

SLA theory: prolegomena to a theory of language acquisition and implications for Theoretical Linguistics*

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1. Introduction: The Relevance of Theories

"You know that much", said the wise man to the explorer, "but your knowledge is not really forceful. You know all these things about the sun, when it raises and when it sets, how far it is and how hot. But I know a formula, and when I spell it, the sun begins to shine. Do you know such a formula?" — "No", said the explorer, "I am impressed. And this formula really works?" — "On occasion, at least", said the wise man, "but you don't even have a formula, do you?"

Any scientific research which deserves the name can't be satisfied with accumulating knowledge about its particular field. It must try to uncover the general principles behind the masses of particular findings: it must eventually come up with a "theory". I can't imagine that there is any serious disagreement among researchers of any orientation or discipline on this general point, although opinions vary substantially as to what such a theory should look like, what might constitute a sufficient factual base for it (Newton's "hypotheses non fingo" seems no longer trendy), and how to evaluate the relative merits of more theoretical versus more descriptive contributions.

Language acquisition research is no exception here. Indeed, be it due to the steadily increasing stock of knowledge about acquisition or to the mysterious meandering of scientific fashion or to both, the need for a theory of acquisition is more urgently felt than ever. This holds for both first and second language acquisition studies, and especially in the latter case, the lack of a theory has almost grown into a trauma for some researchers. The reason is historical: the field originally developed from language teaching, and it was, and often still is, seen as an application of linguistic and psychological insights rather than as a research domain in its own right.

It lacks the glamour of a true scientific discipline, and hence, it tries to get this glamour from other more reputable areas: first from psychology, notably psychology of learning, and more recently from theoretical linguistics.

This attitude is understandable, but basically wrong. No reasonable person would deny that a responsible researcher should keep an eye on what is going on in neighboring fields. But I think, too, that the study of developing systems is a research field in its own right, and linguistic theory can benefit from it as much as vice versa. This is the view taken in this paper. More precisely, two points will be made:

1. A theory of acquisition should be a theory of acquisition.
2. The study of developing systems can substantially contribute to our understanding of language structure and function in general.

The first point simply means: Take the phenomena of your field seriously! The process of language acquisition has a number of obvious and salient characteristics, and a theory which does not take these characteristics seriously does not qualify as a theory of acquisition, no matter how elegant, precise, and appropriate for other fields it may be. Section 2 will consider some of these characteristics. The second point can't be proven here; a convincing argument can only consist in making such contributions. But in Section 3, I will try to illustrate what I have in mind and what I think is the wrong route.

2. Some Truisms on the Process of Language Acquisition

A theory of language acquisition can't deal with all aspects of its object: a certain amount of idealization is necessary, as is the case with any theory of any field. It should focus on those aspects which are, in a sense, constitutive, and ignore those which are accidental. But then, which aspects are constitutive, which ones are accidental? Clearly, this is not an easy question to answer. Classical mechanics was successful, because it ignored the shape of bodies, but not their mass; classical optics was successful because it ignored the mass of bodies, but not their shape. What follows are elementary facts which hold for any kind of language acquisition and which are constitutive for, and can't be ignored by, any serious theory of language acquisition — although they often are ignored.

2.1 *"Language acquisition is a difficult and cumbersome process which extends over many years until full mastery is reached."*

This is clearly true for second language acquisition where full mastery is rare, anyway. It is no less true for first language acquisition. We are sometimes surprised and pleased how rapidly children grasp certain features of the language to be learned, especially if they are our own children. But this impression must not mislead us to premature conclusions. Clearly, a normal child hardly shows phonological errors at the age of four, i.e. after about three years of learning, and not many morphological errors at the age of six. This is also the age at which most important rules of syntax are mastered — but clearly not all. The difference between "him" and "himself, felt to be such a crucial phenomenon of English syntax by many linguists, is not systematically mastered at the age of seven (Solan 1987, cf. Section 4.3 below); at this age, by far most children consider "him" in *The dog told the horse to hit him* to be co-referential with "the horse". Many children don't master the "tough to"-construction at the age of eight (C. Chomsky 1969). There are numerous other examples of this sort, as one may find in any comprehensive textbook on language acquisition.

The point here is not whether children are able to understand and to make themselves understood — this they achieve much earlier; nor whether they know some unusual lexical item or inflectional form — this often adults never do. The point is rather at which age they master the full structure of their language. Even if we assumed, counterfactually, that they do so at school age, the process of language acquisition extends over minimally five years. Why this long time? There is at present no straightforward answer to this question. But any serious theory of language acquisition has to take a stand on it.

As is well-known, a number of theories on language acquisition have been partly inspired by exactly the opposite assumption.¹ To give but one quote: "this complex but fairly uniform mature capacity is attained in a remarkably short time, without much apparent effort, conscious thought, or difficulty" (Hornstein and Lightfoot 1981: 10). The problem which arises under this assumption can be accounted for stipulating that a great deal of the structure of the language to be learned is already there, and only some gaps must be filled by appropriate triggering; in more recent terminology: some open parameters must be set (cf. Section 3 below). Such an assumption is clearly not justified by time constraints, quite in the opposite: the

child has all the time in the world to acquire the structure of the language, and actually takes it. Note that this does not falsify the notion of a rich innate structure; but its existence can't be motivated by the speed and the ease of the acquisition process. The justification, if any, has to come from somewhere else, for example from the possible poverty of the input.

It is interesting to compare this case to another famous argument on a developmental theory — Darwin's theory of evolution. A crucial problem of Darwinian evolution is the long time it needs to bring about the present state of the animate world. Clearly, the 6000 years which Bishop Usher, in accordance with the testimony of the Bible and with Newton's calculations, allowed to this world would not suffice. Under this assumption, the idea of a Darwinian evolution must be false, the creation of the world must have been a short-term process, and a reasonable theory of it could even consider it to be instantaneous (or six days long). A more serious argument along these lines was raised by the leading physicists of Darwin's time, who estimated the age of the earth to no more than about 20 million years — still by far too short for Darwinian evolution. This was clearly a much harder challenge to Darwin's theory than Bishop Wilberforce's eloquence, and indeed, no solution was found in his day. It was only Rutherford who provided convincing evidence that the earth was much older, hence that there was probably enough time for Darwinian evolution.

2.2 *"Language acquisition is essentially cumulative."*

This point is related to the previous one, but it is not the same: It is not just an accidental fact that not everything is learned at one stroke; you must have accumulated some knowledge, in order to be able to add further knowledge. This may be done in various ways, and how it is done is an empirical question. But in any case, the process is essentially, and not just accidentally, cumulative. Ignoring this creates a number of puzzles which immediately disappear as soon as we consider the time-course of the acquisition process. One of these examples can be illustrated by the following argument used by Chomsky on various occasions, for example Chomsky (1975):

Imagine a neutral scientist, observing a child learning English. Suppose that he discovers that the child has learned to form such questions as those of (A), corresponding to the associated declaratives:

- (A) the man is tall — is the man tall?
 the book is on the table — is the book on the table? etc.

Observing these facts, the scientist might arrive at the following tentative hypothesis as to what the child is doing, assuming now that sentences are analyzed into words:

Hypothesis 1 : The child processes the declarative sentence from its first word (i.e., from "left to right"), continuing until he reaches the first occurrence of the word "is" (or others like it: "may", "will", etc.); he then proposes this occurrence of "is", producing the corresponding question (with some concomitant modifications of form that need not concern us).

This hypothesis works quite well. It is also extremely simple. The scientist has every right to be satisfied, and will be able to find a great deal of evidence to support his tentative hypothesis. Of course, the hypothesis is false, as learned from such examples as (B) and (C):

(B) the man who is tall is in the room — is the man who is tall in the room?

(C) the man who is tall is in the room — is the man who tall is in the room?

Our scientist would discover, surely, that on first presentation with an example such as "the man who is tall is in the room", the child unerringly forms the question (B), not (C) (if he can handle the example at all). Children make many mistakes in language learning, but never mistakes such as exemplified in (C) (if he can handle the example at all). If the scientist is reasonable, this discovery will surprise him greatly, for it shows that his simple hypothesis 1 is false, and that he must construct a far more complex hypothesis to deal with the facts. The correct hypothesis is the following, ignoring complications that are irrelevant here:

Hypothesis 2: The child analyzes the declarative sentence into abstract phrases; he then locates the first occurrence of "is" (etc.) that follows the first noun phrase; he then proposes this occurrence of "is", forming the corresponding question.

Hypothesis 1 holds that the child is employing a "structure-independent rule" — that is, a rule that involves only analysis into words and the property "earliest" ("left-most") defined on word sequences. Hypothesis 2 holds that the child is employing a "structure-dependent rule", a rule that involves analysis into words and phrases, and the property "earliest" defined on sequences of words analyzed into abstract phrases. (Chomsky 1975: 30-31)

To be clear: Chomsky does not make a particular claim about acquisition here, he simply uses this example to demonstrate that a sentence is not just a linear sequence of words but has a structure, in particular an NP from which one must not extract a finite verb. This is correct, although perhaps not in need of demonstration. The point becomes interesting as soon as one asks the question: How is it possible that the child unerringly obeys this

structure, instead of simply fronting the first "is" (see, e.g., Fanselow and Felix 1987: 110-112)? The child must somehow have access to it, but how? This must be mysterious, indeed, for anyone operating with the idealization that the acquisitional process can be regarded, without changing the nature of the problem, as instantaneous. But in fact, it is cumulative, and this is **essential** to the process. What happens, is roughly this. This child initially learns utterances like *Daddy is tall, the uncle is tall*, etc. He also learns utterances like *Is Daddy tall?, Is the uncle tall?*, etc. And he learns that these two sets are somehow related, say by a transformation **fronting**, which turns "NP is ADJ" into "Is NP ADJ"². This, of course, is not the end of his acquisition process. The child, for example, works on his little NPs and enriches them by adjectives, genitive NPs, but relative clauses, and the like, for example by *who is here*. But there is no reason for the child to change his previous rule **fronting** — unless there is evidence to the opposite. There is no particular puzzle here, although there are a number of empirical problems about the details of this developmental process.

We may put this in more general terms: If we really want to understand the process of language acquisition, we must consider its time-course.³ What a learner acquires at a given point in time is only understandable taking into account what he already has at that particular point in time. Any serious theory of language acquisition has to provide for this fact.

The point has been made here for first language acquisition. It is easy to see that it also applies to the second language learner: in Section 3.4 below, I will discuss an example which bears on this point.

2.3 *"The process of language acquisition presupposes a vast amount of input as well as species-specific learning capacities."*

The first part of this claim is hard to prove. It just appears that nobody ever has learned a language, first or second, up to full mastery without a vast amount of input. This, admittedly, is no proof, just as there is no proof that we all must die. But since there is no convincing evidence to the contrary either, and given all our experience so far, the assumption made here seems better justified than its contrary — that only a little bit of input would suffice.

The second part of the claim is no less arguable. It has been said, indeed, that other species are able to acquire a language, as well. Bees, dol-

phins, and monkeys have been mentioned in this connection. The argument would be more convincing, if it had been made by a spokesman of these species themselves. In fact, the point is trivial if we understand by language any system of communication.

There is no doubt that other species have such systems. On the other hand, no evidence was ever given that any other species can learn something similar to human languages. There are two reasons why I am mentioning the claim above in connection with the requirements which a reasonable theory of language acquisition must meet. First, there must be something special in the genetic endowment of human beings which allows them to learn a language. In other words, there must be a genetical component in language acquisition. The question is only where this component is. There are other species-specific capacities of human beings. No other animal masters fire, has invented the wheel, plays dominoes, has built a single neutron bomb, or created value-added tax. The question is whether the innate component of our mind which allows us to learn a language is a different capacity from the one which allows us to do all these other things. Note, in particular, that all species-specific capacities could simply be due to a single quantitative difference in some cognitive component between humans and other species, for example in memory. We shall return to this question below, but in any case, it is clear that a reasonable theory of language acquisition has to take a stand on it.

The second reason is this: Why do we need that much input? If you imagine how much a child hears, or overhears, during the first three years of his life, it is amazing how little he makes of it. It is not likely that a child after that time is exposed to structures not heard before (except there is a change in social environment, and also except some lexical forms; but this is not the point). The fact is simply that basically everything is there, but it can't be fully processed. Some researchers correctly pointed out (Corder 1973) that there is a difference between "input" and "intake", i.e. that part of the input which is processed, or processed beyond mere hearing. But this is only a name for the problem. Why does the "intake" change?

There are two reasons. First, the flashlight of linguistic theory makes us sometimes blind to the fact that mastering a language is not just knowledge; it is a skill which includes knowledge. Hence, it needs practice, and thus, time. This is much more obvious in second language learning than in first language learning. But it also applies there, although children are perhaps more flexible in this respect, just as they are more flexible in learn-

ing many other skills.⁴ In any event, having heard some structure once, or twice, or even ten times, does not guarantee that you really "have" it. So, there might be a purely statistical reason for the vast amount of input needed. This is not a very poetic reason, but there is surely some truth to it, especially on the production side.

The other reason is more interesting. Apparently, the capacity of processing the input changes over time. This immediately leads us to the next question which a reasonable theory of language acquisition must address: Which part of our "learning capacity" changes?

What are the possibilities? At any point of his acquisition process, the learner can draw on two types of resources. The first are his biologically given faculties to process language; these include peripheral capacities, such as hearing or articulation, and central capacities, such as memory, thinking, and maybe others (including possibly some language-specific capacities). The distinction between peripheral and central is not always perfectly clear, but this should not bother us here. The second type of resources is the "available knowledge" — i.e. all the knowledge which he already disposes of at that time. This includes knowledge about the world and the course of events in it, but also whatever he knows about the language to be learned.

One might have very different ideas about what these components look like, how they interact, and what their relative weight in the whole process might be. But there can't be any doubt that they all play a role in the acquisition process.

Returning now to the question which component changes and is thus responsible for the changing "intake", the first answer is clearly that all of them change over the years. But they change at different times during the life span, and to different degrees. With the possible exception for the very first months, the peripheral capacities remain relatively stable for a long time; at least, there seems to be no decisive development of peripheral capacities between, say, 6 months and 10 years which could account for the changing capacity to process the input. It is much less clear whether the central capacities as such considerably change between, say 2 and 50 years. We all like to believe that we are thinking faster, more clearly and more correctly than a three-year old. But first, it is not at all clear whether **biological** changes in our central capacities are responsible for this desirable development, or whether it is just a consequence of the obvious fact that we have more experience and more knowledge than a three-year-old. Second,

we do not become better language learners with increasing age: a six-year-old is generally a better language learner than a 30 year old. And third, the "intake" (not the input!) changes considerably from, say, three to six; but it is at least open whether there are major biological changes during this time in memory or thinking. So, the question to what extent the change of central biological capacities is responsible for the changing "intake" is simply open. There might be, of course, a special central capacity different from memory or thinking, which is just responsible for language and which changes (or "matures",⁵ as some would say). We shall return to this possibility below.

The clearest change concerns the available knowledge, especially after the first year (but also during the first year, world knowledge clearly grows). Hence, it would be natural to assume that the difference in what can be and what actually is processed from the flood of input is largely due to what knowledge is available already at a given point. At least, it would be very strange if the permanent changes in this respect did not affect the ways in which the learner deals with what remains to be learned from the input. Moreover, it would be most economic for a theory of language acquisition, if the whole acquisitional process could be explained in terms of changing accumulated knowledge, rather than in terms of changes in the biological component of our language learning capacity.

The point of this whole argument is this: A reasonable theory of language acquisition can't assume a stable "language learning capacity". It has to operate with a "learning capacity at t1", where t1 is some period in the acquisition process. What the changes are, and how they affect the acquisition process, is an empirical issue.

2.4 *"The input consists of sound waves and of parallel situational information."*

The language to be learned becomes accessible to the learner in the form of sound waves — longer or shorter stretches of audible, but unanalyzed speech. (For simplicity's sake, I am ignoring written language here, which may play an important role in second language acquisition). This sound stream does not include little labels like N, VP, "classifier", or "infix". Its segments do not structure or carry meaning for the learner, as they do for those who know the language in question. There are two exceptions. First, the learner may have a teacher who analyzes the sounds for him. This is

why tutored second language acquisition is in at least one crucial aspect different from first language acquisition and from second language acquisition by everyday communication. Second, the learner may know already bits and pieces of the language to be learned. In this case, his present "available knowledge" allows him to analyze parts of the sound stream — but exactly those parts which he need not learn anymore.

If the learner had only the stream of sounds, he could never learn anything new. This constitutes what one might call the "locked room mystery of language acquisition". Suppose someone would lock you into a room and play Malayalam to you over a loudspeaker — for hours, days, weeks, months. Even after years, you would not have learned Malayalam, except perhaps some features of the phonology. In order to achieve this, you also need a lot of "parallel information" — you must see and hear what people do when they produce these streams of sounds. This concerns not only the lexical meaning of words, but also the structure of sentences. In standard English, many sentences are verb-initial and have null subject. In order to understand this important feature of the English syntax correctly, you must know that it goes with a special communicative intention on the part of the speaker ("Close the window, please"). Nothing in the sound stream tells you this: it is part of the "parallel information", which must be processed simultaneously.

What the nature of this "parallel information" is and how it operates in the process of language acquisition, is an open, interesting and extremely difficult question (see, for example, Slobin 1986, and, for second language acquisition, Klein 1986, chapter 4). One point should be clear, however: A theory of language acquisition, in which this "parallel information" is not a key concept, can't even remotely explain what it is meant to explain: language acquisition. It is worthless for this purpose. I am stressing this point because there are several ambitious theories of acquisition which suffer exactly from this deficit. These include most theories in the "learnability" paradigm (surely not all, though: Pinker (1985) is one of the few attempts to deal with this problem). They also include some formal theories of language learning in which the input — to the extent that it is felt to be necessary at all — is processed by a special parser (see, for example, Berwick and Weinberg 1984). It is not easy to imagine a parser which successfully handles the Malayalam sound stream in the locked room — that is, derives the language-specific rules of Malayalam without any access to and systematic integration of on-going "parallel information".

2.5 *"The target of the acquisition process is to interrelate expressions and meanings."*

The point is related to the preceding one; but it is not to be confused with it. By the deliberately global term "parallel information" used above, I mean all the information which, in a given learning situation, is accessible to the learner in addition to the sound stream. It allows the learner to find out how expressions are structured in the language to be learned, and how they are related to particular meanings. But it is not to be equated with the latter, just as the sound stream is not simply to be equated with "expressions", so long as the learner cannot analyse it. Sound stream and parallel information are the **sources**; expression-meaning pairs are the **targets** of the acquisition process.

Language acquisition, both first and second, is a directed process with a clear target, and we cannot understand this process, and hence have no reasonable theory of it, without keeping in mind what this target is. The learner's eventual aim is not to know how to build a certain expression when he wants to express a certain meaning in a given situation.⁶ It is with aim in mind that the learner operates on sound stream and parallel information. In other words: All the learner's ideas about how a certain expression is structured are driven by the idea that this expression and the rules on which it is based, should make some sense — not some sense in general, but some **specific** sense.⁷ This does not mean, of course, that learners could not sometimes use constructions the meaning of which they are totally wrong about or don't know at all. But then, they will give it up after a while in the former case, and they continue to operate on it in the latter, until it is found out. Observations of this kind do not violate the general principle that during the acquisition process, the rules which the learner assumes must be meaningful; they confirm it.

The common way to state specific acquisition problems often blurs this issue. We would often say, for example, that a child or adult who tries to learn French has to learn the rules of clitic placement. Such a statement is surely correct, but it is also highly misleading. What the learner has to learn is roughly this: "if, in French, some entity is referred to, and **if** it is not in focus, **then** put the word *le* or *la* (*les*) in front of the "verb" (and analogously for the comprehension side). Or to take another example: There are a number of studies on how people acquire relative clauses in different languages. But the aim of this effort is not to learn how to construct relative

clauses in a particular language, although they clearly learn how to do this. What the learner wants to know, and eventually learns, is how to express, in a particular language, a property of some object or person, such as the property of a book to be cheap or to be liked by John; languages have various ways to do this, including constructions which we call relative clauses, and so, the learners eventually learn relative clauses. Just as children learn to tilt cups as they learn to drink from them.

Ignoring the fact that expressions are usually considered to be meaningful leads to a number of strange problems. As is well known children sometimes express the idea that some event of going was in the past by *go-ed*, just as they express the idea that some event of walking was in the past by *walk-ed*. After a while, they give up this way of expressing "going + past" in favor of *went*. This is sometimes felt to be surprising because they are hardly ever explicitly corrected. I don't see that there is anything surprising about this retreat, given (a) that children also have to analyze the recurring "went" in the input and that as a consequence, they discover that exactly this meaning "going + past" is expressed by everybody else as *went* and (b) that they normally take their ideas of how to express something from how it is done by everybody else. The retreat might be a mystery, however, if one does not consider what *go-ed* and *went* are used for.⁸ Or to take up the relative-clause example. Some languages express relative clauses by constructions like *the book such that John likes it*, i.e., with an explicit object pronoun. Structurally, it would also be possible to have some other object in this position, for example *Mary*. But such constructions never occur. How does the learner avoid them? On purely structural grounds, we might be forced to stipulate some special constraint on relative clause formation, either explicitly to this effect, or as a consequence of some other, more abstract structural constraints. In fact, the problem never occurs for the learner who tries to find ways to say something about some objects: it is a possible property of some book that John likes it, but it is, as far as I can see, not a possible property of some book that John likes Mary.

Let us conclude this by a brief look at another argument which is often put forward in the acquisition literature — the "poverty of the stimulus" problem (cf. note 1). One aspect of this argument is the assumption that the input which the learner gets is deviant in many respects — full of hesitations, pauses, false starts, ungrammatical constructions, etc. Let us ignore the possibility that the linguist's ideas of grammaticality are occasionally somewhat different from the ideas of the average native speaker. Then, a

great deal of the phenomena mentioned are simply irrelevant to the point. Pauses or hesitations normally do not affect grammaticality; similarly, repairs often underly rigid grammatical constraints (see, e.g., Levelt 1983). But surely, the input also contains utterances which one would not predict from a grammar book. Take, for example, a repetition like *Give me the the the plate!* Utterances of this type are not infrequent, and yet, children never develop the rule that there is a determiner consisting of *the* repeated three times. It does not make sense, in contrast to the simple determiners *the* or *a*. These determiners have a clear and recognizable function. Let us assume, for the moment, that "the N" has the function of indicating that the referent of the NP was mentioned before, whereas "a N" indicates that this was not the case; in fact, the situation is more complicated, but this is irrelevant to the argument. We can assume that the child keeps track of what has been said before, at least for a while; hence, he will identify this functional difference after some time. There is no such functional contrast between "the the the N", on the one hand, and "the N" or "that N" or "three N" or "a N", on the other. Hence, there is no way in which the child could ever make sense of this possible construction and consequently, he will not include it in his growing repertoire of linguistic devices. This is a problem, of course, for any acquisition theory which only looks at the occurring sequences of sounds and syllables; but if learners did only that, we could never understand how someone can learn a language. The child does not learn "bare" structures — he learns meaningful structures.⁹

2.6 *"Language acquisition is the result of many essentially interesting processes."*

Mastering a language involves mastering rules of various types: phonological, morphological, lexical, syntactic, as well as rules of discourse organization, to use conventional delimitations of linguistic knowledge. All of these rules are either there right from the beginning, or they must be learned. As for the latter, they are not learned one domain after the other; first phonology, then morphology, etc.: the acquisition process simultaneously affects all of these domains (or modules, maybe). This is clear and not denied by anybody. But the development is not just parallel — it is **essentially** interactive. The development of some syntactic rule, for example, is often — though not always — dependent on the development of a rule in some other domain, say a phonological rule, and vice versa.

There are many phenomena which illustrate this point. Let me take one from German. One of the most salient rules of German syntax concerns the position of the finite component of the verb (in contrast to the lexical component which may, but need not go together with it). It is sentence-initial in some interrogatives, in imperatives, and in some conditionals; it is sentence final in subordinate clauses, and it is preceded by exactly one major constituent (subject, object, adverbial) in declarative main clauses. This is an important rule for the learner to acquire. In order to do so, the learner must be able to identify what the finite component of the verb is. The only way to do this is by its morphology. Hence, he must know the verb morphology — not completely, but sufficiently to identify the finite verb. In contrast to English, this task is far from being easy in German (Mark Twain's remarks on German highlight the point). In particular, German finite verbs — but also uninflected words — can end in extremely complex consonant clusters, like /ltst/ or /rgst/. Decomposing such clusters is often a terrible problem for learners, especially for second language learners with a native language like Spanish or Chinese. This clearly hampers the recognition of the verb morphology, hence the identification of the finite verb and, as a consequence, the acquisition of a central syntactical rule of German. This does not mean, of course, that the learner first has to complete his phonological analyses, before he can seriously attack other, higher rules; actually, his morphological or syntactical rules at a given point may facilitate his phonological analysis. The example only illustrates that processes in one domain are affected by processes in some other domain. Thus, we should not be surprised that in second language acquisition some learners have problems with some structures and others don't, although their native languages do not differ with respect to the corresponding structure: the reason may be in some other domain (or module).

There is a clear consequence: If we want to make a claim about the development in some domain, say syntax or lexicon, we must also have an eye on the other domains. The fascination which the idea of a "modular approach" has created — whether justified or not — must not make us forget that these modules, in whichever way the borderlines between them are drawn, interact at some **point**. This is also true for acquisition, and a theory of language acquisition which does not deal with this interaction misses its objectives in a crucial respect.

2.7 *"Principia non sunt multiplicanda praeter necessitatem."*

Occam's razor applies to all theories, and theories of language acquisition should be no exception. There is a widely-held view in the literature, according to which the species-specific capacity of learning and using a language can't be solely accounted for by those cognitive capacities of our mind (or brain) which are also operative in other cognitive domains. There is a special part of our mind (or brain) — we may have called it the "language module" — which, of course, interacts with other parts (without a memory, language is neither easy to learn nor to use), but which is not shared by any other part of our mind (or brain). We briefly touched upon this point above, in connection with the changing capacities of exploiting the input (cf. Section 2.3 above).

I think the question of whether there is such an extra "language module" is not easy to settle, given our present state of knowledge about language in particular and cognitive capacities in general. What is clear, however, is the research strategy in such a situation. A theory which can do without the assumption of a specific "language module" is much better than a theory which has to make it. This is a simple consequence of Occam's razor. If everything can be explained without an extra cognitive capacity, why assume it? It could turn out, of course, that indeed not everything can be done without. This would be a highly deplorable fact which one possibly has to face at the very end. But it would be unwise to begin with it.

The temptation to stipulate an extra capacity for language is surely understandable. After all, language is something special, and at present, we cannot account for all of its peculiarities. But life itself is something special. No one would deny that there is a really major difference between a living tiger and a dead tiger. It is not surprising, then, that at least until the end of the last century, many researchers assumed that there is some special property, a "living force", "elan vital", "Lebenskraft", which distinguishes living from dead beings and which cannot be explained by general biological or chemical processes. By now, most researchers have given up this notion, not because it was definitely refuted — it would be difficult to falsify, indeed — but simply because it is no longer needed. Clearly, one century ago, the issue was much more open, and given the little known in those days about biological and chemical processes, the assumption of a "living force" was surely not an unreasonable one: it could account for many phenomena which we find in a living but not in a dead organism.

To be clear: I do not want to belittle the idea that there is an extra "language module" in our mind which plays an important role in language acquisition and use. But a theory of language acquisition which does not need this extra "principiium" would be the better theory. Hence, we cannot rule out the existence of such a capacity, but we should avoid this assumption as long as possible.

In this section, I have discussed a number of requirements which, to my mind, any reasonable theory of language acquisition has to meet. There are others, no doubt. But even for those mentioned, I do not see any existing "theory" which would even remotely satisfy them. I personally think that at present, we simply do not know enough to proclaim a serious and comprehensive theory of language acquisition. But I would be pleased if I could be shown to be wrong.

3. Setting the Controls in the Other Direction

In this section, I will deal with one alleged theory of language acquisition which draws heavily on recent developments in theoretical linguistics — parameter setting. My intention is not so much to discuss or criticize this theory in detail, although this is done to some extent. In the preceding section several requirements for a reasonable theory in the field of language acquisition have been mentioned, and it is up to the reader to evaluate the theory of parameter setting against these requirements. The point is rather the following. In dealing with language acquisition, notably in constructing a general theory of it, many researchers strongly rely on the claims, notions and ideas of theoretical linguists. This is not of necessity bad. But it can be very misleading, and it was in this case. Moreover, if language acquisition studies are to attain the status of a serious scientific research domain, they should not only contribute to a better understanding of how people acquire language but also to a better understanding of how language in general is structured and functions. In more radical terms: Fully developed languages should be seen only as a borderline case of learner varieties.

Originally the idea of parameter setting was launched by Williams, Chomsky and others for first language acquisition only (see, for example, the introduction to Roper and Williams 1987). It was not intended to cover other types of acquisition, notably not second language acquisition of adults (in the classroom or by everyday communication). Recently, how-

ever, some researchers have extended the idea of parameter setting in this direction. There may be some doubts as to whether this turn is in accordance with the spirit of the original idea. But in what follows, I shall ignore this possibility, since it does not bear on the specific points to be made here.

The idea of parameter setting is roughly the following (for a fuller account, see, for example, Roeper and Williams (1987), and specifically for second language acquisition, Flynn (1987)). Any language has a great deal of special features, which must be learned item by item. This applies, for example, to the individual words in the lexicon or to the specific forms of inflectional endings. After all, English and Malayalam do not share a single inflectional morpheme nor a single word (except Coca Cola), and if you want to learn Malayalam, or you have to, because you happen to be born there, you simply must learn all these items one after the other. This part of the language to be learned, which may also include a number of idiosyncratic syntactic features, is often called — with a perhaps not too fortunate expression — its "periphery". What remains, is the "core" (cf. Chomsky 1986, section 3.5.1). It results from a limited number of universal principles ("universal grammar"). These principles are innate, but they still allow for some variation from language to language: they have a number of open slots, parameters, which the learner has to fill on the basis of the input which he gets from his social environment. Such a principle from universal grammar is, for example, "subjacency". It roughly says that no element may be moved across more than two "bounding nodes". What counts as a bounding node may vary within certain limits from language to language, and the learner has to find out what the bounding node in his case is: he has to "set the parameter" or, in the case of second language acquisition, to "reset" it (if still possible).

The specific way in which a parameter is fixed in some language has a number of consequences for its sentence structure. In other words: A part of the structural peculiarities of a language, compared to some other language, follows from the way in which the open slot in the universal grammar is filled — the way in which the parameter is set. It is exactly this point which makes the whole idea attractive for a theory of language acquisition. A learner need not learn all of the structural peculiarities (except "peripheral ones", of course) step by step. As soon as he has fixed one parameter, all structural properties connected to that parameter follow. This is a tempting idea, no doubt. If language acquisition was indeed short and easy, parameter setting might be a good explanation. The question,

however, which I want to follow up here, is not so very much whether the whole idea is a good and correct one, but: How does one get the parameters which make it possible to test the theory?

In the following, I shall discuss three studies, two on second language acquisition, one on first language acquisition. They deal with three different parameters, as postulated by theoretical linguists, and test whether these parameters are operative in acquisition. In each case, the author shares the basic assumptions of the parameter setting view.

3.1 *The Head Parameter: Flynn (1987)*

The Head Parameter which Flynn considers in her study (and in some previously published studies; see also her contribution to this volume) is the most recent variant of the well-known idea that languages tend to serialize dependent elements in a uniform way — either before or after their head. For example, Japanese is consistently left-branching, whereas English is considered to be right-branching. The Head Parameter differs from these familiar concepts in that it is part of a more general concept of phrase structure, usually called X-bar theory. I will briefly sketch the idea to the extent necessary to discuss Flynn's study.

In X-bar theory, it is assumed that there are basically four types of phrases, NP, VP, PP, and AP. They all consist of a lexical head (noun, verb, preposition, or adjective) and various complements before or after the head. The form of these complements, hence the structure of the whole phrase, is essentially determined by the lexically fixed properties of the head: the phrases are "projections" of the lexical properties of the head. Opinions vary as to how many levels of projection should be distinguished as well as on some other issues, which need not concern us in the present context. The crucial point here is, that the relative order of the possible complements — such as modifiers, determiners, grading adverbs, etc., — in relation to the head is not considered to be a lexical property of the latter. It is assumed that this order may vary from language to language, but is consistent within one language: complements either follow or precede the head. Exactly this is the Head Parameter which the learner has to set or, in the case of second language acquisition, possibly to reset. If this turns out to be correct, it would indeed greatly simplify the acquisition process. The structure of phrases is largely determined by the lexical properties of the head (which the learner has to learn, of course), and the rest is done by fixing one parameter.

In her study (which also deals with some other phenomena, not considered here), Flynn compared Japanese and Spanish students learning English in classrooms. In Japanese, complements regularly supposed to be congruent with English in this respect. Hence, the Japanese learners must reset their parameter, the Spanish students need not. This should result in greater learning problems for the first group, as evidenced in more errors and greater uncertainty in metalinguistic judgments. Flynn's crucial variable was the position of the adverbial clause (e.g. "when"-clauses). I will not report here the details of the testing procedure. In principle, the assumption that Japanese learners have more trouble in the right positioning is confirmed, or at least, we can take it to be confirmed for present purposes.¹⁰

The problem with this finding is that it illustrates a possible influence from the first language, but it says nothing about the Head Parameter. The point about the parameter is, that **all** complements go in one direction (with some exceptions, perhaps, which must be considered, in one of the many uses of this word, as "marked"). Hence, it would be necessary to give evidence that, for example, Japanese speakers show a uniform behavior with respect to all complements of N within an NP: they should **similarly** try to place an adjective and a relative clause before N. Nothing of this kind has ever been observed. Or take the case of the Spanish learner. We do not find that they normally try to place the adjective after the noun when learning English, although the way in which the head Parameter is set in their language and is supposed to be set in English should tell them so. Hence, one would have to assume that they **partially** reset the parameter, leaving the relative clause after and placing the adjective before the noun. But this totally destroys the idea of a **parameter** which applies all complements within a phrase, not to speak about other phrases, like VP. (I personally think — if I dare to speculate — that the Spaniards hardly ever put the adjective after the noun because they never hear that.)

The problem is not in Flynn's study. It is the parameter, as conceived of here. It is true that some languages are quite consistent in their positioning of complements relative to their heads. But most languages are rather mixed, and hence there is no way for the poor learner other than to learn the relative order more or less individually for each head-complement configuration. The point I want to make here is this: One should not necessarily believe and attempt to apply to SLA research what theoretical linguists at one point in their deliberations tell about the organizational principles of

language. Rather, acquisition research in itself should try to contribute to a better understanding of these organizational principles. Now the head parameter may have been a bad candidate for a possible parameter. Let us turn to another candidate, often discussed in the literature.

3.2 *The Pro-drop Parameter: White (1985)*

Again, I will briefly and informally sketch the main idea of this parameter. It was named after the well-known phenomenon that some languages — typically languages with strong verb inflection — omit the subject pronoun, unless it is in focus. Typical examples are Latin, Spanish, and Italian, in contrast to French, German, or English. It has been observed, or at least claimed, that this property goes hand in hand with a number of other structural properties of the languages involved. Opinions vary to some extent here, but the properties most mentioned, including White's study, are these:

1. Omission of subject pronoun
2. Relatively free word order, notably subject inversion
3. Extraction from THAT-clauses
4. Rich verb morphology

English, for example, can't omit the subject pronoun (this may be arguable, but I shall adopt this view here), has no subject inversion (one can't say *Danced Peter*), extraction from that-clauses is highly restricted (it is awkward to say *Who do you think that might come?*), and the verb morphology is relatively poor. All of this is exactly the reverse in Italian or in Spanish. This collection of features could be accidental. The idea of the Pro-drop Parameter is that it is not an accident but follows from a more abstract property of the grammar, which has to do with the level at which the finite component of the verb (responsible for agreement) and the lexical content of the verb are brought together. There are various ways in which this property has been spelled out; but this need not concern us here (see, for example, Chomsky (1981: ch. 4.3 and 4.5); for a survey of the various versions, see von Stechow and Sternefeld (1988: ch.7.2). The crucial point here is that all of the individual structural characteristics listed above (and maybe others) are systematically connected: they follow from the way in which this parameter is set in a given language. It is exactly this fact which makes this parameter so interesting for language acquisition. The learner would not need to learn all of these individual characteristics separately (except, of

course, the forms of the verb morphology); as soon as he has set — or reset — the parameter, they follow.

White studied this. Informants were French and Spanish students learning English in the classroom. Spanish is pro-drop; French, like English, is not. The results are quite clear (I take it that the methodology was sound). There is a noticeable effect for the first feature, the omission of the subject pronoun. There is no effect for subject inversion and for **that**-clause extraction. (Morphology was, for plausible reasons, not included). Thus, there is an influence from the source language concerning one structural feature. This feature, then, is dissociated from the other features which, according to linguistic theory, are subsumed under the Pro-drop Parameter. Hence, either the Pro-drop Parameter as stated by linguistic theory is false, or it does not apply for language acquisition. Or both.

Faced with this choice, the partisan of parameter setting will choose, of course, the first option. Then, he (or she) will probably sit down and wait until the theoretical linguist comes up with another idea. Of course, he could also have a look at which features really go together in language acquisition, and based on this evidence, stipulate a new parameter. This would be a serious contribution from the field of language acquisition — independent of whether it eventually survives further study or not. There is some doubt whether the language researcher, despite his firm belief in parameter setting, will ever dare to do so. Theoretical linguists actually do not appreciate this kind of evidence. They like successful application of their findings to other fields, but not disconfirmation and alternative proposals. My last example of a parameter and its possible role in language acquisition is devoted to this point.

3.3 *Binding domain: Solan (1987)*

The third study considered here deals with first language acquisition, and the parameter involved is the Binding domain. Again, I will give a Reader's Digest version of the essentials of this parameter.

Most languages distinguish between two types of object pronouns, roughly like English "him" and "himself, the difference being that the latter is co-referential with the subject of the same sentence, whereas the former can't be co-referential. This is illustrated by well-known sentence pairs like *The horse hit him* versus *The horse hit himself*. The point at issue is the domain of this co-referentiality, the "binding domain". Should it be,

as said above, the sentence? This is clearly falsified by sentences such as *The horse told the dog that he should hit him/himself*. Here, "himself" is clearly co-referential with the subject of the subordinate clause, which in turn is co-referential with the object of the main clause: "himself" can't go beyond the clause, as deviant sentences like *Mary wished that Peter looked at herself* illustrate. Hence, a better approximation to the correct binding domain in English is the clause. This is still too rough, as examples like *The dog told the horse to hit him/himself* or *The dog showed the horse a picture of him/himself* show: apparently, the infinitival complement and the noun phrase "a picture of x" count as binding domain. To cover all of these cases, the definition of the binding domain has to be in much more abstract, theory-bound terms. We shall not go into this here. Languages may vary considerably with respect to this domain. In Icelandic, for example, the reflexive pronoun can "leave" subordinate clauses and be co-referential with the remote subject of the main clause. Hence, the binding domain is subject to parametric variation; it must be set as the language is learned.

Solan did not compare languages. He studied how children between four and seven years of age fix the parameter for English. Roughly, he presented several sentences containing reflexive and non-reflexive pronouns to children and asked them, who did what. (He also conducted some other experiments, not at issue here). Here are the results, according to sentence type and age group:

	4y	7y
	% correct answers	
1 <i>The dog said that the horse hit himself</i>	71	92
2 <i>The dog said that the horse hit him</i>	48	96
3 <i>The dog told the horse to hit himself</i>	51	92
4 <i>The dog told the horse to hit him</i>	48	93
5 <i>The dog found the horse's picture of himself</i>	52	54
6 <i>The dog found the horse's picture of him</i>	53	51

Note that 50% correct simply means "random". The findings must warm the heart of everybody who believes in parameter setting. Obviously, the children at the age of four have not set the parameter in the right way, and there is a clear jump, except for the "picture-noun-phrases" at age seven. It is not clear why the "picture-noun-phrases" are not affected, but these structures are a bit peculiar anyway, even for adults. But the evidence for the other cases is really telling, at least if we ignore the somewhat high 71

percent for the four-year olds in the first example. So, the case is perhaps not perfect, but a strong argument.

Now, these are not Solan's real results. I have invented them. In fact, there was no difference at all between the two age groups, and the real figures were as follows:

	% correct answers
1 <i>The dog said that the horse hit himself</i>	92
2 <i>The dog said that the horse hit him</i>	48
3 <i>The dog told the horse to hit himself</i>	76
4 <i>The dog told the horse to hit him</i>	38
5 <i>The dog found the horse's picture of himself</i>	85
6 <i>The dog found the horse's picture of him</i>	1

What would a responsible researcher conclude when faced with **these** findings? The theoretical linguist would not feel concerned. As for acquisition, he only processes positive evidence. What the language acquisition researcher, pondering the appropriate theory of language acquisition, would conclude should be clear: Take the results as primary evidence about the process of acquisition and treat them as one of the many building blocks for an eventual theory of language acquisition.¹¹

4. Conclusion

It could be that a phenomenon such as language acquisition has nothing interesting to offer from a scientific perspective. In this case, any attempt to uncover the principles which guide it is lost effort, and researchers in this field should look for something more rewarding. If it is not the case, then acquisition researchers should carefully explore the full range of this complex evolutionary process, try to isolate the various factors which govern it, and eventually develop a theory which is able to explain it. In doing so, it is important to have a look at what other people think and claim about language and about cognition; but none of this should be taken for granted. Language acquisition researchers should consider their findings to be an independent and important contribution to our understanding of language and human cognition in general.

Notes

*This paper is based on the first and the last part of a talk given at the Stanford Conference "Second Language Acquisition and Linguistic Theory", July 1987. The middle part of this talk dealt with the European Science Foundation project on Second Language Acquisition by Immigrant Workers. For a short sketch of this project and some of its results, see Perdue's contribution to this volume. For the sake of clarity, I have made many simplifications throughout this paper, but especially in Section 4. I do not think that they affect the argument, but it should be kept in mind. An earlier version of this paper has been read by several colleagues. I wish to thank them, especially W. Levelt, for critical and helpful comment.

1. It should be stressed that the "relative speed and ease of acquisition", though permanently referred to, was never the key argument for a rich innate structure, as far as I can see. More important was the supposed "poverty of the input", for example the many ungrammatical structures which it contains, or the non-appearance of some structures. I do not think that this argument really stands, either, but it would lead us too far to discuss this point here. See, however, note 6 below.
2. This, admittedly, makes a long and complex story short; but it illustrates the point. In fact, it is not easy to see how a child manages to relate these two types of structures to each other. It should be clear that the idea, the child might go from left to right until he "encounters" the first "is", is a baroque metaphor, good enough for the argument but hard to be made precise. The reader who is interested in a more comprehensive picture of what is going on in the acquisition of questions, is referred to the literature, in particular to Labov and Labov (1977).
3. Felix (1987), being aware of this point, suggests a distinction between the "logical problem of language acquisition", on the one hand, and the "developmental problem of language acquisition", on the other. It will appear to me that the former is simply the latter, if the time-course is ignored: it is a problem created by an unfortunate abstraction. It is like the problem of how it is possible to climb on a mountain, which gains a new dimension if we ignore the many intermediate stages which it normally involves. There is, of course, a logical problem of climbing on a mountain, if we want to consider only the stage where the possible climber is in the valley, and next, where (s)he is on the top. But it disappears when the developmental problem of climbing on the mountain is solved.
4. This, incidentally, is perhaps a major reason for the obvious differences in achievement when learning a second language in childhood or as an adult. We would not claim that there is a "critical period" for learning to ice-skate or to play piano; but when you start as an adult, your final achievement is (on the average) less perfect than that of a child. Mastering language is not **just** a skill, but it includes an important skill component, and this has consequences in ease of acquisition and final achievement.
5. The notion of a "maturing" language faculty seems to gain some popularity over the last years; see for example Felix (1987) and Borer and Wexler (1987). This is not the place to deal with it in detail. The only evidence put forward so far is the fact that the learner's capacity to process the input changes: this, however, is no argument that the language faculty matures. But this is a minor problem, perhaps. The real problem is that one can hardly see how the idea of a "maturing language faculty" of the mind could ever be falsified. The point is that one can hardly see how the idea of a "maturing language faculty" of the mind could ever be falsified. The point is not that one can't talk about mat-

uration of the mind; it is all too easy. Note that this would be quite different if claims were made about cellular or glandular development. I think we should leave the notion of maturation to real biologists and to camembert producers.

6. For simplicity's sake, I am talking here about the production side only; the analogous argument holds for comprehension, of course.
7. There is only a limited number of studies on this very point in the literature; for first language acquisition, see especially Slobin (1986) and Clark (1987) and the literature quoted there; for second language acquisition, von Stutterheim (1986, ch. 1.3).
8. I do not claim, incidentally, that all problems of "lacking negative evidence" can be handled in this way. But many.
9. Note that the whole point of "meaningfulness" made in this section has nothing to do with the "autonomy of syntax" argument. It would be utterly confusing to equate syntax with semantics, or to assume that syntactic rules are in some way "determined" by semantic rules. After all, a second language learner basically wants to express the same meanings, just in a different language, with different syntax, morphology, etc. But he does not just learn new rules — he learns to express certain meanings by certain new rules.
10. There is a number of conceptual and methodological problems with Flynn's study, just as with White's (to be discussed below), cf. Bley-Vroman and Chaudron (1987) or Jordens (1988). Since objections in this respect do not affect the present argument, I will ignore them and take all findings for granted without further discussion.
11. See, for example, the excellent study by Deutsch and Koster (1982); Solan's findings exactly correspond to this analysis.

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