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Colloquium 3

### **UG access in L2 acquisition: reassessing the question**

Questions (L. Eubank, Oct. 1997)

1. Is the original access question a reasonable one to ask at the present time?
  2. Does the current state of linguistic theory warrant the original question? If not, should the question be reformulated?
  3. How would such a reformulation affect our understanding of previous research, as well as any future attempts at falsification of a reformulated question?
- Eubank: "The UG-access question, as understood and examined in earlier UG/L2 research, cannot be maintained today and should be reformulated."

**Whose question was it anyway?**

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## 1. Introduction

I had originally planned to make a statement to the effect that I have always thought the "original access question", as Lynn Eubank calls it, was misframed in much first and second language acquisition research, and a significant distortion of Chomsky's notion of Universal Grammar. I was then going to show how easy it is to arrive at the conceptualization which now dominates generative SLA research, given certain unstated assumptions about what our data licences as inferences to UG, as well as some unstated assumptions about the relationship of UG to psycholinguistic mechanisms - assumptions which amount to a tacit theory of cognitive architecture and processing. This conceptualization is that a theory of Universal Grammar is a *transition theory*. A transition theory, as Gregg, (1994) tells us, reiterated here today, indicates how the learning mechanism moves from one state to the next, and while it could be, in principle, a theory of growth, maturation, or metamorphosis, in the case of adult second language acquisition - which I focus on in order to simplify matters - we are all agreed that we are talking about learning. As Meisel has pointed out in his presentation here, and the other participants agree, a theory of Universal Grammar, conceptualized as a set of constraints on linguistic cognition, has to be embedded in a theory of learning. It is not a substitute for it. Essentially, problems have arisen in SLA because various authors have moved from this abstract view of UG - to a reification of UG as a psycholinguistic mechanism comparable to instance-based learning, rule-generalization and so on. When one reads papers which attempt to argue on the basis of production data that adult L2 learners do/do not "access" UG

although/because their developmental paths are different from monolingual children, and that this evidence does not motivate/motivates the hypothesis that therefore adult L2 acquisition is caused by domain-general learning mechanisms, then it seems to me that my observation of the state of the field is justified. Certainly, others have the same view of the field, and Bley-Vroman has repeated here his view that UG ought to have something to say about development. I do not share his view. There are all sorts of accounts imaginable for why different groups of learners follow different developmental paths, there are equally all sorts of accounts imaginable for why adults *say* certain things, and there are all sorts of accounts imaginable for why ultimate attainment is different in first and second language acquisition, including the very fact of bilingualism itself. We can, and must, in my view, divorce these observations about the nature of transition from basic claims about the essential nature of linguistic cognition in SLA if we are to make any progress.

This was going to be the crux of my contribution to this discussion, along with some detailed analysis of some of our critical literature (especially Clahsen & Muysken, 1986, and Bley-Vroman, 1990) to show how easy it is to be led astray by certain generative metaphors, and how difficult it is to sort out discussion of data in order to understand what are legitimate inferences about UG and what are not. After some queries over e-mail among the participants, who were worried that we would end up saying the same things, and after discovering that my contribution would occur last, I decided to take a slightly different tack and map out what I think might be a

reasonable UG-based research agenda for the future. In laying this out, I will try to give my spin on answers to Eubank's questions. An initial formulation appears in (2).

- (2) The original "access question", properly construed was then and is now a reasonable question to ask. The proper construal is:

Do interlanguage grammars conform to the principles of UG?

## 2. The proper construal of the "access" question

The proper construal of the "access" question is, in my view, the classic view that Universal Grammar is a set of constraints on linguistic cognition which define what a possible linguistic system is (repeated recently in Chomsky, 1998). We automatically assume that this means that UG also constrains what a possible psychogrammar is, although the connection between UG and the knowledge systems actually responsible for what we understand when we hear speech or say something is anything but trivial. The reason for this has to do with Chomsky's claim that UG is knowledge "in the absence of experience"; it is some sort of basic knowledge which makes learning of a particular psychogrammar possible. There are various ways in which we can envisage the relationship of UG to psychogrammars, a topic I will return to below. Let me say here that the investigation of the *constraints issue* continues to be an important one, and, as White has pointed out here, has been the focus of at least some important SLA research in the last 10 years. I fully expect it to continue to make a significant contribution to SLA in the future. However, we have to understand that in investigating the nature of the essence of linguistic systems, we are not making

statements about the nature of development and our research requires an appropriate methodology to focus on the question of whether interlanguage grammars are, as Clahsen & Muysken (1986) put it "natural language" grammars. I assume that the appropriate research will include detailed writing of partial grammars of interlanguage knowledge at some selected point in time (Initial Stage, Stage i, Final Stage). In contrast to White, I am not so happy with the existing literature because it has tended to focus on the nature of constructions (relative clauses, passives, raising constructions) or on some narrow aspect of UG deemed to be parameterized (reflexives, adverb position, negation). I think this is insufficient because the question of UG-in-SLA does not hinge on the correctness of the parameter-setting hypothesis. I would therefore like to see more qualitatively-focused case studies. We have a mere handful that we constantly refer to - the "singular" ones (Huebner, 1983; Sato, 1990) and the multiple ones (Clahsen, Meisel & Pienemann, 1983; the ESF project Klein & Perdue, 1992; Dietrich, Klein & Noyau, 1995; Becker & Carroll, 1997) and we need more. In particular, we need studies which explicitly adopt a UG perspective (rather than rejecting the Autonomy of Syntax hypothesis - as do many of the studies just cited) and which attempt to examine the general UG-based principles assumed to underly large areas of the grammar and which will assess the nature of reference, coreference, quantification and variable-binding, predication and modification in interlanguage grammars. We may discover, once we move away from a narrow focus on particular constructions and turn our attention to the ways in which "meaning" and "form" in language are subject to formal constraints, that the systemic function of interlanguages is remarkably "normal", even at the very early stages of development.

In addition, I hope that at least some of the studies would mimic studies of input-deprived children such as Geni, Marta, Antony etc. Such studies ask a single question: What is the essential nature of their subjects' knowledge systems? More specifically, do they exhibit essential properties of normal grammars (structure-dependency, c-command, No Crossing Lines Constraint, etc.). Is it possible to write a grammar of the data without recourse to these constraints? Because work in linguistic pathology has been directly concerned with the question of whether limitations of general intelligence necessarily entail limitations in linguistic knowledge and ability (Curtiss, 1977, 1982, 1988; Yamada, 1990; Smith & Tsimpli, 1995), they are directly relevant to the question of whether interlanguages are well-behaved or deviant in some independently definable way. It is important that SLA research continue to ask the question: What is the essence of interlanguage grammars? It is important too that UG-friendly researchers be part of this particular type of research because others are making remarkable statements in answer to this question, claims which amount to saying that early grammars do not exhibit essential properties like c-command (Perdue, 1996) or that innate principles which occur in a modular acquisition device but are not specific to language (= general nativism) can account for the properties of linguistic cognition (O'Grady, 1996). I personally am not impressed by Perdue's line of argumentation since it is based exclusively on production data and I need to be convinced that someone could regularly and systematically compute interpretations of utterances on the basis of the properties of the sentence in the absence of c-command.<sup>1</sup> The claim is nonetheless out there and we shouldn't ignore it. Similarly, O'Grady's approach is linguistically sophisticated and may be compatible with some of the odder properties

of interlanguage grammars, namely that they tend to exhibit "mixed" properties falling between what an L1 and an L2 parameter-setting would predict, or that they exhibit some aspects of the expected clustering effects of parameter-resetting but not all (Meisel, 1997, and today).

### 3. Linking UG to psychogrammars

The essence-of-interlanguage question is fundamental but some of us are more interested perhaps in developmental issues and, in any event, we all know that if we cannot make generative research relevant to acquisition issues, it will be ignored in SLA. Gregg and Meisel have both claimed here that we need to re-interpret UG in the context of an acquisition theory. Well and good! The question is: How? Here are at least 2 ways to think about the problem and where UG-friendly researchers can make a significant contribution to our field.

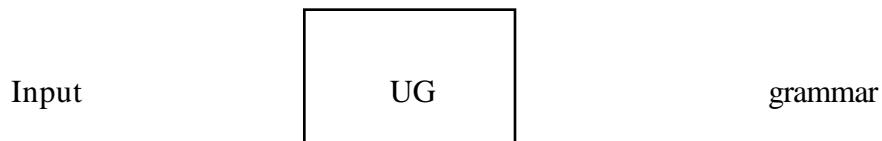
#### (3) Operationalizing UG

- a. Serious conceptual work has to be done on the development of a model of UG in its relation to the psychogrammars associated with parsing and production.
- b. Serious conceptual work has to be done on the development of a model of bilingualism where what is in focus is the functional architecture and the relationship of the psychogrammars to each other.
- c. Related to (a) and (b) is the development of a plausible psycholinguistic model of the lexicon such that parameter-setting could be "lexically-based".

The first problem lies in operationalizing UG. The tacit assumption in the current literature appears to be that UG is a "box-in-the-mind" whose contents are "accessed" (along with unspecified other information) whenever the learner needs to learn

something. This follows from the characterization of UG as knowledge "prior to experience" and the hypothesis that UG is relevant to adults learning an L2. It cannot be assumed, given the hypothesis, that UG ceases to constrain acquired linguistic knowledge after some given point in time, say, puberty or when first language acquisition has reached some steady state. Consequently, researchers have tended to assume that if a given phenomena is absent in the surface syntax, e.g. WH-movement, this means that relevant constraining principles are not "available" in the L1 and therefore if such individuals subsequently learn an L2 with WH-movement, this will constitute a test case for continued "access" to UG. Thus, when a learner needs to reset a parameter, he reaches into his UG box and takes out the relevant other setting. While there are all sorts of assumptions built into this kind of argumentation which one need not agree to, I want to focus here on a single issue, what it means to say that the "information is available". Gregg earlier in his presentation pointed out the dangers in taking one's black box diagrams literally. I concur and would argue that we are well past the time when we should content ourselves with barebones models like that in (4).

(4)



If we focus on the operationalization issue, we have to confront questions of how the information is made available during learning. In principle, this might run the gamut from hypothesizing that UG is "virtually" constraining the system in such a way that the principles we constantly refer to are never represented anywhere but the outcome of learning is just such that our formulations of them are obeyed, to hypothesizing that there is a mental representation somewhere in the mind/brain. I am loath to consent to the latter view myself, since it licenses questions about *where* the information is stored, and questions about principle-specific pathologies. While there may be some evidence for computational pathologies (Caplan & Hildebrand, 1988), the evidence is limited and hard fought for. I know of no evidence that parameters can be linked to specific pathologies. Consequently, there is little reason to think that parameters are located anywhere in the mind/brain, and little reason to think that their "content" is represented (in the normal meaning of that term). This does not diminish the relevance of talk about parameters, it merely means that at some point that talk has to be fleshed out in a functional architecture of linguistic cognition. Surely, we are ready to make some moves in this direction.

We need to attempt to operationalize UG in terms of the psycholinguistic mechanisms causally related to our usual data (what learners understand and say), and recast some of our traditional problems in psycholinguistic terms. For example, I prefer to recast Schwartz & Sprouse's (1996) *full access claim* in processing terms, namely that in attempting to comprehend speech in the L2, learners have no other option but to transfer the parsing procedures of the L1, something they do

automatically and unconsciously. In most of the cases we investigate, transfer of parsing procedures leads to parsing failure because the categories of the L1 and L2 and their arrangement in structures are normally quite different. In my model of SLA (Carroll, in preparation, 1996/submitted), it is when parsing failure occurs, that learning takes place. What the learner then draws on, in order to construct viable parsing procedures, will be the content of a theory of learning. In my work, the operations which occur to rearrange the limits of existing categories (understood to be complex 'theories' about objects, events, properties, and relations), or to construct novel representations are highly constrained and must respect the constraints in UG in precisely this way: Induction cannot introduce new primitives into the representational systems; it must respect the autonomy of levels; and structure-building operations are constrained within a given level to unification.<sup>2</sup> It is entirely possible as well that some notion of economy will enter into the operations of the learning device, perhaps in the traditional sense that the learning device makes the minimal number of changes to its grammar to accomodate the still-to-be-parsed input. This is an empirical claim of my theory which needs to be closely examined.

One of the consequences of this way of looking at things is that learning takes place in the "input" side of things. Our traditional focus on production data and inferencing to grammars and learning is, in my view, entirely misplaced. There will, of course, be learning on the output side of things. Learners have to acquire novel production schemata to organize categories into new sequences; they have to encode detailed phonological and phonetic representations in order to have a set of instructions for the

motor-articulatory system, and they have to lay down and use particular schemata often enough so that they are readily activated and can play their role in producing fluent smooth speech. Learners also have to suppress the readily-activated schemata of the L1. Thus, an English-speaker like me learning German has to acquire production schemata placing a direct object NP, adverb phrase, or a PP immediately after a past tense auxiliary with the past participle coming only at the end. When all 3 such elements occur, they have to be ordered according to target norms. In addition, however, I have to suppress the English-based schemata which would produce the sequence NP Aux Verb NP PP Adv. What we have been investigating for years might be the "learning" of grammatical rules. It may, however, be something quite different, namely the complex interaction of the learning-to-suppress-production-schemata while learning to produce and automate new production schemata. Until we start to focus on the role of parsing and production as separate aspects of our operationalization of learning, we will not be able to investigate these issues.

The statement in (3b) is directed at learnability research in the context of bilingualism. The current general assumption is that there is both "cross-linguistic influence" and independent development in two distinct and autonomous systems, such that resetting a parameter has no influence on first language knowledge. Why this assumption should be held is not clear to me, except insofar as our intuitions tell us that learning a second language does not make us aphasic in our first. Nothing in our models and theories requires it and it is empirically and perhaps also logically untenable. One thing which needs to be investigated in particular is what it might

mean to be the "same" parameter in several languages. This topic cannot be broached without examining the various cues to parameters, and the nature of the input to learning, a topic I return to below. Meisel's careful analyses of the distinct developmental paths of child bilinguals and adult L2 learners have raised serious questions about the construct *parameter-resetting* in second language acquisition. He has argued here today that the SLA use of this notion is not motivated by research in related domains and is perhaps not viable. I think we might sort our way through this underbrush if we give some serious thought to the idealizations we make about the conceptual architecture of bilinguals, and then measure those idealizations against the small but important literature on bilingual memory and language processing (see, e.g. De Groot & Kroll, 1997; Green, 1998 and commentary, and Grosjean, 1998, for discussion of some of the issues). UG has to function somehow in the context of considerable evidence that the bilingual's knowledge systems are constantly interacting. The more alike those systems are in terms of structural or formal overlap, the more difficult it becomes to talk of two distinct knowledge systems (think about Dutch and German, or Spanish and Portuguese in this context). The truth of the matter appears to be that the notion of "separateness" in "separate grammars" will be very difficult to articulate and will certainly force us to see the inadequacies of viewing UG as a "box in the mind/brain".

The issue in (3c) also arises out of trying to operationalize grammatical theory. At present, psycholinguistic models of linguistic memory are so different from Chomsky's conceptualization of the lexicon that it is impossible to move readily from

linguistic treatments to psycholinguistic ones. Is the 'lexicon' in Chomsky's sense even usefully considered to be a memory store? In particular, why should it be the case that it is the language-specific categories, namely the functional categories, which have to be stored in the lexicon precisely because they are not universal, which parameterize? And if these elements are stored in memory, why shouldn't one find parametric-pathologies if access to the particular functional categories is disturbed?

#### 4. Linking UG to learning

Talk of learning specific functional categories ought to remind us that we have to link UG to a theory of learning in SLA. Chomsky's continued insistence that there is "no learning" in first language acquisition in the usual sense should be recognized for what it is - a rhetorical stance vis à vis behaviourism - and now connectionism - that will not resist close scrutiny. A theory of language learning compatible with UG requires a position on what "input" is and how it is identified. This is particularly relevant to the parameter-resetting approach.

- (5) Serious conceptual work has to be done on the development of a theory of triggers for parameters in the context of emerging bilingualism.

Valian (1990a,b) has pointed out that both speech and analysed strings are ambiguous and therefore children must have some way of deciding which strings to pay attention to and which to ignore. Learnability work by Wexler & Gibson (1996) and J.D. Fodor (1998) has also pointed to some serious problems in the original conceptualization of parameters which suggests that even with parameters, learners might not converge on

a given grammar. More problems will emerge when one tries to reconcile the recognition of triggers with a model of the functional architecture of bilingualism since bilingualism will increase the degree of potential ambiguity, and raises important questions as to how the learner knows which system to fix and which to leave alone. Identifying unambiguous triggers is not a trivial problem, and it is one which has been completely ignored in generative SLA research. Moreover, even assuming that we can deal with the learnability issues involved, it still remains to be seen whether the speech that given learners hear might contain identifiable cues and triggers. Given the relative isolation of many adult L2 learners, and the infrequency with which they have any kind of normal discourse with native speakers, one might need to set up a second type of research focusing on the logical relations among cues and parameters if cues come in different temporal orders.

(6) We need a learning theory which is UG-compatible

Serious conceptual work has to be done on the development of a theory of learning which is UG-compatible independently of parameter-setting.

One of the peculiarities of the "access/no access" debate has been the assumption by those rejecting access to UG that necessarily learning will therefore be domain-independent and based on some unidentified but presumably radically different principles. I see no reason why this should be the case and the assumption show a surprising ignorance of much current work in cognitive psychology which suggests that language is far from being unique in being grounded in domain-specific autonomous cognitive principles. My reading of this literature (see Carroll,

1996/submitted) is that there is no such thing as a domain-general theory of learning.

What we will have to adopt are a number of distinct domain-specific theories of learning, including one for language. I suspect that the one we need for SLA will be compatible with UG even if it turns out to be the case that the parameter-setting metaphor has to be abandoned altogether.

### Summary

I have argued here that the original view of UG as a set of constraints on linguistic cognition is the correct view and will continue to provide SLA with insights into the nature of grammars. I then pointed out that this research has to be independent of developmental conclusions. To turn our property theory into a transition theory, we need to link UG to a theory of the functional architecture of linguistic cognition, in particular to a functional architecture compatible with the facts of bilingualism, we need to conceptualize UG in the context of theories of parsing and production, and we need to develop a theory of learning which is compatible with UG. All of these types of research activities require a commitment to the idea that there are language specific, innate constraints on linguistic cognition, determining the nature of interlanguage grammars. It behooves those of us who have this commitment to undertake such novel forms of research.

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## Notes

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<sup>1</sup> There is, in other words, a fundamental processing difference between inferring the meaning that Sally ate a cookie from hearing [saeli] and [k ki] and seeing Sally behind a cookie-crumb laden plate (inferencing from non-linguistic knowledge) and parsing the string Sally ate a cookie which will lead to the correct interpretation in the absence of both Sally and cookies. It may well be that in the very early stages of acquisition learners are simply not able to parse the "lower" levels of acoustic and phonetic analysis rapidly enough to identify and encode strings of syllables and "hear" something like a string of words. At that early stage, I assume that they will parse only stressed syllables and only items occurring in particular positions in intonational phrases will be encoded (namely the locus of shifts in fundamental frequency at the end of the utterance, or in positions of phonetic reinforcement, namely at the left edge of an intonational phrase where contours are reset). At this stage, learners may "hear" sounds which correspond to particular words - nouns in English (direct objects and objects of prepositional phrases), particles and intransitive prepositions and verbs. This may be contentful enough vocabulary to set in motion our powerful inferencing mechanisms and derive some kind of meaning from the context. There is, however, no reason to assume that this kind of interpretation has anything to do with what happens in normal speech interpretation, or that we shouldn't envisage a radical transition in the nature of interpretation processes as learners acquire more phonetic and phonological knowledge and become able to parse longer strings of words.

<sup>2</sup> This is a tentative claim at the present time because there are certain fundamental differences between morphosyntactic structures (binary branching, depth of embedding, asymmetrical relations like c-command) and phonological structures (n-ary branching, relatively flat, no a-symmetrical relations). I haven't sorted out the implications of these differences yet as to their consequences on the number and type of operations needed by a parser.