DOCUMENT RESUME

ED 327 059 FL 019 011

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TITLE Universal Grammar and Age Effects in Second Language Acquisition: A Response to Flynn and Manuel.
PUB DATE Feb 89
NOTE 22p.; Paper presented at Annual Meeting of the Second Language Research Forum (Los Angeles, CA, February 23-26, 1989).
PUB TYPE Information Analyses (070) -- Reports - Evaluative/Feasibility (142) -- Speeches/Conference Papers (150)

EDRS PRICE MF01/PC01 Plus Postage.
DESCRIPTORS *Age Differences; *Grammar; *Language Research; *Language Universals; Linguistic Theory; Second Language Learning

ABSTRACT

This response to a paper concerning age-related effects and their relationship to universal grammar (UG) in second language (L2) acquisition looks first at both the paper in question and research on UG and L2 acquisition in general. After these observations, discussion focuses on the following four topics in relation to the paper: (1) the notions of a "critical period" and "age-dependents effects" in language acquisitions; (2) the notion of success in UG/L2 theory; (3) evidence from speech and speech perception; and (4) evidence from judgments of grammaticality. It is concluded that while the study suggests that linguistic theory offers promising avenues of inquiry into the course of age-related differences, it also lacks clarity, rigor, and finesse. (MSE)

Universal Grammar and age effects in second language acquisition: A response to Flynn and Manuel

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Flynn and Manuel are to be commended for drawing attention to a central question in second language (L2) acquisition, namely age-dependent effects. They accurately point out that the field has only fragmentary knowledge of issues relating to this issue. They are also justified in framing the question within the theory of Universal Grammar (UG), inasmuch as (controversial) demonstrations of access to or lack of access to UG by L2 learners are often construed in terms of its maintenance or decay over time.

The authors adopt a defensive posture toward the issue, marshalling a number of arguments to support their position that UG is alive and well and mediating adult L2 acquisition. In so doing, the Flynn and Manuel paper often departs from the general notion of a critical period for language learning to focus more specifically on the idea of a critical period for access to UG. They are again correct in observing that this is an issue in need of clarity and rigor in its theoretical formulations, and of finesse in interpretation of pertinent empirical data.

In this response I too would like to speak to questions of clarity, rigor, and finesse, with respect to both the Flynn and Manuel paper and UG/L2 research generally. After making some general observations, I will develop my response around four topics: the notions of "critical period" and "age-dependent effects", the notion of success, evidence from speech and speech perception, and evidence from judgments of grammaticality.

General remarks

Let us assume the Chomskyan version of first language (L1) acquisition whereby acquisition proceeds by an interaction of UG and appropriate

triggering experiences from the environment (see Chomsky, 1981; Cook, 1988, Chapter 3; Lightfoot, 1989, *inter alia*). Research by Susan Curtiss (1982, 1989) has revealed that under conditions of deprivation of triggering input, access to UG appears to decline, with resultant failure to acquire language. For example, the linguistic output of Chelsea,[1] who was thus deprived for the first thirty years of her life, is characterized by violations of Structure Dependency, e.g., finite verbs preceded by determiners.

Because adults learning a second language have not been deprived of linguistic input, their task is vastly different from that of a Chelsea or a Genie (Curtiss, 1982). Accordingly, the notion of access to UG must be framed differently for L1 and L2 acquisition. Several theoretical possibilities have been raised in the literature. Sharwood Smith (1988, p. 25; see also Schachter, 1989) names and describes them as follows:

> The Parasitic Hypothesis holds that UG is no longer active in second language acquisition and that traces of conformity to UG in IL [interlanguage] may be traced back to features of L1 carried over into the developing grammar. The Recreative Hypothesis holds that UG is active in second language acquisition and that grammatical development unfolds very much along the same lines as it does for first language acquirers. The Reconstructive Hypothesis holds that UG is still active but in a different way in that the learner sets parameters shared by L1 and L2 in the way [sic] that have been set for L1: this entails complications where there is no evidence in the input for resetting the IL parameters so that IL is aligned with native-speaker L2.

Let us assume that interlinguistic output and elicited L2 data do not violate constraints on natural language, and grant that interlanguage grammars are consistent with a principles and parameters of UG in L2 acquisition. However, evidence favoring these assumptions cannot be taken to mean that UG has *necessarily* been accessed, à la the Reconstructive or Recreative Hypotheses; after all, it is conceivable that UG-like knowledge is derivative from L1 knowledge, along the lines of the Parasitic Hypothesis (see Bley-Vroman, 1989, and this volume). Can proponents of the UG/L2 position live up to the standard applied by Flynn and Manuel themselves in their discussion of accented speech and loss of cortical plasticity (pp. 4-5); that is,

can they demonstrate that UG-like effects are "*causally* linked" to UG? As Flynn and Manuel suggest, this question can also be asked, *mutatis mutandi*, of advocates of some version of the Parasitic Hypothesis.

However, it may be unreasonable to invoke such a criterion of causal linkage. In L2 research, demonstrations of causality are just as problematic as in other epistemological domains. With respect to the role of UG in L2, the available evidence is ambiguous and open to question on theoretical and methodological grounds.[2] The major obstacle resides in the fact that the data that can be brought to bear on theory are data from performance (or E-Language). The relationship of performance to the designated object of theoretical inquiry, competence (or I-Language), is indirect at best. This is the case with naturalistic and elicited data of all kinds--free speech, compositions, elicited imitation, etc. It is certainly the case with grammaticality judgments, which are thought by many to be the performance most directly linked to competence (see, e.g., Cook, 1988, pp. 80-81). As Lasnik (1981, p. 20) observes: "Grammaticality judgments are often incorrectly considered as direct reflections of competence . . . responding to a grammaticality query is an instance of linguistic *performance*." If grammaticality judgments are just performance in the form of a kind of meta-E-language, then by theory-internal standards they are not relevant. Thus it is not clear that theory can be empirically tested in any strict deterministic sense.

As is so often the case when evidence is merely suggestive, the principals in scientific debate retreat to pronouncements about who should shoulder the burden of proof. Tradition seems to place the burden of proof on proponents of the least parsimonious position. For second language acquisition, is it more parsimonious to assume that adults learn L2 as they did their L1, *viz.*, mediated by UG (e.g., Schwartz, 1989), or to assume that adults learn L2 as they learn everything else, *viz.*, by reliance on general problem solving (e.g., Bley-Vroman, 1989)? Flynn and Manuel, citing Lust (1988), argue for the parsimony of the former (p. 21). However, neither position is particularly parsimonious. The UG/L2 position presupposes the modularity of language and the existence of UG, and hinges on complex procedural questions of access to and application of UG in L2 acquisition (see concluding remarks, this paper). The alternative position appeals to a domain-general problem solving capacity (cf. Flynn and Manuel's reference to "the *supposed* advanced

cognitive development of adults [p. 13; italics mine]). On this view, general problem solving systems are hypothesized to have powers of induction capable of yielding linguistic knowledge surpassing that available in input and consistent with abstract properties of natural language (see Bley-Vroman, 1989, and this volume).

Ultimately, the debate moves from posturing about burden-of-proof bearing to a general defense of one's position. The Flynn and Manuel paper is a such a defense, with special attention to what is ostensibly one of the most vulnerable points of UG/L2 theory, age-dependent effects. The qualifier "ostensibly" is warranted because, in a certain sense, the theory is not vulnerable in this respect. As long as it can be shown that L2 learners of all ages possess knowledge exceeding that available in input--and, specifically, that their interlanguage grammars do not violate what is known about natural languages--then the "anti-critical period" position is secure.[3] Since White's seminal paper on the "logical problem" of L2 acquisition (1985), this epistemological mismatch between input and knowledge has been the foundation of the UG/L2 theory. (Indeed, some would argue that it is the entire edifice.) Flynn and Manuel restate the logical problem for L2 acquisition in their "Summary of Arguments" (pp. 21-22), and this is the core of their defense.

There are several other plates in the Flynn and Manuel armor. The major ones will be reviewed in the remainder of this response.[4]

The notions of "critical period" and "age-dependent effects"

One defensive tack by Flynn and Manuel takes the form of arguments against a "hard and fast" conception (p. 4, pp. 16-17) of critical periods. While the term "critical period" may suggest decisive temporal borders, it is not used in the current literature as a developmental analogue of the International Date Line.[5] Thus to inveigh against the notion of a "hard and fast critical period" is not to deny the existence of age-dependent effects.

Though perhaps preferable to "critical period", the terms "age-related" or "age-dependent" effects should not be misunderstood either. Developmental changes are not linked strictly to the number of months or years an individual has been alive. All these terms (as well as the popular term "sensitive period")

should be understood as refering to clines, with more pronounced rises or descents roughly coinciding with rather ill-defined periods such as the onset of literacy, puberty, and Piagetian stages. (Note that even diehard Piagetians recognize that Piagetian stages are characterized by *décalages*, which militate against strict associations of age and development.)[6] Long (1988, p. 3), in a comprehensive review article, notes that even such putatively "hard and fast" animal behaviors as imprinting are often gradual and are sometimes reversible. Following Oyama (1979), Long observes that "age-related declines in language learning ability" are likely to be "variable in onset and effect"; moreover, "one would no longer predict, for example, that all areas of language will be affected during the same period (say, puberty) in all individuals. Nor would one need to show such a catastrophic one-time loss in ability in order to claim that sensitive periods exist for first or second language learning" (p. 4).

Extensive research on age-related effects in second language learning has been undertaken from linguistic, motivational, sociological, cognitive, and neurolinguistic perspectives (e.g., Bever, 1981; Harley, 1986; Jacobs, 1988). The relevant research presents a rather confusing picture, as adamant assertions have been made both pro and contra the existence of maturational effects. The confusion is largely the result of differing research foci: some studies address questions of rate of acquisition, while others look at ultimate attainment; some involve phonology, while others involve syntax, lexis, pragmatics, etc.; some deal with production data of various sorts, while others analyze elicited data such as imitation and grammaticality judgments. Long (1988) sifts through the various results and attempts to clarify the picture. He concludes that both L1 and L2 acquisition "are subject to maturational constraints, specifically sensitive periods during which learning is successful, and after which it is irregular and incomplete" (p. 1).

The Flynn and Manuel paper acknowledges that age-related effects in L2 acquisition occur (p. 1). It goes on to speak to the linguistic domains in which these effects are observed, and to offer theory-based explanations of why such effects should or should not occur. Thus the question of where these effects are seen, and the question of why such effects occur in the first place, are not independent. Flynn and Manuel note that age-related difficulties may be found in "peripheral" areas such as the lexicon (presumably considered independently of its relationship to syntax via the Projection Principle). This

finding does not necessarily constitute a threat to theory, since problems of UG/L2 theory arise only if the ability to acquire core grammar is compromised. The where and the why of age-related effects will be discussed further in the concluding remarks of this paper.

Success in UG/L2 theory

So uniformly successful is L1 acquisition that it is often compared to learning to walk, and nativist arguments have been developed to account for it. L1 acquisition is also robust; in fact, "there is virtually no way to prevent it from happening short of raising a child in a barrel" (Pinker, 1984, p. 29).

Although derivative nativist arguments have been invoked to account for L2 acquisition, it cannot be characterized as uniformly successful or robust. It is true that multilingualism is a prevalent state of affairs in many areas of the world; yet it is usually the case that these speakers began to learn their second languages as children. Those adults who do succeed--by standards of success besides possession of more knowledge than they have been exposed to--are rare (see, e.g., Coppieters, 1987). Barrels are not required to prevent L2 acquisition from happening.

In their paper Flynn and Manuel dismiss the general failure of adult L2 acquisition to achieve native competence. They maintain that such a failure is irrelevant to UG/L2 theory: "UG is not a theory of end-state competence" (p. 20). What is important is constrained hypothesis testing, linguistic knowledge that surpasses input, and IL grammars that do not violate UG (pp. 20-22).

Curiously, Flynn and Manuel appear to contradict themselves by insisting that "adults, like children, are capable of learning new languages under a wide range of learning conditions" (p. 15). This notion is a key element in the Flynn and Manuel defense, as they formulate "general arguments against the 'anti-UG' position" on the basis of "the common observation that adults can and do learn new languages (taking into account, of course, individual differences)". For them this constitutes "sufficient reason to believe that there might be an underlying deep commonality to both child L1 and adult L2 syntactic acquisition" (p. 14).

Apparently, Flynn and Manuel want to have it both ways. As a foil to arguments based on age-dependent effects, they conveniently invoke

evidence that some adults do end up learning a second language. At the same time, they shoo away the pesky gnat of near-universal failure by saying that UG is not concerned with end-state competence.

Quite apart from seeming to engage in disingenuous argumentation, Flynn and Manuel have inadvertently underscored a certain awkwardness in their formalization of UG/L2 theory. While arguing that "UG underlies the L1 in the adult end-state" (p. 21), Flynn and Manuel assert that "UG is not a theory of end-state competence" (p. 20). (I am assuming that the latter statement is not merely the standard dismissal of idiolectal or "proficiency" differences among native speakers, but rather that it refers to underlying grammatical considerations such as binding, the theta criterion, structure dependency, etc.) In its application to L2 acquisition, what exactly is the domain of UG theory? Is it merely a theory of initial-state competence? Certainly it is more than that, since UG/L2 researchers are concerned with learners' resetting of parameters, thus going beyond the initial state, and since UG/L2 researchers (e.g., Felix, 1988) have suggested that UG-type knowledge is not operative *in toto* at beginning stages of L2 acquisition, but (like L1 acquisition) appears to manifest itself piecemeal over time. Thus it is apparently a theory about the initial state of the language learner, and a theory about the way interlinguistic knowledge accrues, but not a theory of end-state competence. So stated, the domain of the theory is rather unorthodox: it is a theory of everything-but-end-state competence.

Yet surely UG/L2 theory addresses itself to end states as well. Indeed, the original formulation of the logical problem of L2 acquisition by White (1985) was based on a comparison of the end product of acquisition with available linguistic input. UG/L2 theorists are hardly oblivious to observations that interlanguage grammars, up to and including end points of fossilization or native speaker competence, do not violate principles of UG. If a fossilized interlanguage grammar were to develop that violated structure dependency, it is a safe bet that UG/L2 theorists would be concerned.

It is fair to say that UG/L2 theory is *selectively* concerned with end states. UG/L2 theorists would no doubt all agree that what "counts" in the end state are features of core grammar; peripheral features are disregarded (p. 18). Any further restriction of the theory's purview would seriously weaken it. Thus it would be self-defeating to declare end-state competence off limits

for theoretical inquiry, even as a gambit against criticism based on maturational data.

It should also be noted that theory-internal shifts in the determination of core vs. peripheral elements may pose problems of "imprecision", which Flynn and Manuel underscore in their second counterargument (pp. 18-19). Principles are "primitives" and are part of the core grammar. Subjacency used to be considered a primitive but since Chomsky (1986) it is not. The fluidity of the theory makes life difficult for UG/L2 theorists, who must vigilantly align current and projected research with the latest pronouncements, while reinterpreting (or even disavowing) previous research. But for Flynn and Manuel, this very fluidity is a blessing, as they appeal to the mutating theoretical status of subjacency when grappling with research that has yielded little evidence of subjacency effects predicted by theory (see, e.g., Johnson, 1988; Schachter, 1989). UG/L2 theory is immune from such criticism, since "the exact nature and status of subjacency is [sic] still widely debated in the literature" (p. 18). By appealing to such arguments, UG/L2 theorists frustrate attempts to pin them down. Critics of UG/L2 research can only try in vain to pour salt on the tail of a very elusive bird.

The relevance of evidence from speech and speech perception

In the first part of the Flynn and Manuel paper, Arguments 1 and 2 (pp. 3-5) address the possibility that native-like speech can be produced by adult L2 learners. Flynn and Manuel observe that available data are "more compatible with a gradual decline in propensity to acquire accent-free speech than with a hard and fast critical period" (p. 4). Flynn and Manuel use the same argument to discount neurobiological factors (hemispherical lateralization or loss of plasticity) in the loss of ability to produce unaccented speech (p. 5). However, as discussed above, to reject the idea of "hard and fast" critical period is not to discount age-related effects.

Recalling another line of reasoning mentioned above, the authors wish to impose again a criterion of causality with respect to the relationship between hemispherical specialization and a decrease in ability to produce

unaccented speech. Yet it is not clear how or whether such a causal link could be emprically established.

Argument 3 is an excursus on categorical perception. Flynn and Manuel review ambiguous evidence concerning the ability to discriminate within categories: in some studies it appears to deteriorate with age, in others it does not. Flynn and Manuel argue that in any case, perceptual abilities, intact or impaired, have little to do with L2 accent: "the way in which this function [categorical perception] changes over maturation does not alone explain the differences between child and adult L2 learners' accents" (p. 9).

The length of Argument 3 may obscure its premises, if not its conclusions. Much of the discussion involves asymmetries in perceptual and productive abilities generally, and particularly asymmetries specific to crosslinguistic contexts (in addition to references given by Flynn and Manuel, see, e.g., Caramazza et al., 1973; Caramazza & Yeni-Komshian, 1974; Flege, 1984; Flege & Hillebrand, 1984: Jamieson & Morosan, 1986; Miller et al., 1986; Serniclaes & Bejster, 1979). Given the introductory paragraphs and subtitle of Argument 3, however, and in keeping with the modular linguistic theory espoused throughout the paper, it would appear that Flynn and Manuel are just as concerned with demonstrating that categorical perception--a putatively modular and species-specific ability--is immune to critical period effects. It is of importance to Flynn and Manuel's defense that adult native speakers of Japanese can discriminate contrasts that do not exist in Japanese, since this would suggest no critical period effects in this linguistic domain.

However, such an argument should be developed cautiously, and should respond to evidence that the ability to discriminate within categories is neither modular nor species-specific. Recently, researchers have found that a number of human phonetic contrasts can be discriminated by non-humans, e.g., chinchillas, Japanese macaques, and Japanese quail (see Diehl & Kluender, 1989; Kluender, Diehl & Killeen, 1987; May, Moody & Stebbins, 1989). For example, Kluender, Diehl, and Killeen found that Japanese quail, after having learned to categorize syllables consisting of [b], [d], or [g] followed by one of four English vowels, were able to correctly categorize novel syllables consisting of the same consonants and different English vowels. These findings represent a challenge to speech mode or motor-theoretical hypotheses which attribute speech perception to a modular faculty based in speech production (e.g., Liberman & Mattingly, 1985; Mattingly, in press).

Since non-humans lack evolutionary adaptations specific to the perception of speech, it can be argued that lower-level auditory mechanisms should be invoked to explain the similar performance of humans and non-humans. Thus Diehl and Kluender (pp. 1196-1197) believe that it is inappropriate to "opt for higher level psychological [phonetic or articulatory] explanations when lower level ones will do." (For counterarguments to this position, see Fowler, 1989; Remez, 1989; Studdert-Kennedy, 1989.) The significance of such findings in the present context is clear: if Flynn and Manuel wish to discount convincingly the notion of critical period in human categorical perception and defend the modularity of this ability, they must at least come to grips with the evidence that Japanese quail can demonstrate perceptual discrimination abilities similar to those of Japanese native speakers.

The evidence from grammaticality judgments

Flynn and Manuel correctly observe that the data from grammaticality judgments are often unreliable and difficult to interpret. The vagaries of this popular methodology are well-established, and are reviewed in Birdsong (1989a). Flynn and Manuel note that convergent evidence from a variety of experimental methodologies is desirable, as is a "more precise elucidation and understanding of what aspects of a learner's competence each [experimental task] principally investigates" (p. 20). With respect to grammaticality judgments, a meaningful interpretation of judgment data will require advances in our understanding of the categorization and decision-making processes that underlie this metalinguistic performance. Some preliminary steps have been taken in this direction (Birdsong, 1989b).

Other measures of precision may also profitably be taken. In L2 acquisition research, the presumed relationships between judgment data and theory should be explicit, especially as concerns comparisons of learners' judgments with those of natives. For UG/L2 research, at least four hypotheses might be entertained (see Birdsong, 1989a, p. 121): (1) If UG is available in adult L2 acquisition, its effects should resemble those attested in natives' grammaticality judgments (this assumes that natives have access to UG via their L1); (2) If UG is available in adult L2 acquisition, then theoretical contrasts, such as the grammaticality of *What did you think would happen?*

versus the ungrammaticality of **What did you think that would happen?* should be confirmed in judgments by natives and learners alike; (3) If UG is available in adult L2 acquisition, learners should be able to demonstrate by their judgments linguistic knowledge that surpasses what is available in input; (4) If UG is available in adult L2 acquisition, learners' judgment data should suggest that their interlanguage grammar conforms to constraints on natural languages generally. Since each hypothesis is based on a different theorized relationship of data to theory (and since some are more plausible than others), it is essential that the relationships be spelled out pre-experimentally.

Flynn and Manuel are justifiably skeptical of the use of judgment data as indices of linguistic competence (**pp. 19-20**). However, their skepticism does not extend to the Johnson and Newport (1989) study, from which the authors cite evidence that learning of canonical word order is not affected by maturational variables (**p. 18**).[7]

Flynn and Manuel go on to speculate that maturational effects in grammaticality judgments may be an artifact of "a type of critical period on the experimental tasks themselves" (**p. 20**). This claim is in need of elucidation. As detailed in Birdsong (1989a, Chapters 1 & 2), there are indeed age-related effects aplenty in grammaticality judgments, the most notable being a shift of focus from semantic to formal aspects of linguistic strings. Many effects are a function of training and cannot be said to reflect straightforwardly differences in linguistic knowledge; they may therefore be considered "independent of the role of UG" (Flynn & Manuel, **p. 20**). Given this, Flynn and Manuel are mistrustful of unidentified "studies that purport to isolate a critical period or indicate differences between adults and children" (**p. 20**). This is a rather curious warning, since comparisons of children's and adults' judgments are not the concern of influential critical period studies such as Johnson and Newport (1989); rather, such studies involve comparisons of judgments among adults who began their L2 study at varying ages. The crucial maturational variable is the age of onset of L2 learning, not the age at which the subjects are tested.

Conclusions

Age-related differences among L2 learners are routinely attested. Why these differences exist, and in what linguistic domains, are questions that await definitive answers. As Flynn and Manuel's paper suggests, linguistic theory offers promising avenues of inquiry.

It should be noted that at least part of the answer to the "Why?" question might be theory-independent. We cannot cease to explore such variables such as length of exposure,[8] motivation, and neurological structure just because they have no place in a linguistic theory of language acquisition.

This is not to say that the question of "Why?" cannot be addressed theoretically. One little-explored possibility involves the interaction of linguistic epistemology (e.g., knowledge of UG or L1) and general problem solving systems. As is well known, problem solving systems are fallible. It has been suggested (Birdsong, 1989c) that adults' access to linguistic knowledge can be compromised procedurally by imperfect search sets (see Tversky & Kahneman, 1974), with the effect that presumed linguistic knowledge cannot be demonstrated empirically.[9] At issue here is a procedural question of *access* that is crucial to the theoretical question of *availability*. Let us assume that adults' knowledge of UG has not deteriorated as a function of age (i.e., that it is "available"); even so, it still remains to be shown that these adults can make use of the knowledge (i.e., that it is "accessible"). McLaughlin (1988, p. 3) has raised such questions from the perspective of information processing: "Are some people [adult L2 learners] able to access the Universal Grammar more efficiently? Is the UG more efficiently organized in some people, more automated? These are questions that the theory has yet to address."

Undeniably, the question of what linguistic domains are affected by maturational variables lends itself to productive discussion within linguistic theory proper. Our understanding of L2 acquisition is enhanced by demonstrations that interlanguage grammars and hypothesis testing are constrained in ways predicted by linguistic theory. Such effects are all the more significant if they are resistant to maturational variables. Of course, it is regrettable that the fluidity of the theory can be used in self-defense; after all, one of the greatest virtues of Chomskyan theory is its presumed testability. Moreover, any theoretical approach is on shaky ground when putative empirical validations of theoretical predictions are undermined by methodological problems or by self-serving interpretations.

For both the "Why?" and the "What?" questions in L2 critical period research, Flynn and Manuel's paper has shown that principled theoretical approaches can contribute clarity, rigor, and finesse. It has also shown that, as with any scientific endeavor, there is room for improvement.

NOTES

1 Chelsea was born deaf to hearing parents. She was deprived of hearing assistance devices, as well as sign language input and training, for some thirty years. Her only linguistic output has been in the form of American Sign Language.

2 See, e.g., Birdsong (1989a, Chapter 4), Bley-Vroman (1989, pp. 62-65), Bley-Vroman and Chaudron (1989), Eubank (1989). The types of evidence brought to bear on questions of UG's role in L2 acquisition will be discussed further below.

3 Proponents of UG/L2 theory can retreat to this position in the face of any number of criticisms. Note, however, as mentioned above, that the UG/L2 position is less secure in attempts to establish empirically a causal link between such UG-type effects and actual access to UG.

4 One feature not dealt with in this response is the example of UG effects in L2 acquisition given at the end of the Flynn and Manuel paper. Data and argumentation relevant to this example are reviewed at length in Bley-Vroman and Chaudron (1989), Eubank (1989), and White (1989, Chapter 4).

5. It is true that the discussion in Lenneberg (1967) does suggest strict temporal borders, as he refers repeatedly to puberty and to the ages of 12 to 13 years. However, since the time of Lenneberg's seminal study most researchers have been careful to emphasize *approximate* ages and stages such as pre- and post-literacy. More recent efforts such as Johnson and Newport (1989) demonstrate age-related effects in a rough correlational fashion, thereby avoiding strict associations with "critical" periods.

Some researchers (e.g. Schachter, 1988; cf. Bley-Vroman, 1989) refer to a "window of opportunity" for access to grammatical principles. Although this may suggest a strict temporal cut-off, no ages or stages are stipulated or investigated empirically.

It is worthwhile to comment parenthetically on the frequent association of the idea of a "hard-and-fast" critical period with the notion of "all-or-nothing" effects. The "all-or-nothing" view is poorly supported in UG/L2 research. Both group and individual data suggest that access to UG is not absolute; that is, UG seems neither totally accessible nor totally inaccessible. At least some of this ambivalence may be attributed to guesswork, experimental set effects, or response biases (see Birdsong, 1989, Ch. 4 for a review). Such procedural artifacts aside, however, the data lend themselves to reasoned speculation about age-dependent effects in terms of an asymptotic function. Under such a view, access to a given grammatical feature may not dissappear in an all-at-once fashion, but rather may decline asymptotically toward an undefined (but presumably zero) floor. This notion of declining access is consistent with the correlational results of Johnson and Newport (1989). It would be interesting to investigate a related possibility, namely that hypothesized asymptotic functions vary for different grammatical principles. Such a possibility is consistent with the spirit of Seliger's (1978) notion of multiple critical periods and with McLaughlin's (1988) suggestion of differential access to various UG principles (see concluding remarks of this paper; also discussion below of Long, 1988 and Oyama, 1979).

6 On the notion of developmental stages, see Gelman and Baillargeon (1983).

7 There is no consensus on the ability of adults to acquire L2 canonical word order. See Clahsen and Muysken (1986), du Plessis et al (1987), Jordens (1988), as well as the papers by Bley-Vroman and White in this volume.

8 As Klein (1986, p. 9) points out, "Assuming a child is exposed to language for something like five hours a day, also practising speech in the process (those who have children will agree that this is probably an underestimate), we arrive at a total of about 9,100 hours of active language learning in a child's first five years." Adults learning a second language, especially in formal contexts, rarely have opportunities for such input and practice. Note however that the Johnson and Newport (1989) study found more robust effects for the variable of age of beginning language study than for years of exposure.

9 Tversky and Kahneman cite the example of subjects' failure to respond correctly to a query such as "Given English words of more than three letters, is it more likely that a word randomly sampled from text starts with *r* or that *r* is the third letter?". Most people search for words by their first letter, not by their third letter, with the result that words beginning with *r* are thought to be more frequent than words with *r* in third position. In fact, *r* occurs more frequently as a third letter than as a first letter. Imperfect search sets such as this could lead to anomalous results in production or judgment under uncertainty by L2 learners. For example, adults beginning their study of English might incorrectly deem acceptable a sentence like **I don't know where he's* on the basis of the licit occurrence of the contraction at the beginning of utterances and in the majority of structural contexts.

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