Access to what? Access by what?

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The question before us is the so-called 'access to UG' question, and whether it needs to be reassessed. To let the cat out of the bag, I'll say yes right off the bat, and then try to qualify that yes from now on.

First of all, we should distinguish between the question whether the research program that has been to a great extent inspired by this question was worthwhile, on the one hand, and the question whether the question itself was well-conceived on the other. I'm going to say next to nothing about the first question, or at least next to nothing about the research itself, since I'm not an appropriate person to assess it. I will simply note that this research has become voluminous over the last decade, as well as increasingly sophisticated. One admittedly impressionistic index of that growth in sophistication comes from looking at footnotes to individual papers replying to the comments of referees. Another sign is the increasingly frequent appearance of SLA papers—not all UG-related, mind you—appearing in non-SLA journals. This suggests that SLA research, especially UG-related SLA research, is starting to attract the interest of non-SLA researchers in linguistics and cognitive science.

In any case, what I want to talk about is the so-called 'access' question itself, and about whether it is felicitously framed. Here's my plan: I will start by trying to situate the access debate within the framework of the property-theory/transition-theory distinction. I will then suggest some ways in which the debate has been less than fruitful, and some areas where we still need answers.

PROPERTY THEORIES OF SLA

An SLA theory, as we all know, has two fundamental explanatory burdens: to explain the linguistic knowledge possessed by a learner about his L2, and to explain how the knowledge comes to be possessed. In Felix's terms, we need to solve the logical problem of SLA and the developmental problem. In my terms, or Cummins's terms stolen by me, we need both a property theory and a transition theory of SLA. The property theory's job is to answer the question, In what does L2 competence consist? The answer to that question will probably—but not necessarily—be a linguistic theory. The transition theory answers the question, What causal mechanisms bring about the acquisition of the sort of competence explained by the property theory?

To start with the property theory first: Oddly enough, most of the participants in the access debate actually agree as to what the appropriate theory of linguistic competence is: UG. In this sense there's no debate. **Not**, mind you, that there are no other suggested explanations for L2 knowledge: there's connectionism, for instance. And there are of course all kinds of variations within UG theory, both variations in the theory itself—GPSG, LFG, etc.—and variations in the specific characterization within a single theory of the L2 competence, as seen for instance in the debate over the initial state of the L2 learner. Nor are these differences trivial or uninteresting. But I will still maintain that there is no other **theory** around besides UG—in this larger sense of UG—for explaining the knowledge an adult L2 learner has of the L2. What Eubank & Gregg said a few years ago is still essentially correct: "For acquisition researchers working within the UG framework, there is a rich, well-developed theory of linguistic competence at hand ... with one

enormous strong point: **it is the only one there is**." (p.51) (Since this quote seems to have got up the noses of a lot of people, most of them too careless to actually read it correctly, let me stress 2 things: 1) that we were not claiming that UG is the only theory of language acquisition, or even that it is **a** theory of language acquisition, but rather, and merely, that it is the only well-developed theory of language competence; and 2) we weren't bragging—I, at least, find it highly regrettable that there isn't any well-developed competition yet.)

In any case, if we ignore those who say there's no access because there's no UG in the first place, the disputants seem to be in agreement that the L2 learner's competence can be, and properly is to be, described by (a subset of) a set of terms from within a theory of UG. The disagreements arise over the extent to which a full, precise description of L2 competence matches that of a native, and, as we will see, over the acquisition mechanism. So far as I can tell, even Robert's FDH and William O'Grady's general nativism fit in here; what is really fundamentally different is the learning theory underlying the FDH. (Or if I'm wrong about, say, the FDH, then it's because, like anti-UG SLA theories in general, it has **no** theory of L2 competence to explain the detailed kinds of linguistic knowledge even so-so NNS's have.)

Thus in a sense, there **is** no access debate, at least if 'having access to UG' merely means something like 'being constrained by UG', as for instance it is defined by Epstein et al. in their BBS paper ("we mean by 'X is accessible' only that 'X constrains the learner's hypothesis space" —their fn.5). As several commentators on that paper pointed out (White, Sprouse, Moi, inter alia), this sort of definition is rather too all-inclusive to be of much interest; it predicts little more than that IL grammars will not be 'rogue' or 'wild' grammars. That is not such a trivial prediction, of course, as there can be some fairly subtle distinctions between what is and is not licensed by a normal grammar. (The tendency to point to the absence of truly loony rules in an ILG, in line with the obligatory mention of Chomsky's structure-dependence example, is misleading in this respect.) But it's also a prediction that most parties to the debate make, and which seems in general to have been borne out. Further, it leaves open all kinds of possibilities for ILG's that should be unwelcome to anyone arguing for 'access to UG': After all, given that all settings of all parameters are presumably licit, hence that all 'hypotheses' about settings for TL parameters are presumably within the hypothesis space provided by UG, an L2 learner could stay within that hypothesis space and still get every single parameter setting wrong. A grammar that doesn't violate UG but that does come about in disregard of the input should hardly be acceptable to a supporter of 'access to UG'. Which suggests at least that framing the 'access' debate in the negative terms of not violating a hypothesis space obtaining in L1 acquisition is a less than satisfactory way of framing it. We'll get back to that when we look at what 'access' could mean. But first a look at the other side of the problem, the transition theory.

TRANSITION THEORIES

A transition theory explains a system's changes in state. In language acquisition, of course, the system is a learner and the changes in state are changes in the state of knowledge, or competence. In the case of SLA, though, the explanatory burden is twofold: 1) to explain what causes these changes, but also 2) to explain the failures to change, that is, the differences between NS final state and NNS final states, even granting the irrelevance of cases where the stimuli are so truly impoverished as to guarantee non-acquisition. This twofold explanatory burden means that we will also need two kinds of explanation: a contrastive explanation (why P rather than Q? why don't L2 learners reach the same level/pass through the same stages/act on the same causal stimuli as

natives?), and a non-contrastive explanation (why P, punkt?). These are very different kinds of explanation, designed to answer quite different questions.

Sources of change are either internal or external. Given that adult learners have matured, of course, we can pretty much rule out internally initiated changes—unless, of course, decline, decay, and degeneration of mental and physical systems can be a source of acquisition!

This leaves us with external causes—in a word, input. (Insofar, of course, as there are changes to explain. After all, one central claim of those supporting so-called 'full access' positions is that there's a lot of stuff that does **not** change.) There are various kinds of input, as we all well know, but not all of them will be relevant to the access debate.

- Explicit input, whether negative or positive, for instance, will not play a significant role in the debate. It isn't a question of whether negative evidence works or not—of course it works, at least sometimes. It's a question of whether it suffices, since the UG claim is a poverty-of-the-stimulus claim. The debate over the effect or non-effect of negative evidence on parameter-setting is an interesting one, but it is not strictly speaking an access debate, since both sides are either assuming, or taking a neutral stand on, access by other means, i.e. positive evidence.

- Similarly, mere brute force frequency will not be a likely factor in the debate, even if it is a factor in acquisition itself. It could easily, for instance, take some minimally frequent amount of input of X to 'trigger' some change, such as acquisition of X. But what is needed for a UG/SLA account of acquisition is input that produces POS-type changes in the IL state; in effect, input of X to produce the acquisition of Y. It is possible, of course, that there are child/adult differences in sensitivity to input, such that it will take a significantly greater amount of X input before the adult learner twigs to the consequences of X for Y. But even if this were the case—and I don't know of anyone making this kind of claim—we'd have only a contrastive explanation of the difference between L1 success and L2 failure to acquire Y. Telling us that one needs more X to achieve Y does not tell us what X does to produce Y.

- Once again, modified input of one sort or another may have a role, even for all I know an important role, in SLA, but still only a secondary one: Explanations from modified input are in effect explanations of how requisite input is 'cleaned up' for the learner, of how noise is removed from the signal. Here again we can hope for no more than a contrastive explanation.

What we need, of course, is **triggers**. Or rather, triggers is what the learner needs; what **we** need is a theory of triggers, input on the basis of which 'deductive' learning can take place that can revise the ILG, e.g. by resetting a parameter. But again, given the failure of the Learnability Condition to obtain in SLA, we need two kinds of explanation:

1. What specific kinds of input should suffice to produce changes in the ILG? and
2. Under what conditions would input **not** suffice, hence either
   a) requiring negative evidence of some sort to effect the desired transition, or else
   b) dooming the learner to a non-native grammar with respect to the non-triggered change?

To take a concrete, albeit rather sad, example, what should a learner of Japanese like me need to hear in the input in order for my IL Japanese grammar to adjust whatever parameter it is these days that accounts for the scope of anaphors like *zibun*? If it's the same sort of input that works for

Japanese 5-year-olds, why hasn't it worked for me after 20 bleeding years of input? If it isn't the same sort of input—if, say, it's rather recondite input unlikely to be encountered even by me—what sort of input is it, and why are the triggering conditions different?

Note how little is explained by saying that I don't have 'access to UG', especially given the non-uniformity of the gaps in my L2 knowledge, and given that I neither restrict myself to English-type interpretations (so far as I can tell), nor have I produced a 'rogue', unnatural IL anaphora system. (And conversely, if I were as accurate as the Japanese themselves are with Japanese anaphora, it wouldn't be a very impressive explanation merely to say that I do have access, unless we can show how the Japanese catch on, and can show—or at least make a plausible case—that learners like me too should catch on when provided similar input.) And note too that we can't appeal to a traditional Subset Principle kind of explanation, given that—if indeed the Subset Condition holds here—English is the subset language and Japanese the superset.

Anyway, it's this sort of change of state—or failure to change—that we need to account for, and it's precisely a theory of triggers that's supposed to do the accounting. In the SLA literature, there are occasional suggestions made, but they're fairly thin on the ground, and not often very specific when the are made. (One example of a specific proposal is Schwartz & Gubala-Ryzak; interestingly, the input they propose is not likely to be met, as White points out.)

THE ACCESS DEBATE AGAIN

So having very quickly glanced at what an SLA property theory and an SLA transition theory need to explain, let us return to the question of 'access'.

In effect, it would seem that the debate about access has in fact been a debate about what the specific states of the L2 learner are—what his initial state is vis-a-vis the L2 and what his (expected) final state is. It has not been a debate about 'access' to UG so much as a debate over what kind of UG-licensed knowledge system is acquired. This is, needless to say, an important debate; it just isn't an important debate about access to UG. Or rather, the debate is about access if we make the assumption that UG is something—some thing—to which one can have access; in effect, as a machine that makes grammars, a language acquisition device. Total access means the machine is in perfect working order; zero access means you'll have to find some other machine to construct your grammar; partial access means that some parts of the machine will no longer function to produce those aspects of the grammar that they were designed to produce. This is not so gross a caricature as you might think; read Epstein et al..)

It may be that we're still too influenced by the standard black-box diagrams of our youth, showing input on the left of the box and a grammar on the right. UG on this view is rather like a vending machine: put in a coin and you get a Pepsi, put in input and you get a grammar. The problem, of course, is that this concept of UG, even in a less caricatured form, is not the one currently accepted by most generative linguists (including, so far as I know, every speaker in this colloquium), where a grammar is in effect an epiphenomenon, simply UG in one of its avatars as a result of parameter-setting.

In short, the 'access to UG' debate has been clouded by the fact that 'access to UG' can only be a metaphor, and perhaps a metaphor that has outlasted whatever usefulness it may have had. Remember Fodor & Pylyshyn's warning that 'metaphors tend to be a license to take one's claims as something less than serious hypotheses.' Now, I don't for a moment think that, say, the people on this stage have been taking their claims as less than serious hypotheses, and I most certainly do not think that they've been misled by the 'access' metaphor into taking it literally. I do

think, however, that the debate has been couched far too often only in merely metaphorical terms of access, and I would suggest that for some people outside the arena, and perhaps even inside, the 'access' metaphor has encouraged a certain fuzziness of thought, and especially a certain confounding of the roles of property theory and transition theory in SLA.

What, after all, would it mean to say we had settled the 'access' issue? 'No access' could be demonstrated, perhaps, by the widespread existence of 'rogue grammars' showing no good evidence of instantiating any of the subtle aspects of UG. But such grammars don't seem to be found. 'Full access', presumably, would be supported by general native-like success in acquiring all UG-based properties of the L2. (Not that the 'full access' hypothesis **requires** such evidence, mind you.) 'Partial access' hypotheses would perhaps be the preferred choice if one could point to universal failure to acquire all UG-based properties of the L2. In other words, choosing among these hypotheses seems to involve committing something like Bley-Vroman's comparative fallacy: 'access' is being defined as the degree to which the L2 competence of the learner conforms to the competence of a native.

Since, though—or insofar as—ILG's do not violate UG (and since, after all, even L1 grammars don't instantiate all aspects of UG), the 'access' claim in fact is really a claim about how input causes changes of state in the L2 learner, or how and why it fails to cause such changes. In other words, the access debate is, or should be, centrally concerned with the transition theory of an SLA theory. In essence, it seems to me, 'full access' hypotheses are claims that input that serves as triggers for the L1 acquisition of Language X will, ceteris paribus, serve as triggers for the L2 acquisition of Language X as well. Partial access hypotheses are claims that there are cases where such input will not serve as a trigger and that hence certain properties of the TLG will not be acquired, or at least will not be acquired by triggering, or at least will not be acquired by the same triggering as in L1A.

Framed this way, the access debate may blur the distinction between full access and partial access; but then, I'm not sure that that's altogether a bad thing, given what I see as the vacuity of the term in the first place. A full access hypothesis is **not**, for instance, committed to the counterfactual claim that L2 learners acquire nativelike competence. (Indeed, it's not, strictly speaking, even committed to the claim that some L2 learners acquire nativelike competence.) Nor is it forced to take refuge in handwaving appeals to non-specified 'performance factors' to explain away the failures to reach nativelike competence. I stress the 'ceteris paribus' clause in my description. Ceteris paribus does **not** mean that X happens, except when it doesn't. But if we can specify certain conditions that obtain in SLA that do not obtain in L1A, we may be able to predict that L1 triggers won't trigger. That is, if the initial state differs between L1 and L2A, we may have a well-defined ceteris paribus condition. Bonnie Schwartz, I believe, has made claims of this sort. In any case, a full access claim—I use the term only for convenience from this point on—will need to explain, but is not automatically embarrassed by, failure to acquire UG-relevant L2 properties.

Similarly, a partial access hypothesis needs to specify the conditions under which acquisition should **not** be triggered, and needs to be able to explain putative counterexamples. But just as a full access position does not entail perfect nativelike acquisition, neither does a partial access position need to deny seemingly nativelike competence, **if** that competence can be explained as the likely outcome of processes other than triggering. Some linguistic knowledge, after all, however it is acquired by 2-year-olds, might in principle, and indeed in practice, be acquirable by an adult through induction or even brute force enumeration. (The head position parameter might be such an example, as some writers have suggested.) Thus a learner could be behaviorally indistinguishable from a native in respect of some property without endangering the partial access

hypothesis; nor need one mire oneself in the probably often insoluble problem of whether the knowledge underlying that behavior is ontologically different in native and non-native—i.e., whether 'learning became acquisition', to use an unfortunate phrase. Insofar as the 2 speakers' competences can be described in the same theoretical vocabulary, it may be otiose to argue that they are nonetheless different competences. And on the other hand, if it can be shown (or if it can be plausibly claimed, at least) that the L2 learner's competence was not (ideally, could not have been) triggered by input—i.e. deductively acquired—then we have evidence for the partial access position. Or at least the evident identicality of the 2 competences is not an embarrassment for that position.

In this view, a full access position might seem to differ from a partial access position only quantitatively, in the degree to which triggering is claimed to be successful in SLA, or the number of triggers that should still work. But a principled distinction is possible, certainly: e.g. where a full access theorist is committed to the across-the-board effectiveness of triggers, with ceteris paribus exceptions possible, and needing to be theoretically motivated, of course, a partial-access theorist could claim that only those triggers that provoked parameter-setting in the L1 would still be effective in the L2, hence that parameters should not be 're-settable'. (Janet Fodor, I believe, hints at something like this.)

A no-access position, finally, would be one that denies the existence of triggering in SLA; it would claim, as I believe Robert does, that L2 learning is learning: it is essentially inductive not deductive, although one centrally important inductive base is the learner's deductively acquired L1 grammar. (Again there no doubt need to be some ceteris paribus conditions; it shouldn't be incumbent on a no-access theorist to deny **all** forms of deductive learning, although presumably he'll have to deny deductively set parameters.)

Some years ago Piattelli-Palmarini stressed the analogy between language acquisition theories and immunological theories; both are theories of selective, rather than instructive, change in the organism. To take the analogy he borrows from Edelman, you can conceive of the immune system, or the language acquisition device, as being like a bespoke tailor: the customer/ antigen/input comes in and orders a suit/antibody/parameter setting. Or you can conceive of the immune system/LAD as an enormous Men's Wherehouse, where every single conceivable suit/antibody/parameter setting is waiting to be chosen by the customer/antigen/input. The off-the-rack system is a selectivist system. The access issue then comes down to deciding whether, once the 3-piece pinstripe has been taken, the pearl-gray check is still available (full access); or whether once the input equivalent of cowpox has been acquired, we are no longer susceptible to smallpox (partial access or no access).

Whatever the value of that analogy, it seems to me that focussing on the question of triggering input, rather than on a vague 'access to UG', has a couple of advantages, not the least of them being that it takes us away from a vague 'access to UG': It also takes us away from the vexed question of modularity of L2 knowledge states, away from arguments over whether my knowledge of when to drop subjects in Portuguese is the same **kind** of knowledge as Maria's, to the question of acquisition processes. (The modularity issue doesn't disappear, of course, or become uninteresting; but it may be finessable while investigating the 'access' issue in terms of triggers.)

Of course it will not have escaped your attention by now that there is a fairly hefty fly in the ointment: We don't have a theory of triggers, at least in SLA. But that's not my problem, is it? I didn't promise you an answer to the access question, merely an attempt to frame it more perspicuously. But there is interesting work going on in L1 acquisition, e.g. Gibson & Wexler, and Fodor, and this work should have important implications for SLA.

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