UG AND SLA THEORY: THE STORY SO FAR*

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Roughly since the publication of Chomsky's Pisa Lectures (Chomsky, 1981), there has been a dramatic growth in the amount and quality of research into the status and role of Universal Grammar (UG) in second language acquisition (SLA). Indeed it is not that much of an exaggeration to say that SLA as a scientific (rather than an academic) discipline was born in Pisa; and it may be worth our while to take stock and assess just what has and has not been accomplished by work in this tradition. Specifically, I want to discuss the role of UG in the construction of a theory of SLA.[1]

Although empirical SLA research in general has multiplied in the last decade or so –indeed, UG/SLA research is hardly the main trend, if measured by the pound– it can be argued that actual work toward a theory of SLA has been restricted to the UG framework. (This is one reason, among many, that I am unwilling to refer to a UG 'paradigm'.) This is a strong claim, I realize, and one of my aims here will be to back it up, by looking at SLA work in the light of some fundamental criteria for theory construction.

The claim that the UG/SLA research program is the only current program that takes seriously the commitment to theory construction does not, however, entail either that there is unanimity within the program, or that the program as currently carried out is sufficient to meet the criteria I will outline for a potentially successful SLA theory. There are well known disagreements, about fundamental theoretical claims, among scholars who share the UG/SLA perspective. And there are important theoretical questions that have yet to be addressed within this perspective. Thus I will also try to indicate at least the more important explanatory problems currently left out of, or not yet addressed in, the UG/SLA framework.

SLA THEORY: GOALS AND STRUCTURE

It is taken for granted by UG/SLA theorists that the central goal of an SLA theory is to EXPLAIN the ACQUISITION of SECOND LANGUAGE (L2) COMPETENCE.

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The three words need –alas– to be stressed; in fact it is an indication of the immaturity of our field that they do.

EXPLANATION: Theories are ways of explaining phenomena; they are not descriptions of phenomena, still less collections of data. This of course does not mean that description is a minor or uninteresting aspect of theory construction; it does mean, though, that we want to go beyond description. What this means in practice – and this is a point that is missed with great regularity, at least in the polemic SLA literature– is that we have to appeal to non-observable phenomena (Gregg, 1993a).[2] Nor are theories sets of predictions. Of course, predictive power is a major criterion for assessing a theory, but it should be kept in mind that some of science's most valued theories –evolutionary theory, plate tectonics, Big Bang cosmogony– are valued almost exclusively for their explanatory power not their predictive ability. From a practical point of view we might prefer e.g. to be able to predict which students will succeed and which fail in acquiring a foreign language; but that's not the concern of the theorist (cf. Newmeyer and Weinberger, 1988).

ACQUISITION: This should be an unproblematic term, but it is remarkable how often research –especially 'sociolinguistic' or 'pragmatic' SLA research– makes use of the term when there is no mention of acquisition itself. Ellis (1985) to the contrary notwithstanding, language use does not equal language acquisition; it does not always even presuppose it. Which leads us to

COMPETENCE: By this time the competence/performance distinction, and the knowledge/ability distinction, should go without saying, and it's a sad reflection on our field that they do not. There has been an astonishing amount of just plain silliness in the SLA literature on this issue, none of it relevant. Certainly no one has presented any reason to deny that what an L2 learner acquires (or fails to acquire) is not sentences, or utterances, but knowledge; which is to say competence. The definition of the scope of this knowledge is the job of a theory; which is one major reason that no one has yet proposed anything remotely like a theory of 'communicative competence'.[3]

The goal of SLA theory dictates its structure. Specifically, a general theory of SLA will have a two-part structure, since there are two quite different central explananda, what Felix (1984) refers to as the logical and the developmental problems of SLA: How is it possible to acquire a language? and How does acquisition proceed? These two explanatory problems correspond nicely to the distinction Cummins (1983) makes between property theories and transition theories, respectively. Transition theories are perhaps the more familiar type; they explain changes of states, normally by postulating a causal mechanism. For SLA the transition theory would explain how a given system –i.e. a learner– changes from a state of L2 ignorance to a state of (probably imperfect) L2 knowledge. Given the complexity of L2 knowledge and the time required to attain it, we will of course need an explanation of a series of changes of states, and indeed an explanation for the particular sequence of states.

Property theories, on the other hand, deal not with changes of states of systems, but with the system itself: here the question is 'In virtue of what does system S have property P?' (Cummins, 1983: 15). Thus, to use an example of Cummins's, where a transition theory explains why the gas got hotter, a property theory –specifically, the kinetic theory of gasses– explains why gas has a temperature. The question of how heritability is instantiated is answered by a genetic theory. A sociological theory would

explain e.g. how authority is instantiated in a society. In SLA, the central property question is, How is L2 knowledge instantiated in a learner? Actually, as we will see, SLA theory, or indeed any language acquisition theory, will require minimally two property theories, one for the initial state and one for the final.

Where transition theories deal with cause and effect, property theories are in another line of work altogether: they explain the function of a system by breaking the system down into its component parts and specifying the interaction of the parts. Again to take an example from Cummins, one can view the description of the set of conveyor belts, robots, parts, etc., that make up an automobile factory as being a 'functional analysis' of the factory. In the case of, say, the linguistic system –that is to say, the linguistic knowledge in an individual's mind– one can explain that knowledge by breaking it down into its component parts; which is exactly what theories of UG do. The various subtheories of current 'GB' or 'Principles and Parameters' theory – binding theory, bounding theory, case theory, etc.– and their various subparts interact to form a system, and the grammaticality, ambiguity, etc. of given sentences can be explained by appealing to this system and to the interaction of its parts. Thus the impossibility of 'himself' referring to John in (1) is explained by the interaction of Principle A of Binding theory with one of the values of the Governing Category Parameter (Wexler and Manzini, 1987).

(1) John wants Bill to introduce himself

Note that there is no causal explanation here, at least in the sense of explaining an event; nor is there any explanation of any observable data. That is, the explanandum here is the impossibility of coreference, not the impossibility (or even unlikeliness) of uttering (1). This does not mean that property theories are totally non-causal; for instance, there must be some causal relation between Principle A and a speaker's judgement (whether expressed or not) about coreference in sentences like (1). But this is separate from the functional analysis of UG itself.

Note also that there's an extremely important advantage to property theories like UG when dealing with linguistic competence. Functional analyses proceed by breaking down complex systems into progressively less complex subsystems and ultimately into atomic components. What this means for cognitive theories like UG is that one can escape the so-called homunculus problem in explaining competence. A homunculus theory is one that in effect postulates a little person inside one's head, who acts on e.g. perceptual information to make judgements, execute behavioral decisions, etc. The problem of course is then to explain how the homunculus knows what to do, and this can only be done by postulating a homunculus inside the homunculus's head, and on and on in an infinite regress of homunculi. This problem arises because there is no difference in complexity between the originally posited explanandum knowledge and the knowledge used to explain it, which means that we are no better off than before. UG, on the other hand, explains the more complex knowledge in terms of less complex knowledge, ultimately in terms of the interaction of totally mindless principles and structures.[4] One way of viewing the change in linguistic theory from transformations to move-alpha, for instance, is to see it as a way of solving the homunculus problem in one domain of linguistie competence. This is not normally the way it is described in linguistics texts, of course; rather the change is

justified in terms of learnability: an English specific, highly complex 'passive transformation' could hardly be innate, yet could also hardly be learned on the basis of simple input of the sort that in fact is all that is required by English-speaking children. But in fact the learnability model of acquisition fits in quite nicely with the property/transition distinction.

LEARNABILITY AND SLA

Learnability models can take somewhat varying forms, but essentially they include the following components:[5]

(2) a. the learner's initial hypotheses prior to receiving language input
b. the adult grammar at the end of the acquisition process
c. input to the learner
d. a learning mechanism to analyse and interpret input in conformity with (a); e.g., a parser

An account of (a) and (b) will require property theories, of the initial and final stages of acquisition; while (c) and (d) will be involved in a causal transition theory explaining the transition from initial to final state.

This model has been employed almost exclusively by UG-oriented acquisition theorists, but it should be stressed that there is no logical necessity for this. For instance, there is room for disagreement as to what initial hypotheses the L1 learner brings to the acquisition task at the onset; especially there is disagreement as to whether (2a) includes any specifically linguistic hypotheses, and similarly whether (2d) is a dedicated language-learning mechanism, or a general learning mechanism applied also to language. Thus it's perfectly possible to accept the model in (2) while denying the existence of UG.

If we apply this model to SLA, a number of interesting problems immediately arise:

(3) a. The (adult) learner's initial hypotheses

Since (adult) L2 learners already have a fully developed L1, it becomes a question as to whether this is to be included as part of the initial state. Theoretically it could be excluded, but very few if any UG/SLA theorists today would want to claim that adults are starting from scratch. Conversely, the question is whether the adult has *lost* any of the properties of (2a) available to the child; whether e.g. the L2 grammar effectively delimits the learner's hypothesis space. Indeed, the bulk of the UG/SLA literature is devoted to settling the question of whether UG 'operates' in SLA; which is to say the question of whether the hypothesis space available to the adult is as usefully constrained as is the child's.

Also, since the adult has not just an adult language but an adult mind all around, the question arises as to whether the L2 learner's relevant hypothesis space includes domains excluded in the case of L1 acquisition (cf. Bley-Vroman, 1989; Felix, 1985).

(3) b. The interlanguage (IL) grammar at the end of the acquisition process

Where in L1 acquisition all members of a speech community arrive at a virtually identical final state, in SLA final states vary widely across learners. Although this unhappy fact doesn't alter the learnability model as such, it does complicate it, in that explanations for failure, and for differential success, are also required.

(3) c. Input

It is well known (although not yet universally accepted) that L1 acquisition proceeds in the virtually total absence of negative evidence; that is, without correction or explanation or instruction. This fact constitutes what Pinker (1979) calls the Learnability Condition on (L1) acquisition theories: an acquisition theory must account for the fact of universal acquisition based on only positive evidence, or primary linguistic data (PLD). In SLA, though, the Learnability Condition clearly is not met. This raises the theoretical possibility of a role, perhaps even an essential one, for negative evidence.

(3) d. The learning mechanism

Just as we can ask if UG survives puberty, we can ask whether the learning mechanisms employed by children to advance from initial state to final state are the same as those employed by adults. And just as it is possible that UG as given in the initial state in (2a.) atrophies in the adult, it is theoretically possible that the parser, say, loses its initial flexibility, becoming unable or less able to deal with e.g. constituent orders grossly different from those of the L1, or becoming unable or less able to provide its output as input to a grammar-forming faculty.

UG/SLA researchers have treated these four components variously, and to varying degrees of thoroughness; a comparatively enormous amount of research has gone into the question of the initial state of the L2 learner, while virtually nothing has been done on the L2 parser, and so on. As might be expected, there is still plenty of room for progress on all fronts.

ASSESSING SLA THEORIES: THREE CRITERIA

One convenient way to assess the UG/SLA approach and to determine its limitations is to measure it, and its competitors, against some basic criteria for judging the explanatory value of an acquisition theory.[6] Following Atkinson (1982), I will discuss three fairly self-evident criteria, which I will call the Theoretical Framework Condition, the Sequence Condition, and the Mechanism Condition.

(4) a. Theoretical Framework Condition: An SLA theory is explanatory only if it can account for each state of a learner's competence within the framework of a satisfactory property theory of language compe-

tence, as defined by that theory. Ideally, there should be only one type of property theory for all states.

b. Sequence Condition: An SLA theory is explanatory only if it can explain verified acquisition sequences, where 'acquisition sequence' is defined in terms of the property theory.

c. Mechanism Condition: An SLA theory is explanatory only if it includes a transition theory that has a mechanism or mechanisms to effect change of state in L2 competence.

These three criteria are necessary, but not necessarily sufficient, conditions. By 'SLA theory' I mean something fairly general, although the boundaries of the theory's domain cannot be stipulated in advance. For instance, it is not yet at all clear whether, or to what extent, pragmatic L2 knowledge could be included in the domain of an SLA theory. This is not to say that the acquisition of pragmatic knowledge is not of interest or in need of explanation; rather, I am simply suggesting that it may turn out that such an explanation will properly be the role not of an SLA theory, but instead of some other sort of learning theory that would then be applied in conjunction with an SLA theory to account for the acquisition of what in non-theoretical terms we call a second language. The conjunction of theories itself would not be of scientific interest, though, for it would not be dealing with a natural kind. This should not be a surprising outcome, given the distinction, in UG/SLA terms, between I-language and E-language (Chomsky, 1986).

THE THEORETICAL FRAMEWORK CONDITION

There are two motivations for positing this condition, one much more important than the other. The lesser reason, which relates to the second sentence of (4a), is for consistency; for essentially Occam's Razor reasons, we would like to avoid gross qualitative changes in the learner's grammar over time. If the learner is a 'tadpole', in Gleitman and Wanner's (1982) terminology, the explanatory burden on the theoretician is the greater. For instance, the pivot/open distinction of the 1960s, or claims such as Givón's (e.g. 1985) that the learner starts with a semantically organized grammar and then 'syntacticizes' it, as it were, require two separate types of property theory, as well as a way of getting from the one to the other. Thus all else being equal we would prefer to have one (developing) system to deal with rather than two. But of course all else is not necessarily equal. Like all appeals to Occam's Razor, this one is of only limited compellingness; after all, tadpoles do exist and they do turn into frogs. The real rebuttals of e.g. Givón turn not on arguments from simplicity but rather on arguments from the poverty of the stimulus, which indicate that a 'syntacticizing' account cannot yield the rich syntactic knowledge possessed by all humans.

What is vastly more important than explanatory parsimony, though, is that if we don't have a coherent property theory in terms of which the transition theory –the acquisition theory proper, as it were– can be expressed, we really don't have anything at all. The fundamental –and after all, commonsense– point was made some time ago by Wexler and Culicover: 'If a sufficiently precise theory of what is achieved does not

exist, we cannot evaluate (or even do a reasonable job of creating) a theory of the learning of the achievement' (1980: 596; cf. Gregg, 1989 for SLA).

The consequences of applying the Theoretical Framework Condition to current SLA theorizing are profound, and profoundly disturbing: If we accept that the goal of SLA theory is to explain the acquisition of linguistic competence, then VIRTUALLY ALL CURRENT SO-CALLED SLA 'THEORIES' ARE DOOMED TO FAILURE FROM THE START. The simple fact is that at the moment at least, as far as property theories of linguistic competence go, UG is the only game in town; and yet most SLA theoreticians decline to play the game. For a field that likes to use the term 'applied linguistics', it is truly astounding to see the degree to which linguistic analysis of any sort is shunned in the non-UG SLA literature.

There is a vast literature, for instance, devoted to 'input' studies in SLA: what kind of input learners get, how it's presented, what happens if it's modified in various ways, and so on. Since input is one of the components of our learnability model, this would seem to be a welcome development. The problem is that 'input' is transitive; you don't have just input *tout court*, any more than one can simply be a resident. Input is input to an input-output device; here, a learning system as instantiated in the mind/brain of a learner. Thus studying input in the absence of such a device is likely to be a sterile enterprise: manipulating, tabulating, and cross-classifying how learners are addressed in order to uncover the mechanisms of language acquisition is of about as much use as manipulating, tabulating, and cross-classifying how meals are served would be, in order to uncover the workings of their digestive system. It's not clear, therefore, why so much of this input research goes on, although it may be that Chomsky's explanation of the appeal of motherese studies in L1 acquisition research provides the answer: 'The reason people do it is that it is easy to do. You can do it if you know nothing' (Chomsky, 1984: 47).

In short, advocates of non-UG SLA theories must either show how their theories can interact with UG, or else must propose a property theory of something like the scope, detail, and internal consistency of UG. Failing that, there is simply little reason to take them seriously as contenders. This does not necessarily mean that e.g. input research is of no practical value; one could for instance turn up a good deal of empirical data on what kinds of input or input modification lead to higher comprehension, and such findings could be of major benefit in teaching L2 learners. And of course input research could turn up interesting new explananda for a theory to deal with. But such research in no way alleviates the need for a property theory, and is in no position to provide us with one.

It is, of course, also possible for a theoretician to deny the need for UG; we will still need a property theory, mind you, but not a modular one. About the only explicit challenge to modularity comes from connectionism, which denies the existence (the non-epiphenomenal existence, anyway) of a language module, or rules or principles, and for which the mind is essentially uniform. There has been some connectionist work in SLA recently (e.g. Schmidt, 1988; Gasser, 1990; Sokolik, 1990; Shirai, 1992; cf. Carroll and Meisel, 1990; Fantuzzi, 1992) but nowhere near enough to suggest a need to reassess the modularist claims of UG/SLA. And given the cogent critiques of connectionism offered by Fodor and Pylyshyn (1988) and by Pinker and his associates (Pinker and Prince, 1988; Marcus et al., 1995) there seems little reason to think that a connectionist property theory for SLA will be forthcoming. Especially troubling is the apparent inability of connectionist theories to explain the negative aspects

of linguistic knowledge; that is, how it is we know that such and such is impossible. This is, after all, the crux of the learnability problem: how do we come to have knowledge in the absence of evidence? Gasser (1990) is to my knowledge the only SLA connectionist even to have addressed this question, but he simply brushes it aside by claiming that whether or not we have such knowledge is an 'empirical question'. Which it is, of course; but one to which we already have an answer. But at least it should be said in favor of connectionism that connectionist theoreticians do offer a radically different model of the mind that, if successful, would meet the Theoretical Framework Condition; and they do make a serious attempt to shortcircuit the homunculus problem. For these two reasons alone connectionist SLA research deserves a good deal more credit than a lot of more traditional 'empirical' SLA work.

One other option open to the anti-UG theoretician is to finesse UG by appealing to underlying neural systems (Jacobs, 1988; Jacobs and Schumann, 1992; Pulvermueller and Schumann, 1993). This neurolinguistic approach, like connectionism, treats UG as in essence an epiphenomenon; unlike connectionism, it attempts to deal with linguistic knowledge at the physiological rather than the cognitive level. In either case the approach is reductionist in motivation, and in either case it fails for that very reason. In order to successfully reduce a theory –in this case a theory of UG– one must show that the terms of the theory to be reduced are translatable into terms of the reducing theory (Hempel, 1966; Fodor, 1981). Needless to say, no one has yet found a way to replace e.g. such terms as 'empty category', 'clause', 'subject', etc., with terms from neurobiology. But since linguistic theory can explain a good deal about linguistic knowledge by appealing to just such terms, there is absolutely no justification for abandoning them.

Of course we want to know what the neurological basis is for linguistic knowledge and language acquisition. But it by no means follows that, once we gain that knowledge (and we're not that close), we will then be able to forego the more abstract UG theory. The problem is one of finding the proper level of abstraction for best understanding the phenomena under consideration. Just as it is unenlightening to say that simplified input enables the learner to acquire Subjacency, it is similarly unenlightening to say that acquiring Subjacency consists in, say, the interaction of dopamine with morphine receptors.

To take an example adapted from Fodor (1981), any economic theory will have to account, *inter alia*, for such phenomena as payment. Would it make sense to reduce economics to physics? The phenomenon of payment is of course a physical phenomenon, or expressed in physical action anyway, but payment can be instantiated in any of many grossly different physical actions: handing objects from one person to another, writing symbols on a piece of paper and mailing it; punching computer keys, etc. Thus there is no way, at the level of physics, to arrive at any useful generalizations about economic phenomena. In such cases it is not a convenience or a makeshift for us to talk in more general, abstract terms of debt, payment, or whatever; it is *essential*, if we are to develop the basis for a successful economic theory (cf. Pylyshyn, 1984). And *mutatis mutandis* for SLA theory, or indeed any theory outside of physics. This is not to deny that UG, or any other kind of knowledge, must ultimately be instantiated in the brain; we are not reverting to some sort of dualism or platonic formalism. Thus research on the neurological basis of SLA is to be welcomed, but it should not be seen as a substitute for an abstract theory of the acquisition of L2 knowledge (Eubank and Gregg, 1995).

CRACKS IN THE THEORETICAL FRAMEWORK: THEISM AND DEISM

Thus the bottom line would seem to be that currently it is only UG/SLA theories that meet, or come close to meeting, the Theoretical Framework Condition. However, as is well known there is a major division in the UG/SLA camp, precisely on the question of whether UG is directly implicated in (adult) SLA, whether it is 'available' or 'operates' in SLA. White (1989) refers to the two positions as the UG hypothesis and the UG-is-dead hypothesis; Gregg (1994, 1996) refers respectively to theists and deists. The theist position is of course that UG is alive and well, and is responsible for acquisition; or at least, in our terms, that UG theory is appropriate for characterizing adult competence and for explaining the constraints on the forms of interlanguages, just as it is for characterizing the initial state of L1 acquisition and for explaining the constraints on L1 development. The deists hold that UG as such has disappeared, so that in so far as adult competence in the TL resembles the competence of adult native speakers of the TL, this is in effect an artifact of the learner's L1. Both positions have problems (see Gregg, 1996 for detailed discussion).

The obvious challenge to the theist is to explain the wide, if not universal, failure to achieve total L2 competence (Bley-Vroman, 1989). This challenge can, though, be met, at least theoretically: by appealing to interfering effects from non-linguistic factors such as affect, insufficient input, or most ingeniously perhaps, to failure not of UG itself but of the learning mechanisms involved in applying UG to acquisition (White, 1989). (We will come back to this problem in discussing the Mechanism Condition.)

More problematical, however, is the question of just what it means to say that UG still lives. After all, as Kean (1988) forcefully points out, if we are to take seriously the claim that UG is actually instantiated in the brain –and despite my strictures above against reductionist SLA neurobiology, I agree that we are– then we must face the fact that the brain of an adult is quite different from that of an infant, and that UG should 'be taken as a characterization of an *emergent* property of the nervous system' (1988: 62; my emphasis). Proposals for a maturational view of UG (e.g. Borer and Wexler, 1987) can be seen as offering a possible way of avoiding a neurologically implausible concept of UG. Of course in so far as we are restricting ourselves to adult SLA, we must assume that the maturational processes, whatever they may be, have finished, and that all elements of UG have already kicked in. This places all the more of an explanatory burden on the theists to account for failure to acquire, and in fact holds them to a stronger constraint in one respect than L1 acquisition theorists: Where a maturational view of UG allows for the (limited) possibility of 'wild' child grammars (Goodluck, 1986; Wexler, 1990), SLA theists are committed to the non-existence of any IL grammar that violates UG.

However, while 'wild' IL grammars would be major counterevidence against theism, mere conformity across L2 learners to the constraints of UG is not a major threat to the deist position. Deists can argue (and have; e.g. Bley-Vroman, 1989) that the source of L2 knowledge is not UG itself but rather the UG-constrained L1 grammar; if this is so it would be unsurprising that IL grammars are also in conformity to UG, if they are. For that matter, an L2 learner could in theory simply impose his L1 parameter settings on the L2 input in total disregard of their appropriateness or otherwise, and the mess that resulted could still be in conformity to UG. Such an IL grammar would hardly be cogent evidence that UG is at work in SLA. The deist claim is that in

those cases where the L1 grammar does not instantiate a UG principle instantiated in the L2, that principle cannot be acquired, and that where the L1 grammar cannot be relied on for inducing parameter settings appropriate to the L2 –roughly, where the two languages differ in the settings for the relevant parameter– the learner will not be able to 'reset' the L2 parameter correctly.

On the other hand, clear evidence for truly 'wild' IL grammars –as opposed to grammars that conform to UG but don't necessarily conform to the specific exponents of UG expressed in the L2– would be something of a mixed blessing for deists, too. After all, it is not enough to show, as wild IL grammars would show, that UG is not 'operating' in SLA. The fact remains that the learner has some sort of grammar, which is to say some sort of L2 'knowledge', even if it's incorrect knowledge; and we still need some sort of property theory to account for this knowledge. If neither UG-*an-sich* nor UG-as-exemplified-in-the-L1 will fill the bill, what will? The heretofore most articulated deist proposal, Bley-Vroman's Fundamental Difference Hypothesis (1989), is not too specific on this point, merely positing the L1 grammar as the sole linguistic source of specifically linguistic knowledge, and predicting gaps in the learner's knowledge of the L2 as a consequence. One possible corollary of this position is that a wild IL grammar would be a 'kludge', an *ad hoc* system cobbled together partly from UG sources and partly from non-modular knowledge. This would be tantamount to saying that IL grammars are not natural kinds; the status of L2 competence as an object of scientific inquiry would then be called into question. If this is the case, then deists as much as theists should be anxious to avoid wild ILs.

I will postpone further discussion of theist/deist disagreements until we get to the Mechanism Condition. Here we can simply note that theists and deists essentially agree that UG theory provides the appropriate framework for describing the linguistic competence of an L2 learner, both in his initial state and his terminal state, while disagreeing as to the precise nature of those states.[7]

THE SEQUENCE CONDITION

Acquisition takes place over a fairly extended period of time, so that we do not have just an initial state and a terminal state, but any number of intermediate states, each presumably describable in terms of our property theory. (Whether or not these intermediate states can best be described as distinct stages is another matter.)[8] The Sequence Condition requires that we can explain why the sequence of states is as it is rather than some other order.

Historically, of course, acquisition sequences have played an important role in SLA studies; probably too important in retrospect at least. The period of 'morpheme addiction', for instance, when it seemed you couldn´t take two steps without tripping over another morpheme acquisition study, was characterized not only by the sort of mindless enthusiasm one finds today in the input modification and discourse studies, but also (and this is the source of the mindlessness) by a failure to interface with any usable property theory. This meant that not only were the morpheme acquisition studies methodologically flawed (Long and Sato, 1984); they were uninterpretable (Gregg, 1984). The Sequence Condition, after all, does not commit the theoretician to explain

every acquisition sequence –still less every (or any) production sequence– but rather every theoretically relevant sequence.

Consider, for instance, the three kinds of possible explanation for a given sequence that Atkinson (1982), following Flavell (1972), sanctions: An environmental explanation is one where e.g. relative salience or frequency in the input determines order. A reductive explanation would explain the order as simply a special case of some other, 'deeper' order, e.g. an order of concept acquisition –for instance the acquisition of concrete nouns before abstract nouns could be explained reductively in this sense. 'Teleological' explanations claim that in effect there is no other order logically possible. Thus, to use Atkinson´s example, if there were an order where at stage 1 there are PS-rules but no transformations, and at stage 2 there are both, this could be explained teleologically since transformations operate on phrase-structures. Or if there was an order of parameter settings where Adjacency was set after Configurationality (White, 1989), this would be for the simple reason that non-configurational languages make no use of the Adjacency parameter.

For Atkinson these three explanation types are all equally valid; which is true if one is only interested in explaining such and such a given acquisition sequence. But as far as SLA theory as a whole is concerned, we are often not going to be interested in certain sequences, no matter how well-verified they may be. Thus in so far as e.g. frequency of items in the input determines the acquisition order of those items (Larsen-Freeman, 1976), the order is an artifact, and therefore in no need of explanation by a theory of SLA. Indeed in this respect the whole history of the morpheme studies should be a cautionary tale for the field.

The old morpheme studies can be profitably compared with the work of Clahsen, Pienemann and others, for instance, on acquisition orders for word-order rules in German (Clahsen, 1984; Clahsen and Muysken, 1986; Pienemann, 1984). Clahsen et al. propose a set of processing strategies, or perhaps filters would be more accurate, and suggest that the production stages reflect a progressive dropping of the filters. Whether these proposals provide a satisfactory explanation is of course another matter, as is whether they implicate UG as such (see e.g. White, 1991a; Bley-Vroman, 1991 for discussion); but at least they are made within some sort of theoretical framework. With regard to the English grammatical morphemes, it is not until Eubank (1994), basing herself on work by Wexler (1992), that we have theoretically relevant proposals to account for the appearance of English third person–*s* in the sequence. That is, her proposals, whether right or wrong, connect directly with the property theory.

The Sequence Condition, in short, is parasitic on the Theoretical Framework Condition; the theoretician is obliged to explain a sequence only if the sequence is interpretable within the theoretical framework.

In any case, when discussing the Sequence Condition it is important to keep in mind that explanations meeting this condition will be contrastive; they answer questions of the form, 'Why P rather than Q?', rather than of the non-contrastive form, 'Why P?'. There's nothing in particular either better or easier about one type or the other, but they do require different types of explanation. To use a standard example (cf. Lipton, 1991), given two tertiary syphilitics, Smith and Jones, we can explain why Jones has paresis– because he has untreated tertiary syphilis; and we can perhaps explain why Smith doesn't –the odds are 3 in 4 against getting it. But we can't explain why Jones rather than Smith has paresis. That is, we have an explanation of the cause, but not

of the contrast. Conversely, we can often explain the contrast but not the cause: it's easy enough to explain why Max swallowed poison before jumping off the Golden Gate Bridge rather than the reverse, but we still don't know why Max killed himself.

By the same token, we may have a readily available contrastive explanation for a theoretically interesting sequence without thereby having a satisfactory non-contrastive explanation. Indeed, the contrastive explanation may be all too readily available. Why did Greta's IL setting of Parameter X change from [-] to [+] rather than from [+] to [-]? Because her L1 is [-] and the L2 is [+]. Why did it change from [-] to [+] rather than not changing? Because the L2 input reflects the [+] setting. Why did it remain at [-] rather than changing to [+]? Because UG is dead. What none of these explanations do is explain how the resetting took place, or what would have caused it to take place if it had. We can't appeal to UG here, because UG sanctions both [+] and [-] values. Nor is it enough to attribute the sequence to UG plus input; this simply identifies the (partial) cause, without actually explaining the effect.

In other words, the Sequence Condition is parasitic not only on the Theoretical Framework Condition, but also on the Mechanism Condition; which suggests that if we can meet those two, we should not have too much trouble with the third.

THE MECHANISM CONDITION

As a property theory, UG can only establish constraints on development. The explananda of a UG theory include such things as e.g. the grammaticality, ungrammaticality, ambiguity, possible and impossible interpretations, scope, etc. of a given sentence, or (which is the same thing) a native speaker's knowledge of the grammaticality, ungrammaticality, etc. They do not include the acquisition of this linguistic knowledge; this is the domain of an acquisition theory in the wider sense, that is, the property theory plus the transition theory. The Mechanism Condition requires that we have some sort of learning mechanism or mechanisms to interact with UG (or, in a deist SLA theory, with the L1 grammar which itself is a product of the interaction of UG and learning mechanisms).

Ironically enough, the SLA literature is filled with causal theories; in fact most of what passes for SLA theorizing is causal in intent. The problem, as we have seen, is that they fail to meet the Theoretical Framework Condition. That is, they don't interface in any useful way with what I have claimed is the unique property theory in our domain, UG. The plausibility of such theoretical explanations as have so far been offered depends largely on taking a sufficiently broad and superficial view of either the initial or terminal state or both. It *may*, for instance, be possible to appeal to acculturation as a cause of SLA, if we define L2 competence in the broadest, vaguest terms like 'proficiency'. So we can say, for example, that overall proficiency is a function of acculturation to the culture of the speakers of the target language. And this may even be true, for all I know. True or not, it isn't satisfactory. We certainly can't appeal to acculturation to explain e.g. parameter setting –we couldn't even if there were a perfect correlation between degree of acculturation and success in resetting parameters.

And note that even to the extent that such non-UG causal theories can be seen as explanatory, they provide at best contrastive explanations, not causal ones. It might

be plausible to assume that, *ceteris paribus*, the more highly motivated a Korean learner of English is, the greater the chance of his acquiring Subjacency, thus explaining why Kim but not Paik can handle Subjacency in English. But this is a far cry from saying that the motivation was the cause of the acquisition. And indeed there is not the slightest reason to posit a direct causal relation between Subjacency and motivation or acculturation.

In other words, if we are to postulate language acquisition mechanisms, they are going to have to be more specifically linguistic mechanisms. Given the learnability framework in which UG/SLA research is carried out, we need to look at the role of input on the one hand, and at the learning principles and/or mechanisms that have been proposed for L2 learning on the other. Proposals as to the causal role of input center on two questions: negative evidence and input modification. Learning mechanisms can be categorized as either general (non-modular) or language-specific (modular).

Input: Modified

There is an enormous literature on modified input and its putative role in SLA (see e.g. Long, 1983; Gass and Madden, 1985; Day, 1986), whether on 'foreigner-talk' (Ferguson, 1975) or other forms of simplification, or on the role of discourse in SLA (e.g. Hatch, 1978, 1983a; Larsen-Freeman, 1980). Virtually all of this research can be dismissed very quickly as far as any explanatory value goes. There has been a great deal of heavy weather made about 'comprehensible input'; but comprehensible input in SLA theory is like edible input in a theory of digestion –a given.[9] The problem is to get beyond such platitudes to see how comprehensible input works. It's perfectly legitimate and useful, of course, to investigate how input is made more or less comprehensible –e.g. is it better to slow the syllable-per-minute rate, or to pause between intonation units? But this is all that the input modification research, at its best, has done, and the reason is of course not hard to find: results are almost always measured in overall 'proficiency', which does not help us discover how given linguistic knowledge (as described in a property theory) is acquired. The concern with overall proficiency, like the claims of communicative competence, reflects an understandable interest of applied linguists and language teachers with everyday problems of real-world L2 learners; but for that very reason the concept is as useless as the concept of communicative competence is in the scientific study of SLA (Newmeyer and Weinberger, 1988; Gregg, 1993a).

The discourse literature lacks even this utility, and again the reason is clear. Aside from a lot of handwaving about 'form-function relationships', there's no theory of linguistic knowledge to relate to whatever discourse 'function' is being investigated; there's not even much detail about what forms go with what putative functions, for that matter. As Hatch once put it (and the admission is still valid today), 'We have yet to work out the middle ground between low-level data and acquisition theory' (Hatch, 1983b: 83). I can think of no criticism more devastating than this.

What has yet to be done in SLA is to show any interesting –that is, theoretically relevant– relation between some specific type of input modification on the one hand, and some specific bit of acquisition on the other. That in itself, of course, wouldn't meet the Mechanism Condition; but it would at least provide an explanandum for a theoretical learning mechanism to explain. At the moment, input modification research offers little promise of leading to any such explanandum, let alone any explanation.

INPUT: NEGATIVE

Given a system (that is, a learner) at stage i (e.g. thinking that whales are fish, or thinking that *teached* is the past tense of *teach*), and the same system at stage i + 1 (thinking that whales are mammals, thinking that the past tense form is *taught*), one obvious way to explain the change of state is to hypothesize that someone told the learner about whales, or about *taught*. This is negative evidence, and it seems reasonable enough to imagine that it plays some sort of role in at least some aspects of second language acquisition.

There are various ways to categorize negative evidence in language acquisition (see Birdsong, 1989 for a thoroughgoing discussion), but perhaps the most useful way to distinguish negative from positive evidence is along the traditional use/mention axis: Positive evidence (PLD) is language used, that is, utterances in context; negative evidence is language mentioned. This categorization entails that negative evidence includes not just correction of learner errors, but also instruction and metalinguistic explanation –what Schwartz and Gubala-Ryzak (1992) refer to as explicit positive evidence.[10] Thus just as one can think whales are fish and be disabused, one can also not know a thing about whales and be informed. And one can either think that *jejune* means puerile and be told that it means sterile, or one can have not a clue as to what *jejune* means and be told. In either case *jejune* is no more being used than whales are.

In L1 acquisition we know that negative evidence is unnecessary; we know that because (a) it's not provided and (b) all children do just fine nonetheless. But in SLA not all adults do fine, and one can even conceive of the claim that it's only those adults who receive negative evidence that do do fine. Even without going that far, though, the question of the role of negative evidence in SLA cannot be dismissed as it can in L1 acquisition.

Once again, we have to distinguish between theoretically relevant and theoretically irrelevant negative evidence. It may be that one can learn that *gokiburi* means 'cockroach' by being told explicitly, just as effectively as by hearing the word used in appropriate contexts (not to mention less traumatically). It may even be the case that certain aspects of big-L Language, say honorifics or politeness forms, will *need* to be explicitly taught to some extent, at least in an SLA context. (To take a very trivial example, it took me ten or so years to discover that one of the standard greetings in Japanese, *konniti wa*, isn't used within the family. I'm not sure how I would have found out if someone hadn't told me.) And in any case, of course, explicit instruction and correction in such areas may well be useful even when unnecessary, in terms of efficient use of learning time, for instance.

But the theoretically interesting question is whether negative evidence is or is not necessary in SLA in those domains defined as central by our property theory; in other words, does negative evidence have a role to play in the construction of a UG-based IL grammar?[11] For instance, is negative evidence either necessary or effective in getting the learner to reset parameters to the L2 values? This question is probably most germane in cases where PLD by itself would seem insufficient to trigger resetting, as when the L1 (or IL) grammar sanctions a wider range of possibilities than does the L2 grammar (see the discussion of the Subset Principle in D.2 below). Of course, no one thinks that one should or could explicitly give useful instruction in UG itself. The question is rather, if e.g. you tell a learner that 'John ate quickly dinner' is

ill-formed, or that in general English adverbs cannot be inserted between verb and direct object –information not immediately available from positive evidence– will that information lead to resetting the relevant parameter (see White, 1991b)? That is the empirical question; the theoretical question is, what effect will settling this empirical question have on the theist/deist dispute?

The deist position is fairly clear: the learner should often find himself in situations where negative evidence would be necessary or useful if it were available. Since there will be cases where such evidence is not available, no learner is likely to reach native-level competence even with the aid of negative evidence. And of course there's no guarantee that negative evidence when available and provided will indeed be used. What would be a major challenge to the deist position is a case of successful acquisition, in such circumstances, in the absence of any negative evidence.

The theist position is more nuanced. While some theists, notably White (1989), are willing to accept the need for negative evidence in certain situations (specifically, when the Subset Condition is met; see below), others, e.g. Schwartz (Schwartz and Gubala-Ryzak, 1992), are committed to the claim that negative evidence is useless. Schwartz's position derives from applying Fodor's (1983) concept of modularity to language acquisition (Schwartz, 1986). Specifically, given that modular input systems are cognitively impenetrable, Schwartz reasons that the acquisition mechanism should in principle be impervious to the sort of consciously acquired, metalinguistic knowledge that negative evidence provides. This position has the advantage of being in clearer contradistinction to the deist stand than a theist position such as White's. It is also interesting in that it is not that easily falsifiable (but not unfalsifiable). For instance, a French speaker learning English could be told about the position of adverbs in English, and could even successfully apply this knowledge so that he never makes a single error in production, and also correctly judges sentences of the type 'John drank quickly his coffee' to be ungrammatical; and yet that learner's IL grammar may still be 'wrong'. That is, the learner, while having failed to make use of the negative evidence to reset the Verb Movement parameter to the English value, could theoretically use the knowledge gained from negative evidence to make local repairs as needed for production or judgement (Schwartz and Gubala-Ryzak, 1992). Whereas behavioral evidence of failure to reset the parameter (with or without negative evidence) is at least suggestivecounterevidence to a strong theist claim of the sufficiency of PLD (and whereas behavioral evidence of successful resetting in the absence of negative evidence is even stronger counterevidence against the deists), behavioral evidence of resetting after receiving negative evidence is much less compelling.

Once again we see the importance of the Theoretical Framework Condition: It is simply not enough to investigate whether or not 'instruction helps'. It is not even enough to investigate whether instruction with reference to one aspect of a given parameter as defined by the property theory helps to alter the linguistic behavior of learners with respect to that aspect. The question is whether negative evidence can lead, e.g. to parameter resetting in the IL grammar; and since on most accounts parameter settings entail a 'cluster' of properties, it is necessary to see whether L2 learners acquire the set of properties upon receiving negative evidence about one member of the set. Sufficiently specific research on this question is still quite limited (see e.g. White, 1991b).

LEARNING MECHANISMS: GENERAL

By general learning mechanisms I have in mind e.g. such mechanisms as are assumed to apply equipotentially to linguistic and non-linguistic learning situations. For example, McLaughlin (1987, 1990) talks of 'restructuring', where information acquired piecemeal is later reorganized and integrated into a system. Restructuring is intended as a general process in that it can apply in any cognitive domain; just as the English past tense marker-*ed* is first learned verb by verb and then later converted to a past-marking rule (with the concomitant 'U-shaped behavior'), so e.g. is mathematical knowledge restructured when a child goes from adding 7 ten times to multiplying 10 x 7 (McLaughlin, 1987). In the SLA literature the term 'strategy' is often used (e.g. O'Malley and Chamot, 1990), although there is not necessarily any claim of consciousness being made here. O'Malley and Chamot (p. 46) include among their list such strategies as inferencing ('using information in text to guess meanings of new linguistic items'), rehearsal ('repeating the names of items ... to be remembered'), deducing ('applying rules to the understanding of language'), and imagery ('using visual images ... to understand and remember new verbal information').

This is rather a mixed bag, but one thing is fairly clear already: Whether or not such 'strategies' operate in SLA, and it wouldn't be surprising if they all did, they either will not satisfy the Mechanism Condition or else they will not satisfy the Theoretical Framework Condition. Restructuring, automatization, etc. no doubt occur, but they are processes not mechanisms; to say that *-ed* is restructured as a rule is to describe the acquisition of *-ed*, not to explain it. On the other hand, inferencing, deducing, etc. can be seen as mechanisms all right, but no attempt has yet been made in the SLA literature to have these mechanisms operate on input that can be characterized within the property theory (= UG). One problem with the strategy literature, aside from its ignoring linguistic theory, is that it is concerned with comprehension and not with grammar construction. It is perfectly possible that such mechanisms or strategies or what have you exist and are used by L2 learners, nor do UG/SLA theorists have to deny this possibility. They do, however, have to deny the *sufficiency* of such mechanisms; but, given the inadequate theoretical framework within which general learning mechanisms are discussed in the SLA literature, such a denial is easy, natural, and indeed almost obligatory.

More explicit proposals for learning mechanisms that operate as well outside the language domain come from O'Grady (1987) on the one hand and the connectionists on the other. O'Grady's position is explicitly reductionist; he claims that 'descriptively adequate grammars can be constructed from concepts and relations that are not specific to the language faculty' (p. 1). It follows that grammar acquisition should not require faculty-specific mechanisms, and indeed O'Grady proposes general learning principles such as the Conservatism Thesis ('children make use of the available concepts to formulate the most conservative hypothesis consistent with experience' (p. 187)) to account for language acquisition. In SLA research, there has to date been only one attempt to apply O'Grady's ideas, that by Wolfe Quintero (1992); which is perhaps a bit surprising, given the amount of resistance in the field to the UG/SLA position. One possible explanation, I'm afraid, is that O'Grady takes theTheoretical Framework Condition as seriously as anyone within the UG/SLA framework, which is to say that he doesn't offer any easy escape from the demands of theoretical relevance.

We have already looked briefly at connectionism in discussing the Theoretical Framework Condition. Connectionism offers the extreme of simplicity in terms of learning mechanisms, since in effect there is only one mechanism: spreading activation, which either strengthens or inhibits the connections between nodes. As with O'Grady, the challenge facing connectionists is to reduce the property theory to a non-autonomous, general domain; if linguistic competence contains irreducible domain-specific rules, then connectionist theories cannot be maintained. Where O'Grady recognizes the complexity of the task, however, connectionist work on language has tended to vastly underestimate the knowledge to be accounted for. And as a consequence, as Pinker and his associates have shown (Pinker and Prince, 1988; Prasada and Pinker, 1993; Marcus et al., 1995), connectionist proposals for the acquisition of even comparatively low-level features of language, such as past tense endings on English verbs, have so far failed.

LEARNING MECHANISMS: LANGUAGE-SPECIFIC

Language-specific learning mechanisms can, somewhat arbitrarily, be categorized as modular or non-modular; that is, as to whether they are intended to operate only within a specified language faculty or module as such (e.g. the Subset Principle as formulated for parameter-setting), or whether they are simply not intended to deal with non-linguistic learning.

Operating Principles

Probably the best-known non-modular learning mechanisms are the Operating Principles (OPs) proposed by Slobin (1973, 1985). (In SLA research, Andersen (e.g. 1989) has made extensive use of Slobin's work, while extending and revising it in an attempt to determine the OPs for SLA.) Actually, these principles started off as closer to general learning mechanisms, as indicated by the title of Slobin (1973): "Cognitive prerequisites for the development of grammar". And indeed it is significant that as over the years Slobin has revised the OPs, he has changed the emphasis 'from general cognitive prerequisites to those that seem more adapted to the task of language acquisition in particular' (1985: 1243). Thus I have opted to include them here rather than in the previous section.

In any case the OPs are intended as in effect instructions to the learner as to the perception, storage, and organization of input, as well as for the production of speech. Some examples:

> Pay attention to stressed syllables in extracted speech units.
> Keep track of the frequency of occurrence of every unit and pattern that you store.
> Store together ordered sequences of word classes and functor classes that cooccur in the expression of a particular proposition type, along with a designation of the proposition type.
> Keep the order of morphemes in a word constant across the various environments in which that word can occur. (Slobin 1985: 1251-1254)

There are 40 or so of these, which is itself a problem, especially as there seems to be no principled way to limit this number; just as Slobin himself has gone from 7 OPs to 40, there is no evident way to stop someone else from going on to 80. But more disturbingly perhaps, they do not seem to form any sort of natural class. 'Pay attention to stressed syllables' and 'keep track of frequency' sound like quite different mental operations, for instance. And the former hardly seems to make sense as an instruction or strategy; the learner can't help noticing stressed syllables. In fact, as Pinker says, 'OPs are almost certainly not implemented in the child's mind' (Pinker, 1989a: 458; cf. White 1991a). Applying the philosopher's distinction between obeying and following a rule (the planets obey but do not follow Kepler's laws), Pinker claims that children only obey OPs, but do not follow them.

Indeed, it's not clear how a well-intended learner *could* follow the OPs successfully, or what would result. Given three OPs that require paying attention to, respectively, the first, last, and stressed syllables in an 'extracted speech unit', one would pay attention to the whole of 'banana', but not to the third syllable of 'banana boat'. Conversely, there are two separate OPs enjoining attention to the first syllable of 'horsefeathers'.

What is true of Slobin's work is also true of Andersen's in SLA. Although Andersen makes use of fewer principles than Slobin does (Andersen, 1989 has 12, for instance), they too fail to form a coherent set. Principle 7, for instance ('Formal Determinism'), states that 'when the form:meaning relationship is clearly and uniformly encoded in the input' (p. 52), the learner will discover this relationship earlier than for less clearly related pairs; which may be true, but certainly isn't a learning principle. Principle 12, on the other hand, sounds very much like something an L2 learner would follow: 'When you cannot perceive the structural pattern used by the language you are trying to acquire, use your native language structure with lexical items from the second language' (p. 56). The problem, of course, is that it isn't a principle of acquisition, but of production.

All in all, the operating principles proposed in first or second language acquisition do not go very far toward satisfying the requirement that SLA theory include mechanisms for using input to effect changes in the learner's grammar. At best they are, like the general learning mechanisms, descriptive of processes that learners likely undergo; but what we want to know is the mechanism that makes the process proceed. Once again, we have a case of description masking itself as explanation. In so far, of course, as the description is accurate, well and good; and, as with general learning mechanisms, there is no need for UG/SLA theorists to deny the existence of such processes. What they must deny, though, is their sufficiency as acquisition mechanisms; and so far there is no reason for anyone to hesitate to deny this.

Modular Mechanisms: The Subset Principle and the Uniqueness Principle

We come at last to two proposals that have been made within the UG framework (although not exclusively so) for learning mechanisms, or at least learning principles, that operate on linguistic input to guide the construction of a grammar: the Subset Principle and the Uniqueness Principle. Both of these have been discussed to some extent in the SLA literature, although there is so far very little empirical research on either.

The Subset Principle

The Subset Principle was proposed by Berwick (1985; cf. Wexler and Manzini, 1987) in response to a problem first made explicit by Baker (1979): given the absence (in L1 acquisition) of negative evidence, what is to prevent a learner from overgeneralizing on the basis of positive linguistic data to rules or structures that are in fact not licensed by the TL? After all, once a child made an overgeneralization, it would seem to require negative evidence to 'retreat' to the appropriate rule or structure. Baker suggests that the child must be a conservative hypothesizer, and Berwick formulated this suggestion as a principle: 'the learner should hypothesize languages in such a way that positive evidence can refute an incorrect guess' (Berwick, 1985: 37). What this means is that, given two grammars G_1 and G_2, where the set of sentences allowed by G_1, is a subset of the set of sentences allowed by G_2 –where, in other words, G_1 and G_2 together meet the Subset Condition– the learner will start with G_1, and only change to G_2 if there is positive evidence justifying such a change. So for instance the possibility of Dative Movement with verbs like *give* should not mislead the learner into generalizing a Dative Movement rule across all verbs.

Of course if G_1 and G_2 are grammars of the L1 and L2, then the subset problem can apply in SLA also. For instance, where French allows only piedpiping, English allows both piedpiping and preposition stranding; thus French in this respect stands in a subset/superset relationship with English. One might predict that French learners of English could acquire stranding from positive evidence, while English learners of French would either need negative evidence to learn that stranding is not allowed, or else the Subset Principle to keep them from the temptation to strand prepositions (see White, 1989, ch. 6 for detailed discussion of the Subset Principle in SLA).

The Subset Principle is certainly attractive, and given 'Baker's Paradox' it would seem that some sort of conservatism principie would be necessary. Still, there are a number of questions that need to be dealt with. First of all, it is not always clear what the scope of the Subset Principle is intended to be. For instance, does it cover syntactic phenomena only, or linguistic phenomena in general? Berwick, as we have seen, talks about 'language' in general, but Wexler and Manzini seem to be limiting it to parameter setting, where different values of the same parameter would be in mutual subset-superset relationships. Either way there is a problem.

If the Subset Principle is supposed to be a general (language-specific) principle of conservatism, it should be able to account for the kinds of possible overgeneralization problems that Baker first mentioned, such as the dative alternation in English. The problem is that in fact children do make precisely such overgeneralizations in their L1 (Mazurkewich and White, 1984; Bowerman, 1987), and then somehow overcome them. So in fact it would appear that children are not necessarily conservative in their hypotheses; and if children aren't, there's all the less reason to think that adult L2 learners will be.

This suggests that we will need to define the kinds of distinctions between grammars that the principle is intended to deal with, to exclude cases where there is evidence for (harmless) overgeneralization. The problem then becomes one of finding a principled way to distinguish between rules or structures that come within the purview of the Subset Principle and those that don't. (And of course we still have those areas that fall outside to account for.) For instance, Inaba (1992) points out that Japanese and English conditional sentences can be treated as subset and superset, respec-

tively, in that English allows but Japanese forbids sentences of the form, 'If I go to New York, I'll go by plane' (*'Nyuu yooku e ikeba, hikooki de iku'). In Japanese, the action expressed in the apodosis must be later than that expressed in the protasis. Again, in Japanese, given numerous examples of the paradigm *yomu/yomiyasui/yomeru*, (read/easy to read/can read), etc. how is one to learn *wakaru/wakariyasui/ *wakareru* (understand/easy to understand/can understand)? There is no doubt a good reason why *wakaru* doesn't take the potential form, but it's going to be a very different reason from the time sequence constraint on Japanese conditionals. There are vastly many ways to categorize linguistic data, and an equal number of ways in which to be conservative; it's hard to see how a general command, Be Conservative, can be followed, without a great deal of very specific information about how to be (Pinker, 1989b). (Of course the problem is even more severe for a general learning principle like O'Grady's Conservatism Thesis)

Thus it might seem better to restrict the operation of the Subset Principle to parameter setting only. Of course this leaves all the other kinds of possible overgeneralization still in need of explanation, but at least we might hope for a principled account of the SLA process that would also meet the Theoretical Framework and Sequence Criteria. So for instance, since [+prodrop] languages, like Spanish, license both overt and null subjects while [-prodrop] languages, like English, require overt subjects, resetting from [-] to [+] should be easier than vice versa, unless the Subset Principle operates in SLA.

But even here we have difficulties. How, for instance, does the learner determine whether a subset/superset situation obtains, and how does he determine which of two grammars is which? As White (1989) points out, either each parameter will need to be 'wired in' such that the learner automatically starts with the setting that would generate the smallest subset of sentences, or else the learner will have to have the ability to compute those values from input. In the former case, though, there is no Subset Principle as such, but rather a set of orders of parameter settings. Furthermore, there are difficulties with the idea of prewired default settings for parameters (Hermon, 1990; Valian, 1990).[12] In the latter case, as with a more general Subset Principle, it is hard to accept the kind of computational power required to decide on subset relationships. Even in such a seemingly simple case like prodrop, the facts are more complex; not only are there imperatives in English, but sentences with expletive subjects in English have no counterpart in Spanish. Which means that Spanish sometimes forbids overt subjects, and thus that Spanish is not a true superset of English. Similarly, not only are there cases where only the NP PP dative is allowed and cases where both NP PP and NP NP are allowed, there are also cases where only NP NP is allowed ('Coffee gives me a headache'). Thus NP NP datives are not a subset of NP PP datives.

The bottom line would seem to be that the Subset Principle cannot do the work it has been called upon to do as a learning mechanism. Certainly the subset *problem* exists, and children overcome it in their L1 while adults often fail to overcome it in an L2.[13] But the Subset Principle seems in fact to be no more than a restatement of the subset problem; as Fodor and Crain put it, it is 'a guiding principle for linguists to follow in devising theories. lt is not, presumably, a principle that a learner can abide by in constructing his grammar ... ' (1987: 54).

This means that one must exercise caution in interpreting the research on the Subset Principle in SLA. The evidence is clear that L2 learners overgeneralize, but

that is not the same as saying that they violate the Subset Principle, if in fact there is no Subset Principle. But if there is no Subset Principle, White's (1989) proposal that UG survives in SLA while the learning mechanisms atrophy is called into question.

The Uniqueness Principle

This leaves us with the Uniqueness Principle. This has been offered in various forms, both within and without generative linguistics:

Every two forms contrast in meaning. (the Principle of Contrast; Clark, 1987: 2)

[I]n the unmarked case, every deep form has a single surface structure. (Roeper, 1981: 141; citing Wexler)

[W]hen [a child learner] is faced with a set of alternative structures fulfilling the same function, only one of the structures is correct unless there is direct evidence that more than one is necessary. (Pinker, 1984: 113)

An interlanguage system should be constructed in such a way that an intended underlying meaning is expressed with one clear invariant surface form (or construction). (The One to One Principle for SLA; Andersen, 1984: 79)

However phrased, the Uniqueness Principle has two basic motivations, or functions: what we can call preemption and distinction.

Preemption: the learner often produces illegitimate forms, whether as a result of overgeneralization or, in the case of an L2 learner, of the inappropriate application ('transfer') of an L1 rule or structure. However produced, they must be purged from the grammar. The idea here is that hearing e.g. 'ate' (frequently enough, etc.) will cause the child to drop *eated* in favor of *ate* as the past tense of *eat*. Without some sort of Uniqueness Principle, the danger would be that the child would simply add *ate* to his grammar as an alternative form, rather than as the unique correct form. Of course a simple Uniqueness Principle could lead to the purging of the wrong form, which is why Pinker adds the proviso that the learner must be able to distinguish between self-generated forms and forms heard in the input, and must purge the self-generated form (Pinker, 1989b).

Distinction: The other function served by a Uniqueness Principle is the obverse of the preemption function; the appearance of two different forms in the input, with superficially the same function, should lead the learner to try to distinguish between the two, rather than take them as pure synonyms.

As with the Subset Principle, the motivation for the Uniqueness Principle is strong, but there are a number of problems. One problem, at least with such formulations as Clark's, is that they talk about meaning; and on most accounts of meaning there are quite a few synonymous structures in a language: the two English datives, for instance, or piedpiping and stranding, or sentences with or without the complementizer deleted, don't vary in meaning. On the face of it, it would seem that the Principle of Contrast is, strictly speaking, false. Either that or we have to include nuance, register, style, etc. as aspects of meaning, a step to be avoided if possible. (Of course it's possible that the Principle of Contrast is false as a characterization of languages, but still necessary as a learning principle, even if in fact the learner has to violate it time and again.)

As with the Subset Principle, there is also the question of the scope of the Uniqueness Principle. The principle seems to work well with such *loci classici* as the purging

of overregularized past tense forms in English; but can it be applied to syntax? One problem is determining what the same 'function' would be for two different syntactic structures, if we use Pinker's definition, or what the 'meaning' of a syntactic form is, if we use Clark's or Andersen's. Which suggests that those three at least are thinking mainly of lexical acquisition.

Perhaps more pressing a problem, though, is the question of just how the Uniqueness Principle is supposed to work. In order to purge the incorrect form, the learner must both recognize that it has not appeared in the input, or rather that it is a self-generated form, as Pinker (1986) points out, and also interpret both it and its correct counterpart as being in fact true counterparts. Granting that the learner can mark self-generated forms as such –and Pinker seems right to claim that such a capacity is simply necessary, however implausible it may seem to some– the question remains how the Uniqueness Principle can guarantee that the learner doesn't assign a false distinction to the pair.

For instance, a learner whose L1 has more than one morphological past tense –say a preterite and an imperfect– could theoretically assign *goed* to one and *went* to the other. After all, even though *goed* hasn't appeared in the input, there's still very powerful evidence in the input for its existence in the lexicon, viz. the thousands of tokens of regular verbs that have justified the learner's use of the correct past tense rule in his IL grammar.

Conversely, in the case of two forms in the input, rather than one in the input and one self-generated, the learning problem becomes one of deciding whether or not they violate the Uniqueness Principle. The learner presumably will not purge either form from his grammar, since both have appeared in the input, but he still needs to determine whether they do indeed differ from each other (not all pairs do), and if so, how. The problem is that even when there is a difference, there still may be plenty of overlap between the two forms; think for instance of *will* and *be going to*. And indeed, it's perfectly possible that for a given learner at a given point in time every single instance of either form heretofore heard has been a case where the other form could have been used with no change in meaning. So the learner has to decide whether he's facing a *will/be going to* case or a *someone/somebody* case, whether to push on looking for a distinction or give up and accept the pair as synonymous (cf. Bowerman, 1987).[14]

In any case, as with the Subset Principle, it would appear that in SLA the Uniqueness Principle does not obtain (see Rutherford, 1989 for useful discussion). The only empirical data that I know of come from Trahey and White (1993), but there is anecdotal evidence (e.g. Bley-Vroman, 1986) that certainly suggests the absence of any Uniqueness Principle operating in SLA. Over the 17-year course of my own naturalistic acquisition of Japanese, for example, I have

> (i) incorrectly assumed synonymy of forms that are in fact quite different; e.g. *tokoro de/tokoro ga* (by the way/nonetheless), *sikasi/sikamo* (however/moreover)
> (ii) incorrectly (I think!) assumed a distinction where none exists; e.g. *ka mo sirenai/ka mo wakaranai* (maybe)
> (iii) correctly assumed that there was a distinction, but got the distinction wrong; *taberu/kuu* (eat; I assumed it was parallel to *essen/fressen* –Lord knows why, since I don´t know German– where in fact it's a register distinction, the former being the more polite form)

(iv) correctly acquired a distinction, but consistently fail to apply it in output; e.g. *hontoo/honma* (is that so?; standard/Osaka dialect), *watasi/boku* (1st person sing. pronoun; register difference)

Perhaps, given the gross stupidity of some of my errors, I should stress that I actually am pretty good in Japanese.[15] Notice also that at least some of the time I have acted as if in obedience to a Uniqueness Principle, although not necessarily successfully, and not all the time. Notice further that my verbal behavior can be wildly misleading in this respect; e.g. it is almost a physical impossibility for me to refer to myself as 'watasi', but I have a very good (but probably not nativelike) knowledge of when I should use the term.

It would thus seem that if the Uniqueness Principle is a learning principle that guides L1 acquisition, it ceases operating, or at least operating effectively, in adult SLA, just as does the Subset Principle, if it is a learning principle. One question that arises here is how this discussion of these two principles relates to the theist/deist controversy. Even if we reject the two principles outright, denying their status as learning principles, that in itself would not invalidate the distinction between principles of grammar and principles of grammar learning; so far as I know this distinction is accepted by both sides. Nor is White's (1989) claim that the former survive while the latter decay necessarily overthrown, although it would certainly be seriously threatened by the elimination of the Subset Principle and Uniqueness Principle as learning principles. A deist might want to claim that it is implausible that of two closely related modules one only should peter out while the other carries on, and hence might want to take evidence for the decay of one as evidence for the decay of both. But this is not a particularly compelling argument; it sounds rather like an example of what Dawkins (1985) calls the Argument from Personal Incredulity. The mind, after all, is a queer place, and queer things go on in it. Still, it might be a fairly Pyrrhic victory for theism if the adult is only constrained from making crazy grammars, and isn't constrained from making bad ones.

WHAT IS TO BE DONE?

Considering how young UG/SLA research is, it may be justifiable to say that it has come a fairly long way, at least in comparison to SLA theorizing outside of the UG framework. Still, that isn't necessarily saying a lot, given the dedicated futility of so much SLA research. In any case, there is still a good deal to be done on all fronts, and I will simply indicate what I see as some of the larger areas of theoretical inquiry that need to be addressed.

The theory of UG itself, of course, is progressing, nicely it would seem, and this progress is largely out of the hands of SLA workers. Needless to say changes in this theory will entail adjustments in SLA theory. Of more specific concern for an SLA theory is the theist/deist controversy, which really does need to be settled. In effect theism today seems to have the upper hand, but it is not clear to what extent that is simply by default. Neither party has yet to deal satisfactorily with the ontological question of UG in adult language acquisition: for instance, to what extent does it make sense to talk about UG as separate from an L1 grammar?

One problem I have avoided up to now, but which impinges directly on questions of theory, is the problem of measurement. As has often been lamented, empirical results tend to fall between two stools: better than chance but not like native-speaker results. One obvious potential cause of this is that the tools used to date for measuring competence have lacked precision. It would certainly be helpful if e.g. acceptability judgement measures could be refined (cf. Sorace, 1991), or other methods employed such as reaction time measurements (e.g. Eubank, 1991) or neurological measurements. No matter what measuring device we use, of course, we are compelled to infer competence from what we can observe; but it would be a major step forward if we could have greater faith in the inferences.

But the property theory is in quite decent shape in comparison with the transition theory. As we have seen, there have yet to be any truly detailed proposals in the SLA literature for explaining how input is acted upon to form an IL grammar. Even such discussion as there has been of the Subset and Uniqueness Principles is limited, after all, to learning principles, not learning mechanisms as such. For instance, there is very little mention of the parser in SLA (Bley-Vroman, 1991 and Zobl, 1988 being a couple of the exceptions). In any case SLA theory would benefit immensely from specific proposals about the interface between input and language processor, proposals moreover that can avoid the homunculus problem.

Remember that one of the points of a property theory is to explain complex systems –in our case, L2 knowledge systems– by breaking them down into simple, and indeed mindless, components. UG theory does this; but if the transition theory doesn't also do this, then the homunculus has slipped back into the mind, and the advance made by the property theory is undone. When Chomsky and others talk about the 'rich deductive structure' of the language faculty, they mean deductive structure, not deductive reasoning; the language faculty obeys, but does not follow, the laws of logic. It is thus a bit disturbing, for instance, to find, even in the best SLA research, terms like 'notice' or 'realize'. For instance, Finer (1991), in an interesting and important study, says that 'the *realization* that the [Proper Antecedent Parameter] takes on the marked value in the target language would entail the further *recognition* that failure to shift away from the marked position ... on the [Governing Category Parameter] could eventuate a violation of [binding theory]' (p. 367; emphasis added). Noticing and realizing, of course, are precisely what the L2 learner is supposed *not* to be doing –especially if one is a strict theist who rules out the utility or useability of negative evidence. Now, I'm sure that in most cases words like 'realize' are being used metaphorically as a kind of shorthand; but such metaphors are still promissory notes that sooner or later will have to be cashed in. This is perhaps the central challenge facing UG/SLA theory at present. It's still early days, of course, but if UG/SLA theory can ultimately provide us with a homunculus-free transition theory, it will have succeeded in bringing SLA into the big leagues.

Notes

* I am happy to acknowledge the helpful comments of Lynn Eubank and Lydia White on an earlier version of this paper. The discussion of learning principles started off as a presentation at a symposium on cognition and SLA organizad by Russell Tomlin at the

Univesity of Oregon, February 1992; the paper as a whole is based on my presentation at a workshop on theory construction and SLA held in Washington, DC, September 1992, organized by Alan Beretta and held under the auspices of the NIMH. I am grateful to Russ and to Alan for giving me the opportunity to participate in these two meetings, and to the NIMH for enabling me to attend the latter.

1. I'm interested here not in any specific example of the various forms UG theory may take, but rather in the simple commitment to some sort of UG. In actual fact, though, virtually all UG/SLA research has been conducted within the so-called 'GB' or 'Principles and Parameters' framework. (No doubt this will soon change with the rise of the Minimalist Program in linguistic theory (e.g. Chomsky, 1995).
2. Actually, this commitment to observables is –fortunately– often honored in the breach; for instance, Tarone (1988) professes allegiance to an extreme empiricism in her demand for 'empirically verifiable' (*sc.* in your face) causes, but ignores her own impossible standard to accept (justifiably) e.g. relative social status of interlocutors as a cause of interlanguage variation.
3. For a sampling of typical communicative competence chat, see Angelis and Henderson (1989).
4. It is precisely this feature of UG-type property theories that renders nugatory the often-heard whine about how the 'innateness hypothesis', or UG, just sweeps the acquisition problem under the rug, or merely postpones any explanation, etc. Such a complaint would be valid if UG theoreticians simply left the black box alone, as did e.g. Krashen (e.g. 1982; cf. Gregg, 1993b).
5. For detailed discussion of the learnability framework, see Pinker (1979) and Atkinson (1992). I'm deviating from Atkinson here in that I make no mention of the success metric, or evaluation measure –a device necessary to tell the learner when to stop, as it were. I know of no discussion of this subject in the SLA literature.
6. See Long (1990, 1993) for a different approach to assessing SLA theories.
7. Needless to say, we are assuming certain idealizations in all this: e.g. that the learner receives sufficient comprehensible input, has sufficient motivation, etc.
8. The putative absence of stages has been cited as an argument against parameter-setting models; see e.g. Wolfe Quintero 1992.
9. Despite occasional claims to the contrary –e.g. Larsen-Freeman and Long, 1991; VanPatten and Cadierno, 1993– no one has ever suggested that incomprehensible input is necessary! The claim (as in e.g. White, 1987) is that unparsable input is needed; that is, input that violates the learner's current IL grammar is needed to stimulate revisions in that grammar to accommodate the input. Whether or not the message conveyed by that input needs to be understood by the learner is a separate question (cf. Gregg, 1984); but in any case it's not likely that speaking gibberish, talking at a mile a minute, etc. is going to do the learner much good.
10. Negative evidence is often defined simply as evidence about ungrammaticality, but this is misleading. For one thing, evidence about grammaticality –Schwartz and Gubala-Ryzak's (1992) 'explicit positive evidence'– intuitively seems not to belong with normal utterances in context as a single class of evidence, which is why Schwartz and Gubala-Ryzak make a point of excluding it, along with standard ly defined negative evidence, as a source of L2 learner hypotheses. But also, positive evidence can also provide information about ungrammaticality, as in parameter setting: information that one value is grammatical is automatically information that the others are ungrammatical. Thus it would seem that rather than the object of the evidence, the mode of providing it should be the defining criterion.
11. Note that we will still need a theory of learning for those parts of language that fall outside UG; if e.g. learning that in Japanese one never attaches the honorific *-san* to one's own name is no different from learning e.g. who to bow to and how low, the problem still remains of explaining how such knowledge is acquired. But that will be a problem for someone else, specifically for a learning theorist.

12. One problem that Hermon raises is that of how hardwired settings could be altered simply by input. In SLA, if default settings were biologically predetermined, then transfer of an L1 superset parameter setting to the L2 would be evidence for deism; thus a theist would presumably either opt for the subset-computing form of the Subset Principle, or else deny the existence of such a principle in the first place.
13. And yet sometimes L2 learners do reset from superset value to subset value, and in the absence of negative (or even much positive) evidence. Hirakawa (1990), for instance, found that a number of her subjects (10 out of 65), high school students in Japan with virtually no natural English input inside or outside of class, nonetheless managed to reset the Governing Category Parameter (Wexler and Manzini, 1987) from the Japanese value (largest superset) to the English (smallest subset). Hirakawa offers no explanation for these rather surprising successes.
14. Here perhaps is a case where research on the success metric would be helpful.
15. My favorite example of a Uniqueness Principle violation comes from a European friend of mine, fluent in English and several other European languages, who coined the word *overlisten* for *eavesdrop*. When it was pointed out that there is no such word, her response was lucid and compelling: (1) given *hear/listen* and *overhear*, naturally one expects *overlisten*; (2) *eavesdrop* is simply ridiculous. Several of us were sufficiently impressed by this line of argumentation that we thereupon acquired *overlisten*.

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