promettre/promise the

most similar and vouloir/want falling somewhere in between[7]. Since these structures in French are generally considered to be less marked than their English equivalents in a Greenbergian sense, i.e. when we compare these languages and other languages, we have in these facts the tentative basis for a comparison of the relative complexity of parameter settings: the acquisition of English would seem to require more of an adjustment than the acquisition of French. In sum, the implication with respect to foreign-language acquisition (hereafter SLA) is that the Francophone adult would have more difficulty mastering the complexities of English grammar, given its distance from UG, than his Anglophone counterpart would have mastering French, given its proximity. Theoretically, differential effects in accuracy should also parallel interlingual differences among these three pairs of verbs, as outlined above, but that is a somewhat subordinate issue (see below). Suffice it to say, one would expect the acquisition of promettre/promise to be less problematical than the acquisition of the other pairs whether because of the influence of the first language, intrinsic complexity, general learning strategies or the role of UG.

1.5 Leaving aside the complexities that arise with respect to Wh Movement and V NP AP structures, the system in French is clearly more regular, and conceivably more universal, than the system in English, as Table 1.2 and Table 1.3 above show. If, for the purpose of discussion, we take the setting associated with French (Table 1.3) to represent a neutral or initial setting, then a child learning English would require positive evidence for the acquisition of ECM, and this would be abundantly available in the input. On the other hand, positive evidence would not be available for the child to learn that believe was not a PRO verb and that want did not permit clausal complements: the child's acquisition of these constraints would presumably require negative evidence, which is generally considered to be absent from

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7 Discussion of à- and de-insertion and tense assignment in the clausal complement is avoided here because these are relatively uncontroversial issues and do not bear directly on the topic.

the child's language-learning environment except possibly in the form of implicit or indirect negative evidence, i.e. the absence of positive examplars. Whether, however, we take the French setting to be universal or not, the implications for the acquisition of French among Anglophone adults are clear: at the very least, we can say that, while learning that ECM does not exist in French would require negative evidence of some kind, resetting the parameter for PRO and clausal complements could occur automatically as a consequence of positive evidence. These comments relevant to learnability theory, of course, beg the central question, namely, the issue of UG and its role in the process, except insofar as we can hypothesize that the regularity and comparative simplicity of the system associated with French would ease acquisition and that these differences would be reflected in accuracy levels on a judgment task.

It may be that the two languages stand in a "superset/subset" relationship, as that is defined by Wexler and Manzini (1987), although it is not clear, given the set of verbs as defined, how that might play out. Essentially, their idea is that if a child is understood to overgeneralize by setting his parameters in such a way as to generate a grammar which is too big, i.e. a superset of the actual grammar of the language, then he will never be able to correct this mistake with positive evidence because his grammar will always be consistent with all available evidence. Rather, their view is that the child opts for the subset before the superset and gradually expands this grammar in the light of positive evidence. Thus, for example, according to these researchers, a reflexive anaphor like herself must be bound locally whereas its Japanese equivalent, zibun, can be bound anywhere. Therefore, the setting associated with Japanese yields a bigger grammar than the setting associated with English and, furthermore, Japanese, where this parameter is concerned, contains English implicationally since the English constraint is consistent with its Japanese equivalent though not quite big enough, i.e. sufficiently comprehensive, to be identical. On this basis, Wexler and Manzini suggest that the smaller grammar is therefore less marked than the bigger one. They further allege that a child will naturally try the less marked setting first; then, as more and more positive evidence

accumulates and nudges him positively toward the more marked setting, he will reset his parameter accordingly (See White 1989b for a reformulation of this hypothesis in SLA terms and a summary of a related study of adjacency in SLA).

The problem with this formulation - created to account for the facts of L1 acquisition[8] - is that, where the phenomena addressed in this study are concerned, neither setting can be subsumed implicationally under the other.

Clearly, competence in and/or dependence on the French system will take one relatively far in English: positive evidence will be sufficient to iron out problems with ECM. It will not help one learn, however, that believe is not a PRO verb (and requires additional morphological apparatus) or that want does not take tensed clausal complements. Negative evidence will be necessary for that. Or possibly indirect negative evidence.

On the other hand, English parameter settings will not take one very far where croire and vouloir are concerned. Positive evidence will suffice for the acquisition of tensed clausal complements (with vouloir) and the PRO-ness of croire, but it will not help much with the impermissibility of ECM. Again, negative evidence will be necessary. Or possibly indirect negative evidence.

In other words, neither system is wholly productive while additional properties are acquired incrementally by means of positive evidence. Each setting requires negative evidence if it is going to be fully fleshed out and refined.

1.6 In sum, patterns like the following are universally possible, particularly in Romance.

(36) Je veux aller

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[8] There is an obvious interest in adult SLA/UG research in looking for adult phenomena which are similar to L1 phenomena on the assumption that the two processes are overlapping. On occasion, this is carried too far, i.e. either when the theory (created to explain L1 phenomena) has not been permitted to ripen or when methodological approaches suitable to children are applied inappropriately to adults (see Flynn 1984, for example).

'I want to go'

This means that the person who does the 'wanting' also does the 'going'. Such sentences, with this interpretation, are unproblematical in virtually all languages. We also know that such sentences as the following are also likely candidates for universality.

(31) Je veux que Jean soit à l'heure

*'I want that John be (is?) on time'

While this sentence is acceptable almost everywhere, in almost every language (and is thus likely to be part of the child's innate linguistic armamentarium), it is strictly forbidden in English. This means that a child, "prewired" to expect such sentences, has to reset his parameters to accommodate language-specific input in English. The child's mind, in other words, is geared up to be unconsciously alert to such complexities involving tensed clausal complements - to see this as a flash point in the acquisitional process - and to move quickly, on the basis of very little evidence, to alter his assumptions as required. The abstract universal principle, therefore, leads him to expect one pattern but revise that expectation within a narrow range of possibilities in the light of evidence to the contrary. This set of expectations is commonly referred to as "core" grammatical knowledge (see below), whereas the process of parameter 'revision' carries him into the marked "periphery". The overarching principle, or so-called universal, is, roughly, that complex shifts in syntactic relations can occur when one verb is subordinate to another, shifts having to do with who does what and how you represent these facts in language.

One more example:

(37) I want him to leave

*'Je veux le (lui?) partir'

In this case, the person who does the 'wanting' is different from the one who does the 'leaving'. Here, as above, English seems to be exceptional: while a pronoun in the objective case is

permitted in this context in English, it is clearly not accepted in many other languages[9]. This configuration is commonly disallowed: it is an odd property of English, something exceptional or "marked".

The key point in all of this is that, while French and many other languages, are perfectly regular in their patterning [i.e. they will accept (31) and (36) but not (37); cf. promise], English is the odd man out. This fact has a clear implication for foreign language acquisition: French will be easier to acquire than English. Question: Is this really true? And is it true among adolescents and adults?

In short, this study is based on four assumptions:

At least where statements are concerned, the syntactic system governing croire, vouloir and promettre + infinitival and clausal complements is far more regular and less marked - hence more accessible from the point of view of learnability - than the system governing believe, want and promise + infinitival and clausal complements.

Francophone learners of English are therefore likely to have a harder time than Anglophone learners of French, i.e. because of the intrinsic complexity of the system associated with English. The apparent proximity of French syntax to UG may also confer an advantage on the Anglophone learner. This constitutes one general tendency.

UG, i.e. those principles that characterize the grammar of all languages and get parameterized in response to language-specific input, plays a role in the learning of any language, whether that language is a first or foreign language, regardless of the age of the learner.

Francophone learners of English and Anglophone learners of French, in reactivating UG, would be operating with common - or, at the very least, overlapping - assumptions, i.e. parameter settings, about what constitutes an acceptable sentence whether these are closely

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9 Kayne (personal communication) suggests that Egyptian Arabic may be one other language that patterns like English in this respect.

associated with the learners' native languages or not. This set of assumptions would naturally conform to the less marked, more universal model. This is a second general tendency, one which runs counter to the first.

These assumptions are reformulated as hypotheses below.

Influences on informants' deviant judgments about sentences in the foreign language which cannot be traced to dependence on the first language or the reactivation of UG have several other likely sources: quality of input, general learning strategies, means of acquisition, circumstances of use and so on. These environmental, or system-external, variables are not addressed in this study (see below). It is obvious that many possible judgments are consistent with more than one causal explanation - indeed, it may never be possible to interpret all outcomes unambiguously or identify a closed set of all such variables. The questions this study addresses are these: Is there any evidence whatever of a role for the first language? Is there any evidence whatever that UG is still accessible to these learners? Stricter experimental conditions would have to be met to get a more precise fix on many outcomes and their etiologies.

CHAPTER TWO

Review of the Literature

2.1 Conceptual Matters

2.1.1 As indicated above, Chomsky makes a distinction, in discussing parameters, between "core" and "periphery":

> When the parameters of UG are fixed in one of the permitted ways, a particular grammar is determined, what I will call a "core grammar." In a highly idealized picture of language acquisition, UG is taken to be a charterization of the child's pre-linguistic initial state. Experience - in part, a construct based on internal state given or already attained - serves to fix the parameters of UG, providing a core grammar, guided perhaps by a structure of preferences and implicational relations among the parameters of the core theory. If so, then considerations of markedness enter into the theory of core grammar.
>
> But it is hardly to be expected that what are called "languages" or "dialects" or even "idiolects" will conform precisely or perhaps even very closely to the systems determined by fixing the parameters of UG. This could only happen under idealized conditions that are never realized in fact in the real world of heterogeneous speech communities. Furthermore, each actual "language" will incorporate a periphery of borrowings, historical residues, inventions, and so on, which we can hardly expect to - and indeed would not want to - incorporate within a principled theory of UG. For such reasons as these, it is reasonable to suppose that UG determines a set of core grammars and that what is actually represented in the mind of an individual even under the idealization to a homogeneous speech community would be a core grammar with a periphery of marked elements and constructions.
>
> (1981a: 8)

In other words, the grammar of a particular language is made up of a central set of principles which guide its elaboration and a marginal set that reflects the history of the language and the peculiar circumstances in which it has been used. The core grammar of a language is intimately related to universal principles, parameterized through exposure, while the periphery is more tenuously related to these principles and may even embody quite idiosyncratic, language-specific rule orderings. As Chomsky has written elsewhere,

> The systems called "languages" in common sense usage tolerate exceptions: irregular morphology, idioms, and so forth. These exceptions do not fall naturally under the principles- and-parameters conception of UG.

> Suppose we distinguish core language from periphery, where a core language is a system determined by fixing values for the parameters of UG, and the periphery is whatever is added on in the system actually represented in the mind/brain of a speaker-hearer. This distinction is a theory-internal one; it depends crucially on a formulation of UG.
>
> (1986a: 147)

The assumption in this study is that ECM, while associated with English, partakes of certain general syntactic principles that operate across languages (see below). It so happens that a child - or, for that matter, a young adult - learning English has to set his parameters to accommodate this phenomenon.

2.1.2 In the first passage above, Chomsky appears to be talking about markedness in at least two distinct ways. In the first place, he refers to "a structure of preferences and implicational relations" which influence the parameterization of core principles. These "preferences" are presumably innate and, together with experience, guide, say, the child in his selection of one setting over another. Whether an intuitive decision about one of these settings necessarily implies a decision about another - or, indeed, whether one decision triggers another, so that effects can be said to cluster - is, as we have seen, at best an open question. Given the arbitrary definition of ECM employed in this study, there is little hope of establishing an implicational or hierarchical relationship among its components, as I have suggested.

Secondly, Chomsky also mentions "a core grammar with a periphery of marked elements and constructions" and thereby implies that, while the core is unmarked, the periphery is marked. Thus, in addition to degrees of markedness within the core, we also have degrees of markedness that transcend the core/periphery distinction.

Subsequently, Chomsky distinguishes three dimensions of markedness:

> The distinction between core and periphery leaves us with three notions of markedness: core versus periphery, internal to the core, and internal to the periphery. The second has to do with the way parameters are set in the absence of evidence. [Berwick 1985 essentially maintains that, if one parameter setting can be said to be a subset of the other, then that is the unmarked value.] As for the third, there are, no doubt, significant regularities

> even in departures from the core principles (for example, in irregular verb morphology in English), and it may be that peripheral constructions are related to the core in systematic ways, say, by relaxing certain conditions of core grammar. The problem of formulating these notions precisely is an empirical one throughout, although not a simple one, and many kinds of evidence might be relevant to determining them.
>
> (1986a: 147; some italics added)

In other words, degrees of markedness can differ along three dimensions: "core versus periphery, internal to the core, and internal to the periphery" (1986a: 146). Clearly, the regularity of the French verb system, at least where these three verbs are concerned, puts it squarely in the core. The English system is more problematical. On the one hand, attempts such as Kayne's to assimilate it into the larger syntactic processes of the language argue for its inclusion in the core. On the other, Chomsky's analysis suggests that it is distinctly peripheral, though its designation as marked itself suggests that it can only be defined with reference to principles it appears to violate. In other words, its exceptional character derives from its relation with the language's regularities. This somewhat casuistic argument, however, does not address the central issue in this study, whose purpose is to see if there are any differences in the judgments of two groups of learners. Such differences (if found) may contribute to speculation about differences in degrees of markedness, but such speculation about the languages themselves is not its primary purpose.

Not all studies involving UG and SLA have been scrupulous about such distinctions. If the rule systems under study in the two languages, for example, cannot be differentiated apriori with respect to markedness, it is sometimes difficult to sort out associated effects, and circularity results. As Hyltenstam predicts (see also below), with reference to typological categories, quite disparate acquisitional patterns can be expected of learners going from an unmarked to a marked (U-M) setting as opposed to the opposite (M-U). In the first case, long-term transfer can be anticipated; in the second, more ephemeral effects are predicted. Comparisons of marked (M-M) and unmarked (U-U) rule systems, by contrast, are less likely to prove interesting since, according to Hyltenstam, an unmarked setting will "fairly quickly

be abandoned in favor of the marked category" in the first case and "no acquisitional difficulties will be experienced" (1987: 68) in the second. Clearly, the best place to look for differential effects owing to differences in (degrees of) markedness is in a comparison of two precisely distinguished rule systems. It is only on this basis that one can assess the relationship between transfer and markedness, if it exists, or between markedness and a role for UG, if it exists.

Markedness distinctions, in short, are, as is widely attested in the literature, something of a crap shoot: virtually anything can be identified as marked or unmarked because the distinction is purely relative. Indeed, since the terms were first used by Trubetzkoy (1939), they have been applied in almost every corner of linguistics: primarily in phonology (Chomsky and Halle 1968, Eckman 1977), but also in morphology (Rutherford 1982), morphophonemics (Cairns 1986), lexicon (Greenberg 1966), semantics (Kellerman 1978), syntax (Comrie 1981), discourse (Chao 1986) and "error evaluation" (Santos 1987), to name just a few areas where the designation has proved useful. In fact, there seems almost no limit to this epidemic: given a comparison of two phenomena, one is always likely to appear more accessible than the other whether criteria of familiarity, intrinsic simplicity or perceptual complexity are applied. As Ellis says, "until reliable and generally accepted means are found for establishing which of two or more forms are marked and unmarked or more or less marked, the whole construct of markedness must be considered of doubtful value for empirical research" (1985: 212). Differences in degrees of markedness, in other words, depend crucially on how items are selected for comparison; ultimately, the validity of these distinctions depends on whether or not these items were selected on a principled basis or just thrown together. If the latter, then they are simply distinctions without a real difference and have little or no value where SLA is concerned.

In fact, White has pointed out a potential contradiction in Chomsky's definition of markedness as given above:

> If the unmarked case is defined as the child's initial hypothesis, then he would be expected to adopt the unmarked case even where data from the language he is learning suggest that the marked case will be required. Thus, one has a developmental claim that the child acquires the unmarked as a necessary prior step to the acquisition of the marked. On the other hand, if the unmarked case is defined as the choice made by the child in the absence of evidence to the contrary, then one does not necessarily have a developmental claim at all. That is, given evidence to the contrary, the child might immediately assume the marked form, without first adopting the unmarked.
>
> (1986b: 320)

This dilemma has implications for both learnability (see above) and the general notion of markedness, as we shall see.

The classic case of markedness, i.e. the one associated with the Prague School and linguists working in that tradition, involves the isolation of a pair, or pairs, of items whose apparent opposition can be traced to differences in distribution and/or paradigmatic complexity and/or syntagmatic complexity (Moravcsik and Wirth 1986). In general, highly frequent items are considered to be less marked than items of low frequency. So, for example, Rutherford (1982) makes the claim that adjectives such as tall, old and big are less marked than their opposites because they appear in questions as well as statements: How old are you? How tall is she? etc. On the other hand, the most common forms in a language are also comparatively complex in paradigmatic terms. The most common, everyday verbs in English, for example, i.e. those of Anglo-Saxon origin like be, though suppletive, are relatively easy to learn, perhaps because is is more salient in the input than, say, exists. Similarly, singular articles in German form a more complex paradigm (masculine, feminine and neuter in the nominative case: der, die, das respectively) than plural articles (nominative die); this asymmetry suggests that the singular forms are relatively unmarked, as does their comparative frequency. Finally, where syntagmatic relations are concerned, the unmarked forms, it is alleged, are simpler in structure. So, for example, the articulation of voiceless stops, in combination with other

sounds, is intrinsically less complex than the articulation of voiced stops; their relative simplicity, in articulatory terms, correlates with their frequency across languages. Indeed, the presence of voiced stops in the inventory of a language implies the presence of their voiceless counterparts, but not the other way around. Voiceless stops are also comparatively complex on the paradigmatic dimension, at least in English: /p/, for example, has both aspirated and unaspirated allophones (though this variation is motivated by what are usually referred to as syntagmatic relations, i.e. the stop's position in a word). In other words, because these stops are easy, there are a lot of them in the world's languages and they have many variants. For all of these reasons, the voiceless stops are commonly considered to be less marked than voiced stops.

This classic definition of markedness, however, does not go very far toward explaining the PRO/ECM/clausal relations that form the topic of this study. In the first place, the three verbs are widely distributed in both languages, though there may be some variation in the frequency across clausal types, i.e. I believe him to be late is probably less frequent than I believe that he's late. Secondly, the paradigmatic/syntagmatic distinction collapses when we are talking about essentially structural constraints. If we take Table 1.2 and Table 1.3 above to be illustrative of paradigms, then clearly the French system is more regular - and basically simpler -than the English paradigm, suggesting a marked designation. On the other hand, these tables are expressive of structural or syntagmatic relations as well, so an argument could be made that, since the French system exhibits less complexity, it should be declared the unmarked alternative. This is the view adopted in this study. However, the essential point is that so-called classic markedness can only go so far toward explaining these phenomena.

Moravcsik and Wirth (1986) raise an important issue in their discussion of what they call the "core hypothesis of markedness theory," namely, that underlying all definitions of markedness is the assumption that there will be high correlations among tests applying the three criteria outlined above. Implicit in all tests of correlation is the notion that, for example,

two measures being correlated are independent of each other (see also below); if this condition is not met, then the correlation coefficient simply confirms what is already obvious and buys us nothing. If, for example, we 'discover' that height correlates with success in basketball, when only tall people are chosen to play basketball in the first place, then the statistic is meaningless. Where markedness is concerned, the assumption is that there is no inherent reason why frequency and complexity (to use the unmarked form) should correlate - when, in fact, it would seem more logical for them not to - so that, when they do correlate, we have uncovered an interesting and otherwise inexplicable relationship. If, on the other hand, we ignore some other factor which motivates the relationship - as, for example, phrasal length mediates the lack of a relationship between frequency and complexity in synthetic and periphrastic adjectival morphology in English - then we run the risk of saying nothing new about markedness, indeed, of saying nothing new at all. It is for this reason that the notion of a correlation occupies a central role in most research within a markedness framework. Indeed, as Ferguson has written, "the value of the concept lies in the strong tendencies toward agreement among the various attributes of less or greater markedness (frequency, overtness, complexity, early acquisition, neutralization, etc.)...and consequent possibilities of prediction" (1984: 249).

In the case of this study, correlation is carried a step further. Since I am not primarily concerned with establishing a markedness relationship on the basis of a syntactic analysis but, rather, assuming that relation to exist, with unearthing evidence of a universal preference, the correlation I am looking for is a statistical correlation between response patterns and only serendipitously one that would constitute evidence of markedness (see Zobl 1982b). My assumption is that the subjects being tested are free to choose between the English syntactic path and the French syntactic path, which I take to be the less marked, more universal default option. They have powerful reasons to choose the English option: transfer in the case of the Anglophone group, input in the case of the Francophone group. They have the same powerful

reasons, in reverse, to choose the French option. If, however, both groups tend in a statistically interesting way to opt for French syntax over English, then we have some reason to suspect that universals have an influence.

As I have suggested, the definitions and foci of markedness and markedness-oriented research vary considerably. As Moravcsik and Wirth say, "...markedness theory - if theory is the correct word - is really a family of hypotheses: if one correlation is not borne out, others may still hold" (1986: 10). Thus, researchers in this field are constantly groping toward an understanding of markedness, if it exists, but more fundamentally of linguistic phenomena which may or may not be illuminated by markedness distinctions. Comrie, for example, in an attempt to establish that the form which is "least marked formally is also least marked in terms of properties of the real world" (1986: 86), looks at "control," which he interprets as principles governing the "interpretation of the missing subject of the infinitive construction" (89) in sentences like Tom promised Sally to return before nightfall. Essentially, in this study he is at pains to describe semantic and pragmatic constraints on the interpretation of these sentences and to show that these parallel formal differences in markedness. Harbert (1986), by contrast, applies markedness notions to rule formulations themselves, in this case to various definitions of binding domain, in an effort to show that a less uniform definition, Huang's (1982), which distinguishes pronouns from anaphors, is preferable to Chomsky's (1981a), which does not. After a tour of the world's languages, that takes us along both synchronic and diachronic paths and through various formulations and reformulations, Harbert concludes that several problems can be resolved by declaring that in English "both AGR and syntactic subjects are potential SUBJECTS" (147), whereas in Icelandic only AGR constitutes a potential SUBJECT; the first of these he deems the unmarked case, the second the marked. He also makes the claim that linguistic change, in languages related to English, has occurred as a result of "pressure to eliminate markedness" (140). The study is only marginally related to the topic of this study. It is cited here only to demonstrate how free-

wheeling are the applications of markedness theory in recent research.

One promising development in markedness-related research in SLA is Eckman's (1977, 1985) Markedness Differential Hypothesis. Though constructed with reference to phonological data, it is sufficiently general to be applicable in syntax research. In summary, it says: "A phenomenon or structure X in some language is relatively more marked than some other phenomenon or structure Y if cross-linguistically the presence of X in a language implies the presence of Y, but the presence of Y does not imply the presence of X" (1985: 290). McLaughlin provides a summary of what this hypothesis means in an interlingual context:

> (1) Those areas of the target language that are <u>different from</u> the first language and are relatively <u>more marked</u> than in the first language will be difficult.
>
> (2) The <u>degree of difficulty</u> associated with those aspects of the target language that are different and more marked than in the native language corresponds to the relative degree of markedness associated with those aspects.
>
> (3) Those areas of the target language that are different from the first language but are not relatively more marked than in the first language will not be difficult.
>
> (1987: 89; italics added)

This formulation, in other words, integrates the notion of markedness and the notion of transfer; the definition of markedness employed here is also interesting because it bears a close resemblance to the definition of implicational universal in the typology literature (see below).

White (1986b) has also attempted to make markedness considerations more responsive to SLA data with reference to Corder's (1979) criteria for any theory of SLA: such a theory must account for variability in interference patterns, similarity in the developmental sequences of learners regardless of their L1s and the relative complexity of learning unrelated languages. White, in response, finds in markedness-related research into the processes of adult SLA a method for satisfying these criteria. Markedness, for example, may delimit those areas of a language where interference is most likely to occur and may be better at doing so than a

contrastive analysis which is uninformed by markedness considerations. Secondly, overlap in parameter settings, together with markedness considerations, may account for these apparent similarities despite differences in the L1. Finally, unrelated languages may be unrelated precisely because of differences along these dimensions, and not simply because of superficial differences in morphemic or syntactic structure or genesis. In other words, such considerations as these may, in the long run, uncover completely different configurations of similarity and dissimilarity than have usually been assumed to exist and, in the process, provide answers to vexed, unanswerable questions.

As Kean (1986) points out in a review of markedness assumptions in syntax, four questions arise with respect to these constructs and, depending on their answers, imply different acquisitional patterns in SLA.

(a) Do foreign language learners assume the unmarked values of parameters (i.e. reactivate UG)?

If so, then there would be no significant effect of transfer, unless, of course, both the L2 and the L1 instantiated the unmarked value (in which case the data would mask the role of transfer.)

(b) Do they transfer only the unmarked values of their L1s?

If so, then these learners presuppose "convergence" between the two grammars where unmarked cases occur in the L1. They would then have to attend to positive input to acquire marked values in the L2 where the L1 grammar lacks markedness and its absence (i.e. indirect negative evidence) to acquire the converse.

(c) Do they transfer only the marked values?

If so, they would have to adopt a strategy which stands the preceding one on its head; that is, they would have to depend on positive evidence to acquire the unmarked values in the L2 (which differed from the L1 in terms of their configuration) and its absence to find the marked values (which likewise differed).

(d) Does transfer, whether present or absent, really depend on the relative markedness values of parameters in the L1 and L2?

An answer to this question, in Kean's view, is intimately related to the answers to (b) and (c) since, if they are answered in the negative, that would move the whole SLA research program away from UG-related issues and, if they are answered in the positive, that would raise a whole set of new and interesting questions.

Kean concludes (on the basis of a review of early work on "pro-drop" and preposition stranding) that there are two possible sources of transfer: that which obtains when the learner fails to observe that some property of the L2 contrasts with the L1 ("blind transfer") and that which stems from his failure to distinguish between the property's realization in the L1 and the L2 ("short-sighted transfer"). Markedness considerations, she concludes, are relevant only to the first of these sources.

2.1.3 Still other questions arise with respect to the definition of universal. Universals come in many shapes and sizes though the SLA literature commonly refers to only two: typological (Greenbergian) and generative (Chomskyan). Although SLA research has been carried out within both traditions, the assumptions that underlie the two approaches are radically different (though not irreconcilable), which makes comparability across studies difficult if not impossible. This is true despite the fact that the two approaches are not themselves incompatible: they simply represent different and complementary ways of looking at the same phenomena.

Chomsky's definition of universal essentially rests on a few familiar claims, namely, that the language faculty is innate and prewired to manipulate the intake in accord with universal principles; these principles are then themselves parameterized or fixed to accord with the intake. Chomsky (1965) also claims that there are two types of universal: formal and substantive. Formal universals specify the structure of all principles governing all languages (components, constraints on application, articulation of all sub-systems, etc.), and substantive

universals specify the content of these principles (the syntactic categories they refer to, etc.). The purpose in all of this is to define the properties of a possible (human) language. So, for example, substantive universals delimit the range of possible sounds in all (human) languages, from which a single language will select a subset; formal universals define the phonological processes that operate across syntactic categories in all languages, of which a particular language will select a subset. As Chomsky's own work is almost exclusively concerned with English and depends crucially on his own native-speaker intuitions, his notion of universal is constrained by what is accessible in English - that is, by his penetrating analysis of how elements move, assimilate each other and align themselves in that language. Since Chomsky's initial work, studies have been conducted by generative grammarians with reference to languages as disparate as Catalan and Japanese, to name just two. In any event, the essential claim is that it is impossible to conduct cross-linguistic studies until the object of study itself has been defined. As the literature has grown, we have also come to realize that the principles that Chomsky and others have uncovered in English operate, in parametric variation, in other languages, as many recent studies show (see Rizzi 1982a, Sportiche and Aoun 1983, Saito 1985, among others).

By comparison, typological universals are the province of non-generative grammarians, chiefly those working in a structuralist framework. Rather than consult their own intuitions about single languages, these researchers are concerned to categorize the formal variation that exists among many languages. While the generative approach is deductive and distinctly Platonic in orientation, this approach is inductive and clearly Aristotelian. Since Greenberg (1966) did his seminal work with 30 languages, the inventory has expanded to include, at least where word order is concerned, over 350, or roughly 5% of the world's languages (Hawkins 1983)[10]. These have all been classified with reference to certain surface-level parameters, e.g.

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10 The current count re. the number of languages in the world according to the Summer Institute of Linguistics, Inc. in Dallas, Texas (personal communication) is 6,449. This

what gets relativized in relative clauses, noun-adjective word order, etc., and implicational universals have been constructed. These universals - of the form "if p, then q" - codify the relationships among syntactic categories and organize the structures into hierarchies. Perhaps the best known, at least in SLA research, is the so-called Accessibility Hierarchy of Keenan and Comrie (1977). This specifies the order in which relative clause types are acquired on the basis of how they group cross-linguistically in implicational terms. Gass (1979) has found considerable support for this hierarchy in an SLA context (see below). In addition to implicational universals, Greenberg and others have also looked at language frequencies, that is, the numbers of languages that load onto each attested word order type. In short, this approach focuses, as Keenan says, primarily on "the regularities in the ways languages may differ from one another" (1978: 90) and secondarily on "frequency hierarchies," or the frequency with which certain co-occurrences appear in the languages of the world. Thus, we are dealing here with both implicational and statistical universals, both relationships and tendencies, both variation and probability.

As Comrie has commented:

> At first sight, the study of language universals and the study of language typology might seem to be opposites, even in conflict with one another: language universals research is concerned with finding those properties that are common to all human languages, whereas in order to typologize, i.e. to assign them to different types, it is necessary that there should be differences among languages. The contrast can thus be summed up as one between the study of similarities across languages and the study of differences among languages.
>
> (1981: 30)

The two approaches, in short, differ in a number of important ways: the number of languages each linguist typically looks at, the syntactic theory each espouses and the type of explanation each provides for the existence of universals. As we have seen, a generative grammarian looks

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is clearly an underestimation, particularly when dialect variation (entailing important syntactic differences) is taken into account. Furthermore, it is obviously not the case that Hawkins' inventory was a random sampling of all languages; had it been, one could have more confidence in the typological notion of "universal".

at comparatively few languages, chiefly his native language; a typologist looks at many, even hundreds[11]. Of course, the typologist is constrained in his use of grammatical categories by the preferences of field linguists, many of whom carried out their research in the decades when structuralism held sway; a generative grammarian has few such constraints, indeed, slices up the language in a different way from the field linguist, so is free of certain historic encumbrances. While the typologist is busy tallying co-occurrences and devising implicational universals across many languages, the generative grammarian is quietly uncovering subjacency relations and gradually establishing the validity of subjacency as a construct with reference to only a few languages. What this research lacks in breadth, however, it more than makes up for in depth. Unfortunately, so long as the typologist is only concerned with surface-level phenomena, he has little to say to the generativist, who is busy revising the categories we use to think about languages (while the typologist is merely scrambling for more and more data). The typologist faults the generativist for being too abstract; the generativist faults the typologist for being too superficial. On the theoretical level, the typologist also looks for alternatives to the innateness argument (e.g. monogenesis, functional constraints on processing, etc.) that underpins all of Chomsky's work.

Nonetheless, an argument can be made that the two approaches are at least complementary. It is clear that work in typology, though data-driven, depends on theoretical constructs; indeed, it is impossible to classify the world's languages without reference to the categories of theoretical syntax. The current dependence of many typologists on constructs associated with American structuralism is a simple artifact of history. As more and more work on more and more languages is conducted in a generative framework, language typologies will inevitably change to keep pace with the multinational generative enterprise. We have seen this

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11 Needless to say, the scope of generative research has radically expanded in the last decade, as indicated above, with the result that it is no longer possible to examine a phenomenon in a single language without reference to collateral studies of similar phenomena in other languages, even genetically unrelated languages.

already in the enthusiasm (somewhat tempered in Comrie's case) for X-bar theory (see Hawkins 1983: 183), as well as in the incorporation of "unattested" examples. Similarly, we have seen the influence of variationists on generative theory in the notion of parameterization itself, as well as in Chomsky's grudging acceptance of the value of "universals of the sort explored by Joseph Greenberg" in determining "how much must be learned as grammar develops in the course of language acquisition" (Chomsky 1981a: 95). Inevitably, the two approaches will move toward a synthesis because their aims are overlapping: typologists are no more interested in a simple descriptive catalogue of cross-linguistic differences than generativists are obsessed with English as an end in itself. Indeed, the isolation of an "absolute" universal within a Greenbergian framework would constitute an important challenge to generative theory and require explanation. A "statistical" universal occupies a different status since it cannot be said to be 'true' in the same way and merely constitutes an interesting fact about language. Thus, the remarkable variation that exists among the dialects of Italian, for example, must be assimilated in a theory of Italian syntax, but the principles that theory yields must have wider applicability. By contrast, a challenge to the notion of subjacency as it is now formulated would have to be explained with reference to still more abstract properties; that is, it is entirely possible that subjacency itself may flow from some other, more primitive principle or set of principles which are not only poorly understood but conceivably imperceptible.

Since research in both traditions, however, is at a relatively formative stage, neither has yet provided a completely uncontroversial definition, together with examples, of what constitutes a language universal, although clearly the generativists, in their carefully principled and explanatory approach, have the upper hand.

In short, there are few hard and fast universals - at least in the sense of "absolute"

universal - in either of these approaches[12]. The generative enterprise in its current orientation has given us very little to go on: it may be, for example, that null subjects are more universal than pronoun subjects, but the fact is that languages vary, and both configurations are possible. It may be, as the typologists allege, that the two most widespread relative clause types are the pre- and post-nominal (Comrie 1981: 137), but the internal-head type also exists (see Bambara, Quechua and Hindi, for example), so there is considerable variation. While the world's languages may exhibit certain tendencies and predispositions, the fact remains that we are a long way from knowing what is present in the child's head at birth, i.e. how to formulate a principle, apart from saying, for example, that word order is a feature of all languages and furthermore is constrained or parameterized in such-and-such a way. That is, to say that word order is an issue and the child has to set his parameters in some way to account for this fact is not to say very much. By the same token, suggesting that verb-NP relations are a "flash point" in the acquisitional process (see above) and may trigger re-parameterization is not particularly revealing. While formulations at this level of generality are a necessary first step, they take us only a short space down the road toward an understanding of what is truly universal about language.

Specifically, neither of these approaches leaves us with a clear sense of how the PRO/ECM/clausal complement contrast outlined above fits into UG or the scheme of typological universals, except, of course, that in Chomsky's work ECM, as the name implies, is an exceptional phenomenon. Whether or not the syntax associated with French can be said to be a good candidate for universality, it at least has the virtue of symmetry: whether it is

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12 Gregg (1989b) claims that the distinction between absolute and statistical universal is, in any case, merely quantitative. If only one language can be found that violates the apparently universal rule, then the rule loses its absolute status and becomes statistical; if additional such languages can be found, then it ceases to be a candidate for universality altogether. He further criticizes Eckman (1984) for using these statistical criteria to define language itself, in other words, to poach on territory staked out by generative syntacticians, who have their own criteria of universality.

universal or not, it is certainly less marked in its overall structure. Thus, in a sense, this is a dissertation about markedness rather than universals. On the other hand, even if Hypotheses 2 and 2' are confirmed (see below), we will still not know (as I have suggested above) whether that indicates anything about markedness, so, in a sense, this is not a dissertation about markedness. To define markedness, as I have indicated, with reference to the ease or directionality of an acquisitional process while attributing these characteristics to markedness itself is to commit an elementary error in logic. The best one can hope for - particularly in a study so hedged with statistical inference - is to be able to say that here lies some evidence that universals and/or markedness considerations may or may not have some role in the process: absent any evidence to the contrary, it is at least conceivable that French is closer to the universal norm than English. In the worst-case scenario, one must content oneself with saying that these informants found this syntax easier to master than that syntax for whatever reason, perhaps only the intrinsic complexity of the two systems.

Recently, a number of controversies have arisen over the question of what constitutes evidence for and against the claims of SLA researchers working within a principles-and-parameters framework (see also Methodological Issues below). The general SLA literature (see below) is replete with attempts to come to terms with this issue, that is, by asking a variety of questions: Are interlanguages natural languages? Do L2 grammars exhibit structure-dependency? Are production data indicative of underlying competence? Are judgment data? Are the acquisitional orders of L1 and L2 learners the same? Are their developmental stages? Are considerations of markedness relevant? Are core/periphery distinctions? and so on. Some of these studies come closer than others to the central issue; some in fact are very indirect. Differences over what constitutes evidence of UG reactivation, as we have seen, interact in a variety of interesting ways with more fundamental issues of what constitutes language, with the underlying question of competence. As Clahsen and Muysken have written:

> Originally, the account given by generative grammarians was that variation between languages is simply what is left open after the invariant

> principles of UG have outlined the basic framework for grammars of natural languages. The rise of parameter theory involved the idea that some of the variation between languages is determined by a set of options defined by UG itself (Rizzi 1982). The general principles, such as move alpha or subjacency, in this view, do not hold directly and invariantly, but because variables in their definition are filled in by specific values in individual languages...The fact then that abstract general principles interact with properties of individual languages to produce variation implies that parameter settings have consequences for the overall grammar, not just for one construction.
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> (1989: 24)

In other words, in the principles-and-parameters framework there is an assumption of an underlying relationship among acquisitional elements such that the acquisition of element A may trigger - or at least co-occur or "cluster" with - the acquisition of element B. In all such deductive models, there is an assumption of automaticity, of organic maturation, but in this case there is the additional assumption that language-specific input may cause collateral shifts in the system as a whole. This assumption complicates the process of trying to capture the overall shape and direction of SLA and has given rise to a number of complex and highly speculative proposals.

Most recently, three clear-cut points of view with reference to the question of the role of UG in SLA have been isolated. The first of these is sometimes referred to as the "pure" UG hypothesis (see White 1990); in essence, the claim is that second (or foreign) language acquisition and first language acquisition are identical processes involving primarily the interaction of UG and input from the environment. Few researchers in the SLA vineyard cling to this hypothesis. The second position is the so-called "UG is dead" hypothesis, which says essentially that UG is no longer available to the adult foreign language learner, that second language acquisition is distinct from first language acquisition and that, in adults, it is comparable to the learning of non-linguistic subject matter. This position is associated, to some extent, with Schachter (1988) and Clahsen and Muysken (1986). Finally, a third position, a modified contrastive point of view, claims that UG is still available to the adult by way of the L1 in the somewhat attentuated form associated with that language's parameter settings

(together, presumably, with some kind of abstract memory of the process as a whole). In its most extreme form, this claim is associated with Bley-Vroman (1986), Clahsen and Muysken (1989) and Schachter (1989b), whose view (in part, because of the lack of "completeness" in adult second language acquisition) is that UG cannot be reactivated as a full participant in the generation of a second or foreign language. A somewhat modified form of this claim (see White 1986b) takes the view that UG is still reactivable but that the learner will exhibit dependence on the L1 parameter setting at the early stages of SLA. Whether in its strong or weak forms, this third hypothesis represents a retreat into contrastive analysis and transfer. While it may give us in the long term a clearer understanding of these phenomena from a somewhat different perspective, it all but abandons the claim of a governing or determining role for UG in the process of second language acquisition among adults (See below for more on these competing claims).

2.1.4 With respect to the overall direction of SLA research within this framework, Rutherford (1986) in an evaluative survey suggests that, while the agenda is promising, certain problems remain. These include, in his view, the need to "clarify and refine the UG notion of 'parameter' itself before a theory of 'parameterized UG' can with any kind of confidence be brought to bear upon the problems of IL syntactic development" and the need to gain "insight into the crucial phenomenon of 'failure of ultimate attainment'," (12) perhaps outside grammatical theory, perhaps with reference to general "operating principles" (see Clahsen and Muysken 1986 and Felix 1985).

On the issue of the overall shape of the process, Schachter (1988) compares child L1 acquisition and adult SLA and concludes that the two processes differ with respect to four characteristics: completeness, equipotentiality, previous knowledge and fossilization. Whereas children attain perfect command of the L1, adults rarely - and perhaps never - match this achievement in SLA (see Schachter 1989b below for more on this). This difference is reflected in the differential ability of native and non-native speakers to identify ungrammatical

sentences (Kellerman 1984, White 1985a) and in Coppieters' study of near-natives (1987; see below). Children also exhibit equipotentiality: they are all able to learn any language in approximately the same period of time. Adults, by comparison, exhibit considerable variation in their abilities to acquire a second language, as we have all seen to our regret. The issue of previous knowledge is of course a familiar one in its customary guise, transfer. Schachter cites Schachter and Hart (1979), White (1985a) and Hilles (1986) to establish its influence. Finally, she discusses fossilization, which is everywhere manifest in adult SLA and needs little explanation. Fossilization, however, may constitute evidence of system-external, conceivably even general social, influences (see Schumann 1975, among others, on the effects of social distance.)

Schachter's conclusion from this survey is that "the facts of second language acquisition are nowhere near the same as those of first language acquisition" (222) and that "the role of Universal Grammar in explaining how second language acquisition occurs will turn out to be much more modest than present claims indicate, with parameter setting, or resetting, as currently envisioned, an impossibility" (219). Rather, she favors the view espoused by Bley-Vroman (1986), in effect, that only a "pared-down version of UG" is accessible to the adult as a residue, essentially those options instantiated in the L1. Thus, L1 acquisition can be viewed as development (cf. Chomsky's distinction between development and acquisition) and adult SLA as learning, with the possibilty that "cognitive systems" or general learning strategies play a role. Schachter is particularly interested in the role of negative evidence since positive evidence obviously underdetermines parametric resetting in a number of domains.

As Schachter has indicated, it may well turn out that UG is only peripherally related to adult SLA. She is certainly right to point out that there are certain facts about L1 acquisition and adult SLA staring us in the face - facts which are too often ignored in the general rush to establish identity in the two processes (see Dulay and Burt 1974 for one egregious example). However, like many others in the field, she seems to want to jump to

conclusions about the general approach on the basis of a few fragmentary studies addressing a few hypotheses. As in all SLA research, it would be more prudent to wait until the facts are in - or at least until the theory-building process has gone further - before veering off course.

On the other hand, Schachter is not alone. As indicated, Felix (1985), Clahsen and Muysken (1986, 1989) and Bley-Vroman (1986), among others, have also raised questions about the availability of UG in the second language acquisition process. Bley-Vroman (1988), for example, surveys the L1 and L2 literature and arrives at ten characteristics of adult foreign language learning. Four of these are adopted by Schachter (1988; see above); the others include variation in success, course, and strategy; variation in goals; the correlation of age and proficiency; the indeterminate status of intuitions; the importance of instruction; the role of negative evidence; and the role of affective factors. Bley-Vroman concludes that "the domain-specific language acquisition system of children ceases to operate in adults" and that "adult foreign language acquisition resembles general adult learning" (25). This is commonly referred to as the "fundamental difference hypothesis" (1986: 10). He goes on to speculate that the adult foreign language learning armamentarium may include L1 knowledge and "general problem solving systems" (26). These components, taken together, enable the learner to "'reconstruct'" UG - principles of constituent structure, recursive embedding, thematic roles, syntactic structure generally, etc. - by means of the L1. Indeed, he goes so far as to echo, though not advocate, the view that principles which define a "possible language" may still be available though the ability to order input for constructive purposes may not be, a variant of the so-called "no wild gramnmars" view (see White 1990). While these speculations are interesting and principled, they do not in their cumulative effect necessarily put the SLA/UG enterprise on a permanent side-track. What will do that is the consistent failure of hypotheses that have been formulated on the assumption that UG has a role to achieve confirmation. Nonetheless, it is quite clear from this and other surveys that adult language learning is not the same as child language acquisition in its overall shape and direction.

In an expanded version of this thesis, Bley-Vroman addresses what he calls, following Chomsky, the "logical problem of foreign language learning" (Bley-Vroman 1986), in other words, the failure of the input to fully determine the acquisition of a foreign language. In this case, he traces four alternative explanations to the thesis advanced above and evaluates them with reference to the literature. Two that have a direct bearing on this dissertation are the "L1 interference hypothesis" and the "competing cognitive systems hypothesis" (10); the other two involve input and affect. With respect to the first of these, Bley-Vroman simply repeats the frequent claims that "interference" is predicated on a notion of proactive inhibition associated with the behaviorist tradition, that there are methodological problems with the error studies (having to do, for example, with the assignment of exemplars to causal categories) and that many errors cannot be attributed to L1 interference. With respect to the second, he relies primarily on the work of Felix (see 1985, for example) and his claim that the general problem-solving capacity of adults interferes with acquisition: in other words, adults suffer from a surfeit of cognitive systems. Bley-Vroman attacks this hypothesis on four grounds. He claims that good problem solvers are not necessarily poor language learners (as one might predict from the hypothesis); that if motivation and attitude affect only this problem-solving capacity (and not the unbridled language acquisition device, as Felix alleges), low motivation and poor attitudes could then be expected to result in successful foreign language learning; that Felix' claim does not adequately account for adolescent success; and that the idea of competing cognitive systems runs counter to common assumptions about human phylogenesis.

Having dealt (somewhat summarily) with these competing claims, Bley-Vroman then looks at three areas "where actual evidence has been held to show that foreign language learners have at least some access to the same sorts of mechanisms available in child language development" (13), the third of which is (primarily Felix') research within a GB framework. Since this is summarized below, there is no need to go into it here; suffice it to say, the 48 native speakers of German learning English performed better than chance (about .70 overall)

on the judgment task Felix administered. This has been interpreted as evidence of UG availability among these learners. Bley-Vroman, however, takes the view that this outcome can be explained in any one (or more) of four ways: L1 analogy, the availability of rich data, "learning of UG consequences as peripheral facts," and parsing difficulty. With respect to the first of these, he suggests that German (the subjects' L1) conspires to give the learner the necessary information for English by providing comparable contrasts, though they lack strict equivalence and/or an identical origin (i.e. in UG). With respect to the second, he raises the issue of negative evidence in adult SLA (see also White, forthcoming), which, despite its ubiquity, continues to be controversial. With respect to the third, he suggests that certain linguistic facts can be learned piecemeal fashion and still give the cumulative impression that the parameter setting has been swallowed whole; he offers apposite examples from the work of White (1985) on pro-drop, Munoz-Liceras (1983) on preposition stranding and the author on the Coordinate Structure Constraint to support this analysis. Finally, with respect to the fourth, he suggests that learners (or indeed native speakers) impose coherence on the comprehensible scraps in a generally incomprehensible sentence in a way which is sometimes mistaken by researchers for accuracy of comprehension. With all of these arguments, Bley-Vroman attempts to give the flavor of what he sees as the general complexity and variety of adult language learning and restrain the precipitant tendency of SLA researchers to look for unitary UG-based explanations. No one who has read the literature could seriously differ with this attempt.

Nonetheless, Gregg, with his customary panache, comes to the defense of parameter-oriented SLA research in a wide-ranging and somewhat speculative article (1989b). After situating SLA research in relation to the whole theory-building enterprise, and clarifying certain terms, he advances three reasons why "a generative theory of grammar" is both necessary and useful in SLA: it provides rigor, clarity and explanatory power; it gives SLA a "sense of direction"; it helps delineate a "division of labor" in the field. On this first point, he

attacks atheoretical infatuation with discourse, sociolinguistic notions of variable rules and "capability continua" (see Tarone 1985 and below) and typological universals on the grounds that these approaches lack the necessary formalism and/or preoccupation with issues of competence. As he says:

> ...when White, for instance, talks about the "pro-drop parameter" (1985), she means something much more specific than that in Spanish a pronoun subject can be deleted. Whether White is correct about the acquisition of English by Spanish speakers, she cannot be accused of using an ad hoc term. The pro-drop parameter is part of a larger and well-motivated theory, and stands or falls with that theory. And this is right, for what we want in an SLA theory is not simply rigor - although God knows we want that - but explanatory power, and it is just that ability to appeal to a related, well-developed theory that allows us to explain phenomena in the domain of SLA.
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> (1989b: 34)

In other words, this approach is good because it addresses the central issue of how languages are mentally represented and not just trivial issues of performance. Whereas Bley-Vroman is concerned to trace the failure of parameter theory to explain phenomena that fall outside its province (see discussion on system-external variables below), Gregg is concerned to rescue parameter theory for what it can contribute, which is certainly not the whole picture.

In much of the general commentary on this area of research, there is a continual tug-of-war between those who, on the one hand, want to strike off in new directions (and sue for a divorce from linguistic theory altogether) and those who, on the other, seek constantly to remind us of the fundamental orientation of contemporary linguistic theory; between those who want to rush into print with conclusions that are much too broad and those who, on the other, want to abandon the whole enterprise on the grounds that it is unlikely to provide all of the answers; between agnosticism and orthodoxy. Somewhere among these extremes, there is room for a careful consideration of the issues in the light of what we know from the empirical literature.

2.2 SLA and Syntax: Empirical Studies

Several researchers have looked at the relationship between second language

acquisition and UG.

2.2.1 Ritchie (1978b) opened the debate by examining Ross' (1967) Right Roof Constraint, now subsumed by subjacency, in the context of adult SLA. The constraint (affecting the movement of elements at the level of S-structure) is assumed to be part of the child's armature and is, conceivably, lost with age. Essentially, he wanted to know if the adults he tested had retained or lost this constraint, which forbids the rightward detachment of elements from their matrix, i.e. original, clauses, as in *That a gun went off surprised no one which I had cleaned. To do this, he asked 20 Japanese graduate students and faculty members and six controls to indicate whether 120 pairs (!) of sentences were equally grammatical or not and, if not, which of the two sentences was grammatical. The list was carefully contrived so as to eliminate various alternative strategies, and Japanese informants were chosen because Japanese does not contain rightward movement rules while English does (cf. A gun went off which I had cleaned). Since the study was undertaken when the principles-and-parameters framework was a mere gleam in Chomsky's eye, the constraint is described as being "incorporated a priori in the grammars/strategies" of English L1 learners over the course of "an early period during which the constraint is violated in production, followed by a process of refinement leading eventually to production conforming almost entirely to the constraint" (1978: 37). Ritchie found that most of his informants confirmed retention, though he neglects to report the significance of this result in precise terms ("the probability of this result is extremely small"). Unfortunately, it is also impossible to disambiguate the relative effects of transfer (which would incline the informants to avoid rightward movement altogether), success (which would stem from purposeful intake) and retention with any precision in this study. In any case, the issue is the presence/absence of a constraint associated with English/L1 acquisition, not the comparative effects of L1 parameter settings and UG within a markedness framework (see Johnson and Newport 1988 below for more on the relevance of Lenneberg's critical period hypothesis).

More recently, White (1985b) has studied the role of subjacency in the process with a view to determining whether it constrains hypothesis testing or not. In this case, she administered a grammaticality judgment-cum-correction test to 73 adult students of English, 54 of whom were native speakers of Spanish, 19 of whom spoke French. They represented various levels of English proficiency and, presumably, various periods of exposure to English; there were 11 native controls. Since S is a bounding node in English (as are S' and NP in some analyses)[13] but not in Spanish and French, these learners were going from more marked languages to a less marked language, assuming that the least marked setting is the most restrictive, i.e. all nodes are bounding, and only one bounding node can be crossed. [Of course, an argument could also be made that the least marked setting is the least restrictive, i.e. no nodes are bounding and movement of any element anywhere is permissible. However, the assumption here and elsewhere (Chomsky 1980) is that bounding, i.e. restriction, is the rule across languages and that the more bound movement is in a language the more universal are the properties of that language. Even if the opposite view were taken, White's findings would still not be very convincing, as we shall see.]

What she discovered, after coding the responses, was that 34% considered S to be a bounding node, 25% didn't and 41% were "indecisive"; indecision occurred at all levels of proficiency in the L2. In short, these figures could be cited to support virtually any position, though with little prospect of convincing anyone. Nonetheless, White interprets them as support for the notion that hypothesis testing is not unrestrained and the proposal (Felix 1982) that among adults there are two competing acquisitional systems at work, general problem-solving and UG.

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13 Chomsky (1981a, 1981c) claims that bounding nodes for all languages are S' and NP and that parameter settings vary as to whether or not to include S as well. Sportiche (1982), in accord with Rizzi (1982), claims that English has S and NP, French S' and NP, so parameter settings vary re. whether or not to substitute S for S'. The issue is of course still unsettled.

Still more recently (1988), Bley-Vroman, Felix and Ioup investigated the ability of native Korean speakers to make judgments about sentences exemplifying subjacency constraints and the Empty Category Principle (ECP). Koreans were chosen because their language does not instantiate the properties of English where these principles - which involve contraints on Wh movement and proper government within, in this case, Wh questions - are concerned. Subjacency of course involves the familiar claim that, in English, only S and NP are bounding nodes (in this study; see above) and that movement can occur across only one of these; in the case of this study, the sentences presented for judgment exemplified Wh islands, factives and relative clauses. The ECP (which applies at the level of Logical Form; cf. subjacency, which applies at S-structure) requires antecedent government (see Kayne 1984) for vacated departure sites; in this case, the test sentences exemplified subject/object assymmetries, PP/adverbial islands and the SSC (the Specified Subject Constraint). As for Korean, since there is no movement required for Wh question formation in that language, subjacency relations simply do not apply. The ECP, by contrast, does apply in Korean (though in a slightly different way), so the design of this study permits a clear-cut comparison of principles which are both likely and unlikely to have been absorbed by these learners as a consequence of transfer. Of course, given this design, the subjects' readiness to embrace subjacency (as it is played out in English) and/or their failure to impose Korean assumptions about ECP, to the extent that these phenomena exist in the data, could both be interpreted as success in the second language[14] independent of the factor of UG. For that matter, an apparent reluctance to accept subjacency relations (regardless of what happens with respect to the ECP) could be interpreted as evidence of transfer. In fact, these researchers go so far as to say that even if the data can be construed to show transfer in the ECP test, "the transfer

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[14] The term "success" is used here in the informal way it is commonly employed in the literature, i.e. to mean a positive consequence of system-external factors. Needless to say, success (to some extent) characterizes virtually all adult SLA; the question is what the source of this success is.

process itself could be argued to require access to abstract concepts of Universal Grammar, such as Logical Form and the ECP" (14). This view is what is commonly referred to as having it both ways.

In the event, the 92 subjects (and 34 native-speaker controls) were presented with 32 randomized sentences exemplifying the properties described above for judgment on a three-item scale (possible, impossible, unsure). The data were then submitted to three analyses (overall, individual sentences, within subject), with the result that the authors conclude that "there was significantly better than chance performance of the non-native speakers in almost all cases" (26). In the process of their analysis, they uncovered a tendency on the part of these subjects to achieve slightly lower scores on the ECP than on subjacency (.722 vs. .778) and to reject ungrammatical sentences significantly more often than they accepted grammatical sentences. The first of these facts suggests that transfer was less of a factor than anticipated (although there are no operationalized hypotheses reported in this study); the second suggests that "the strong form" of the "fundamental difference hypothesis" (i.e. that UG is inaccessible to adult foreign language learners; see above) cannot be sustained. As I have indicated, however, these results could conceivably be interpreted to support an inference of success. In any case, in an heroic effort to avoid retreat from the broad notion of a "fundamental difference," the authors then trace a couple of possible explanations for the results that exclude UG [the subjects had had some (formal) exposure to English as children; processing constraints may have played a role] but reluctantly conclude that no, "adults appear to have some sort of access to...UG" (27). Finally, they ask the question: If UG is a factor, then why didn't these subjects do as well as the native-speaker controls? In response, they suggest that UG may be available only in an attenuated form or that Felix' "competing cognitive systems" idea (see above for accounts of both of these notions) may account for the outcome. Although the study is conceptually sound and thorough in its procedures, it is hard to avoid the conclusion that there are alternatives to the notion that these subjects' accessibility to UG caused what looks

like a tendency on their part to elude the the influence of the L1.

While considering the issue of "completeness," i.e. the attainment of native-speaker competence in adult SLA, Schachter (1989b) took a look at subjacency. In this case, she first summarizes two general hypotheses relevant to the availability of UG: UG is fixed full-blown in the head of the child from the beginning and is thereafter constantly available (the "continuity" hypothesis) or UG matures (or develops) as other parts of the brain do and is available in stepwise fashion (the "maturational" hypothesis). Consistent with the work of others (see Johnson and Newport 1988 for discussion of a similar hypothesis), she then proposes a third option: UG matures, but there are preceding and succeeding stages at which it is unavailable to the learner (the "window-of-opportunity" hypothesis; see chapter six). Where adult SLA is concerned, the first two of these hypotheses are conflatable and together contrast with the third. That is, if UG is fully available to the adult, that fact is consistent with either of the first two; if the parameter setting associated with the L1 is the only setting available, that fact is consistent with the third. Schachter favors the third.

In order to investigate this question, Schachter asked 79 informants and 19 native-speaker controls to render grammaticality judgments (four-item scale) on 66 sentences that included examples of Wh Movement, grammatical and ungrammatical "syntactic constructions" (sentential subjects, noun complements, relative clauses, embedded questions) and distractors. The informants were Dutch (tested in Holland; see below), Indonesian, Chinese (?) and Korean; where subjacency is concerned, Dutch patterns like English, Indonesian and Chinese show partial evidence of subjacency and Korean has no subjacency at all. What she found, overall, is that the Dutch informants did significantly better than the other groups and that native language (as opposed to age at first exposure, number of years of study, months in an English-speaking country or age) accounted for about 43% of the variance on this measure, by far the largest share. This suggests that the availability of UG is indeed severely limited among adults, transfer is paramount and the "window-of-opportunity" hypothesis offers the best fit.

This issue is taken up in chapter six below.

2.2.2 Other researchers have looked at the pro-drop parameter. Phinney (1987), for example, used production data in a comparison of adult students of English as a second language (in Puerto Rico) and adult students of Spanish as a second language. Her assumption, following Chomsky (1982) and Hyams (1983), was that the setting associated with Spanish, i.e. null subjects, was less marked than pronoun realization. As a consequence, she predicted that it would be harder for the Hispanophones to acquire the English rule system than for the Anglophones to acquire the Spanish system, consistent with the the assumptions of this proposal. Although no tests of significance are reported in Phinney's study, this is apparently what she found.

White (1985a) also studied the pro-drop parameter among adults learning English as a second language in Canada at several levels of acquisition. Three-quarters of the subjects were Hispanophone, and the rest, who served as controls, were Francophone Canadians; thus there were considerable differences between the two groups in terms of their exposure to the L2. In addition to null subjects, White looked at associated effects, subject-verb inversion and that-trace, on a grammaticality judgment task. Predictably, she found evidence of transfer among the Hispanophones, particularly at the lowest levels, on the null-subject items; since she does not report significance levels for the difference between the two groups on all items, it is impossible to draw firm conclusions about differential parameter settings from this study. More importantly, she uncovered little evidence that pro-drop was linked to its putatively associated effects: there were no significant differences between the experimental group and the controls on those measures. She also found little evidence that her Francophone subjects were reverting to an "open" setting in the process. Her conclusion - a classic case of trying to have it both ways - is that the study shows that UG accounts for differences as well as similarities in the acquisition processes of speakers of different native languages. It is hard to know what this has to do with UG.

Hilles (1986) studied the transcript of a 12-year-old Colombian who had been a subject in an earlier study (Cancino, Rosansky and Schumann 1978) with an eye on pro-drop. Data had been secured by means of spontaneous conversation, games, experimental elicitation tasks and "preplanned sociolinguistic interaction," i.e. museum trips and the like. Hilles found, as one might expect, that the subject's pro-drop decreased as he began to load lexical material onto AUX, e.g. have, be, modals; there was also some, though inconclusive, evidence that his use of expletive it and there triggered the process. Hilles interprets these findings as support for the notion that certain effects emerge in tandem or cluster, as one might expect them to do if some kind of "projection device" (Zobl 1983, 1984) plays a role.

2.2.3 Still other researchers have looked at preposition stranding. On the assumption that preposition stranding was more marked than pied-piping, Mazurkewich (1984a, 1984b, 1985) asked 45 native speakers of Canadian French and 38 speakers of Inuktitut, at three levels of English proficiency, to form questions using who or whom from written input containing dative phrases. Her assumption apparently was that preposition stranding would be acquired later in the sequence than pied-piping: she doesn't seem to have taken differences in the two L1s or, indeed, French dialectal differences into account in the design of her study. What she found, in any event, is that stranding did come later, though the Inuit informants produced (significantly?) more stranding than her French subjects despite the lack of prepositions in Inuktitut. She attributes this difference to their longer exposure to English. Whatever the reason, there are serious flaws in the design of this study having to do, primarily, with a failure to exploit differential parameter settings in the informants' L1s and the introduction of such potential confounds as differences in dialect, L2 exposure and age (her native-speaker controls were significantly younger than her informants). See Kellerman (1985) for more on this study and its problems.

Bardovi-Harlig (1988) gathered production data from two elicitation tasks in a cross-sectional study of preposition stranding among 95 college-age students from a variety of

linguistic backgrounds at various levels of English proficiency. Since the predisposition conferred by the subjects' L1s was not analyzed, apart from determining that none permitted preposition stranding, Bardovi-Harlig was looking at the gross contribution of markedness to the process in the expectation that all subjects would naturally prefer the unmarked structure, i.e. pied-piping. Specifically, she asked her subjects (a) to form questions using who or whom (see Mazurkewich 1984a, 1984b, 1985) and (b) to complete a sentence by making a relative clause with who, whom or which; she ignored the "No-Prep" option for the most part. What she found, in contradistinction to Mazurkewich, is that the marked construction, i.e. preposition stranding, was acquired before the unmarked construction. She goes on to suggest that "salience," by which she means the presence of stranding in the input, may account for this acquisitional sequence and that its influence, when compared to that of markedness, may be paramount.

More recently (1988), van Buren and Sharwood Smith have provided, as a jumping-off point, a more sophisticated discussion of the syntax of preposition stranding. In essence, they describe stranding as an "epiphenomenon" that embraces a number of syntactic processes; this fact provides a basis for its designation as marked. But they also consider the paradox of its infrequency in the languages of the world and its surprisingly early emergence in the acquisitional sequence of English acquisition and suggest that the "extra machinery" required for its syntax may facilitate sentence and discourse processing. In their remarkably inconclusive study (among other things, there was "a clerical error" in the administration of the tests and no tests of significance are reported), they compared, in Holland, 43 university students in their second and third years of English study and 27 "school children" who had had one year of study. The university students were given two acceptability tasks, one contextualized and one decontextualized, and the school pupils were given three tests requiring them to assemble questions, make acceptability judgments and rearrange "jumbled sentences." The third test in this latter series was also administered to 46 second-year university students,

and their scores were compared with those of the school pupils on that test. The researchers were unable to reach a decision on the issue of "back-to-UG" v. "crosslinguistic influence" from these data, but the results were sufficiently robust to suggest that these learners were not simply importing the preponderance of stranding in the input (in Dutch and English) wholesale. Instead, they exhibited a variety of strategies, including pied-piping, promiscuous stranding and crosslinguistic stranding. van Buren and Sharwood Smith conclude that both the GB framework and markedness increase the precision with which SLA issues can be framed, though you couldn't prove it from this study as reported. They also discovered that their subjects provided "many more wrong rejections than wrong acceptances," with possible methodological implications (see below).

Most recently, Klein (1990) has looked at what she calls the "null-prep" phenomenon and situated it in linguistic theory as an unmarked default option available in several disparate languages. Thus, it has a respectable pedigree and its emergence in SLA data can be cited as evidence of the influence of UG.

2.2.4 Another popular area of research is relative clauses. The early studies, in this regard, stem from an interest in transfer effects and error analysis (or the so-called aposteriori approach to contrastive analysis). So, for example, Schachter (1974) analyzed 200 free compositions from four language groups (Persian Farsi, Arabic, Chinese? and Japanese) at two levels of English-language proficiency in an attempt to discover whether they were transferring the L1 pattern or not. Following Keenan and Comrie (1972), she had anticipated that differences in the position of the head noun, subordination markers and the use of resumptive pronouns or pronominal copies between these languages and English would trigger transfer effects; she expected the speakers of Chinese (?) and Japanese to have the most difficulty with relative clauses for a variety of reasons. In the event, she discovered that these learners made significantly fewer ($p < .025$) errors than the others, though they also made far fewer attempts to use relative clauses. She interprets this evidence of avoidance as support

for apriori contrastive analysis since this procedure comes closer to predicting the outcome than a simple error count. She doesn't directly address the issue of UG.

Following Schachter, Gass (1979) conducted an extensive but somewhat scattered study of relative clauses that touches on the question of universals, in this case typological universals. Gass gathered data from 17 high-intermediate-to-advanced adults from nine language backgrounds, including the four that Schachter had investigated, on two tasks: acceptability judgments and sentence combining. One of her aims was to determine the relevance of the Accessibility Hierarchy devised by Keenan and Comrie (1977), which she interprets as a universal, and in fact she found support for its alleged universality (though the genitive proved easier than expected). By comparison, she uncovered little evidence of transfer, except where pronominal copies were concerned. Overall, she favors the view that "both target language facts and rules of universal grammar" (343) should be taken into account in transfer studies. Her study is valuable because it reports levels of significance derived from both an analysis of variance and a t test; on the other hand, Gass' definition of universal is different from the one employed in (more recent) research within a GB framework, which limits its comparability.

Hyltenstam (1981a) also studied pronominal copies in relative clauses in a Greenbergian framework, i.e. with reference to the frequency of certain structures in the world's languages and Greenberg's extension of markedness to include lexicon and grammar. In this case, he used elicited production from 45 students of Swedish whose L1s were Persian Farsi, Greek, Spanish and Finnish. Some of these L1s permit resumptive pronouns, and some do not; Swedish apparently does not permit them. In any case, they emerged in the students' interlanguages, whether their L1s contain them or not, more or less in accordance with Keenan and Comrie's (1977) Accessibility Hierarchy (see above). Hyltenstam concludes from these facts that both markedness conditions and the subjects' L1(s) provide the best basis for a prediction of interlingual behavior. His study did not attempt to sort out the contributions

of both factors because he did not see them as "mutually exclusive" (65).

Zobl (1983) reanalyzed Gass' (1979) data and contributed some of his own in an effort to define markedness in the context of SLA. His interest is the process whereby students are able to infer rules from impoverished input by means of recursion. Zobl's notion of "projectability" - the expanding productivity of simple rules and their extension into new areas of the language - prefigures the "projection device" of subsequent articles (see above). Zobl's claim is that the least marked structures are the most productive in this sense, and he specifies three criteria for determining a structure's degree of markedness: its accessibility, the amount of exposure to the new structure required and the number of rule revisions it requires. If, as is alleged elsewhere (see Keenan and Comrie 1977, for example), a structure's accessibility is itself determined by its lack of markedness, then Zobl may be burdening markedness with unnecessary layers of definition and evading the issue, i.e. what makes some structures more accessible than others in the first place. On the other hand, he is certainly taking a first step toward redefining markedness in psycholinguistic - and not simply formal - terms or, more precisely, incorporating markedness into a psycholinguistic theory of SLA.

2.2.5 In a somewhat related study, Schmidt (1980) addresses the issue of whether, as Adjemian (1976) claims, interlanguages can be described as natural languages by examining the ILs of nine subjects (L1s: Japanese, Mandarin, Finnish, German, Arabic?). For this purpose, she used data from tasks requiring description of a picture, imitation, sentence combining and grammatical judgment. Her focus was extractions from coordinate structures permitted in SVO languages (English, German, Finnish, Mandarin) as well as SOV (Japanese) and VSO (Arabic) languages; her aim was to determine whether these learners conformed to or deviated from a "natural" pattern regardless of the predisposition conferred by their L1s. What she found was that their word orders coincided very closely with those of the L1; since these are all natural languages, she concludes that the students' ILs are indeed natural languages and obey what she calls "universal constraints" on extraction. Since, however, she

stuck to constraints on surface word order and, in any case, found considerable evidence of "negative transfer," the study has little relevance for the issue of UG's role in the process. She also had some problems with inconsistency across tasks and between tasks administered in isolation and in combination (see Tarone 1982 for further discussion of task types). These problems make it hard to know exactly what was going on in these informants' ILs, though the bottom-line issue of their naturalness would seem to be resolved.

In what is generally considered to be an important study, Clahsen and Muysken (1986) looked at the word order assumptions of children and adults acquiring German, a verb-final language, as an L1 and L2 respectively. What they discovered, in sum, is that, whereas children grasp German's underlying verb-final character virtually from the start, adults (i.e. these gastarbeiter whose L1s were Romance languages and Turkish) persist, at least across two of six stages, in the SVO assumption (regardless of their L1s). This suggests to these researchers that "language acquisition by adults involves general learning strategies, while principles specified by the LAD operate in L1 acquisition in addition to general learning principles" (94), though it could as readily suggest the universality of SVO. (See discussion of Schachter 1988 above for more on the overall shape of the process relevant to this issue.) Unfortunately, since the study made use of naturalistic production data, it is hard to know whether the perceived pattern bears any relationship to the issue of a role for UG or not: as Gregg (1989; summarized above), among others, indicates, the issue for SLA is competence, not performance. White also makes this point in her review of the study: "Offering an alternative processing principle that cannot in fact explain the acquisition of an SVO order, though it might explain the use of it once it has been acquired, does not help to settle the issue of whether or not UG is still active in L2 acquisition" (1990: 104; italics hers). In any case, it seems unlikely that this study could be cited to claim, as Clahsen and Muysken do, that adult L2 learners formulate "unnatural" rules in the acquisitional process since German has a variety of surface-level word orders, including both SOV and SVO. "Unnatural" reliance on one

or the other could easily be attributed to the input. The claim of Clahsen and Muysken that it is the input iteslf which leads the learner to formulate "unnatural" rules, i.e. by means of a processing strategy which feeds on input, leaves us with the logical problem of interpreting success as failure to grasp an underlying "canonical" order which is everywhere belied. The marked character of German where head position is concerned, together with certain other problems which White (1990) discusses in some detail, make a clear-cut comparison of the contributions of various factors to the acquisitional process all but impossible in this study. In fact, White suggests in her summary that the unwarranted assumption that German is consistent in head position and the evidence of head initiality in the specifiers and complements of NPs and PPs conspire to create the effects observed and that this explanation is consistent with a role for UG (See chapter six for more on these issues).

More recently, du Plessis, Solin, Travis and White (1989) took a second look at both the syntactic assumptions (with reference to Chomsky 1986) and the acquisitional stages Clahsen and Muysken employ. In their analysis, simple inversion is not alleged to take place in L1 grammars as Clahsen and Muysken claim; rather, double movement takes place only in structures that are nonsubject initial, in which case the topicalized element (e.g. das brot below) moves to the specifier position while the inflected verb (i.e. haben) moves first to INFL and then to COMP, resulting in a sentence like das brot haben die kinder gegessen. It is the ECP, i.e. the requirement that even empty categories be properly governed, which compels this analysis. They go on to reanalyze Clahsen and Muysken's stages with these assumptions in mind and arrive at a scheme which depends crucially on the tendency of learners to move finite verbs into INFL in embedded clauses to avoid an ECP violation; this looks like SVO but isn't. These researchers then asked 28 students of German at McGill (L1s: English, French) to write compositions; direct comparison with the learners studied by Clahsen and Muysken was of course not possible because of differences in the L1s and acquisitional conditions. There were almost no errors (6.44%) in these compositions with respect to verb placement, so little

can be inferred from the study, which nonetheless does not keep du Plessis et al. from trying; "there may be multiple routes to the acquisition of German" (71), however, is the best they can do. Finally, citing some research du Plessis had done with five native English speakers who also knew Afrikaans (involving a judgment task and structured conversation), they conclude that these subjects "know that the inflected verb in Afrikaans has to be fronted from the underlying final position but do not yet know that COMP functions as a proper governor" (73). This analysis (some of it rather arcane and speculative), together with these data, are then cited as evidence that "the interlanguages of L2 learners fall within the range of grammars permitted by UG, rather than being unnatural" (74). As the study is long on analysis and short on interesting data, it is hard to agree with this optimistic conclusion. It is, in sum, more of an argument by assertion, a sort of force majeure, than a carefully conceived effort to test out certain hypotheses. It is valuable, however, because it is a healthy reminder that many of the theoretical assumptions that underlie this kind of research are themselves subject to revision and as likely to subvert as support it. The assumption of Clahsen and Muysken that a particular analysis of L1 German acquisition was more or less correct may, as it turns out, have been naive.

Clahsen and Muysken, in a recent survey (1989) of the data on the acquisition of verb placement, verb inflection and negation, as well as the comments of some of their critics, in fact acknowledge that the analysis of du Plessis et al. "will yield the right developmental sequence". "As it stands, however," they go on, "the 'COMP as proper governor' parameter seems to us rather an ad hoc way of accounting for the final stage of L2 development" (17). Their criticism centers on the notions that its implications, its triggering mechanism and its connection to differential age-related effects are still not clear. They also reject the claim of several of their critics (e.g. du Plessis et al.) that the SVO bias evident in the early acquisitional stages of their subjects is due to a transitional flirtation with transfer ["Many factors make L2 acquisition different from L1...including the fact that L2 learners already

know another language." (du Plessis et al. 1989: 57)]. In this case, they argue (not very convincingly) that four other studies involving a total of 14 native Turkish speakers contain "clear cases of SVX but no unambiguous cases of SXV" such as one might expect if transfer were influential "since Turkish is dominantly SOV, and German (as well as Dutch) certainly contains many OV patterns" (14). Unfortunately, as indicated above, the presence of an OV pattern in the input - now acknowledged - may account for its appearance in these learners' output, as indeed residual dependence on the L1 may account for its emergence among the speakers of Romance. Furthermore, no data on age or acquisitional condition are given for these studies, they involve the learning of Dutch and German and the subjects appear to have been at all levels of L2 proficiency, so it is hard to know how to evaluate their relevance to the issue.

Clahsen and Muysken also take on the issue of "restructuring," i.e. from stage to stage in the developmental sequence, and deal persuasively with several of their critics who have proposed alternatives to their schmatization; since these do not bear directly on the topic of this dissertation, there is no point in going into them here.

Finally, however, they discuss Felix' notion of "competing cognitive systems" (see above), which, needless to say, given their assumption that UG is "coterminous with maturation," they are unlikely to find appealing, and in fact they do not disappoint. They attack Felix on the predictable grounds that, if there were in fact two competing systems (one of which encodes the principles of UG), then one would naturally expect to see more, though perhaps not preponderant, evidence of an underlying SOV word order than was actually produced by their informants. In something of a contradiction, they then say that it is hard to imagine exactly what sort of evidence could distinguish between Felix' proposal and theirs. Finally, they (somewhat obtusely) suggest that it is unclear why the availability of additional strategies would make it harder for adults than children to acquire German word order in the first place. Although these are not serious criticisms of Felix no matter how you look at them,

the general review is helpful in focusing the debate.

In short, as Clahsen and Muysken say, "no attempt to give a UG-compatible analysis of the L2 sequence so far adequately accounts for the data" (22). Then, shifting gears, they take up the question of how to reconcile their preference for processing and general problem solving with the idea that UG is available to L2 learners and essentially embrace the view that "a set of stable principles" is still available to the adult although the parameter-setting capacity of the child is not. This set of principles, of course, is instantiated in the learner's L1. In short, while L1 development follows a predictable path which is consistent with parametric variation (there is evidence of radical restructuring, for example), adults "come up with linguistic systems which are not consistently defined by a specific option of UG" (25). Rather, all the evidence, to the extent that it suggests the availability of UG, is consistent with its availability in and through the L1. Clahsen and Muysken do, however, see some hope for the subset principle (Wexler and Manzini 1987; see above), i.e. its apparent failure to operate in L2 acquisition (see White 1989b), as a basis for reconciling their position and that of those who favor UG-based models.

Flynn (1984, 1987a) and Flynn and Espinal (1985) have also looked at word order universals - specifically, the head-direction parameter and related topics such as anaphora and adverbial clause position - in a number of studies. Flynn's assumption, consistent with Zobl's projection device, is that effects cluster: once the learner has determined the typological category of a particular language and reset his parameters, his acquisition of related phenomena is accelerated. Collectively, these studies present data from Japanese, Spanish (?) and Chinese (?) speakers, for the most part from an imitation task involving when clauses with and without anaphora (both backward and forward; pronominal in the when clause). The researchers' assumptions were (a) that the position of the adverbial clause containing the anaphor in the sentence, either before or after the matrix clause, reflects the principal branching direction (PBD) of the language and (b) that, after aural input, "active (?) repetition

of the stimulus reflects input of the sentence to both the S's comprehension and production systems" (1985: 101). Both of these assumptions are somewhat dubious, as we shall see.

In Flynn (1984), two hypotheses are addressed, essentially that a learner of English coming from a language whose PBD mismatches the PBD in English would have more difficulty than one whose L1 is not a mismatch with English. Without going any further, it is obvious that these hypotheses do not explicitly address the issue of UG: if they are confirmed, they are consistent with the notion of transfer; if disconfirmed, they are consistent with a role for UG or input or, indeed, the view that the PBD is an irrelevant construct in SLA. Thus they tackle UG implicitly since disconfirmation would at least eliminate transfer as the dominant factor and, presumably, lead to further study and more refined hypotheses.

In any event, Flynn tested 51 Spanish speakers and 53 Japanese speakers; Spanish, like English, is principally a right-branching language, Japanese is principally left branching. Unfortunately, Flynn gave both groups the grammar and listening comprehension sections of the Michigan English Placement Test to control for her subjects' English proficiency levels; these tests were inappropriate for a study involving "active repetition." To get around this problem, she also administered an imitation task involving juxtaposed clauses devoid of PBD apparatus, i.e. to ensure that her subjects were able to reproduce relatively long strings (10 words, 15 syllables). She then introduced these results as a covariate in her factorial design.

What she reports, in her summary of four (!) test sentences, is that indeed the Japanese subjects at the "intermediate" and "advanced" levels had significantly more difficulty with this test than did the Spanish speakers regardless of the directionality of anaphora. The Spanish speakers also evinced a significant preference for forward anaphora, which Flynn interprets as evidence that the PBD constrained their "hypotheses about anaphora" (1985: 82). In other words, the Japanese simply couldn't get the overall task right very often while the Spanish speakers not only got it right more often but also tended to get the sentences containing forward anaphora right more often than those containing backward anaphora. [In

a subsequent paper reporting on the same data, Flynn (1986) apparently found that the "advanced" Japanese learners were also significantly better at imitating right-branching when clauses than left-branching; this she interprets as evidence that they were ready to acquire associated effects, though she presents no evidence that that is in fact what they had done.] Unfortunately, 52% of the errors made by the Japanese at the intermediate level on right-branching when clauses represented a failure on their part to provide any of the right-branching material. This suggests that these learners may have been operating with a severe limitation on the amount of material they were able to process and reproduce, i.e. on channel capacity. Nonetheless, Flynn does not forbear from announcing in this study that the PBD "is effective in both first and second language acquisition" (85) and that the lack of a preference for right or left branching in the (low) scores of the Japanese informants indicates the bankruptcy of contrastive analysis as an idea. Be that as it may, many readers have viewed this study, with all of its flaws, as evidence of the effects of transfer, not the influence of UG.

Flynn and Espinal (1985) essentially report the Chinese data, but their report is also dressed up with a lot more syntax. Unfortunately, part of the problem here, as elsewhere, is the fragmentary reporting of these data and the analyses necessary to interpret them. In this paper, the writers are at pains to rescue their study from the claim (Travis 1984) that the differences Flynn had found could more accurately be attributed to a difference in word order. Since Chinese (?) is, like English and Spanish, an SVO language but, like Japanese, head-final, it constitutes an appropriate terrain for the testing of this claim[15]. The essential issue,

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15 Many of the claims relevant to Chinese are controversial in part because the language is hard to classify. Although Flynn et al., following Huang (1982), characterize the language as "substantively" head-final, it also has prepositions. Furthermore, its head-finality suggests a preference for SOV word order which, as indicated, is not borne out. The question then becomes whether the language is underlyingly SOV, and the debate essentially turns on the question of the level at which X-bar expansion can take place and related issues of little relevance to this study. The essential point is that Chinese appears to represent a mixed system in which head-direction and word order operate independently.

according to the writers, was whether or not the Chinese subjects would behave like the Japanese; the question of whether or not they would behave like the Spanish-speaking informants was not taken up.

What they found was that, except for those sentences that contained no anaphora (and then only among the "advanced" students), there were no significant differences between the two groups. While it is true that on the no-anaphora measure there were significant differences, the Chinese informants did significantly better with right-branching when clauses than left-branching, as had the Japanese at that level (and, for that matter, the Spanish speakers at the "intermediate" level) - which suggests a similarity in both groups' general direction. On the other hand, a cursory examination of the analyses reported suggests that there may have been significant differences between the two groups on this measure at the "intermediate" level as well, so it is hard to know exactly what inference can be drawn from this study. Like the Japanese, the Chinese subjects also "had difficulty with embedding in general" (104), by which is meant an inability to reproduce all the appurtenances, e.g. when, of the adverb clause structure. Instead, they converted such sentences to coordinate structures, suggesting a lack of familiarity with the target structure and/or a limitation on the amount of material that could easily be processed for reproduction.

Despite the character of these results as reported, Flynn and Espinal find in them the basis for several far-reaching claims, some of them contradictory, some of them merely sweeping. On the one hand, like the Japanese, "the Chinese do not...map from the L1 onto the L2" (103) since the (low) scores for the "beginning" group do not indicate a preference for the head-final setting, i.e. with left-branching when clauses; indeed, they made many (significantly?) more errors of the coordination type in preposed clauses than in postposed. On the other hand, they were unable "over the ESL levels tested here to very successfully imitate postposed sentence structures both with and without forward pronoun anaphora" (107), indicating residual difficulty with the mismatch in PBD parameter settings between the two

languages. The researchers' conclusion is that the parameter-setting model of SLA "allows one to account for both the role of the L1 experience in L2 acquisition...and the role of principles of acquisition independent of this experience" (108) since they have demonstrated both residual dependence on the L1 setting (in overall accuracy and in the patterns of error) and independence from the L1 setting (in the lack of a preference for preposed when clauses and backward anaphora, consistent with what is alleged to happen in L1/English acquisition). Of course, they also found movement toward the setting associated with English (in the preference among the "advanced" learners for right-branching sentences without anaphora), but, as they do not address markedness conditions in this study, this finding goes uninterpreted except as evidence that the parameter was "available as a guiding principle in the acquisition of other aspects" (96). In short, the somewhat messy and disordered results of this study are cited to support the whole SLA enterprise in all of its variety: the value of the parameter model, the influence of the L1, the underlying identity of L1 and L2 acquisitional processes, the critical role of the head-initial/head-final parameter and the inadequacy of "astructural" processing-oriented approaches.

Despite the patness with which Flynn frames these issues, there is little consensus as to what constitutes the PBD (a term which Flynn and Espinal eschew, by the way, possibly because of a de-emphasis in Chomsky's most recent work on binding) of a given language. English, for example, is considered only predominently head-initial since adjectives usually precede NPs; in Chinese, relative clauses also usually precede NPs, and it has been suggested (Huang 1982) that "languages may parameterize on both the type and level of category" (40) because of this fact. Even if head direction does constitute a single parameter, there is little consensus as to what its associated effects might be. At the very least, it seems unlikely that the position of adverbial clauses in English, which is both permissive and relatively sensitive to discourse constraints in its permissiveness, would be indicative of its PBD. Furthermore, there is little evidence that children learning head-final languages, as Flynn (see also Lust and

Chien 1984) claims, exhibit a preference for backward anaphora, i.e. that this preference is a defining property of the language's PBD (see O'Grady et al. 1986). Nor is there even any longer much interest in directionality as a syntactic construct given the reformulation of binding constraints in Chomsky (1981a, 1985), i.e. the absorption of this construct into various aspects of binding theory. Of course, this fact simply points up an abiding problem with this line of research: the continuous redelineation of parameters and/or the constantly shifting nature of theoretical syntax make it difficult to test out the psychological reality of many of these constructs in SLA. Be that as it may, there is little attempt in Flynn's work to come to grips with these fundamental issues; rather, there seems to be a generalized rush to judgment.

As for methodology, as the summary above implies, there are also a few problems. These are taken up in some detail below. Suffice it to say, they fall into two categories: procedural and analytical (see Bley-Vroman and Chaudron 1988 for a more searching analysis). Procedural problems include Flynn's selection of her subjects and her selection and administration of the tests. The most serious of these concern her determination of her subjects' proficiency levels in English and her use of imitation as a measure of the "input of the sentence to both the comprehension and productive systems" of her subjects. Analytical problems have to do with her use of the juxtaposed-clauses test and the introduction of these data as a covariate to correct for processing differences between the two groups. Her use of this statistic violated basic statistical principles and, in the end, only obscured fundamental differences between the two groups in their capacities to perform the task. Taken together with the theoretical complications, these flaws are so serious as to invalidate the study.

In a somewhat more respectable effort to address similar issues, Eubank (1989) asked 45 Arabic-speaking students (for the most part from the Persian Gulf) to repeat Flynn's test sentences under similar conditions. Since this study is an attempt to replicate the work of Flynn, Eubank employed hypotheses that stem from her findings: (a) overall, subjects will show no significant preference for preposed or postposed when-clauses and (b) only the

intermediate students will exhibit a significant preference for forward anaphora (and no other effects wil be observed). In the process of discussing these and other aspects of his study, Eubank provides a thorough analysis of Flynn's work (see below). In contradistinction to Flynn, Eubank found that his subjects were significantly more successful at repeating preposed adverbial when-clauses than postposed clauses and that there were no significant differences in the preferences of these subjects with respect to anaphora. Thus, his findings contradict those of Flynn and lead, in this study, to speculation about how these results can be accounted for if not by Flynn's assumption of a role for UG (which has, inter alia, considerable theoretical problems). He explores, in turn, an analysis based on "psychotypology," the possible influence of processing constraints and an explanation based on intonation and juncture (parsing) and finds the third of these the most satisfactory. It is, however, not entirely satisfactory for a number of reasons, chiefly its failure to date to be subjected to an attempt at falsification.

This is a very scrupulous piece of work. Flynn's greatest contribution in the end may be the stimulus she has provided for discussion and research around the issues she addresses rather than the quality of her work itself, although White (1990), among others, takes a more favorable view.

2.2.6 Finally, White (1989b) looked at a binary parameter which meets the criteria for the existence of a subset relationship (see above), now elevated to the status of a "Condition". The parameter involves adjacency, in this case, the requirement that adverbs in English cannot come between verbs and their direct objects without interfering with Case assignment (Stowell 1981); French, by comparison, allows adverbs to be placed in this position. English, in other words, instantiates strict adjacency, while French permits a freer setting[16]. Whereas

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16 The current view (see Pollock 1989, for example) is that adverb placement so-called is subsumed under a larger process (accounting for a number of phenomena including negation, clitic placement, etc. in several languages) whereby French verbs move obligatorily to I. The AGRP in this analysis is defective in English but constitutes an inherent barrier in French. This has the effect of licensing post-verbal adverbs in French and restricting them in English.

positive evidence is enough for a native English speaker to acquire the freer French setting, negative evidence (or indirect negative evidence) is presumably required for a native French speaker to acquire the stricter English setting. That is, on the assumption that transfer has an effect. On the other hand, if the Subset Condition is held to influence the process, and UG play its anticipated role, both types of learners should adopt the smaller grammar, i.e. that of English, as an initial hypothesis and gradually expand that grammar, on the basis of positive evidence alone, to accommodate the specific facts of the L2. As these two assumptions predict distinct acquisitional sequences, they can be formulated as alternative hypotheses: Francophone learners of English will either accept sentences like Mary does slowly her homework (a) or they will not (b).

In order to test out these alternatives, White administered three tests to 43 adult Francophone students of ESL: a paced grammaticality judgment test, an unpaced multiple choice grammaticality judgment test and a preference task. The last of these involved presentation of pairs of sentences (see Ritchie 1978b) for relative evaluation and was added to the battery in part because of criticisms that have recently surfaced (see Birdsong 1989) about the use of judgment data in SLA research. In any case, in the results for all three tests there is evidence that the Subset Condition does not play a role in adult L2 acquisition (if it can be said to play a role at all, of course, even in L1 acquisition). On the first two tests, the subjects were very accurate in accepting grammatical sentences, less accurate in rejecting ungrammatical sentences (as they should have done if the condition were operative). On the third test, they were much more likely to judge grammatical/ungrammatical pairs as the same than the native controls, which of course also suggests that they were not 'subsetting' as anticipated. Thus, transfer (in one of its numerous guises), rather than a theoretical learnability condition, appears to have the upper hand.

2.3 Methodological Issues

2.3.1 A number of methodological issues have surfaced with respect to these studies.

These have to do with the use of metalinguistic judgment data in SLA research, the relative value of various task types (including metalinguistic judgments), such procedural issues as the selection of informants, the use of certain statistical procedures, as we have seen, and so on.

Chaudron, who has made something of specialty of methodological issues, has surveyed the metalinguistic judgment literature, including early studies of UG and found several shortcomings. Among these are the "insufficient reporting of results or data, poorly elaborated stimulus materials...[and a] lack of adequate controls" (1983: 367). Comparability across some of the studies surveyed is complicated by differences in the amount of time allotted for the performance of the task and the order in which the stimulus sentences were presented for judgment. However, Chaudron reserves his most severe criticism for the elicitation procedures, measures and means of validation these studies employed. One problem is confusion, significantly on the part of the informant, as to what is being measured (grammaticality? appropriateness?) when a judgment task is administered. Secondly, dependent measures are frequently dichotomous or tricotomous when in fact a wider range is needed for reliability; tasks that require relative rather than absolute ratings are also more effective. Finally, Chaudron claims that studies employing more than one task type or procedure are more informative, though problems arise when these measures are not independent and/or similar in terms of items or subjects. Chaudron concludes, inter alia, that "the cognitive structures and processes of judgment-making" (371) are poorly understood: "perhaps through the provision of controlled types of rule or nonrule representations," he says, "the formation of implicit or explicit knowledge can be manipulated, and their accuracy in use or rate of accessibility can be delineated" (371).

Since Chaudron first raised these objections, considerable work involving such judgments has taken place (see above) with a concomitant refinement of procedures. For example, some researchers (Klein 1990) have asked respondents to indicate their "levels of confidence" in the judgments they render; others have employed act-out (Flynn 1987a) or

picture identification tasks (Finer and Broselow 1986); still others have asked respondents to revise unacceptable sentences, thus raising the specter of hypercorrective response biases (see Arthur 1980). However, by far the most common corrective on the indeterminacy of judgments is the provision of both acceptable and unacceptable sentences for response. This procedure, together with the use of controls, provides a basis for statistical analysis of response patterns to determine their consistency. Although it does not resolve the issue of the "knowledge base," it provides a subtler understanding of underlying assumptions than, say, asking informants to make their assumptions explicit or to choose from among a series of fabricated rule statements.

Since Chaudron 1983, the use of intuitional judgment tasks - whether these involve acceptability, grammaticality or well-formedness - has provoked additional criticism. Kellerman (1985), for example, claims that informants may well apply semantic rather than syntactic criteria in making these judgments or, at least, exhibit "a great deal of variation in what [they] base their judgements on" (99). Birdsong (1989) has also pointed out that subjects may judge sentences according to criteria which are different from the researcher's presumptive claims and fixes especially on problems associated with the use of "semi-literate" informants. Tarone (see below) has also assayed the issue in a series of studies and concluded that judgments alone do not give a comprehensive picture of a subject's competence in the L2. White (1990), while acknowledging that there is a risk of response biases in such data (e.g. the tendency of subjects to accept <u>all</u> sentences regardless of grammaticality), also points out that there are ways around this danger. For example, analyses can be conducted which look at both acceptance of grammatical sentences <u>and</u> rejection of ungrammatical sentences. Unless there is a high correlation between both types of response (as in fact is evident in the data provided in this study: 112), then conceivably the lack of sufficient "metalinguistic awareness" to make

these judgments may constitute an intervening variable[17].

The literature, I hasten to add, is replete with examples of several statistical procedures besides the usual ANOVA and correlations (see below). Kellerman (1978), for example, in his widely cited study of break/brechen/breken, employs a procedure called multidimensional scaling (Roskam, Lingoes and Raaymakers wrote a program called Minissa for this purpose) in order to represent informants' judgments as to the similarity/dissimilarity between pairs of sentences exemplifying the meanings of polysemous break and breken. This procedure enabled Kellerman to come up with a rank order of acceptability for natives and non-natives with implications for markedness assumptions (see also Meisel 1983 for the application of Kellerman's claims to syntax). Still another example of alternative procedures is Hyltenstam's implicational scaling, which makes it possible (see Hyltenstam 1984) to compare the responses of subjects, subject by subject, and the categories employed by Keenan and Comrie (1977) in their Accessibility Hierarchy. By this method, Hyltenstam is able to confirm the universal order Keenan and Comrie postulate: that is, an informant who had difficulty, in Swedish, in one environment in the hierarchy also had difficulty in all environments to the right. Because of its thoroughness and the precision with which it frames the issue, this study is a model of empirical research in support of cross-linguistic, quasi-theoretical claims, in this case, typological universals. Neither of these approaches was entirely suitable for this study since its purpose was not to look at the relative accessibility of discrete items - say, verbs - but to look for evidence of overall direction.

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17 Quite apart from the question of the value of such judgments in SLA, there is the anterior problem of their general utility. In such early studies as Bever (1970) and Ross (1979), there is evidence that even native speakers and/or linguists may differ radically in their judgments of acceptability, as of course is also evident in Coppieters (1987). As we have seen in this study, there are considerable differences in the judgments of the subjects defined as native speakers, particularly where believe + ECM is concerned. Consequently, one can have little confidence that the variation apparent in their L2 judgments was motivated exclusively by the variables indicated. This, of course, is a risk that all such research runs.

As I have already said, Chaudron and Bley-Vroman have taken a look at Flynn's work in the aggregate (Bley-Vroman and Chaudron 1988) and found it wanting in a number of respects. Specifically, her selection of subjects, the method she used to compensate for unanticipated differences in these subjects' proficiency levels in the L2 and her selection and administration of the task are the foci of this critique. On this last point, she seems to have committed two errors: elicited imitation may not constitute a measure of an informant's awareness of specific syntactic features and the task was administered somewhat carelessly to each informant individually. As Bley-Vroman and Chaudron comment, "part of the procedure allowed for a further repetition of the stimuli in the event that a subject responded minimally" (9). This is a clear violation of elementary research protocols, which require that no individual be given an advantage over another. Bley-Vroman and Chaudron go on to say that, "in view of the rather complex nature of the test batteries..., such an interactive, individualized presentation has the potential of introducing uncontrolled variability and unexplained error into the elicitation, in the form of changes in rate and prosody " (10). On the issue of the task itself, i.e. imitation, and its relevance to a subject's "comprehension and production systems," there is little reason to share Flynn's confidence in this equation. The only study she cites to support her use of this procedure (Gallimore and Tharp 1981) in fact supports its use as a measure of a subject's general grammatical ability, not as a measure of his consciousness of such discrete items as anaphoric relations. This point is similar to Kellerman's general view that "experimentation using acceptability judgements has suffered from the unwarranted supposition that learners automatically judge just those structures that researchers want them to judge, even when these structures are embedded in sentences" (1985: 99).

Tarone, in a series of studies (1982, 1984, 1985) has also addressed methodological issues in interlanguage research, including "intuitional" data. Consistent with the Labovian paradigm in sociolinguistics (see Dickerson 1977, for example), she found systematic variability

in styles among informants with differences in task type, with the most casual style being the most systematic and spontaneous oral communication frequently the least permeable to L1 influences, and proposes that these styles can be arranged along a formality continuum, a "capability continuum". In the process of reaching this conclusion, she investigated (1985) four task types which varied with respect to subject behavior, attention to form and interlocutor type but did not vary widely with respect to these variables (there were, for example, no elicited imitation, word-list reading, sentence-combining, sentence-transformation or free writing tasks; interlocutors were "non-native listener" or "native speaker"). The four tasks were a written grammaticality judgment-cum-correction task, oral description, oral narration and an oral interview. In sum, what Tarone found was that "more than two styles are evidenced when learners perform more than two tasks" (373) and grammaticality judgments do not necessarily reflect a subject's most accurate hypotheses about the target language. Tarone defines style with reference to Labov's familiar casual/careful distinction and in essence suggests that the variability she found across task types, with reference to morphological and syntactic accuracy, was systematic. It may be that metalinguistic judgment tasks, in addition to asking the subject to be more conscious of the language per se, also makes the subject more self-conscious about his knowledge of the language, with an influence on accuracy. Of course, there is also a presumptive factor of self-consciousness in performance data, such as Tarone relies on, as well. In any case, Tarone has become the chief apostle of the view that evidence should be gathered by means of multiple measures, that is, if a comprehensive picture of IL behavior is to be achieved.

Most recently, Gregg (1989b) has taken up this issue in a brief review of Tarone's work. He first reminds us that language acquisition "involves the acquisition of knowledge...that will never find expression in output: knowledge of ambiguity, anaphoric relations, possible versus impossible interpretations of sentences, possible versus impossible sentences" (18) and the like. He goes on to attack Tarone's notion of a "capability continuum"

and argue that, in creating this concept, she has traded her birthright for a mess of porridge by discarding "the essential distinction between performance and competence in exchange for little beyond terminological confusion" (21). Then, after indicating that Tarone herself denies the explanatory power of this concept, Gregg asks the key question: Do we really need to account for all this variability in an overarching theory of acquisition? His (predictable) answer is of course that, while there may be some point in studying all of this behavior, such a study could not possibly bear directly on the central issue, namely, that of what constitutes a subject's underlying representation of the L2. This article is a helpful corrective to the vagrant tendency of SLA researchers to use virtually any approach for any purpose.

In sum, nothing in the foregoing (pace Chaudron) suggests, on balance, that acceptability judgment tasks, such as the one employed in this study, are necessarily and invariably inappropriate for the investigation of underlying assumptions about the language, although clearly they must be used carefully and interpreted circumspectly. In another study (Schmidt 1980, see above) also discovered, like Tarone, that responses varied across task type; however, her conclusion - in contrast to Tarone's suggestion that accuracy declines on a "grammar judgment" test and Chaudron's concern with comparability and control - is that elicited imitation is problematical and many empirical measures, including performance data, should be used to get a fix on interlanguages. The point in all of this is that, if one's aim is to describe the overall shape of an IL grammar, then clearly multiple measures are appropriate (a view implicitly endorsed by Gregg in his remarks on "multiple competences" and explicitly by Eubank in his call for a consideration of "multiple sources of variation"); if, on the other hand, the purpose is to glimpse the subjects' underlying representations of relatively abstract concepts in the grammar, then it is hard to see what other procedure or combination of procedures would be more effective than such acceptability judgments since they are consistent

with the notion of competence[18], they have been widely used in SLA research (which means that their use enhances comparability), they can be used to focus an informant's attention (insofar as that is possible) and the data they yield are relatively 'clean'. At the very least, this approach avoids the problems that arise when performance errors are mistaken for evidence of competence, and it certainly has the virtue of having been used in numerous studies since the publication of Chaudron's reservations (see above) with, as I have suggested, an obvious effect on comparability.

2.4 Related Studies

2.4.1 Coppieters (1987) queried 21 near-native speakers (NNSs) of French, selected on the recommendation of "friends, students, or close colleagues" (550) in France, and 20 native speakers as controls. The NNSs' native languages included both American and British dialects of English, Italian, Chinese (?), Farsi (Persian?), Spanish (?), German, Japanese, Korean and Portuguese (?); there was no formal test of their proficiency levels in French. The cohort included 17 professors and two university student researchers; thus, they were a highly educated lot. They had lived as few as 5.5 and as many as 37 years in France (17.4 years on the average); none had had to use French for everyday communication before the age of 18. The testing condition consisted of a 107-item questionnaire and a follow-up interview. The questionnaire comprised 41 sentences requiring the respondents to supply a missing item from a choice of two and 66 sentences requiring well-formedness judgments. The purpose of the interview was to plumb their intuitions as to why, say, they preferred one form to another or

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18 Needless to say, as Chomsky indicates, "acceptability is a concept that belongs to the study of performance" (1965: 11). However, a learner's grammatical intuitions are not part of performance. As Gregg remarks, these are "what underlie performance data, any (non-aberrent) performance data. In order to identify those intuitions, acquisition researchers have to look at performance data" (1990: 369), that is, at acceptability. But the purpose in doing so is to draw inferences from these judgments of acceptability as to the learner's intuition about the language's underlying rule structures which they reveal; they have no interest as an end in themselves. By definiton, performance data of the more conventional sort carry the researcher further away from this intuition and introduce variables attendant on output as a confound.

had rendered the well-formedness judgments they had.

The domains selected for this study included (among the 41 sentences) such surface areas as imparfait/passé composé, à + infinitive/de + infinitive, adjective placement, il/elle/ce and articles and (among the 66 sentences) such examples of "complex syntax" as "object + predicate," causatives and clitics (see Rouveret and Vergnaud 1980) and Ross' A-over-A Constraint, as well as "the 'Noun de Adjective' construction." The examples of so-called complex syntax are of course the most relevant to this study.

In order to quantify his subjects' responses, Coppieters developed an "evaluation index" for each item, i.e. the preference of the majority of NSs; he then counted the number of times each NNS diverged from this norm and arrived at an average for each (a binary statistic). Whereas the NSs diverged from the norm between 5% and 16% of the time, the NNSs diverged between 23% and 49% of the time. Even the one NNS who diverged least (22.9%) still diverged so much as not to be comparable to the NSs ($p < .005$). Unfortunately, it is hard to know how to interpret so gross a measure of difference: a questionnaire requiring an assessment as to the degree of well-formedness and/or some break-down of the interaction effects of domain and proximity-to-norm might have given a more comprehensive picture. It is interesting to note, however, that the NNSs diverged least in the so-called complex areas of syntax. The study of NNSs, Coppieters suggests, "might well provide interesting insights on the question of parameter setting, parameter variation, and parameter flexibility in UG, but it might also indicate which areas do not naturally fall within the bounds of formal syntactic/semantic competence" (566) and presumably come under the heading of "functional" or "cognitive" aspects of grammar (see Givon 1979, Lakoff and Johnson 1980, Langacker 1986).

2.4.2 Johnson and Newport (1988), in a study of the critical period hypothesis (Lenneberg 1967) and SLA, asked 46 informally selected Korean and Chinese (?) speakers (and 23 native controls) to perform a grammaticality judgment task. The informants had arrived in the U.S. between the ages of 3 and 39 and had lived in this country between 3 and 26 years.

Each subject was presented with 276 (!) sentences to evaluate on a binary scale, i.e. they were either right or wrong. Of these 276, 140 were ungrammatical and exemplified rule violations involving 12 areas of grammar: the morphology of the simple past, pluralization, the 3rd person singular in the simple present, the present progressive; determiners; pronominals; particle movement in phrasal verbs; subcategorization constraints on let and allow; auxiliaries and modals; word order; and auxiliary realization and inversion in yes/no and Wh questions. The remaining 136 sentences were illustrative of the same rules and formed pairs with the ungrammatical sentences.

Although there are no operationalized hypotheses in this study, there are two reformulations of Lenneberg's hypothesis, one attributing language acquisition to developmental factors and one attributing it to the interaction of developmental and acquisitional factors, that surface from time to time. Essentially, four questions are addressed having to do with the effect of age on the learning of L2 grammar: its functional shape, direction, relation to other variables and scope. The researchers found a strong relationship between age at arrival and performance on this measure ($r = -.77$, $p < .01$). They also found significant differences between the pre-pubescent and post-pubescent groups, suggesting that there may still be some life in the critical period hypothesis after all, as one might have suspected from Oyama (1976) and Patkowski (1980). In a series of analyses, Johnson et al. look at all the other issues, but the only one with a possible bearing on this study is their summary of the correlations between age at arrival and the number of errors by rule type, which indicates that the sentences involving the morphology of the simple past and pluralization and those involving pronominals were the most affected by differences in age at arrival ($p < .01$) and those involving the 3rd person singular in the simple present and, as an independent category, the present progressive, the least affected. This suggests that some domains are more sensitive to maturation than others and, as Coppieters (1987) speculates, that some, as a consequence, may be more susceptible to parameter resetting than others. On

the issue of development v. development/acquisition, they come down squarely in favor of development, i.e. that the Language Acquisition Device naturally atrophies with age, although they acknowledge that adults do manage to learn languages to some extent (see Schachter 1989b for a discussion of "completeness") and that there is considerable variability in adult performance (see below). On balance, the study is well organized, informed, thoughtful and thorough.

2.5 Summary

As the discussion above indicates, little can be said at this point as to the precise role of UG in adult SLA. Indeed, the results achieved so far are rather ambiguous. A few general conclusions are possible, however.

In roughly half the studies reviewed, there is at least some evidence that universals play a role. Unfortunately, any attempt to draw conclusions from these studies is complicated by major differences in conceptualization, e.g. the definition of a universal, and research design that limit the studies' comparability. Some of the research is so deeply flawed as to be virtually useless. Some of it is convincing. Little by little, the enterprise as a whole seems to have crept closer to an understanding of the relationship between emerging linguistic theory and SLA, though the dynamic nature of theory building, together with the tendency of many researchers to make premature generalizations, has left us with many more questions than answers.

The evidence on transfer is on the whole comparatively unambiguous: transfer is ubiquitous. The issue remains, however, whether it is possible to disambiguate the effects of the L1 from the effects of a reactivated UG (see below) or whether UG-based research will ultimately yield only a reconceptualization of transfer within a principles-and-parameters framework.

On the issue of markedness, Ellis provides a helpful summary of the research to date encompassing five statements, only four of which concern us:

(a) Learners transfer unmarked L1 forms when the corresponding

L2 forms are more marked.

(b) The effect of the L1 will be observed more strongly where peripheral rules in the L2 are concerned.

(c) In general, marked forms are not transferred into interlanguage, particularly when the L1 possesses both marked and unmarked constructions.

(d) Marked forms may be transferred in the early stages of SLA.

(1980: 209)

The summary has interesting implications for this study. Specifically, (a) and (c), taken together, suggest that the Francophone group is more likely to show evidence of transfer than the Anglophone group, as anticipated below.

There is widespread use of judgments requiring an assessment of acceptability/grammaticality/well-formedness as a means of getting a fix on the universality of the informants' intuitions. While questions have been raised as to how revelatory they are of underlying aspects of language competence and/or the shape of the IL and/or the subjects' capabilities across task types, these criticisms have been effectively answered by Gregg (1989b, 1990) and others with recourse to the competence/performance distinction. Furthermore, their wide-spread use itself supports their continued employment as a research technique. Judgment data alone, however, will never yield the complete picture of interlanguage behavior and its causes. Only the evaluation of these data combined with the analysis of data from other sources (once it becomes clear what the contribution of competence is and, by inference, what the contribution of performance is not) will yield a richer sense of ILs and their role.

Interpretation of the results achieved in several studies and aposteriori speculation about their causes have resulted in a number of interesting and potentially productive proposals. These have drawn the enterprise both into deeper involvement with syntactic theory and toward alternative theories such as processing and parsing models (see above). This speculation - some of it going way beyond the findings that have stimulated it - has greatly complicated the picture (without necessarily clarifying it) and left the the research program at something of an impasse.

These considerations have informed the design of the study outlined below.

CHAPTER THREE

The Study

3.1 Hypotheses

There are four related hypotheses in this study.

3.1.1 Hypothesis: Markedness

Although both groups will exhibit deviation from native-speaker norms, the Francophone group will achieve significantly lower scores than the Anglophone group (Hypothesis 1) in the L2 because English is more marked syntactically than French.

3.1.2 Hypotheses: UG

There will be no significant differences in the two groups' syntactic assumptions (Hypothesis 2) about their L2s. This is a refinement of Hypothesis 1, a gross comparison; whether or not the two groups approximate each other in overall scores, they may be operating with comparable assumptions about the syntax of their target languages. In other words, disconfirmation of Hypothesis 1 is unnecessary, and certainly insufficient, for a decision about Hypothesis 2 (though it is consistent with a decision to accept it). Whereas Hypothesis 1 presupposes that deviance from native-speaker norms derives from the comparative complexity of target syntax, Hypothesis 2 anticipates internal consistency in the two groups' underlying assumptions, which may not derive from French.

Furthermore, both groups will opt for the simpler and more regular rule system (Hypothesis 2'), namely, the one associated with French syntax. This hypothesis obviously has implications for the issue addressed by Hypothesis 1 above. If both groups are opting for the French parameter setting, Group A should have a clear advantage in the L2, and that advantage should be reflected in their overall scores (which of course could be interpreted as success). As for Group F, acceptance of this hypothesis is consistent with transfer; its rejection for this group supports the notion of success in their acquisition of the L2.

3.1.3 Hypothesis: Transfer

If Hypothesis 2 is disconfirmed and Hypothesis 2' is disconfirmed only for Group A, there may be evidence that Group A is also mapping L1 rules onto the syntax of French. In that case, there will be no significant difference between their responses on the L1 and L2 measures (Hypothesis 3). In other words, the syntactic assumptions they make about their L2 will approximate their assumptions about the L1.

3.1.4 To summarize, the study contains four hypotheses:

Hypothesis 1: The Francophone group (hereafter Group F) will achieve significantly lower scores than the Anglophone group (Group A) when measured against native-speaker norms.

Hypothesis 2: There will be a significant and positive correlation between the two groups' responses when implicit grammatical preferences are taken into account.

Hypothesis 2': The aggregated responses required for a decision about Hypothesis 2 will correlate significantly and positively with French native-speaker norms.

Hypothesis 3: The responses of Group F and Group A on the L2 measure, taken separately, will correlate significantly and positively with native-speaker norms in French and English respectively.

3.2 Subjects

3.2.1 96 subjects participated in this study: 48 native French speakers tested in Paris and 48 native English speakers tested in New York City. Group A was tested in February and March, 1990; Group F, in early January, 1990. Group A comprises informants from three sources: French classes at the Alliance Française (2), the United Nations International School (30)) and Fordham University at Lincoln Center (16). Group F is made up of students enrolled at the Ecole Française des Attachés de Presse (EFAP)[19]. Since both groups live in large

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[19] White (personal communication) has suggested that the exposure patterns of the two groups might confer a certain bias. As a native speaker of a (southern) coastal British dialect, she claims that want + tensed clauses is more acceptable than believe + PRO; she therefore suggests that, if the Group F's teachers spoke some variant of British

international cities, they had both had a variety of opportunities for exposure to the L2, including radio, television, movies and popular music, as well as print media such as magazines and newspapers.

The two groups served as native-speaker controls for each other. Both groups, in other words, were given both tests and thereby provided baseline L1 data for the study as a whole.

At the same time, data on age, age at initial exposure, subsequent exposure patterns, years of formal instruction and the like were collected (see below). These attribute variables, in other words, were controlled to ensure comparability.

Since these variables were controlled, both groups are relatively cohesive in terms of acquisitional and exposure patterns. They are also both above the intermediate range in terms of L2 proficiency by inference from these data as well as their test scores (see below); the variance among these scores is also comparatively low[20].

It was particularly important that the subjects in Group A knew the subjunctive mood in French. For this reason, it was critical to select informants who one could be certain had been exposed to and formally studied that aspect of the language's grammar. Thus, established French language programs were used to form a pool of potential subjects (see below). Of those tested, roughly half were selected for study because of their broad similarity with the members of Group F, who had been tested first. The lack of randomness in the selection of subjects of course limits the study's generalizability. However, in the absence of a more principled method

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English, they might have skewed the input in such a way as to prevent a clean comparison. As a native speaker of American English, I cannot detect a preference. Furthermore, the current teachers of the informants in Paris were all native speakers of American English, as in fact Group A's French teachers were all native French speakers, though of several dialects.

20 As indicated below, only the most recent studies have employed independent measures of general proficiency in the L2; this has been relatively easy to do in uni-directional studies. Nonetheless, there is sometimes wide variation in the L2 scores achieved and/or a lapse in time between the L2 testing and the tests employed in the study itself (see Bley-Vroman et al. 1988). In White's (1989b) bi-directional study, an informal cloze was used with wide variation in the scores that resulted.

of assessing L2 proficiency - by means of, for example, two tests in the two languages which are highly correlated, a virtual impossibility - there would seem to be no better way to define them. Since there are almost no two-way comparisons in the literature (see Phinney 1987 for one problematical exception), the (infrequent) use of formal independent measures of proficiency, particularly with reference to targeted grammar (cf. an estimate of L2 proficiency based on a subject's placement within an instructional program), in these studies (Bley-Vroman et al. 1988, Eubank 1989) is a more straightforward process than their use would be in this case. On balance, given the independent verification of similarity between the groups on the basis of the attribute data and their test scores, an insistence on formal pre-testing would have scuttled the study and risked the loss of whatever insight can be gained from a look at contrary acquisitional processes.

3.2.2 Test Condition

In the case of Group F, the tests were administered by the American English teachers of these students at EFAP under the supervision of the researcher. All the students were tested on a single day.

In the case of Group A, the tests were administered by the native French teachers of these students at the Alliance Française, the United Nations International School and Fordham University at Lincoln Center under the supervision of the researcher. These tests took place on three separate days.

In both cases, the students were tested in groups.

An effort was made to make the test as informal as possible. Thus, the proctors were permitted to answer questions about the test as long as these questions did not bear directly on the content. There was no time limit, but, as the instructions indicate (see Appendix B), the subjects were encouraged to give careful consideration to each item without spending more than a few seconds in formulating their judgments. No records were kept on the amount of time each group took to complete the test.

3.2.3 Population Attributes

Sex

The overwhelming majority of both groups was female. Of Group A, 29% were male; of Group F, 17%.

Age

Mean age for Group A was 18.0 years; for Group F, 20.3.

Birthplace

Of Group A, 67% (32) reported that they had been born in New York State. Most of the rest had been born in seven other regions of the country (California, Connecticut, Illinois, New Hampshire, New Jersey, Maryland and Washington, D.C.); three reported that they had been born in "the United States". Finally, two had been born in London (England) and one in Kingston (Jamaica).

Of Group F, 94% (45) had been born in metropolitan France, 40% (19) in Paris and environs. Of the rest, one had been born in Fribourg (West Germany), one in Fort de France (Martinique) and one in Oran (Algeria).

Knowledge of Additional Languages

Group A reported that it spoke an average of 2.6 languages; Group F, an average of 2.9.

In addition to English and French, Group A said they could speak Afrikaans (1), Amharic (1), Arabic (2), "Chinese" (1), "Creole" (1), Dutch (1), Flemish (1), German (2), Hindi (1), Italian (4), Japanese (3), Krio (1), Portuguese (1), Spanish (7), Turkish (1), Urdu (1) and Wolof (1). They claimed to understand (without being able to speak) Dutch (1), German (2), Greek (1), Italian (6), Spanish (8), Tagalog (2) and Ukrainian (1). This diversity reflects U.S. immigration patterns as well as the fact that several of the informants were the offspring of U.N. personnel.

In addition to French and English, Group F reported a speaking knowledge of German

(15), Italian (5), Latin (1), Polish (2), Serbo-Croatian (1) and Spanish (23). They also said they could understand (though not speak) Dutch (1), German (3), Italian (5), Russian (1) and Spanish (1). For the most part, their ability to speak the languages indicated stemmed from formal study rather than simple exposure.

Years of Schooling

Group A had had an average of 12.6 years of formal schooling; Group F, an average of 13.7.

Age When L2 Study Begun

On the average, Group A began study of the L2 (in years/months/weeks) at age 9.9.2; Group F began their study at age 11.7.1. This difference reflects the fact that several members of Group A were students at the United Nations International School (UNIS), whereas most of the French informants had followed the conventional curricular path prescribed by the French Ministry of Education.

Years of Study of the L2

Mean years of study of L2 for Group F was 7.59 years; for Group A, 7.94. 96% of Group A were studying the L2 at the time of the test; 100% of Group F were studying the L2.

Residence in Environments Where the L2 is Spoken Natively

Of Group A, 85% (41) had had some residence in an environment where French was spoken natively. This exposure totalled 25 years, 2 months and 2 weeks (25.2.2) and included residence in Algeria, Belgium, Canada, France, Haiti, Senegal, St. Barthélemy, St. Martin, Switzerland and Tunisia. This computes to roughly 7 months and 2 weeks (0.7.2) per informant.

Of Group F, 75% (36) had spent time in English-speaking countries. This experience totalled 8 years, 6 months and 2 weeks (8.6.2) and encompassed time spent in Britain, Ireland and the United States. This works out to an average of approximately 2 months, 3 and a half weeks (0.2.3.4) per informant.

These differences suggest that informal exposure may have accounted to some extent for the outcome.

3.2.3 The claim is sometimes made by researchers in this field (see Wode 1984, for example) that it is only at the initial point of exposure to a particular phenomenon that the subjects are likely to exhibit transfer effects and/or UG influence and/or acquisition of the L2 setting. Frankly, it is hard to see why this is an important claim since it is the possibility of the persistent influence of UG beyond a transitory period of hypothesis testing that is potentially interesting. Furthermore, the claim implicitly ignores the fact that adult SLA is a complex series of fits and starts, of gains and regression, characterized by the lack of an isomorphic relationship between input and intake, a need for constant re-exposure and reinforcement, an asymtotic learning curve, cognitive limitations on the integration of new material, the powerful influence of affective variables and the likelihood of ultimate fossilization. It is as naive to assume that these effects do not persist as it is shortsighted to take as the goal of this research the description of a brief interlingual episode in a long continuum. The fact is, as is widely attested in the literature (see Snow and Hoefnagel-Höhle 1978, for example), that adults, unlike children, actually make rapid initial gains and then plateau. Indeed, the claim seems to rest on the assumption that adult SLA is identical to child SLA, so may represent an unjustified carry-over from L1 studies. In any event, it is impractical, given the uneven landscape of adult SLA, to think that you could isolate a group of informants at the point of initial exposure. This should be obvious since it has almost never been done in the studies conducted to date[21].

On the other hand, given the design of this study, it is impossible to know exactly at

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21 Bley-Vroman et al., for example, indicate that "a relatively advanced level of proficiency was important to our study: clearly, beginning students might not be able to make the relevant judgements simply because the structures in question were unfamiliar to them. We wanted to make sure that if the subjects did poorly, it could not simply be attributed to low English proficiency" (1988: 6).

what point in the acquisitional sequence these students were being tested. Since it was a requirement in the selection of subjects that they had already been exposed to the relevant syntax (though presumably not mastered it), it is entirely possible that they had already tried UG/transfer and found it wanting. If, by comparison, one had confined the study to students who had not already been exposed to this syntax (as indicated, an impractical option), then one would have had to allow time for the students to absorb the new structure - through exposure to grammatically accurate examples that might bias their response patterns - before their preferences could be consulted and thereby introduce the actual acquisition of this syntax as a confound. In any case, a lack of differential effects in any such study could indicate that UG had already been tried and superceded or, if it were possible to catch the students at the point of initial exposure, bypassed as an option altogether through inaccessibility. There is, in short, no way of knowing whether the ILs these learners were operating with were permeable, fossilized or something else. As I have suggested, the irregularity of adult acquisitional sequences makes more precise comparisons virtually impossible.

3.2.4 Other potential confounds are the circumstances of use of the L2 and, in some cases, the circumstances of acquisition. That is to say, although every effort was made to ensure comparability (to the extent of testing subjects on both sides of the Atlantic), some members of Group A may have acquired French as a second language (though this is doubtful), whereas virtually all members of Group F acquired English as a foreign language. In any case, both currently function in social environments dominated by their native languages. Both, as the figures on residence above indicate, have also had a variety of acquisitional experiences. Furthermore, although differences in these experiences could conceivably have a marginal effect on the results, there is little evidence that as a rule they do. Schachter (1989b, for example, compared Dutch, Indonesian, Chinese (?) and Korean subjects, with the first group drawn from the Catholic University in Nijmegen (and tested in Holland) and the latter three groups from USC (and tested in Los Angeles). There were also considerable differences in the

circumstances of acquisition among these four groups. In order to ensure that these factors did not have a biasing effect on the outcome, Schachter used a generalized linear model regression procedure, found the results to be nonsignificant ($F = 1.37$, $p = .24$) and concluded that "the attribute variables do not account for the variance in the ability of the subjects to identify the grammatical sentences" and that "the subjects formed a fairly homogeneous group" (31). If the issue is competence, as reflected in a judgment task, among subjects who are capable of functioning in the language and have had a range of acquisitional and communicative experiences, environmental, system-external factors such as these may not be directly relevant. However, as indicated, the groups should be cohesive and comparable if other sources of contamination are to be avoided. As indicated above, the purpose of this study was to compare the response patterns of two comparable groups, not to look at co-variation between these patterns and the attribute variables.

3.3 Instruments

Three instruments were employed with each group.

Attribute Questionnaire (Appendix A)

The questionnaire was used to ascertain (a) age, (b) educational level, (c) age at first exposure to the L2, (d) number of years of study of the L2, (e) months of residence in a setting where the L2 is spoken and (f) knowledge of other languages.

Judgment Tasks: L1 and L2 (Appendix B)

Each group was given randomized lists of 50 statements each in the L1 and the L2; they were asked to rate the acceptability of these statements with reference to three designations (1 = *totally unacceptable*, 2 = *uncertainty*, 3 = *totally acceptable*). Thirty-six of these were test sentences, 9 were distractors and 5 were controls. The distractors were added to avoid the impression that the properties of only three verbs were being studied; the controls were added to ensure that the subjects could process V [NP VP] structures easily. Needless to say, none could be said to be partially correct, so a judgment indicating "uncertainty" is never

interpretable as a measure of semi-grammaticality.

The items on the two tests (French and English) are matched-paired and controlled according to the following four criteria.

- The two members of each pair are virtually identical in meaning.
- All sentences contain exactly 10 syllables
- They are all in the past tense, although constraints on usage require the imparfait for promettre and the passé composé for croire and vouloir.
- Vocabulary is kept to an elementary level; all lexical items are highly frequent in the input; there is no slang.

As indicated above, both grammatical and ungrammatical items are included. The following examples (not actual test sentences) illustrate the equivalent distribution of grammatical and ungrammatical exemplars.

(a) *Je croit Jean être...		I believe John to be...
(b) *Je veux Jean être à l'heure		I want John to be on time
(c) Je crois Jean intelligent	*	I believe John intelligent
(d) Je veux que Jean soit...	*	I want that John is...
(e) Je promets que je serai...		I promise that I will...

(a) and (b) above are of course correct in English, incorrect in French; nonetheless, one would expect the informants, if they are operating with the same set of universal assumptions, to declare both pairs unacceptable. (c) and (d) of course present the opposite case (although some native speakers find I believe John intelligent acceptable), but again one would expect the pattern associated with French to be preferred by both groups on the grounds that French syntax is simpler and more regular and, conceivably, representative of the universal option. (e) is unproblematical in either language, whether because of universal principles or L1 transfer. In this case, one can expect virtual identity in the response patterns of the two groups in both languages.

As indicated above, the test sentence sets could not be confined to these six verbs; otherwise, it would have been obvious to the respondent that one was looking for facts about these verbs and only these verbs. There was, in other words, a risk of sacrificing its broader syntactic scope for a trivial study of isolated verbs alone. On the other hand, while it is easy to find references to believe-type verbs in the literature, it is hard to locate a principled and

generally agreed notion of what actually constitutes such a type (see Alexander et al. 1964 and Bridgeman 1965 for examples of attempts in an earlier framework), although consider/considérer seem a likely pair on the basis of simple intuition. As for want, nothing seemed comparable; nor could I isolate anything comparable to promise. This left two alternatives: to either extend the list to include other types, e.g. persuade, hope or expect (though there is no obvious equivalent of expect in French), or load in some other verbs regardless of their categories, simply to throw the respondents off the scent. In the end, neither of these alternatives seemed to have much to recommend it. Ultimately, only three distractors (ask/demander, hope/espérer, persuade/persuader), with a total of three test sentences each, and one control (say/dire), with a total of five, were included.

Ideally, of course, there should be roughly equivalent numbers, overall, of correct and incorrect items in each test to avoid automatic response patterns generated by a false assumption about the aim of the test. However, only where PRO and ECM are concerned is it relatively easy to find correct and incorrect items since an object pronoun is either present or absent. This is true for all the verbs except want although believe/croire presents the clearest contrast; all five verbs allow tensed clausal complements. Want, by comparison, is both a PRO and ECM verb, making it impossible to find incorrect items - unless trivial surface differences are supplied - for this verb; want is also the only verb that does not permit tensed clausal complements. Therefore, while it is not possible to use want to test the PRO/ECM opposition, it is the only verb, together with vouloir, that can be used to test the acceptability of tensed clausal complements. In a sense, want is the most interesting of the six verbs where these complements are concerned: probably the most difficult to master and certainly the most marked overall in its distributional properties. For this reason, and since it is the syntax and not the verb that is the focus of this study, the following paradigm was used to allocate grammatical and ungrammatical exemplars to verb/environment categories; the examples given are not actual test sentences.

TABLE 3.1. Counterbalanced Paradigm for Test Construction		
	believe: CORRECT	croire: CORRECT
PRO	-	Jean croit aimer la musique
ECM	I believe John to be intelligent	-
Clause	I believe that John is intelligent	Je crois que Jean est intelligent
	believe: INCORRECT	croire: INCORRECT
PRO	John believes to like music	-
ECM	-	Je crois Jean être intelligent
Clause	-	-
	want: CORRECT	vouloir: CORRECT
PRO	I want to leave	Je veux partir
ECM	I want John to leave	-
Clause	-	Je veux que Jean soit à l'heure
	want: INCORRECT	vouloir: INCORRECT
PRO	-	-
ECM	-	Je veux Jean être à l'heure
Clauses	I want that John leave	-
	promise: CORRECT	promettre: CORRECT
PRO	I promise to leave	Je promets à partir
ECM	-	-
Clauses	I promise that John will leave	Je promets que Jean partira
	promise: INCORRECT	promettre: INCORRECT
PRO	-	-
ECM	John promised Mary to have a baby	Jean a promis Marie d'avoir un bébé
Clauses	-	-

To summarize, each test contains fifty sentences exemplifying the properties outlined above as well as 9 distractors and 5 controls. Each sentence contains 10 syllables and is

controlled in other ways; for example, each sentence is in the past tense. The test sentences were randomly ordered. The subjects were asked to indicate the degree of acceptability of each sentence from 1 to 3, with 3 designating a judgment of complete acceptability, 1 perfect unacceptability and 2 uncertainty.

3.4 Analyses

As the discussion above makes clear, this study compares the contributions of several variables: native language, test language, three verb types (believe/croire, promise/promettre, want/vouloir) and three syntactic environments (fully tensed clausal complements, ECM, PRO). As indicated, both grammatical and ungrammatical exemplars are included. These variables are independent factors with two or three levels; the subjects' preferences for one setting or another as reflected in their response patterns on the judgment tasks, i.e. their scores, constitute the dependent variable. The study's essential purpose is to assess the interaction effects among these variables - that is, their levels of statistical significance - in order to determine whether there is any evidence among these informants that the groups, either independently or collectively, are reverting to an "open" or neutral parameter setting while making judgments about their second languages, in other words, whether or not some unmarked universal setting is still available.

For this reason, analyses of variance (one-way ANOVA) and a statistical correlation appeared to be appropriate procedures at the outset for the analysis of the data without reference to apriori criteria of accuracy. Subsequently, however, discrepancies between the groups on the L1 measures (see Materials Analysis below) and the failure of the differences between these mean scores to discriminate among the effects in an interesting way were uncovered, and it was decided to use "accuracy scores" (i.e. scores reflecting the extent to which an informant conforms to generallly accepted or "unattested" preferences) in a subsequent analysis. At the same time, it was decided to look at differences between sub-groups (defined on the basis of putative differences in L2 proficiency; see below) and to aggregate the responses

indicating unacceptabilty and uncertainty (i.e. eliminate uncertainty as an intervening variable) since degree of acceptability was not a primary issue and there was no intrinsic basis (i.e. in the test materials) for it. It was decided arbitrarily to encode uncertainty as unacceptable since it was clear that an informant who was uncertain did not find the sentence to be acceptable and therefore could not endorse its use. A comprehensive report of all these analyses is provided in chapter four so that the study as a whole can be evaluated.

To summarize, six analyses were conducted.

(a) ANOVA: three response categories (4.1)

(b) ANOVA: two response categories (4.2)

(c) Comparison of means: two proficiency levels, two response categories (4.3)

(d) Accuracy proportions: two response categories (4.4)

(e) ANOVA: accuracy proportions, two response categories (4.5)

(f) Materials analysis (4.6)

The results of all of these analyses are given, as indicated, in the following chapter.

As for the interpretation of these analyses, the following procedures were used.

Hypothesis 1 In this case, it was necessary to compare the means each group achieved on the L2 test against the means the other group had attained on the L1 measure, together with a subsequent evaluation of the difference. In this case, differences of means were computed in a [verb x environment] matrix and then compared. In this way, it was possible to assess the extent to which each group of L2 learners approximated native-speaker norms; Hypothesis 1 of course predicts that Group A will be closer to these norms than Group F. Matrices such as those generated for the purpose of making these comparisons provided a basis for interpretation of analyses (a), (b), (c) and (e) above with reference to all of the hypotheses.

Hypothesis 2 When the study was conceived, it was assumed that a matched-pairing of items on the two measures (or conceivably a rank-ordering of acceptability) would be

necessary. In the end, however, a finely tuned sentence-by-sentence analysis would have bought nothing more than was available through the tests actually undertaken. In any case, what was required here was a comparison of the response patterns of the two groups on the L2 judgment task [verb x environment] to determine the extent to which they were similar.

Hypothesis 2' In this case, it was necessary to compare both groups' scores on the L2 measure with Group F's on the L1 measure. The purpose of this comparison was to assay the extent to which both groups were approximating the parameter setting associated with French; Hypothesis 2' predicts that, if UG is still available, both groups will exhibit a tendency to (re)set their parameters in that direction.

Hypothesis 3 Here, a within-group comparison of Group A's and Group F's scores on both measures and a subsequent between-groups comparison were required. Although a correlation statistic was initially anticipated for this comparison (since a high correlation would imply that one could predict one score from the other and therefore was measuring essentially the same competence on both tasks), the intervening variable of test language ruled out such a procedure. In the end, an overall comparison of the means achieved was the only recourse.

As a one-way analysis of variance (ANOVA) is a parametric inferential test, there is an assumption that the two groups are equally varied, i.e. comparatively homogeneous (see above), and that the set of scores each yields is likely to be normally distributed (in addition to an assumption that the two groups are independent). For this reason, a preliminary test of variance such as a t-test is sometimes conducted to ensure that these conditions are met; otherwise, it would not be possible to conclude from the data that the distribution actually achieved is due to the effects of one or another of the variables cited. In this case, these tests were not conducted, but the interaction effects of all the variables were computed and evaluated for statistical significance. This information, where relevant, is reported below and in Appendix C.

In short, in order to address the hypotheses in the study, five [verb x environment] matrices reflecting, for example, mean differences were generated and compared appropriately. These comparisons involved five relationships in association with the four hypotheses as indicated below.

(a) L1 English and L2 English

To make a decision about Hypothesis 1, it was necessary to compare this matrix with (b).

(b) L2 French and L1 French

To make a decision about Hypothesis 1, it was necessary to compare this matrix with (a). To make a decision about Hypothesis 2', it was necessary to compare this matrix with (d).

(c) L2 French and L2 English

This comparison was necessary for a decision about Hypothesis 2.

(d) L2 English and L1 French

To make a decision about Hypothesis 2', it was necessary to compare this matrix with (b). To make a decision about Hypothesis 3, it was necessary to compare this matrix with (e).

(e) L1 English and L2 French

To make a decision about Hypothesis 3, it was necessary to compare this matrix with (d).

These analyses and comparisons formed the basis for the discussion in chapter six.

CHAPTER FOUR

Results

As indicated above, several analyses have been made of these data.

4.1 Analysis of Variance: Three Response Categories

As indicated, an analysis of variance (ANOVA) was conducted with subjects as the repeated measure nested in two groups (Group A and Group F) crossed with three variables: language of the test (English, French), verb (believe/croire, promise/promettre, want/vouloir) and environment (tensed clause, ECM, PRO). This analysis indicated that there are no significant main effects for group or language (as one might suspect since each group was tested in an L1 and an L2 and each set of language test results includes L1 and L2 speakers) or significant interaction effects for [group x language] or [group x verb]. All other comparisons do yield significance, however, and there are significant main effects for verb and environment. Overall means for each of the nine cells were then compared (see below), and a Tukey procedure (see Winer 1971) was applied (here and in subsequent analyses) to assess significance for these differences of means.

There are no significant differences between these two groups in any of these analyses (see Appendix C). Although some significant differences emerge in a discrete cell-by-cell comparison with reference to target language norms, the parameter setting associated with French and the groups' L1s, there is no significance in the aggregate (i.e. in the sum over all the cells) and the groups do not differ significantly from each other on any of these measures. In other words, none of these effects is sufficiently robust to distinguish the two groups. That also means, however, that the two groups, defined as second language learners, do not differ from each other in a direct comparison. This fact is potentially interesting since it suggests that they are operating with comparable assumptions about the second language despite syntactic differences in the two languages.

In analyses such as this, however, considerable detail is lost. Such calculations do not

capture the overall pattern of responses; they do not, that is, discriminate between a response pattern, across four items, of 3/1/1/3 and one of 2/2/2/2, although there are obvious differences between these two series of judgments. Therefore, the following analysis was undertaken.

4.2 Analysis of Variance: Two Response Categories

In this analysis, the categories of unacceptability (1) and uncertainty (2) were collapsed. Although native speakers and/or linguists differ in their judgments of some of these sentences (see above), there is relatively little ambivalence (i.e. '?' as a judgment indicator) in the literature: they are either acceptable or unacceptable. As a consequence, it was assumed that, at the very least, a judgment of uncertainty indicated that the L2 informant could not automatically embrace the sentence as acceptable, as apparently an 'ideal' native speaker would; in other words, the sentence might be unacceptable. This assumption, together with the fact that 1-2-3 in this study does not represent an interval scale, prompted an analysis employing only these two response categories. In this case, the analysis of variance (ANOVA) revealed significant main effects for all variables except language and significant effects for all interactions except [group x verb]. As the tables below indicate, significance emerges (cf. Appendix C) in the want/vouloir + PRO cell (an unexpected outcome) in two comparisons (Tables 4.1 and 4.4 below) while promise/promettre + ECM loses significance (an expected outcome) in two comparisons (Tables 4.3 and 4.5).

The following tables are [verb x environment] matrices reporting differences of means. That is, the means for each group and the differences between these average scores for each group were computed. A big or significant difference is unlikely to have occurred purely as a result of chance ($p < .05$); it is therefore likely to have occurred because of one or more of the variables under investigation. Since the difference between 1 and 2 is equivalent to a percentage, these differences are recorded as simple proportions. In each case, the second set of means (e.g. L2 English) was subtracted from the first (e.g. L1 English), yielding plus or minus differences, what it referred to below as "directionality". Thus, a minus difference in

Table 4.1, for example, indicates that the L2 English mean was higher than its L1 English equivalent. Overall, i.e. when the differences are summed across all cells, none of the differences in any of these matrices is significant.

TABLE 4.1. Differences of Means: L1 English - L2 English

	TENSED CLAUSE	ECM	PRO
believe	.31*	.24*	-.22*
promise	.22*	-.26*	-
want	-.39*	.32	.23*

* p <.05

TABLE 4.2. Differences of Means: L1 French - L2 French

	TENSED CLAUSE	ECM	PRO
croire	-	.33*	-.54*
promettre	-	.23*	-.34*
vouloir	-.30*	.25*	-

* p <.05

As Table 4.2 indicates, Group F found PRO and vouloir + tensed clauses significantly more acceptable than Group A. There are also significant differences - in a direction which implies its acceptability in English - where ECM is concerned. Overall, there are fewer significant differences in this table than in Table 4.1. This suggests, as predicted (Hypothesis 1), that Group A was having an easier time with French than Group F was having with English.

TABLE 4.3. Differences of Means: L2 French - L2 English

	TENSED CLAUSE	ECM	PRO
believe/croire	.28*	-	-
promise/promettre	-	-	-
want/vouloir	-	-.38*	-

* p <.05

Little significance is evident in this table, which of course lends support to Hypothesis 2. Group F found want + ECM significantly more acceptable than Group A found vouloir + ECM. The degree of difference apart, this suggests that both groups have sprung free of L1 dependence where ECM is concerned. In other words, Group F is not opting uncritically for their L1 - and the putative universal - setting in this environment while Group A is not presupposing the universality of their L1 setting. These facts support the claim of success for both groups. On the other hand, Group F's reluctance to accept believe + tensed clauses suggests a generalized assumption on their part that the two languages differ where in fact they don't. See chapter five for more on both of these points.

TABLE 4.4. Differences of Means: L2 English - L2 French

	TENSED CLAUSE	ECM	PRO
believe/croire	-.42*	.23*	-.64*
promise/promettre	-	.32*	-.24*
want/vouloir	-.45*	.63*	-.22*
* p <.05			

There is further evidence in this table that Group F is moving away from French, i.e. there is little evidence of UG/transfer (Hypotheses 2' and 3). Acquisition of ECM, want + tensed clauses and believe + PRO (i.e. areas where transfer might be expected) is also moving in a successful direction. On the other hand, these informants judge sentences exemplifying believe + tensed clauses, promise + PRO and want + PRO to be unacceptable on the apparent assumption that the languages differ even where they don't. While such phenomena suggest a lack of transfer, they also imply a lack of success. Thus, there is more evidence in this table of a tendency to make general and unwarranted assumptions about the markedness properties of English. On the whole, with the exception of croire + ECM, Group A finds the French parameter setting more attractive than Group F (cf. Table 4.2) - in a comparison of their L2 scores - though this tendency could be interpreted as success (see chapter five).

TABLE 4.5. Differences of Means: L1 English - L2 French			
	TENSED CLAUSE	ECM	PRO
believe/croire	-	-	.32*
promise/promettre	-	-	-
want/vouloir	.54*	.70*	-
* p <.05			

Although there is considerable evidence of transfer in this table, Group A accepts want + ECM in English and eschews it in French. There is also high acceptability of vouloir + tensed clauses and croire + PRO, which constitutes further evidence of success. On balance, however, it is clear that Group A is more dependent on their L1 (Hypothesis 3) than Group F.

Results as variegated as these are hard to interpret.

In general, if UG (as defined in this study) held sway, one would expect fewer significant differences, e.g. where promise/promettre is concerned. The learners, in other words, should converge in their judgments more than they apparently do in, for example, Table 4.2. The important exception to this generalization is of course Table 4.3, where there is evidence of considerable overlap in the two groups' judgments. Group A is also slightly closer to the native French setting in the L2 than is Group F in English; this fact can of course be interpreted as evidence of success (see below) rather than a biologically motivated preference for the putative universal setting.

As for transfer, Group A is clearly more dependent on their native language than is Group F, as Table 4.4 and Table 4.5 indicate. Although significant differences emerge in cells where no difference (whatever the reason) was expected, significant differences also occur in those cells where differences were anticipated on the assumption of transfer. Furthermore, on the whole, the directionality of these differences (in Table 4.1 and Table 4.2, for example) supports the notion of residual transfer.

Differences of means mask as much information as they reveal. Such differences, to the extent that they exist, indicate very little about where each group is independently situated. A few conclusions are possible, however. Group A, as predicted, has a slight edge over Group F in acquisition of the L2 (Hypothesis 1). The two groups are closely aligned in their syntactic assumptions about the L2 (Hypothesis 2). Neither group, in the L2, is particularly attracted by the parameter setting instantiated in French (Hypothesis 2'), although Group A is more attracted than Group F. Finally, transfer is a stronger influence on Group A than on Group F (Hypothesis 3), and this is a stronger influence than their attraction for French.

4.3 Comparison of Means: Two Proficiency Levels, Two Response Categories

On the assumption that the configuration of results was to some extent influenced by differences in proficiency levels among the informants, a comparison of sub-groups was undertaken. On the basis of teachers' reports and reported years of formal study of the L2, subjects were divided into sub-groups reflecting reported differences in proficiency. Each sub-group contained ten respondents; thus, 40 of the 96 informants were included in this analysis. High/low differences in mean scores were computed, but significance occurs in only one cell, as indicated in Table 4.6.

TABLE 4.6. Differences of L2 English Means: Low - High

	TENSED CLAUSE	ECM	PRO
believe	-	-	-
promise	-	-	-
want	-	.58*	-

* p <.05

The lack of significant differences overall suggests that the two groups were relatively cohesive in terms of proficiency. This claim of homegeneity finds additional support in the data on acceptability above and accuracy below.

4.4 Proportions Correct: Two Response Categories

For the analyses described above, overall means of the informants' recorded judgments (out of a possible total of either 3 or 2) were tabulated without reference to criteria of accuracy; these were then compared, as specified. Such comparisons, however, do not calibrate the respondents' judgments against prescriptive or "unattested" criteria. The following analysis of accuracy proportions, by comparison, ignores the controls altogether (and the variation in their patterns of response) in an effort to assess the informants' proximity to these more widely established norms[22].

The terms "acceptable" and "unacceptable" below refer to test items.

TABLE 4.7. Mean Proportion Correct

	Acceptable	Unacceptable	Aggregate
Group A: L1/L2			
proportion	.75/.63	.76/.66	.75/.64
std	.16/.15	.21/.17	
Group F: L1/L2			
proportion	.90/.58	.97/.63	.92/.60
std	.12/.15	.06/.23	

These figures reveal important similarities and differences. In the first place, the general distribution of scores on the two foreign language tests across acceptable and unacceptable test items suggests that the groups are closely aligned in terms of L2 proficiency: both scored between 60% and 64% on this measure. One must, however, be careful not to over-interpret this fact: defining the two groups with reference to a study which is supposed to reveal their differences (on the assumption that they are otherwise identical) risks circularity. Nor are these data entirely satisfactory as evidence of a role for UG (Hypothesis 2) since gross

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22 "Accuracy" scores, White argues (personal communication), make "it much easier to make genuine comparisons across subject groups and languages" and "allow a much clearer picture of the extent to which learners transfer/do not transfer the L1 subcategorization frames."

percentages like these tell us very little about how closely aligned the two groups are in their foreign languages, i.e. Can they be said to be operating equally with universal assumptions about the grammar of the L2 on the basis of these figures? Secondly, however, there are significant differences between the groups on the native language measures. This either implies a lack of awareness of the rules of standard English usage on the part of Group A (for the most part, late adolescents) and/or signals a shift in the structure of the language, at least in New York City. There is also significantly more variation around the mean among the native English speakers than among the native French speakers (see below), which further argues for a measure of instability. Finally, these figures suggest that Group F is significantly closer to low English native-speaker norms than Group A is to high French native-speaker norms. This evidence, needless to say, runs counter to the evidence previously cited and argues for caution in a decision about Hypothesis 1.

Additional analysis - yielding slightly variant scores - revealed fewer differences, however. In an analysis of variance (ANOVA), significant main effects for all variables except language and significant effects for all interactions ($p < .05$) emerged, as Table 4.8 indicates.

TABLE 4.8. Mean Proportion Correct on L2 Test: Verb x Environment

	Clause	ECM	PRO
Group A			
croire	.81	.64	.40
promettre	.75	.76	.64
vouloir	.59	.76	.82
Group F			
believe	.53	.26	.70
promise	.67	.67	.74
want	.56	.63	.73

As indicated, both groups achieved their highest scores, on the average, in cells where

the two languages diverge least; their lowest, on the average, where they diverge most. This constitutes an argument for transfer (Hypothesis 3). In general, Group A did better in the four key cells than Group F, with the exception of croire + PRO. Sentences like "Marguerite croyait aimer l'opéra" (item 24) were apparently particularly difficult for them, whereas Group F knew they should reject sentences like "Henry believed to like literature" (item 13). In general, these scores, contrary to those above, lend support to Hypothesis 1, namely, the claim that the acquisition of French as a foreign language is easier than the acquisition of English as such. In fact, both groups were better in French (.81) than in English (.73), which also supports the claim of a generalized attraction for the French parameter setting (Hypothesis 2'). Since this fact could also be interpreted as evidence of a difference in L2 proficiency between the two groups, its relevance to the issue is doubtful. The following table gives overall figures for both tests by group.

TABLE 4.9. Mean Proportion Correct: Group x Test

	English	French
Group A	.85	.68
Group F	.61	.94

As the table indicates, while the differences are greater than those reported in Table 4.7 above, the difference between the two groups on the foreign language tests, though significant, is still on the order of only seven percentage points (cf. four). Even if the difference were not significant, it would still not constitute proof positive of alignment as a consequence of a general reactivation of UG, however. As it is, it offers at least modest support for the claim of a preference for French on the basis of markedness (Hypothesis 1).

To summarize what these analyses reveal, let's examine each of the hypotheses in turn. Paraphrases are given in parentheses.

Hypothesis 1 (Markedness: Group A will do better than Group F on the L2 measure) As Tables 4.8 and 4.9 above show, Group A is closer to native French assumptions than Group

F is to native English assumptions. Given the lack of independent corroboration of their actual proficiency levels in the foreign language (except, of course, for the implicit evidence in the attribute data), however, it is impossible to cite this evidence as the basis for acceptance of this hypothesis. Although the same general tendency is evident in Tables C.1, C.2, 4.1 and 4.2, the lack of significant differences overall (i.e. in a sum of scores across categories) between the two sets of means in each case, rendering a comparison of the two matrices meaningless, also argues for rejection of this hypothesis. Thus, although both groups found the French setting easier to accommodate, there is little support on the whole in these data for the claim of an acquisitional bias in favor of French over English on the basis of markedness.

Hypothesis 2 (UG: Groups A and F will resemble each other on the L2 measure) A decision about this hypothesis requires a direct comparison of the two groups as foreign language learners. On the one hand, evidence from the analysis given in Table 4.3 suggests that there is some basis for the claim of similarity, since there are only two cells (believe/croire + tensed clause and want/vouloir + ECM) that contain significant differences between the two groups. On the other, as we have seen in Table 4.9, the overall proportions for the two groups are significantly different ($p < .01$), so there is little support for the claim that they are operating with identical, or even similar, assumptions about the foreign language. "Accuracy" proportions, however, are limited in their power to establish identity: even if the mean scores were identical, that fact alone would not indicate with any precision how overlapping the two groups' assumptions about the L2 were since these scores might reflect completely different patterns of response, as indeed are evident in Table 4.8. In fact, these figures contain little support for the claim of identity except the trivial example of want/vouloir + tensed clause (in which case each group is close to chance probability).

Additional information about the two groups is provided in Table 4.10.

TABLE 4.10. Group A's Proportions on Both Tests: Verb x Environment

	Clause	ECM	PRO
Group A			
believe/croire	.82	.57	.66
promise/promettre	.82	.84	.70
want/vouloir	.77	.85	.89
Group F			
believe/croire	.74	.61	.82
promise/promettre	.72	.83	.86
want/vouloir	.73	.82	.84

As these figures also indicate, we can have little confidence that the two groups are closely aligned. Among the highest scores (above .80) Group A achieved, one occurs in a cell where one might expect difficulty on the basis of a discrepancy between the two languages (want/vouloir + ECM); among the lowest (less than .70), one occurs where ease was predicted (promise/promettre + PRO). As for Group F, two of the highest occur in areas of predicted difficulty (believe/croire + PRO and want/vouloir + ECM). The best one can say is that want/vouloir + ECM would seem to be less of a problem than anticipated for both groups.

In sum, while there is some evidence here that the two groups are making comparable judgments in their L2s, there is not enough to support a strong claim that they closely resemble each other as a consequence of a generalized reactivation of UG. Therefore, only hedging support for this hypothesis is possible.

Hypotheses 2' and 3 (UG: On the L2 measure, both groups will resemble Group F on the L1 measure. Transfer: On the L2 measure, each group will resemble itself on the L1 measure) As suggested above, Table 4.9 indicates an overall preference for the French setting which is consistent with Hypothesis 2'. On the other hand, a comparison of the scores recorded in Table 4.9, which indicates how well both groups did in the foreign language, and Table 4.10,

which gives the overall means for both groups defined as native speakers, reveals no such preference.

TABLE 4.11. Mean Proportion Correct on L1 Test: Verb x Environment

	Clause	ECM	PRO
Group A			
believe	.84	.50	.92
promise	.89	.93	.76
want	.95	.95	.96
Group F			
croire	.95	.97	.94
promettre	.77	.99	.98
vouloir	.89	1.00	.95

As indicated, Group F's scores in the L1 test are exceptionally high: promettre + tensed clause, for no apparent reason (see below), seems to be the only sticking point. By comparison, the lowest scores achieved by Group A on the L2 test (Table 4.8) occur in three of the four predictably difficult areas (on the basis of transfer) and in one cell which was predicted to be easy (promettre + PRO)[23]. Group F, by contrast, had most difficulty on the L2 test with all four of the expected verb environments as well as three areas where no difficulty was anticipated: believe and promise + tensed clause and promise + ECM. Although one of these areas proved difficult for Group F defined as native speakers (promettre + tesnsed clause), this fact alone does not constitute a strong argument for similarity between the two sets of test results, i.e. a general preference for the setting in French.

As for Hypothesis 3, as indicated in Table 4.8, the fact that both groups achieved their highest scores, with some exceptions, in the areas of similarity between the two languages and

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23 Bley-Vroman et al. "recommend that if native-speaker consensus falls below about 90%, then it should be concluded either that something is wrong with the test or that our understanding of this area of UG is incomplete" (1988: 19).

their lowest, with some exceptions, in areas of dissimilarity suggests a general predisposition conferred by the L1. Table 4.11, in addition to the information summarized above, reveals that Group A had particular - and inexplicable (see below) - trouble with believe + ECM and promise + PRO. A close comparison of Table 4.8 and Table 4.11 indicates that the comparatively low scores achieved by Group A on both tests are more proximate than the comparatively high scores achieved by Group F on these tests. Does that mean that the L1 was more influential among the native English speakers than among the native French speakers? The proposition seems implausible on the face of it (particularly given the broad similarity in acquisitional experiences between these groups), but it is at the very least conceivable. What is more likely, however, is that, given the general depression in Group A's scores on the L1 test, together with the failure of "accuracy" scores generally to establish identity across response patterns with any precision, no conclusion can be reached about the effect of transfer on these groups from these data. The lack of a consensus among Group A on certain aspects of English grammar (which are nonetheless considered to be uncontroversial in the literature), in other words, merely creates the specious appearance of transfer, and we can have little confidence in these data as a basis for making a decision about this hypothesis of transfer.

4.5 Materials Analysis

An examination of the scores for native speakers also uncovered six problematic sentences (among the 36) on the English test and two (among the 36) on the French test. That is, judgments varied considerably more on these items than on the others.

On the English test, four of the six involved believe + ECM, the full complement of such items. The number in parentheses on the left indicates the item's position in the test; the number of responses indicating acceptability (out of 48) is given in parentheses on the right.

(1) She believed Pete to be very happy (27)

(20) I believed John to be intelligent (24)

(30) He believed the students to be crazy (24)

(42) They believed actors to be intelligent (19)

Since all four of the believe + ECM items proved troublesome for the informants as a group, it would be hard to avoid the conclusion that this structure is, at the very least, uncommon in the input. It is hard, however, to understand why these sentences became increasingly unacceptable as the test wore on.

In addition, the first item involving promise + ECM and the first item involving promise + PRO struck these informants as difficult, though considerably less so than the four items given above:

(5) Alice promised Freddy to take the train

(It is Freddy who will take the train) (7)

(6) She promised to babysit with the kids (11)

As the numbers on the right indicate, these items were less confusing for Group A as a whole. It is not hard to guess why (5) might have been a problem, i.e. since it was the first item which included a statement of implicature. (6) is harder to sort out; conceivably, "kids" struck some informants as unacceptable, i.e. there is a pragmatic constraint on its use in this context.

As for the French test, the two problematic items involved vouloir and promettre + tensed clause:

(7) Les élèves voulaient que Philippe les aide (35)

(21) Luc a promis qu'elle partirait à Nice (19)

Conceivably, the first of these presented the informants with a confusing real-world situation; conceivably, in the second there is an expectation of a masculine pronoun, i.e. pronominal coreference, though clearly the sentence is grammatically correct.

4.6 Summary

As indicated in the foregoing discussion, only the data relevant to Hypothesis 2 receives any - and only grudging - support. The other three fall short of unqualified support in these data or receive ambiguous or trivial support. Even Hypothesis 2 finds little support

in the analysis involving "accuracy" proportions (141).

Overall, the pattern of significance, however, is consistent with the notion of transfer since significant differences emerge in those cells where differences were expected because of differences in the syntax of the two languages. For example, Tables C.1, C.2, 4.1 and 4.2 above indicate significant differences in all four key cells: want/vouloir + tensed clause and ECM, believe/croire + ECM and PRO. Furthermore the directionality of these effects is consistent with a prediction based on transfer. Unfortunately, differences also occur in cells for which no such prediction was made, so the picture remains unclear. Additionally, Group F in Table 4.4 above exhibits more independence from their native setting (i.e. success) than does Group A in Table 4.5 from theirs. This fact suggests that neither group is opting for the supposedly universal setting instantiated in French though they are somewhat aligned in their assumptions, whatever the source.

The process of making a decision about the hypotheses is complicated by the lack of a formal, e.g. statistical, metric for making comparisons between and among matrices. For example, a decision about Hypothesis 3 depends on an ability to assess (a) how much transfer each group exhibits, (b) which group exhibits more and (c) how influential L1 transfer is compared to, say, the groups' attraction for the setting instantiated in French. There is no way to make the comparisons implied in (b) and (c) except descriptively. Unfortunately, the study as a whole requires several such comparisons, as outlined above (120).

Furthermore, as the discussion reveals, unexpected differences in the data on promise/promettre, believe/croire + tensed clauses and want/vouloir + PRO remain inexplicable. Some of these discrepancies have been teased apart in the materials analysis above, but that analysis alone does not provide an explanation for their occurrence. The evidence on believe + ECM probably indicates either age-related differences in usage (i.e. a pragmatic constraint) and/or a general shift in the language.

In sum, these learners appear to be operating with somewhat similar assumptions

about their L2s. This constitutes prima facie evidence of an underlying convergence but tells us very little about its content. Apart from that, the data are not sufficiently robust to give us an unambiguous picture of the relative effects of UG, markedness and transfer. In chapter five, the data are reexamined and the issue reformulated.

CHAPTER FIVE

Conclusions

It should be clear from the foregoing that these data reveal almost no role for UG in the process of SLA. Since, in the case of Group F, UG is the same as transfer and, in the case of Group A, it is the same as success, this study actually contrasts transfer and success. The fact is that both of these groups are relatively successful in their acquisition of these parameters; the only exceptions are Group F's failure to acquire believe + ECM and Group A's failure to acquire croire + PRO. As these are comparatively peripheral aspects of the languages' grammar (and rarely addressed in texts or instructional activities except by implication), it is not surprising that they have not been acquired.

5.1 This point becomes clearer when one compares expected scores, on the assumptions of transfer and success, with those actually achieved. If, for example, transfer had held sway where believe + ECM was concerned among Group F, one would expect complete rejection of these test sentences; if, on the other hand, success was paramount (i.e. the informants had fully acquired the L2 system), then one would expect complete acceptance. This comparison is spelled out in the following table. "Percent agreement" indicates the extent to which the informants' responses were consistent with each assumption, whether that meant accepting or rejecting the test sentences themselves.

Thus, for example, in Table 5.1, with reference to ECM and croire, the figures 1.00/.36 and .00/.64 appear. The first of these two sets indicates that, on the assumption of transfer, a mean of 1.00 was expected; that is, if transfer governed the acquisitional process, one would expect Group A to find examples of ECM in French entirely acceptable (1.00). Likewise, on the assumption that they have been completely successful in acquiring the target syntax, one would expect them to find such examples entirely unacceptable (.00). In the event, this group was correct 64% of the time (.64) - in other words, agreed with the prediction of success in 64% of their responses. By contrast, they opted for the transfer assumption 36% of the time; that

is to say, they carried the syntax of English over into French in 36% of their responses. As these facts suggest, the learners were right more often than not.As indicated above, the most interesting contrasts occur in those cells where the languages are most divergent, since predictions of transfer and success differ in these areas. The critical cells are, of course, believe/croire + ECM and PRO and want/vouloir + clause and ECM. All of the learners exhibited more success than transfer in these critical cells with two exceptions. In fact, once these two cells are extracted, the scores achieved in the remaining three cells in each case do not differ greatly from the scores achieved overall in the other ten (66.3 v. 66.8 for Group F and 75.6 v. 66 for Group A). These informants were by and large successful learners of the L2.

TABLE 6.1. Expected Mean Acceptance on the Assumptions of Transfer or Success/Mean Proportion Achieved

Group A			
	Clause	ECM	PRO
croire			
T*	1.00/.81	1.00/.36	.00/.60
S**	1.00/.81	.00/.64	1.00/.40
promettre			
T	1.00/.75	.00/.76	1.00/.64
S	1.00/.75	.00/.76	1.00/.64
vouloir			
T	.00/.41	1.00/.24	1.00/.82
S	.00/.59	.00/.76	1.00/.82
Group F			
	Clause	ECM	PRO
believe			
T	1.00/.53	.00/.74	1.00/.30
S	1.00/.53	1.00/.26	.00/.70
promise			
T	1.00/.67	.00/.67	1.00/.74
S	1.00/.67	.00/.67	1.00/.74
want			
T	1.00/.34	.00/.37	1.00/.73
S	.00/.56	1.00/.63	1.00/.73

* T = transfer
** S = success

As indicated above, the most interesting contrasts occur in those cells where the languages are most divergent, since predictions of transfer and success differ in these areas. The critical cells are, of course, believe/croire + ECM and PRO and want/vouloir + clause and ECM. All of the learners exhibit more success than transfer in these eight critical cells with two exceptions. In fact, once these two cells are extracted, the scores achieved in the remaining

six cells (three for each group) do not differ greatly from the scores achieved overall in the other ten (five for each group). So, for example, Group A achieved a mean of 66.3 in the three remaining cells and an overall mean of 75.6 in the remaining five. Group F achieved a mean of 63.0 in the remaining three and an overall mean of 66.8 in the other five. These informants were by and large successful learners of the L2.If, by contrast, UG had played a significant role in the process, one could anticipate clearly differentiated effects between the two groups. That is, since UG is identical with transfer in Group F's case and success in Group A's, two conditions must be met to support the view that UG plays a role: high transfer scores for Group F and low transfer scores for Group A. This hydraulism is not apparent in these data. In fact, overall success scores for both groups in the four critical cells are virtually identical (56.25 for Group F and 59.5 for Group A). It would be ridiculous to assert, without explanation, that Group A had access to UG while Group F did not. Rather, the notions of transfer and success provide a perfectly adequate explanation for the data. This, of course, does not mean that UG plays no role whatever (see below): it simply means that an assumption of UG does not provide a better fit than transfer and success. It is clearly not paramount.

5.2 Two questions remain.

Why do these informants exhibit so much more transfer than success in two of these cells?

None of the six cells reveals a lack of transfer effects. Transfer, as indicated throughout chapter two above, is ubiquitous in L2 acquisition among adults. It would be surprising if there were no transfer evidence in these data. It is also not hard to see, consistent with the assumption of a role for UG, why 74% of Group F reject believe + ECM while only 60% of Group A reject croire + PRO: clearly, ECM is a more marked phenomenon than PRO. It is also clearly easier for Group A to shed their dependence on ECM than for Group F to accommodate it - consistent with the notion of markedness differences as triggering mechanisms. Other possible explanations can be found by recourse to system-external variables, chiefly input. As

indicated, it is unlikely that either of these rule systems is given much attention in instruction.

Why have the informants fallen so far short of expectations in the other six cells?

Here, as elsewhere, there is evidence that Group A has a slight edge over Group F (76% v. 67% success rates respectively). This is consistent with the idea that less marked systems are easier to acquire than more marked ones. Both groups, however, as these rates indicate, are close to success: they are both about two-thirds of the way along the IL continuum. It is entirely possible that a generalized perception that the languages differ even if they don't may account in part for this phenomenon of incompleteness. The view that generalized perceptions affect the process finds support in the work of Kellerman (1983), Meisel (1983) and Eubank (1989) (see below), as well as in the avoidance literature (See especially Schachter and Hart 1979 and below).

CHAPTER SIX

Discussion

6.1 As I have suggested, problems abound in the interpretation of the results achieved in any SLA study that touches so many subordinate issues. Evidence of UG reactivation among Anglophone learners of French, for example, can be interpreted as success. By the same token, evidence of residual L1 dependency among such learners in this study - or, for that matter, the Francophones' accessibility to UG (to the extent that it exists) - can be interpreted as transfer. Thus, it could be claimed that comparable learners of two languages exhibit contrary tendencies: one group rushes forward to embrace the rules of the new language while the other clings to the parameter setting associated with their L1. Although this hydraulism may seem improbable at first, it is of course exactly what one would expect if UG plays a role; indeed, as these data show, it is evidently easier (and in "accuracy" scores, significantly so) for Anglophone learners to acquire French than for the opposite to occur. In other words, this complementary distribution in terms of general learning patterns is consistent with several notions, among them a role for UG. Put another way, what is described as success can also be explained as success promoted by UG. What is described as transfer can also be explained as failure constrained by UG. Unfortunately, what we have here overall is a muddier picture - a more marked, though insignificant, predisposition for transfer and success among the Anglophone learners.

In this particular study, differences in processing time (see below) may account in part for the results. On the other hand, even if there were differences in terms of cognitive processing time (say, it took these learners longer to process English verbs than French verbs), such differences are not necessarily inconsistent with the notion of a role for UG. Indeed, it would be remarkable if there were no differences in processing time between M-U and U-M learners. That is, it is hard to imagine differences in syntactic markedness that would not also entail differences in complexity. Since, however, time was not a factor in this test - for

precisely this reason - and is therefore not a variable, there is very little point in assuming that all such differences evident in these data can be attributed to processing constraints.

Of course, this begs the question of transfer. How, in other words, can one disambiguate the effects of UG from those of transfer in unmarked languages like French? In a word or two, one cannot. With such subjects, the use of an artificial second language would seem to be one option (see below for others). What is measured in this study is an ability to manage both natural systems as a whole. From that point of view, both L2s have a potentially biasing effect since the overall regularity of the French system is offset by the acceptability of ECM in English, at least where the two focal verbs (believe and vouloir) are concerned. Thus, the English system can be seen to be more expansive, less restrictive than the French: it permits more options (though unfamiliar ones). In a sense, it is more heuristic than the Procrustean French setting since it offers more opportunity for hypothesis testing. These facts may lead ultimately to an explanation for the lack of significant differences, at least in the acceptability (cf. accuracy) data.

6.2 Let us now consider these explanations related to potential methodological, conceptual and interpretive problems in more detail.

In the first place, the results could be seen as an artifact of the sampling. Since, however, the interaction effect overall involving [group x language] was itself insignificant (on a one-tailed ANOVA) in the analysis of variance involving three response categories, there is little evidence of distortion or exceptionality in these data. Their internal consistency suggests that even additional testing may not have yielded an interesting difference in overall patterns.

On the other hand, it is quite clear that, as suggested above, the use of intuitional judgment tasks with L2 speakers is sometimes problematical. As the materials analysis indicates, some of the test items did receive divergent responses when convergence could have been anticipated. More to the point, however, there is divergence in the judgments of these learners defined as native speakers. Although all research that depends on intuitional

judgments - or, for that matter, production tasks - runs into this problem (i.e. the lack of absolute agreement), the problem is particularly acute when L2 learners are being measured against widely variant native-speaker norms. In this case, it probably would have been a good idea to take a preliminary sample of native English speakers on believe + ECM, though that effort in all likelihood would have resulted in the elimination of these sentences from the test and thereby have made it impossible to test out claims that are widely attested in the syntactic literature.

Secondly, there is also the possibility that French does not instantiate a universal parameter setting. As indicated above, the claim of universality rests entirely on the perception that the French system is intrinsically simpler and more regular than the English system, together with notion that ECM is relatively rare in the world's languages. Nowhere in the literature are the verbal properties targeted in this research described as a cohesive parameter (cf. subjacency, "pro-drop," ECP, etc.). The aim of this research was never to argue for the elevation of these properties, taken as a group, to the status of parameter. Rather, the aim throughout has been to look for evidence that these learners were making assumptions about the grammar of the L2 which were consistent with the notion of parameter setting. Since such evidence would naturally consist of statistical tendencies - or the mere appearance of a relationship - and the hope of disconfirmation, it could never be cited as the basis for a theoretical claim. Universality is only presupposed in this study, not formally supported.

Thirdly, as indicated in chapter one, certain environmental ("system-external") factors may account for some of these results. Thus, for example, the fact that Group A were far from unanimity in their judgments of the believe + ECM sentences, together with Group F's reluctance to accept them (though this could be traced to UG or transfer), may indicate a lack of exposure, i.e. their absence from the input. As I have suggested, such sentences (despite general acceptance in the literature) are probably not in general currency; they are, among other things, extremely rare (if they occur at all) in EFL textbooks. If their acquisition depends

on frequency, then quite clearly the verbal properties associated in this study could never be construed as a cohesive parameter whose partial acquisition would trigger additional effects. Something like the "double-input" model (Felix 1985), which predicts measured, incremental acquisition and frequent "restructuring," may provide a partial explanation and overall a better fit.

Finally, there is always a risk of reification at the level of interpretation, as Gregg (1989b) says, "unconscious, and not so harmless, reification of acquisition...especially in combination with an equally dangerous reification of language" (16). In this connection, he cites Chomsky's admonition:

> Consider the way we describe a child or a foreigner learning English. We have no way of referring directly to what that person knows: It is not English, nor it it some other language that resembles English. We do not, for example, say that the person has a perfect knowledge of some language L, similar to English but still different from it. What we say is that the child or foreigner has a "partial knowledge of English" or "is on his or her way" toward acquiring knowledge of English, and if they reach the goal, they will then know English. Whether or not a coherent account can be given of this aspect of the common-sense terminology, it does not seem to be one that has any role in an eventual science of language.
>
> (1986: 16)

Similarly, there is a tendency evident in some of the more recent literature to make premature and rather far-reaching generalizations on the basis of the most preliminary and in some cases problematical (see above) research; many of these efforts delve deeply into areas of linguistic theory and cognitive psychology. Although the issues they raise are important and relevant to the eventual development of a theory of SLA, any such effort to capture what is essentially a dynamic process - when the object of this process is itself so poorly understood - with reference to formative empirical research is bound to fail. Data, as is evident in the case of this study, rarely fall out into neat, clean categories. To the extent that they are messy and recalcitrant, they complicate the process of interpretation.

6.3 Several of the proposals reviewed in chapter two may, on the other hand, shed some light on the results achieved in this study. These proposals fall into two broad categories:

they are proposals relevant to the markedness issue and to the overall shape and direction of the process.

6.3.1 Hyltenstam's Claims (1987)

As indicated above, these predict retention patterns arising from differences in the actual markedness properties of the two languages in question (see 22 above); they were formulated with reference to typological assumptions. Since they make predictions about how long a form will be retained, they have temporal as well as 'spatial' implications. Essentially, unmarked L1 forms are predicted to last longer (across the acquisition of marked L2 forms) than L1 marked forms (across the acquisition of L2 unmarked forms). These claims, among others, provide a rationale for Hypothesis 1, namely, that Group A will have an easier time than Group F (on the assumption that differences in the two verb systems reflect differences in markedness). As we have seen, this prediction has not been strongly borne out in these data; if it had been, one would have seen significantly less evidence of English transfer in the L2 data than French. On the other hand, the argument could at least be made - as these predictions are not precisely formulated or operationalized in temporal terms [cf. "for a long period of time" (1987: 68)] - that both groups have passed the stage at which these effects would be evident. In any case, the study does not primarily address the temporal issue.

6.3.2 Eckman's Markedness Differential Hypothesis (1985)

In this case, markedness is given an implicational definition (28 above), namely, that a "phenomenon or structure" is more marked if its presence in a grammar implies the presence of the other less marked element. This definition also employs the notion of degrees of markedness since, as we have seen above, markedness is by definition relative whether we employ the term within a single language or across languages. The hypothesis predicts that more marked L2 structures - as defined with reference to the markedness value of an L1 equivalent - will be harder to absorb (consistent with Hypothesis 1 above) and further that their relative difficulty will depend on their degree of markedness. Unfortunately, as we have

seen, there is little reason, where the two groups in this study are concerned, to think that such considerations play a role. There is some evidence that Group A has a slight edge in the SLA race, but this group also exhibits more - on the whole, insignificantly more - transfer effects than its counterpart.

6.3.3 Kellerman's Transferability Constraint (1983)

With reference primarily to semantics, Kellerman claims that if the two languages are perceived to be similar (whether they are or not), unmarked structures are likely to be transferred (see 68 above for references to one study). He further claims that if the L2 is perceived to be more marked (whether or not it is), then this perception alone will block transfer, although some co-variance between this tendency and the complementary one is likely to occur (e.g. there may be more transfer of marked properties at the outset). In other words, transfer is largely conditioned by the learners' sense of how complex their L1 is vis à vis their L2 (see also Meisel 1983 and Eubank 1989 for more on this). Since this study obviously does not investigate learner perceptions of the L1 or L2, it would be hard to find in these data any disconfirmation of this psychotypological claim, although it is conceivable that Group F's comparative lack of L1 dependency stems from an accurate perception that English is more marked. Likewise. the general lack of a significant difference between the two groups where transfer and success are concerned (with significant differences occurring for the most part only in marked categories, with believe the primary exception) may also reflect these learners' perceptions that the languages are different, the structures are different in terms of their markedness properties and restraint is in order. In that case, one would naturally expect to see more transfer if these conditions did not hold, say, in a two-way comparison of French and Spanish L2 acquisiton. One would probably also expect to see more of a difference between the groups in this study than is apparent.

6.3.4 Zobl's Predictions (1984)

Following Wode (1984) and consistent with Kellerman, Zobl (54) stresses the importance of typological congruity in predicting transfer behavior. That is, transfer will occur if the L2 grammar is relatively transparent: if the L2 rule is "obscure," either because it is peripheral (in a Chomskyan sense) or variable, then L1 transfer can be expected. Additionally, there is a general constraint on the transfer of marked L1 forms[24]; indeed, in Zobl's view, SLA involves the gradual expansion of the L2 grammar from a reliance on unmarked categories - by means of their "projection" - into an acceptance of marked areas of the language. Be that as it may, there is no evidence in this study of any such tendency on these learners' part. As indicated, they are somewhat aligned in their judgments of the L2 - regardless of the fact that the L2s are typologically different - and neither group exhibits significantly more transfer than the other. If Zobl's prediction were correct, then one would naturally expect to see more clearly differentiated effects, with, for example, more transfer and less accommodation of marked L2 forms among Group F.

6.3.5 Ellis' Summary (1985)

As indicated above (76), this summary suggests that there is a general predisposition in favor of the transfer of unmarked items when the learner encounters functionally equivalent marked phenomena (particularly in perpheral rule systems) in the L2. There is also a general bias against the importation of marked items (particularly if the L1 contains both marked and unmarked options), although there may be some evidence of this at the formative stages. As suggested repeatedly, such tendencies did not emerge dramatically in this study. Rather, there would seem to be much more evidence of convergence than divergence in the judgment behavior of these learners overall.

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[24] Zobl makes this claim despite the widely attested phenomenon of L2 calques of L1 material. Indeed, word-for-word translation of marked everyday expressions such as idioms is a common feature of L2 behavior as any ESL teacher knows.

6.4 More plausibly perhaps, we can see in these results evidence of differences in cognitive processing. Clearly, it is at least conceivable that this variable may account for some of these effects, although evidence of the influence of processing constraints does not ipso facto exclude the possibility of a role for UG (see above). Nor is it possible to find in these data a basis for very searching claims about the role of cognitive processing since the study was obviously not conceived to test out such hypotheses. Rather, the comments that follow lead to speculations about new avenues of research that may contribute to an understanding of the process as a whole.

6.4.1 Felix' Competition Model (1985)

As outlined above (41), Felix' idea rests on the claim of a difference in the acquisitional capacities of children and adults: UG is available to children (though constrained by Piagetian stages), a "general problem-solving" cognitive system kicks in where adults are concerned. This so-called PSC-system then competes with UG (instantiated in the L1 presumably) to complicate the process for these learners and make it impossible for them to "complete" the acquisition of an L2 the way children complete the acquisition of an L1 (see Schachter 1989b for additional discussion of what she calls Felix' "maturational hypothesis"). The model therefore predicts that L1 grammars will be closer to UG than L2 grammars, which are mediated by the influence of this poorly suited general system. Since this study does not compare the acquisitional states of children and adults, it can have no relevance to the basic claim of a difference. On the other hand, it is quite obvious that neither group in this study was particularly attracted by the system of verbal properties associated with French, so there is some evidence that these late adolescents may be employing a strategy other than strict adherence to UG. They also show no particular preference for 'the easy way out', that is, on the level of general language processing, for the most parsimonious grammar. This lack of clearly differentiated effects, in other words, may indicate an element of competition between alternative learning strategies consistent with Felix' formulation. It would be helpful to have

additional (and clear-cut) comparisons of the judgments of children and L2 adults, of course with reference to some area of the grammar that both can accommodate cognitively, to resolve the fundamental issue. Presumably, the acquisition of second languages by both age groups would be the most suitable domain for this research.

6.4.2 Clahsen and Muysken's Claim of Partial Availability (1989)

Clahsen and Muysken of course also claim that the influence of UG is constrained in adult SLA; in this case, the availability of UG (instantiated in the L1) is overtaken by "principles of information processing and general problem solving" (1989: 23). As indicated, the comparative success of these learners may stem from the efficacy of these general learning strategies: since they can all be assumed to have had successful learning experiences in formal settings (and were all students when they took the tests), they had all devised strategies of some kind for general learning purposes. The evidence we have of their success in these judgment tasks may flow from that capacity rather than from any of the other sources formally defined in the study.

6.4.3 Eubank's Parsing Solution (1989)

This solution relates directly to the (methodological) presentation of test sentences, which in the case of Flynn's 1984 and Eubank's 1989 studies involved the oral presentation of test material. However, it is interesting because it demonstrates how alternative post hoc hypotheses can be developed to account for inexplicable (and in Flynn's case liberally interpreted) results. Since, however, variation in the presentation of the test material is not an issue where a comparison of Group A and Group F is concerned, Eubank's conclusion (the "findings do coincide with the predictions developed from the idea of a juncture," 1989: 69) is of only marginal interest, although this conclusion, together with his reanalysis of Flynn's data from a processing perspective, points up the importance of regulating input in studies of UG. The question of whether environmental, test-external input is an important factor is of course a separate issue; nor can we draw precise inferences from any of this speculation about the

role of processing generally in a learner's generation of his foreign language. Precision where such predictions are concerned requires, among other things, a clearer sense of the internal processing mechanisms and associated hypotheses than is now possible. Bley-Vroman, I hasten to add, would take issue with this view (see 1986: 9).

6.4.4 Bley-Vroman's Fundamental Difference Hypothesis (1986)

In his distillation of these issues (referring, in part, to several of the views already described), Bley-Vroman arrives at a formula for adult SLA which gives the adult two "advantages" [knowledge of the L1 (including residual knowledge of UG) and general cognitive ability] over the child, who has access to UG and "domain-specific learning procedures" at his disposal. These are familiar claims, but in this case the hypothesis of a difference was tested out (Bley-Vroman et al. 1988) with the result that the hypothesis in its strongest form, i.e. the complete inaccessibility of UG, was not sustained. Therefore, we are left with the somewhat imprecise notion of partial accessibility (see also Clahsen and Muysken 1989), for which there is at least broad support in the data from this study. In essence, to the extent that the claim of UG accessibility is diluted to accommodate obvious child-adult differences and more and more ambiguous results from SLA studies, it becomes easier to find evidence of a relationship but harder to find in this evidence the heuristic basis for subsequent claims. In this case, however, the hypothesis was that UG is completely inaccessible, so a decision to reject begs the question of the extent to which it plays a role in SLA. In the case of this two-way study, by comparison, Hypothesis 2 predicted a similarity between the two groups' assumptions about their L2s and found some evidence of UG's influence in this direct comparison.

6.4.5 Schachter's Window-of-Opportunity Hypothesis (1989b)

This hypothesis, like many constructs employed in adult SLA research, was formulated with reference to L1 acquisition; that is, it offers an alternative to the notion that UG is initially and continuously available and the notion that UG accessibility is phased in gradually as the child matures cognitively (see Felix 1982). Schachter's idea is that "there are stages

before which and after which the principles will not have an effect on the developing grammar" (1989b: 9). The three proposals obviously have different consequences where adult SLA is concerned. Specifically, with respect to the "window of opportunity," one could expect adults who had missed the chance of exposure to the L2 before puberty to have only a residue of UG instantiated in the L1, in which case transfer is a necessary though insufficient influence on the process and "incompleteness" is explained. This is a perfect example of an interesting (though rather abstract) hypothesis which is virtually impossible to test out with any precision (i.e. even if you get comparatively 'clean' results, you still don't know if those results were achieved solely because of the hypothetical sequence under study). As the review above indicates, Schachter found support for this hypothesis in her data and argues that any SLA theory which fails to take the facts about "completeness" into account may lack explanatory adequacy.

Although there is plenty of "incompleteness" in these data, if Schachter's hypothesis held true, then presumably there should have been much more of an advantage for Group A. If, for example, Group F (virtually all of whom began study of the L2 after puberty) missed the opportunity to acquire ECM at the biologically apppointed time in its acquisitional sequence, then obviously that would have put them at a severe disadvantage where the acquisition of English rule systems was concerned. By comparison, few differential effects should have emerged where PRO was concerned - or, more precisely, there should have been a clear difference between the data on PRO and the data on ECM. Since there is no such evidence in these data, it is hard to avoid the conclusion that the "window" may never have been open or, indeed, shut for these learners. Of course, as I have suggested, since there is no claim in this study that these verbal properties constitute, in the aggregate, a cohesive parameter (they merely exemplify a difference along a presumed dimension of universality), the argument could conceivably be made that these data may not be the best place to look (cf. Schachter's study of subjacency relations) for evidence of a biologically determined

acquisitional pattern.

6.5 In sum, predictions based on markedness considerations are less persuasive than assumptions based on "multiple sources" or an attenuated effect for UG (presumably via the L1). This is largely because predictions based on markedness purport to specify what will and will not transfer, and there are relatively few differential transfer effects in these data. By contrast, the other proposals surveyed attempt, in essence, to account for the failure of adults to behave exactly like children and wind up finding a principled basis for integrating what is known about L1 and L2 acquisiton and what is known about UG and, among other things, processing. These subjects' striking similarity as L2 learners suggests that UG has at least some role to play in the process though obviously this influence alone cannot explain the results or be said to wholly motivate the process. A principled synthesis of the various factors that inhere in the process - including UG, L1 knowledge and input - will be required before we have begun to understand it as a whole.

6.6 The purpose of this research was to contrast two types of learners: those moving in an M-U direction and those moving in a U-M direction. Theoretically, if differences in degrees of markedness influence the process of second language acquisition, the acquisitional process should be easier for the first of these two types. Furthermore, if markedness assumes its expected relation to UG, both groups should show a preference for the unmarked setting in their judgments about sentences in the L2. If not, it is reasonable to look for evidence of transfer in these judgments. The particular contribution of this research is its bi-directional design. As the Review of the Literature above indicates, few such studies have ever been attempted. When, by comparison, two disparate groups are compared, i.e. different learners acquiring different languages, confounds emerge which make precise inferences all but impossible. In this case, every effort was made to keep the two groups similar to avoid such contamination.

Two sets of problems emerged in the course of this research, one of them

methodological in nature, the other conceptual.

The assumption that there would be high agreement among the Anglophone learners in their judgments about the syntax of English turned out to be unwarranted. In particular, their reluctance to accept believe + ECM made a comparison of their judgments and those of the Francophone group where this [verb x environment] cell was concerned difficult. As a consequence, the analysis of variance (ANOVA) involving judgments of acceptability was less revealing than the analysis involving "accuracy" data. Furthermore, the use of a difference-of-means test left several questions unanswered. Such a statistic gives only a general impression of how proximate two such groups are in their syntactic assumptions. Even if there is no significant difference in scores overall, there may be considerable variation in the judgments that go into that average.

Secondly, on the conceptual level, a generalized preference for the parameter setting instantiated in French can be interpreted as transfer on the part of the Francophone group and success on the part of the Anglophone group. Indeed, given the lack of a generalized preference in these data, there is certainly no reason to maintain a UG-based interpretation in preference to a more straight-forward transfer/success interpretation. This, as I have indicated, does not imply that UG has no role whatever to play. Rather, it means that these data cannot sustain that hypothesis and further study will have to be undertaken, possibly involving artificial languages, to assay the nature of that role[25]. It may be, as I have

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[25] I am indebted to Richard Kayne for two additional ideas. One is that a wider array of verbal structures (e.g. there is/are, consider, assume, passives with such verbs as acknowledge and think or phrasal verbs with associated particle movement) might be examined in subsequent research; consider, for example, in all likelihood would prove less controversial than believe in these contexts. The other, closely related idea is that informants - conceivably, across several age categories - might be asked to react to a wider range of implicational and distributional properties. So, for example, they might be asked to react to a sentence like John has been wanted to leave and thereby demonstrate their understanding of the parameterized limitations on passivization. I am also indebted to Martin Chodorow for the notion that informants might be asked to paraphrase sentences that have been presented for judgment.

indicated, that what will ultimately emerge is a reformulation of transfer in a principles-and-parameters framework (see 116 above for more on this). In any case, a study which probes, by means of interviews, the basis for the judgments of such learners would give a richer sense of this complex dynamic and its probable influences.

APPENDIX A

QUESTIONNAIRE

(1) Name _____

(2) Sex M ___ F ___

(3) Date of birth _____

(4) Place of birth _____

(5) How many languages do you speak (including your native language)?

(6) What languages do you speak?

Are there any languages that you can understand without being able to speak them? If so, which ones are they?

(7) How many years of schooling have you had?

Primary ___ Secondary ___ College ___

(8) How old were you when you started to study French?

(9) How many years have you studied French?

(10) Are you studying French now?

yes ___ no ___

(11) Have you ever spent time in a country where French is spoken?

yes ___ no ___

If so, which country and for how long?

country length of stay

_____

QUESTIONNAIRE

(1) Nom _____

(2) Sexe M ___ F ___

(3) Date de naissance _____

(4) Lieu de naissance _____

(5) Combien de langues parlez-vous (y compris votre langue maternelle)?

(6) Quelles langues parlez-vous?

Est-ce qu'il y a des langues que vous ne pouvez que comprendre?

Si oui, lesquelles?

(7) Combien d'années de scolarité avez-vous derrière vous?

Ecole primaire ___ Collège ___ Lycée ___ Université ___

(8) A quel âge avez-vous commencé l'étude de l'anglais?

(9) Pendant combien d'années avez-vous etudié l'anglais?

(10) Etudiez-vous l'anglais cette année?

oui ___ non ___

(11) Avez-vous déjà séjourné dans an pays de langue anglaise?

oui ___ non ___

Si oui, quel pays et pour combien de temps?

pays durée de séjour

_____

APPENDIX B

INSTRUCTIONS

Please indicate the degree of acceptability for each of the following sentences.

If the sentence is ACCEPTABLE, check LETTER A.

If you are NOT SURE, check LETTER B.

If you are sure the sentence is UNACCEPTABLE, check LETTER C. Then rewrite the sentence in an acceptable form.

EXAMPLE:

(a) Je sais que Jean est très gentil

Since this phrase is perfectly ACCEPTABLE, you would naturally check letter A.

(b) Marie pense Jean aimer la cuisine

Since this sentence is entirely UNACCEPTABLE, you would

check letter C and rewrite it in an acceptable form.

For example:

In this test, please make your judgments as quickly as possible. However, be sure to consider each sentence carefully.

Thank you.

INSTRUCTIONS

Indiquez s'il vous plaît le degré d'acceptabilité des phrases suivantes.

Si la phrase est ACCEPTABLE, cochez LA LETTRE A.

Si vous avez des DOUTES, cochez LA LETTRE B.

Si vous êtes certain que la phrase est INACCEPTABLE, cochez LA LETTRE C. Dans ce cas, réécrivez alors la tournure qui convient.

EXEMPLE:

(a) I know Ronald to be very friendly

Puisque cette phrase est parfaitement ACCEPTABLE, vous cochez la lettre A.

(b) Marie says to like Italian food

Puisque cette phrase est de toute évidence INACCEPTABLE, vous cochez la lettre C. Vous réécrivez la tournure qui convient, par exemple:

Pouvez-vous faire votre choix le plus rapidement possible, tout en y accordant dans chaque cas la plus grande attention.

Merci.

(1) Elle a promis que son frère l'aiderait

(2) Christophe a dit qu'il travaillerait plus

(3) J'ai promis de prendre de longues vacances

(4) Le chef voulait son assistant venir

(5) Il a dit qu'il irait à l'assemblée

(6) Il l'a persuadée de faire ses devoirs

(7) Les élèves voulaient que Philippe les aide

(8) Je croyais qu'elle était intelligente

(9) Elle lui a demandé s'il venait

(10) La plupart des élèves voulaient partir

(11) Le prof voulait ses élèves réussir

(12) Anne a promis Jean d'acheter une maison

(C'est Jean qui va acheter la maison)

(13) Jacques a promis Guy de prendre le train

(C'est Guy qui va prendre le train)

(14) Il croyait les étudiants être fous

(15) Ils voulaient que leurs hôtes restent plus longtemps

(16) Il a promis de payer la voiture

(17) J'espérais avoir de très bonnes nouvelles

(18) Blanche et Georges croyaient aimer la musique

(19) Il croyait qu'ils étaient trop occupes

(20) L'officier voulait cet homme partir

(21) Luc a promis qu'elle partirait à Nice

(22) Elle a promis de garder les enfants

(23) Ils ont persuadé leur fils de partir

(24) Marguerite croyait aimer l'opera

(25) Elle croyait que Pierre était trop gros

(26) Il a promis Claire d'avoir un bébé

(C'est Claire qui va avoir un bébé)

(27) Je croyais Jean être intelligent

(28) Marguerite et sa mère voulaient partir

(29) Les filles ont dit qu'elles détestaient le film

(30) Il m'a demandé d'aller au marché

(31) J'ai promis que Claire viendrait à l'heure

(32) Les frères croyaient aimer le basket-ball

(33) Son ami voulait qu'il rende la voiture
(34) Les filles ont promis de faire leurs devoirs
(35) Ils espéraient partir pour Angleterre
(36) Leurs chers voisins voulaient déménager
(37) Ils croyaient les acteurs être doués
(38) Il a promis que Jean couperait l'herbe
(39) Madame voulait que Jean trouve un emploi
(40) Ils croyaient que leur voisin était fou
(41) Marie a dit qu'elle voulait une voiture
(42) Elle l'a persuadé qu'elle viendrait
(43) Elle croyait Pierre être très heureux
(44) Gil a promis Guy de conduire la voiture
(C'est Guy qui va conduire la voiture)
(45) Ils voulaient leurs amis faire une visite
(46) Bertrand espérait qu'elle irait mieux
(47) Le monsieur voulait prendre des vacances
(48) Guy croyait aimer la litterature
(49) Il a demandé aux femmes d'attendre
(50) Leurs cousins ont dit qu'ils aimaient la ville

(1) She believed Pete to be very happy
(2) He believed that they were much too busy
(3) He persuaded her to do her homework
(4) His friend wanted that he returns the car
(5) Alice promised Freddy to take the train
(It is Freddy who will take the train)
(6) She promised to babysit with the kids
(7) He asked me to go to the marketplace
(8) The girls said that they hated the movie
(9) The boss wanted to take a vacation
(10) Arnold promised that Jack would cut the grass
(11) Peter promised Harry to drive the bus
(It is Harry who will drive the bus)
(12) They believed that their neighbor was crazy
(13) Henry believed to like literature
(14) The officer wanted that man to leave
(15) I was hoping to have very good news
(16) Their cousins said that they liked the neighborhood
(17) They persuaded their son to go away
(18) I promised that Clara would come on time
(19) Most of the students wanted to go home
(20) I believed John to be intelligent
(21) The teacher wanted his students to pass
(22) Her uncle promised to pay for the car
(23) Margaret believed to like opera
(24) Mrs. Brown wanted that Jim gets a job
(25) My boss said he would attend the meeting
(26) Mary asked Joseph if he was coming
(27) Cathy promised Louis to buy the house
(It is Louis who will buy the house)
(28) The boss wanted his assistant to come
(29) The two girls promised to do their homework
(30) He believed the students to be crazy
(31) The Trumps wanted that their guests stay longer
(32) She persuaded him that she would return

(33) They were hoping to leave for England

(34) She promised that her brother would help him

(35) Their neighbors down the hall wanted to move

(36) I believed that she was intelligent

(37) Martha and George believed to like music

(38) Joe promised that he would drive to Boston

(39) Christopher said that he would work harder

(40) The Smiths wanted their friends to visit them

(41) I promised to take a long vacation

(42) They believed actors to be talented

(43) Ed promised Helen to have a baby

(It is Helen who will have a baby)

(44) Maggie and her mother wanted to leave

(45) She believed that Peter was much too fat

(46) Marianne said that she wanted a car

(47) The students wanted that Philip helps them

(48) The brothers believed to like basketball

(49) Fred was hoping Louise would feel better

(50) He asked the women to wait for an hour

(1) Il a promis qu'elle trouverait sa bague
(2) Christophe a dit qu'il travaillerait plus
(3) J'ai promis de voir le médecin demain
(4) Le capitaine voulait ses hommes aller
(5) Il a dit qu'il irait à l'assemblée
(6) Il l'a persuadée de faire ses devoirs
(7) Son frère voulait que Georges travaille plus dur
(8) Je croyais que le village était sale
(9) Elle lui a demandé s'il venait
(10) Beaucoup de garcons voulaient échapper
(11) Leur mère voulait ses trois enfants venir
(12) Paul a promis Jeanne de laver l'auto
(C'est Jeanne qui va laver l'auto)
(13) Guy a promis Gil d'arroser les plantes
(C'est Gil qui va arroser les plantes)
(14) Il croyait les femmes être agressives
(15) Leur grandpère voulait qu'ils aillent a l'ecole
(16) Sa tante a promis d'assister au bal
(17) J'espérais avoir de très bonnes nouvelles
(18) Les femmes croyaient aimer la planche à voile
(19) Il croyait qu'ils étaient souvent chez eux
(20) Le flic voulait le chauffeur aller vite
(21) Jeanne a promis qu'il payerait la taxe
(22) Elle a promis de nettoyer sa chambre
(23) Ils ont persuadé leur fils de partir
(24) Maude croyait aimer la cuisine française
(25) Elle croyait qu'il était un bon voisin
(26) Georges a promis Marie d'être enceinte
(C'est Marie qui va être enceinte)
(27) Je croyais Marie être très malade
(28) Marie et Clothilde voulaient réussir
(29) Les filles ont dit qu'elles détestaient le film
(30) Il m'a demandé d'aller au marché
(31) J'ai promis que Blaise irait à l'école
(32) Les filles croyaient aimer la mode française

(33) Son chef voulait qu'elle travaille le weekend
(34) Elles ont promis de rentrer à minuit
(35) Ils espéraient partir pour Angleterre
(36) Leurs amis voulaient acheter une maison
(37) Ils croyaient le train être plus rapide
(38) Elle a promis qu'il ferait la vaiselle
(39) Son père voulait que Gil passe l'examen
(40) Ils croyaient que Guy était dangereux
(41) Marie a dit qu'elle voulait une voiture
(42) Elle l'a persuadé qu'elle viendrait
(43) Elle croyait Georges etre plutot nerveux
(44) Maude a promis Guy de faire la cuisine
(C'est Guy qui va faire la cuisine)
(45) Ils voulaient leurs enfants passer le bac'
(46) Bertrand espérait qu'elle irait mieux
(47) La cantatrice voulait gagner le prix
(48) Jeanne croyait aimer la télévision
(49) Il a demandé aux femmes d'attendre
(50) Leurs cousins ont dit qu'ils aimaient la ville

(1) She believed George to be rather nervous
(2) He believed that they were often at home
(3) He persuaded her to do her homework
(4) Her boss wanted that Anne works on weekends
(5) Nancy promised Mike to water the plants
(It is Mike who will water the plants)
(6) She promised to clean her room tomorrow
(7) He asked me to go to the marketplace
(8) The girls said that they hated the movie
(9) The soprano wanted to win the prize
(10) She promised that he would do the dishes
(11) Mary promised Peter to prepare dinner
(It is Peter who will prepare dinner)
(12) They believed that their friend was dangerous
(13) Marie believed to like television
(14) The cop wanted the driver to go fast
(15) I was hoping to have very good news
(16) Their cousins said that they liked the neighborhood
(17) They persuaded their son to go away
(18) I promised that Stephen would go to school
(19) Many of the boys wanted to escape
(20) I believed Milly to be very sick
(21) Their mother wanted the children to come
(22) Her aunt promised to go to the party
(23) Madeleine believed to like French cooking
(24) Mr. Jones wanted that Joe takes the test
(25) My boss said he would attend the meeting
(26) Mary asked Joseph if he was coming
(27) Bill promised Pamela to wash the car
(It is Pamela who will wash the car)
(28) The captain wanted his men to advance
(29) They promised to come home before midnight
(30) He believed women to be aggressive
(31) Their father wanted that they go to school
(32) She persuaded him that she would return

(33) They were hoping to depart for England

(34) He promised that she would find her earring

(35) Their friends wanted to buy an apartment

(36) I believed that the village was dirty

(37) The women believed to like wind surfing

(38) Joanne promised that she would pay the tax

(39) Christopher said that he would work harder

(40) They wanted their children to graduate

(41) I promised to see the doctor next week

(42) They believed the subway to be faster

(43) George promised Mary to become pregnant

(It is Mary who will become pregnant)

(44) Mary and Cathy wanted to succeed

(45) She believed that he was a good neighbor

(46) Marianne said that she wanted a car

(47) His brother wanted that George works harder

(48) The girls believed to like Parisian clothes

(49) John was hoping Mary would feel better

(50) He asked the women to wait for an hour

APPENDIX C

The following five matrices were used for the analysis involving three response categories.

TABLE C.1. Differences of Means: L1 English - L2 English

	TENSED CLAUSE	ECM	PRO
believe	.54*	.48*	-.67*
promise	.34*	-.74*	.07*
want	-.91*	.52*	.31

* p <.05

TABLE C.2. Differences of Means: L2 French - L1 French

	TENSED CLAUSE	ECM	PRO
croire	-.27	.85*	-.95*
promettre	-.03	.57*	-.58*
vouloir	-.48*	.65*	-.20

* p <.05

TABLE C.3. Differences of Means: L2 French - L2 English

	TENSED CLAUSE	ECM	PRO
believe/croire	.48*	.27	.04
promise/promettre	.12	-.35*	-.24*
want/vouloir	.29	-.72*	.12

* p <.05

TABLE C.4. Differences of Means: L2 English - L1 French

	TENSED CLAUSE	ECM	PRO
believe/croire	-.76*	.58*	-.99*
promise/promettre	-.15	.92*	-.34*
want/vouloir	-.77*	1.38*	-.31

* p <.05

TABLE C.5. Differences of Means: L1 English - L2 French			
	TENSED CLAUSE	ECM	PRO
believe/croire	.05	.21	-.71*
promise/promettre	.22	-.39*	.17
want/vouloir	-1.20*	1.24*	.19
* p <.05			

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