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## A Note on Development Syntax

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## A NOTE ON DEVELOPMENTAL SYNTAX

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Let me set aside some issues I will not deal with. First, I will have nothing to say about whether the methodology of the experiments discussed in this conference is sound. I am not a psycholinguist, others have already commented on this, and in any case I am more interested in discussing the syntactic consequences of the findings, assuming they are real. Second, I will not discuss specific works in any detail. I do have technical comments to mostly all of them, but these I believe would interest the authors more than a general audience; in any case our theories at this point are wide enough either for the experiments to adapt to my technicalities or, if the experiments are proven correct, for the syntactic theories to adapt to their lower-scale consequences. Finally, although I will admit variation in technical implementation of theories, I will not admit giving up the central hypothesis of a system of principles and parameters that all of us in the conference, I think, assume uncontroversially. It is in light of this basic hypothesis that I want to evaluate developmental claims of the sort made here.

What does it mean for knowledge of language to be different from a stage  $i$  of infants' development to a stage  $k$  that we may call 'adult'? If our central hypothesis is correct, this must mean that either a change in the system of principles, or a change in the system of parameters, or both, must be allowed to

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happen from *l* to *k*. Needless to say, there are lower-level changes that range from the acquisition of vocabulary items to the mastery of language idioms and cliches that do not fall into the previous categories, at least not obviously. But as is commonly assumed, it is not clear that linguistics has anything interesting to say about the specifics of each of these cases, at least from the point of view of knowledge of language. A general theory of **learning**, in a strict sense now (as opposed to the 'learning' notion that we need, usually compared to **growing**) may be interested in these processes. Nevertheless, I will return in the end to one specific issue related to this matter.

Let us then concentrate on the acquisition of general properties of language. Consider first the issue of principles. These come genetically specified, which of course does not necessarily mean they do not develop. In principle, there could be several reasons for development to take place. First, it could be that the linguistic system is laid out that way. I will not explore this here, mainly because I don't know how to. Second, it could be that something in the (extra linguistic) cognitive systems affects the sequence of acquisition of the linguistic one. In this instance, two typical situations emerge: either stimulus independent maturation of the relevant cognitive domains, or changes propitiated by triggering input of different sorts, which may be available to the learner at different stages. However, the assumption that input data is not organized already discards the second of these possibilities. Even when it appears that the cause for a maturational change is the data in question, what is at issue is not the data, but the nature of the mind which is not capable (at a given stage) of processing these data.

In practice, we use working hypotheses of the sort: "access to principles is instantaneous and/or global, etc." I will assume that, more than just working hypotheses, these are the ones which make fewer assumptions about the language system. It seems to me complicated enough to design the language system for it to be sensitive to cognitive restrictions of the sort sketched above. At any rate, I will try to show that some of these adjustments are sound. To be precise, some of the papers of this conference in fact could be interpreted as presenting a challenge to the

instantaneous acquisition view--for instance, those offering arguments that some principles, such as statements making reference to functional categories, come to be available at a time  $t'$  considerably later than the time  $t$  when principles making reference to lexical categories are available. I will try to show, however, that under a plausible interpretation of these results the instantaneous acquisition (null) hypothesis may be kept. I don't know whether this is good or bad; but I do know that the other option needs further justification and evidence, and it is not clear to me that it is necessarily invoked.

Consider first what maturation's affecting the system of principles entails: (a) The developmental process in question must be universal; (b) the child at time  $t$  must not be able to do things that s/he does at time  $t'$ . This may seem trivial; I don't think it is: (a) entails that all of the observations having to do with processes of this sort changing from language to language (i.e., when the interval from  $t$  to  $t'$  is not constant across languages) have to be explained in terms of a further variable, which I will return to; (b) in turn entails not only that children are going to say different things at  $t$  and  $t'$ , but also, crucially, that they are going to **mean** different things.

Let me first explore (b). There is an old controversy about whether the mapping between the syntax and the semantics is transparent. I happen to believe that it is not; but even those of us that advocate strongly for the autonomy of syntax do want to have some fairly simple mapping between this and the semantics. If something is uncontroversial about this mapping it is that meaning is **somehow** compositional. A version of this within current standards is the Full Interpretation Principle (FIP). What FIP entails for LF is that everything that feeds this level is interpretable. We may even strengthen this to claiming that LF is interpreted in terms of everything that feeds LF (and only that). At any rate, nobody will deny that functional elements such as **INFLECTION** or **DETERMINER** have a rather precise use at LF in adult grammars. Here is the quandary then: if the child does not have these at time  $t$ , how does s/he express ideas that presuppose them?

One possible answer is, of course, "s/he doesn't." This has to be evaluated carefully, though, because functional elements are quite central to the

construction of many of our meaningful expressions. Thus, one may conceive that children do not have access to tense specifications or specificity restrictions; but that is not all we use functional elements for. Take any verb, and two parts within it are going to be clear: one, expressing a specific predicate, idiosyncratic to that verb; another one, expressing an arrangement of argument variables common to other verbs. The latter has been argued to be a functional element, as seems natural (there's a small, fixed set of these; they themselves do not contribute to lexical meaning, etc.). In fact, these days a tendency exists again to decompose even lexical items into more fundamental units, some of which (causative morphemes and the like) arguably fit into the functional paradigm.

If the distinction lexical/functional is going to make any sense (and it does seem to make good empirical predictions), it is going to fall, most likely, into one of two possibilities: (i) lexical elements are exclusively representing matters of predicate logic, matters of higher order logics being reserved for functional elements; (ii) lexical elements are lexicon entries with predicative import, functional elements being 'the rest'. It is easy to see that hypothesis (a) leaves the child at time *t* in the case in point as having only predicative structure. Furthermore, given that virtually all words (at least in a large sub-set of languages) contain both lexical and functional sub-parts, the child would be learning/using lexical items which do not correspond to adult lexical items--in fact contain only a subset of the specifications of the latter. Hypothesis (b), on the other hand, allows the child to learn certain complex units, so long as these are **lexicon** units. It predicts, however, that those words which are syntactically decomposable into (D-structure) elements coming **directly** from the lexicon are not going to be learned/used if **they contain a functional part**. These, in most languages, are far from exotic.<sup>1</sup>

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1. A typical example is causative elements in Basque, Japanese, etc. The relevant words have the import of "cause-V", with essentially any verb being attached to the causative morpheme. The child should not be able to learn these words if the morpheme is functional and s/he doesn't have access to functional elements. Crucially, these elements are different from non-productive forms which have the causality as part of their lexical meaning (e.g.,

The issue of ontogenesis is crucial within the approach I am taking (given that I do not want to ascribe the developmental process directly to properties of the language system). Hypothesis (a) above leads us into the direction of a computational speculation. One may suggest that, for whatever specific reason, the relations encoded by functional structure are harder to construct than the relations encoded by lexical structure--thus the former come in later, together intuitively with the maturation of other 'pseudo-mathematical' capacities. On the other hand, the other plausible approach to the hypothetical problem is that input lexical units are more 'visible' than functional ones--which we know is true even in the adult grammar. As has been proposed in the literature, it is not inconceivable that the child simply does not identify functional elements. Assuming that this triggering experience is necessary for the child to realize that such elements exist, not hearing these elements would result in not being able to use them.<sup>2</sup> This type of approach would be more in line with hypothesis (b) above, in as much as the morphological weight of functional elements appears to be language-specific. Since this hypothesis entails differences in the lexical/functional distinction that are not universal, I will deal with it below when I turn to parametric matters.

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"worry" arguably contains a causative element as part of its meaning). The latter, the child should be able to learn, assuming these are indeed lexical elements under the hypothesis in question.

2. Note incidentally that the latter assumption is far from trivial. On the one hand, we have to worry about languages where functional elements are not specified morphologically, hence are never audible. On the other, we have to worry about languages where functional elements enter virtually into all words, morphologically--the reverse instance. It does not seem to be the case that, in these languages, children drop those elements, like they appear to do in languages where the morphological presence of functional elements is sporadic. This difficulty is in line with the observed fact that, in some languages, processes of the sort of Verb-movement come in very early--thus, apparently Verb second appears virtually from scratch in German, though not in English. Assuming that movements of this sort presuppose the presence of functional categories (to serve as landing sites), this kind of evidence strongly suggests that the whole initial claim about functional elements must be framed in terms of the parameter system, and not the system of principles. For the sake of argument, though, I will proceed ignoring this sort of evidence.

Let us consider, then, whether a child without functional elements can communicate anything within hypothesis (a). Consider (1) below:

- (1) Sonnets are formal and meaningful
- i.  $\forall x S(x) \rightarrow F(x) \text{ and } M(x)$
  - ii.  $\exists P(S(x)) \quad P = F(x) + M(x)$

(1) is ambiguous. Under reading (i), (1) means that if something is a sonnet, it is formal and meaningful. Actually, I think the statement should be weaker; in particular, it is not clear that it is falsified if one sonnet, say, is not meaningful, a general property of generic statements--but I'll return to this shortly. Under reading (ii), (1) means that there is a partition in the set of sonnets,  $P(S(x))$ , whereby these are divided into those which are formal and those which are meaningful.<sup>3</sup>

Now, compare (1) to (2):

- (2) Successful formal and meaningful sonnet writer

In this expression I don't think that a (ii)-type reading is available. That "Shakespeare was a successful formal and meaningful sonnet writer" means that he wrote successfully sonnets which were both formal and meaningful--not that he wrote two types of sonnets. One of the properties of incorporation is that it appears to be restricted in terms of the functional/lexical distinction. Only functional elements incorporate to functional elements; only lexical elements incorporate to lexical elements. This means that whereas "sonnet", as used in (1), can be represented as "sonnet (x)" (not just logically, but also syntactically, with the variable being mapped from a functional element), "sonnet" as used in (2) cannot contain a functional part, therefore, by hypothesis, not a variable. In general, we do not appear to need variables for instances of noun incorporation; thus, we can call Jones "a partridge hunter" even if he has never hunted partridges.<sup>4</sup> "Partridge hunter" can be

3. This reading is highlighted in an example like "Americans are black and white".

4. The difference in question can be seen perhaps more clearly in (i):

- (i) a. Jones hunts partridges
- b. Jones hunts partridge

seen as a complex predicate, where "partridge" specifies the type of predicate "hunter" is, but does not invoke actual partridges. From this point of view, the reason why reading (ii) is lost in (2) is that this reading invokes a set-theoretic operation, one which is not available if to begin with we do not have a way of constructing a set "sonnet (x)" there.

This is not a traditional or even a standard view, but it has been explored in the last few years and I want to pursue it for the sake of argument. In general, elements like "sonnets" in (1) are analyzed set-theoretically: "the set of sonnets". But suppose there is a simpler way in which "sonnets"--or I should say, more precisely, "sonnet"--is interpretable. Think of an "ur" concept for the kind SONNET, which is a rigid designator (invariable across possible worlds).<sup>5</sup> One can think of names just this way: Jones may be represented as "Jones" (*j*), where *j* is not a variable but a constant index. (Literally, what Jones would mean is "that entity *j* labeled "Jones".) Let me call this the **naming** operation. (Of course, we can treat names set-theoretically. From this point of view, the

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The intuitive meaning of (ia) is that Jones hunts individuals belonging to the kind "partridges". In contrast, the intuitive meaning of (ib) is that Jones engages in an sub-case of the activity of hunting; namely, "partridge hunting". (The latter plausibly involves incorporation of "partridge" to "hunts", perhaps at LF--for some unclear reason, English, unlike other languages, does not allow verbs of the [N[V]] type at S-structure.) The object in the second instance cannot control into a secondary resultative predicate:

(ii) a. [Jones [[hunts partridges<sub>i</sub>] ] [PRO<sub>i</sub> dead]]]

b. \*[Jones [[hunts partridge<sub>i</sub>] ] [PRO<sub>i</sub> dead]]]

Assuming an analysis of the (b) instances in terms of incorporation, it is not clear why the trace of "partridge" cannot control PRO--syntactically, that is. This follows, though, if partridges are not invoked extensionally in these instances--not having reference, "partridge" cannot control PRO.

5. Actually, it has been argued that only natural kinds (like, say, "gold") are rigid designators, nominal kinds (like "bachelor") being amenable to a more traditional view that has elements of this sort be non-rigid. Presumably, "sonnet" is also a nominal kind. If the reader is thus unsatisfied with the argument, the example can be changed to "particles are weighable and measurable" (incidentally, a false statement at least on one interpretation), and "reliable weighable and measurable particle accelerator". I don't think there's a sense in which particle is a nominal kind--although this is debatable.



universe could be divided into those entities to which the predicate Jones applies (a singleton set, let us say), and those which do not satisfy said predicate (all other entities in the universe). I am assuming, however, that this set-theoretic distinction is not invoked by way of the naming operation.<sup>6</sup> It is not inconceivable that we can think of entities like sonnets in two ways: as members of the set of what we call sonnets, and as a conceptual unit labeled "sonnet", a kind. This would allow us to get a (i)-type reading for example (2), even if by hypothesis no variable--and hence no set--is at stake. To calculate the meaning of the expression, we need to further name the simple "sonnet" (j) as "formal-and-meaningful" ("sonnet" (j)). Adjectival operations of this sort could be treated by allowing recursive embedding of the naming operation.<sup>7</sup>

A further consequence of the availability of this reading is that there should be a meaning for (1) where the subject does not denote the set of sonnets--but rather a prototypical entity which is named "sonnet" (j). (In the relevant literature, kinds are specifically argued to be akin to names.) In that kind reading, there is no entailment that all sonnets must be formal and meaningful, which as I said seems correct. This is important because the relevant reading is generic, thus cannot be expressed in terms of the existential operator. Intuitively, if the subject were introduced by an existential, it would be true to say "sonnets are formal and meaningful" even if there is only one sonnet which is formal and meaningful--all the rest not being so. This does not seem to be an interpretation for this sentence, though. The point is that the relevant interpretation makes no reference to the existing sonnets, but simply to the basic concept SONNET, as mentioned also in expressions of the sort "the sonnet is formal and meaningful". If a given sonnet does not conform to this description, it will deviate in whatever sense from the prototypical concept--but will not render the statement false

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6. In fact, arguments have been provided that treating names as descriptions yields the wrong semantic import. I think there are fairly good reasons to be able (at least) to treat names as descriptions. However, for my argument to go through, all we need is that names need not be descriptors, but can be rigid designators.

7. This is most likely needed independently in expressions like John Smith, arguably of the form "John"("Smith"(j)).

because set membership was not at issue to begin with. In contrast, if only one sonnet is formal and meaningful, and the rest are not, we cannot give said properties to the prototypical sonnet, however it is that we come up with this notion (not set-theoretically if these ideas are correct).<sup>8</sup> All of this is to say that there are ways in which one can compute (some) meaning without access to, in this instance, variables. Under the assumption that these correlate with functional structure, we have in effect provided a mechanism to compute some expressions without functional structure. It doesn't take much thought to see, however, that the kinds of things we are going to be able to compute this way is rather limited. In essence, we will be able to name identified entities, and stack further names onto these named entities. Note the presupposition for this view is that prototypical "ur" concepts can be grasped rather freely, and can be added amongst them and applied to individuated entities by way of the naming labeling.<sup>9</sup> It is not trivial how exactly these additions, particularly of predicates (e.g. "formal and meaningful"), are going to proceed, especially after we

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8. Another typical instance where a kind reading ensues, and in fact no other reading as far as I can see, is what traditionally have been called 'impersonal constructions':

- (i) a. Man spricht hier Deutsch
- b. Ici on parle Francais
- c. Aqui se habla Espanol

It is rather unfortunate that these expressions are called impersonal, for they must necessarily invoke a human subject. At any rate, in each case the human kind here (wherever that might be) is said to speak such-and-such a language. This is true even if some individuals do not. (It is possible that the man and on (from the Latin homo 'man') markers that we see in these examples mean literally that, 'man'--in line with their etymology. In that case, we could represent the reading in question as "man"(i), with the usual sexist overtones...) Crucially, a partition reading is impossible in instances like (ii):

- (ii) a. Man spricht hier Deutsch und Swahili
- b. Ici on parle Francais et Swahili
- c. Aqui se habla Espanol y Swahili

Suppose that "here" is some country. What these sentences mean is that (prototypical) humans in this country speak both languages at issue, not that they divide in those who speak one or the other.

9. Semantics of the sort exploring cognitive primitives (such as "human" and "cause") could in principle provide an answer to this matter.

have devoided ourselves from set-theory, and hence functions. But one can imagine ways around this; for instance, that **fuzzy** concepts can be created for kinds which contain some properties of each of the **added** predicates, not necessarily all.<sup>10</sup>

In sum, the conceptual consequences of this move are vast. Some of these are maybe correct, and have been discussed. To invoke a deliberately provocative example--which as far as I know has not been mentioned in the relevant literature--recall experiments on dolphin communication. The ones I am vaguely familiar with had these animals being able to understand the equivalent of "red ball" or "small ball", but by no means the equivalent of "no ball" or "three balls". One can conjecture that although these mammals have somehow access to (some sort) of meaning computation of the sort outlined, they surely do not have access to set-theoretic computations.<sup>11</sup> To pursue the speculation, one wonders whether the genetic mutation that gave humans language is related to the quantum

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10. Needless to say, the consequences of this go way beyond the scope of this note. Personally, I am of the belief that the way humans conceptualize, say, "water" has nothing to do with a (realistic) definition of this concept, but rather with (fuzzy) bundles of properties that concern humans. That, of course, is hard to establish--nevertheless is intuitively rather clear. For me to call something "water", I am going to worry about whether it is something tangible, it tastes/smells/looks in such-and-such a way, and so on, rather than about whether it has this or the other atomic structure, or whatever other analytic approach of the sort that worries philosophers.

11. Apparently, dolphins could also understand "every ball". It is not clear to me, though, that this is necessarily a quantificational reading. It could well be that the animal understands, say, "bring ball", and keeps bringing these objects until there's nothing else to bring. I don't know whether the experiments were careful enough to test this. One can imagine, also, that these animals could get some form of "some ball", by interpreting "bring ball" as a command to bring something which is a ball. However, the minute one goes to generalized quantifiers, such as "many balls", or "most balls", or any numerals, problems would immediately arise if no set theory is available. I would like to know, finally, whether dolphins and so on can compute expressions like "red small ball", and if so whether they interpret them as "red and small ball" or as "red [small ball]"; the latter can be tested by making them interpret "large small ball", which would be contradictory if one does not have access to a recursive operation.

leap that entails having access to recursion, with the possible consequence, at least, of getting Set Theory as a result.

Speculations aside, I went through the trouble of suggesting a way to compute sheer predicates (by "sheer" I mean predicates that stand out there as concepts, not as functions defining sets) to point in the direction of what I would like to see theories do that claim that functional elements are not available to infants: can we then be shown how it is that these infants compute meaning? If my suggestions are on the right track, do children, for instance, start by naming entities, and then associate them to predicates, before they go into more elaborate constructions?<sup>12</sup> And so on. My very vague knowledge of these matters tells me that perhaps children do follow a sequence along these lines, but I won't venture to speculate anything in this respect. At any rate, it should be obvious that, under any hypothesis about functional categories that relates these bijectively to set-theoretic operations, if the former are missing, the latter should be too. And it seems to me that this is worth exploring, if only to falsify either the psycholinguistic or the syntactic hypothesis or possibly both.

I should say, though, that any radical version of the syntactic hypothesis just mentioned is bound to create all sorts of psycholinguistic nightmares. Take a difficult case. A verb like "frighten" is arguably represented in L(exical)-C(onceptual) structure as: CAUSE(x, (BE FEARED (x by y))). Not only are abstract morphemes and variables needed here, but furthermore the language learner must find out that two variables in the LC structure of this predicate are identical (the cause and the theme refer to the same individual). What does a child without Set Theory learn when faced with "frighten"? Does s/he only get a non-causative reading (available in instances like "unicorns frighten Bill")? Again, all of this is testable; for instance, a child without a causative reading for "frighten" should not be able to interpret "Bill was frightened by Mary" or "Mary frightened Bill to steal his lunch". In any case, if things are the way I presented them, then we would be really talking about maturation in a rightful sense. Thus the process should appear in all children roughly around the same age, variation being

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12. Or: Can they interpret "large small ball"? (See fn. 11.)

possible only within individuals, and not within language groups. (As I said in fn.2, this seems unlikely--more importantly, though, it is a falsifiable claim in rather non-trivial ways.)

A milder version of the lexical/functional split, hypothesis (b) above, moves us directly into the issue of Parameter Theory. It is plainly obvious that different languages have different lexical space for these distinctions. Where a language like Haitian uses two verbs to express the event of an exchange of an object *x* from a given (not necessarily specified) location to a location *y* caused by an individual *z*, English uses the ditransitive "give". Therefore the set of lexical categories of English in this view is smaller for this subpart of the language than in Haitian. The question is then: are Haitian children slower in picking up the relevant distinctions than English children are? If they are, then this could be interesting evidence for the hypothesis in question; I suspect, though, that matters are a bit more complex, to which I now turn.

The real issue is whether the set of hypothesized functional categories (in terms of (a) or (b) above or any other reasonable hypothesis) coincides with the set of elements that children allegedly do not have in a given language. Suppose it doesn't. However, suppose that the last of these sets in fact is syntactically definable in a plausible way. In other words, is a sub-set of the set of functional categories, such that "so-and-so", where the latter is some kind of syntactic property. It is conceivable that "so-and-so" is hard to learn; in fact, typical if we are dealing with a Subset Principle instance. It could be thus argued that if "so-and-so" corresponds to the marked option, it takes longer to learn it if the necessary input data is exotic or hard to process. As has often been argued, the former should be irrelevant, short of allowing the possibility of coming up with potentially different adult grammars--but the latter, as has been noted, is possible. (Note also that if the issue were one of exotic data, maturation would have nothing to do with the matter.) In this instance, we may expect variation not just within individuals, but also within language groups, obviously.

There is one kind of parameter, however, which cannot have anything to do with developmental matters: what we may call 'underspecification' parameters.

These have really no status within UG, but are merely 'shortcuts' that this system takes. Head position is an example that has been mentioned in this respect. It is entirely plausible that UG simply has nothing to say about linearity of heads/complements, and these get linearized one way or another because of the nature of PF.<sup>13</sup> Since this type of parameter is not part of UG, we cannot make reference to it. One cannot grow into its specifications simply because there is no state to achieve. At best, some additional process might obscure access to the arbitrary setting that these parameters take. This, however, seems highly unlikely for this kind of parameter. If this were true, there would be a stage where children, say, have both options for head position open (there is no unmarked option that UG provides). In fact, such stages do not seem to be attested.

There are probably good reasons for that. I am assuming here that linguistic structures are, to use a term with a precise meaning in Physics, 'chiral'. That is, just like sub-atomic particles move in one arbitrary direction, so too heads and dependents must be fixed in an arbitrary direction--it doesn't matter which, so long as this 'handedness' is kept throughout the system. As a matter of fact, I will assume that 'chirality' is a property of any system that entails (even very abstract) communication, hence not part of the linguistic system.<sup>14</sup> But if setting this parameter is so central to the "chirality" of the system, it simply is unlikely that the necessary information to set it is hard to process. At any rate, the situation contrasts drastically with Sub-set parameters, where not just the parameter, but even the unmarked option comes specified within UG by the Theory of Markedness.

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13. There are fairly elaborate studies on the setting of parameters of the Sub-set type, showing evidence that children start with the unmarked option, as expected. No evidence, as far as I know, has ever been presented that 'head-last' or 'head-first' is the unmarked option. It seems very likely, thus, that neither is, which is what is predicted if this is a sort of epiphenomenological parameter.

14. If different sub-atomic particles didn't agree on whether to move right or left, fields simply couldn't exist; likewise, if two observers in outer space do not agree on what right and left or up and down are, they couldn't communicate anything about the universe they observe.

In real life, parameters can be extremely intricate, and can deceive one into thinking that a maturational issue is at stake. Consider a kind of parameter that does not get much attention lately, but was discussed a few years ago: a 'domain specific' parameter--as opposed to a 'global' one that cuts across the whole language.<sup>15</sup> Take for instance clitics/agreement-markers in different languages. There is a vast range of variation here in terms of whether a given argument is doubled, and if so whether it is doubled by agreement (intuitively, a clitic encoded into the morphology) or by a clitic. Well known implications emerge. For instance, no language with object doubling lacks subject doubling; however, the converse isn't true: there are languages with subject doubling that lack object doubling. Furthermore, if the doubling in question is of the agreement type for the object, then so it must be for the subject; again, the converse not being true. Differences are even more subtle than this, with implications also holding internal to objects, whether they are direct or indirect, and even internal to arguments, whether they are pronominal or full. Suppose that we are talking here about a number  $n$  of variations; for binary parameters, this means we should get 2 to the  $n$ th square variations. In fact we get much less than that. As is well known, also, we cannot reduce this number by linking parameters logically, under the well motivated assumption that these are all independent.

However, suppose that parameters with the following format are possible: property P does/not hold (in domain  $d$ ), for  $d$  a variable ranging over relevant syntactic domains. And now imagine that two given values of  $d$  are logically linked as a matter of UG. Take the concrete case I have mentioned. Let it be the case that internal arguments do not enter into agreement relations, but externalized arguments may. Imagine then a language that forces LC THEME arguments to be externalized in the syntax (say, Basque). This means that for any argument higher in the Thematic Hierarchy within this language, said argument must be externalized. LC restrictions, together with the parametric option of externalizing a 'low' argument, have as a logical consequence the externalization of a

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15. This type of parameter can arguably be either of the Subset or the Underspecification type.

'higher' argument. The converse, of course, isn't true. Thus, for instance, for a language that forces LC BENEFICIARY arguments to be externalized in the syntax (e.g., Galician), it does not follow that THEME arguments must be externalized. In effect, there is an implication that UG introduces once we affect the structures it generates by a parametric decision. Crucial for this is that the parameter in question not be a global one of the sort: "Do/not externalize all arguments." Such a parameter would be immune to implications within the argument system in terms of internal/external specifications. Within this view nothing about the parameter system is implicational.

Typically, language variation and language change happens within this sort of parameter. There are well-known 'cycles' in this. At a given diachronical stage, language L does not have any marks of externalization for its arguments in terms of agreement (like Black English). Dislocation strategies allow displaced arguments to bind pronouns: "John, Mary thinks he left". Some of these get to be systematic even in short distance cases: "John, he left". Some of the pronouns in these constructions cliticize onto the verb, becoming obligatory (this is typical in many English creoles). At a given point, clitics may get integrated into the morphology of the verb, and an inflectional variant arises--sometimes as a consequence of verb movement, which places the pronoun in a suffix position. Fully inflected verbs, though, are subject to morpho/phonological changes (like the stress retraction suffered by Middle English); this may result in the loss of agreement morphemes. And we are back where we started. These cycles, in turn, are restricted by properties of UG, as seen above. Thus, the prediction is that if a language accepts a verb-object relation "like-her Mary" it is because it has already accepted a verb-subject relation "John he-like...".

Now, consider a language where the adult grammar says "thou criest" and "thou seest-her Mary", but not \*"thou cry" and \*"thou see(st) Mary". Suppose that children learning this language at stage *i* have no access to either clitics or affixes, which is tested independently of these constructions by them not being able to say, for instance, "chickens" (they say \*"chicken"), or "John's nice" (they say \*"John nice"). Then at time *i'*, children start getting clitics (they begin to say "John's nice"), but suppose they do not still get affixes (they still say \*"chicken" with the



meaning of "chickens"). At /' the following data would be surprising: they say "thou criest". In this hypothetical example, a fair question is: has there been development from \*"cry" to "criest" between / and /'? At first sight it would seem so: children reach "criest" without an apparent change in the perceptual mechanism that would allow them to get "chickens" correctly. But another variable may be invoked: There is a linguistic implication such that if a language has a direct object clitic, then it must have a subject clitic. The minute the child realizes s/he has a direct object clitic, s/he is forced by UG into positing a subject clitic. The agreement marker is a version of this. In a sense, the child could afford not to 'worry' about affixes corresponding to subject position until s/he decided to worry about clitics corresponding to object position.

In as much as these are typical domains of language variation and change, the child must also be able to chose 'wrong' parametric options of the adult Core Grammar and stick to them. Whether the parameters that allow this are of the Underspecification or the Subset type is a complex matter I do not want to go into here. The point is that real life parametric situations may be extremely deceiving for the issue of development, in as much as internal properties of the grammar may force spectacular changes that have nothing to do with development, at least in intermediate stages. These are precisely the interesting ones, in as much as they follow indirectly from triggering evidence in a different, grammatically related domain. These matters are more deceiving with Domain Specific parameters than otherwise, for in these, properties are sequentially connected. That is, for a Global parameter, one expects a set of changes to happen at the same time, at the moment the parameter is set. These can be spectacular also, but they should appear at the same point, and hence can be seen as a typical parametric situation. However, with Domain Specific parameters, first one sees less global changes (so an immediate parametric approach cannot be taken by the observer); and second one may see changes in sequence, which is of course tempting to interpret developmentally.

In any case, I have suggested that development could be at stake only when the Theory of Markedness is involved. To recast the point: the child would be maturing in the sense that s/he is led into the

unmarked form because of not being able to perceive the triggering evidence setting the marked one. One prediction of this approach is that those domains in which a developmental issue arises should coincide with **marked** parametric settings. Properties at this level can be fairly abstract, and we may deceive ourselves into thinking that a language doesn't have them, simply because they are not obvious. A case in point is long distance Wh-movement. Children of English, French, and Spanish were shown in this conference to be taking a route for these questions which is clear in German and other languages, but not apparently in the former. It has been proposed, however, that in fact this strategy is indeed used also by adult grammars, although this is far from obvious and needs to be argued for. At any rate, even if arguments to this effect didn't exist, the fact that the option that children take is possible in a given language (not necessarily their own) is enough to begin to sketch an account so long as it is assumed that the property in question is unmarked.

What should be clear is that if a child does something which **no adult does systematically**, then a parametric account cannot be at stake. This means that the only other option is for the account to be saying something about the system of principles, perhaps interacting with other properties--the latter being parametric. The matter of the universality of the phenomenon in question is then at stake, and the less variation in terms of **when** it occurs the better for this approach. Ideally, the variation should be none; if it isn't, it must be explained in terms of consequences of the parametric options taken by a given language. This would be the clearest instance of a maturational process. Research in this sense is always interesting, for it may suggest changes that we theoreticians need to make in our model of UG, or may confirm hypotheses which are not otherwise completely obvious--even if this entails that maturation is not necessarily happening, but the facts are simply clearer in child grammars. Thus, for instance, research that claims children do not invoke traces in event adjuncts strengthens the position in the literature that this is true in the adult grammar. Theoretical arguments, however, had scarce, non-theory-internal evidence.<sup>16</sup>

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16. It should be obvious that this is precisely the type of account that cannot be parametric: we do not expect some languages to license adjunct variables in terms of traces, and some others to

The second thing that should be clear is that a child cannot do something that **no adult does at all**. One thing is to say that the system of principles is articulated enough to have two degrees of complexity, and one does not go from level-1 to level-2 until a given state in the general development of the mind is reached (the change has to come externally to the language system). Another very different thing is to say that the language faculty is such that at the growing period it has (positive) properties that it loses at the adult period. There may indeed be properties that are of this sort; for instance, it is not inconceivable that the phonetic system of a babbling child is richer than that of an adult, fixed for a given language (there may be certain sounds that the adult 'cannot' do without retraining). But crucially, the adult set of properties is a sub-set of the children's set of properties (if you wish, open parametric options). This is completely different from one of the examples I gave some attention to: a language that contains both lexical and functional elements is obviously larger than one that contains only lexical ones--which is a sub-set of the adult language. Therefore, no hypothesized property of this sort can be such that it does not show up in some form in the adult grammar.<sup>17</sup> Furthermore, if the claim for lack of (certain) functional categories is to be explained in terms of the Theory of Markedness, it must be that the option of having these categories is marked. Personally, I find this unlikely--though it is in line with claims about languages of the Asian type lacking these elements altogether.

Any claims for 'missing links' between adults and children, no matter how plausible, have to address the issue of how to be incorporated into our general theories of language and learning. In general, puzzling psycholinguistic evidence necessarily tells us something either about UG or about the process of language acquisition (assuming, of course, it doesn't tell us anything about a faulty experiment). Here, I have outlined some of the possible questions that the adduced evidence may raise for UG. Assuming that the studies in point address the issues I raised, their

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license them in terms of, say, event variables.

17. The readings of incorporated nouns may be an example of a "child-like" property of adult grammars, if my sketchy analysis was correct.

consequences are potentially very important. For instance, they would argue for the lexical/functional distinction; further for differences in complexity (semantic, phonological) between these; or for the existence of a kind of parameter which is not epiphenomenological (although they would not argue against such parameters) and further is described by the Sub-set Principle.

An important question that has to be addressed, and according to my initial presuppositions cannot be dealt with in this light, is: how is 'unlearning' possible? This is particularly relevant, for the logic of the approach forces us to take answers that in fact explain away these issues. Thus, for instance, it has been noted in this conference that children who do not know that the locative argument of "put" is subcategorized treat it like an adjunct in a wide variety of contexts. This, presumably, is not done by any adult grammar. Is the child then violating the fundamental Axiom of **no properties which adults lack**?

In this particular example, I think not. The child is making an assumption that is coherent with adult grammars: given sub-categorizations, though a function of semantic selection, are nevertheless specific to predicates within that range. Thus, there does not seem to be anything particularly principled about not having a verb **"to be in hate"** alongside **"to be in love"**, or to allow a causative alternation for **"the horse jumped"** vs. **"John jumped the horse"** but not for **"the horse laughed"** vs. **"John laughed the horse"**. These matters may have to do with knowledge of the world, and so on--but knowledge of the world has to be learned, this time using **"learn"** in a strict sense. Therefore, it should not be surprising if--and has been noted that--children utter expressions like **"John laughed the horse"** to mean **"John made the horse laugh"**, and furthermore if this sentence in the child grammar had 'correct' adult grammar properties to it. (Indeed, the opposite case would be surprising for the latter.) If the use of **"laugh"** in question gets to be dropped when the child becomes a small adult, no 'unlearning' of the type that troubles most of us here took place. Simply, a possible region of the lexical map is not encoded, and is erased from long-term memory.

In that sense, the whole process has to do with those properties of language that I started setting aside, arguing that linguistics has little to say about

them. What I do not know is whether all instances of apparent unlearning can be explained away. Otherwise, they would be real counterexamples to the approach I sketched here, and may in fact be used to argue for a developmental system that has UG itself (not just outside cognitive systems) follow a given path--of the sort I did not know how to explore. This path would include going from impossible adult properties to attested ones; I am very skeptical, but I see no reason in principle why humans shouldn't lose properties when growing. In any case, this would be the strongest kind of argument for the view in question; the alternative one (where the matter is one of gaining properties as we grow up), can always be expressed in terms of instantaneous learning hypotheses plus cognitive developments which are obvious anyway.

To summarize, I have used a now traditional set of assumptions about human grammars to constrain the possible developmental properties of these. If these assumptions are correct, maturation is only possible within the system of principles as a whole, or within Subset-type parameters. In both instances, I have assumed that the development in question is extra-linguistic: something in the cognitive capacity of children a time *t* either disallows them to have access to a sub-set of principles, or to marked parametric options. In fact, the latter instance is arguably connected to perceptual development of the sort needed to process complex data. As for the former, it could either be a perceptual or an internal computational limitation. The tacit assumption in both instances is that if the Language Acquisition Device were 'fixed' with respect to the 'problems' in question, it would acquire the relevant aspect of language immediately. In a nutshell, I haven't really said anything new--but then again I'm not sure there's anything new to say.