The Pace of Syntactic Acquisition
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How fast does syntax develop? Traditional accounts suggest that it begins at about age one year and may not be complete until as late as age eight or nine, although by age four or five the bulk of syntax has been acquired. Reasons for proposing such an early start to the process (at a stage when the child can produce no more than single words) revolve around what Pinker (1984) called the 'continuity assumption', which regards it as more parsimonious to suppose that the child acquires syntax from the beginning with an unchanging mechanism, rather than commencing with one type of mechanism and switching to another (a claim implicit in the 'pivot' grammar of Braine 1963 and the 'maturational' claims of Borer and Wexler 1987, among others). Reasons for setting such a late date for the end of the process involve an obvious confusion between 'language' and 'a language' (see Bickerton MS). It may well take a child until nine or later to master all the finer details of English syntax, or the syntax of any other specific language. However, it can equally well be argued that the child first produces a general form of language which, if no well-formed language provides a lexicon for it, becomes the child's native language; otherwise this general variety of language is subsequently remodelled in the direction of the well-formed target model (Bickerton 1981, 1984).

Thus the process of syntactic acquisition is generally seen as occupying a period of several years. Moreover, during that period no particular variations of pace are predicted by any current theory, whether nativistic or learning-oriented. Indeed, the measures of linguistic development most commonly used directly reinforce this picture of gradual, even long-term development. The most widely used indicator is Mean Length of Utterance (MLU), a measure of development pioneered by Brown (1973) which divides the number of morphemes uttered in a given period by the number of complete utterances. For most children MLU increases smoothly and monotonically throughout the acquisition period with only minor and erratic perturbations which are easily attributable to extralinguistic factors (Brown explained the one blip on Eve's otherwise straightline graph by the fact that she had a cold on the occasion in question). There can be no doubt that MLU serves as an easily-computable figure by which children's overall progress may be readily compared. However, it has several problems as a measure of syntactic development.

In the first place it is a measure of performance, not of competence. If we want to know what are the most complex sentence structures available to a child at a given time, there is no way in which MLU can tell us. But it is competence, not performance, that we need to access in some way if we are to determine how far and how fast syntactic acquisition is progressing at any given stage. Then, quite apart from general competence-performance considerations, there are some purely pragmatic, situational factors that serve to depress MLU scores and therefore render them still less accurate as indices of syntactic attainment. Two of these will be discussed here.

First, there are in child-caregiver interaction a number of situations in which short, indeed one-word utterances are strongly favored, if not made unavoidable. One of the commonest of these is that in which the caregiver shows the child an

illustrated book containing pictures of animals. In theory it would be possible for the child to say things like 'That is a giraffe' or 'The other one is a bear'; in practice, the child says (as would many adults in similar circumstances) 'Giraffe!' 'Bear!'

Then there is code-switching. No sooner has a child evolved out of one linguistic stage (one-word, two-word, or whatever) than that stage becomes available for code-switching. The motivation for such switching is interesting. A good diary which records the linguistic and social contexts of child utterances as well as the utterances themselves will show that frequently, when the child is tired, unwell, or frustrated in some way, (s)he will revert to utterances typical of an earlier stage of development. The motivation for this seems clear; by reverting to a more primitive form of speech, the child symbolizes a reversion to a more immature and dependent state. The subtext of one- and two-word utterances in such a context is 'I'm only a helpless baby, please look after me like you did when I was small'.

For these and similar reasons MLU cannot measure or compare levels of syntactic development attained. An alternative measure also proposed but little used in Brown (1973), longest utterance, is little better, insofar as there is no necessary connection between the length of a sentence and its complexity. Moreover, one feature of evolving syntax is the capacity to produce an ever wider variety of structures. The fact that a child can produce structures of up to six words in length does not distinguish between the child who has produced only one type of structure six words long and the child who has produced several different types of similar length.

An alternative possibility, if one harder to quantify, would be to take the most complex sentence from each week of development. The measure of complexity used here involves only the simplest and most theory-neutral assumptions: that subject predicate utterances are more complex than mere predicates, that simple sentences with two objects, or an object plus prepositional phrasae, are more complex than single-object sentences, that a sentence with two clauses is more complex than a sentence with one, and so forth. Since this requires at least a weekly record, it is a method which cannot be used on those longitudinal studies (by far the commonest nowadays) which sample the subject's speech at fortnightly, three-weekly or monthly intervals. Even studies which take weekly samples are dubious as sources, since there is no reason to suppose that the half-hour or hour that is electronically recorded in each week constitutes a valid reflection of the extent to which syntax has developed.

This is because complex sentences will not be uttered unless two conditions are met. First, the child has to have an appropriate vocabulary. For instance, factive complement clauses cannot be produced until the child has acquired verbs such as *know* or *tell* that subcategorize for such complements. Two year olds tend to know few such verbs. Second, the child must have some pragmatic motivation for using complex sentences; they will not just emerge automatically, of their own accord. Two year olds tend to have few communicative needs that require complex sentences, so initially, even when the child is obviously capable of producing them, they tend to be few in number and therefore statistically unlikely to appear in samples that may cover no more than a hundredth part of the child's waking week. It is true that the alternative method of data collection (daily noting of child utterances by a parent or other caregiver) is far from foolproof and may be biased by all kinds of irrelevant preoccupations on the diarist's part. However, there does seem to be a general bias towards recording the most 'interesting' utterances, and these are likely to include the more complex.

For this reason, two diaries were studied: that kept by Leopold (1939-49) on his daughter Hildegarde, and an unpublished diary kept by Robert Willson, then a graduate student at the University of Hawaii, on his son Seth. The results of this study are given in Tables 1 and 2 respectively.

1;10.0	ride papa's neck	VERB + OBJ
1;10.1	mama wake up	
1;10.2	my buggy way down	
1;10.3	this my rock-baby	
1;11.0	dolly ride buggy	
1;11.1	where my ball	SUBJ (+ VERB) + OBJ
1;11.2	papa forget this	(8 weeks)
1;11.3	watch bake cake mama (???)	
2;0.0*	NO DATA	
2;0.1	mama bite Haita	
2;0.2	Leona put water a my hair	
2;0.3	mama, scratch my back	
2;1.0	NO DATA	
2;1.1	I put my hat my room**	
2;1.2	you watch me open sandbox***	
2;1.3	papa, you like this song?	
2;2.0	you got my shoe out	SUBJ + VERB + OBJ (+ PP/OBJ2)
2;2.1	I want drink of water out my glass	(13 weeks)
2;2.2	I make cocoa all gone	
2;2.3	I go at Milwaukee	
2;3.0	will you hold my clock?	
2;3.1	I got wehweh at my eye	
2;3.2	I won't bump that	
2;3.3	you go up here like I did	COMPARATIVE CLAUSE
2;4.0	I won't cry a mama wash my hair	TIME CLAUSE
2;4.1	that's the way I got my apron out	RELATIVE CLAUSE
2;4.2	mama put all my animals away because I broke them	CAUSATIVE CLAUSE
2;4.3	put this right here so I see it better	RESULT CLAUSE
2;5.0	I think I must cut this out	EMBEDDED FACTIVE

* Hildegard's second birthday was August 3rd, 1932; diary entries for several weeks after that date are patchy due to absence of father.
**Examples for weeks 2;1.1 and 2;1.2 are both contained in an entry dated August 22nd but covering events as far back as August 11th.
***This sentence is probably formulaic: L cites only two other examples, both with watch.

Table 1: Hildegard Leopold, most complex sentences, 1;11.0 to 2;5.1

1;3.3	shit	
1;4.0	night night	
1;4.1	chi	
1;4.2	NO DATA	ONE WORD
1;4.3	lalayt	(7 weeks)
1;5.0	chichi	
1;5.1	'i'i	
1;5.2	N ball	
1;5.3	N ball	
1;6.0	N tree	
1;6.1	N Zack	X + ONE WORD
1;6.2	Ng Teddy	(7 weeks)
1;6.3	N trunk	
1;7.0	O shower	
1;7.1	a swing high	
1;7.2	Ng see me	
1;7.3	Ng dry	
1;8.0	Ng close it	
1;8.1	Thank you ball	
1;8.2	N clothes on	
1;8.3	N come on	
1;9.0	Ride a car	
1;9.1	Bounce a trampoline	VERB + OBJ/ADV
1;9.2	Turn da light	(19 weeks)
1;9.3	Down da slide	
1;10.0	Medicine a bottle	
1;10.1	N jump down stairs	
1;10.2	O sit ta potty seat	
1;10.3	O talk a phone	
1;11.0	O take a off	
1;11.1	N go swimming a water	
1;11.2	N push a high chair back	
1;11.3	N talk a phone	
2;0.0	Daddy open door	
2;0.1	Daddy close da gate	SUBJ + VERB + OBJ/ADV
2;0.2	Daddy tooting a lot	
2;0.3	Help me push a play button	INFINITIVAL CLAUSE
2;1.0	I toldja put the flower in a vase	EMBEDDED COMMAND
2;1.1	Didja sit down tray a give me a little pudding	CONJOINED CLAUSES?
2;1.2	a time to go night-night and push the light	CONJOINED INFINITIVAL RELATIVE CLAUSES
2;1.3	I want take yi nother one	INFINITIVAL CLAUSE

Table 2: Seth Wilson, most complex sentences, 1;3.3 to 2;1.3

The overall patterns of the tables are strikingly similar. Both children exhibit relatively slow development up to and including the appearance of single-clause sentences; then suddenly there is an explosion of longer sentences, including a wide variety of types, which takes place within the span of four or five weeks. In

both cases, this explosion takes place early in the third year; note that it involves simultaneous changes in the length, complexity and sheer variety of sentence types. (For more detailed analysis of the diary data, see Bickerton MS). The suddenness with which this developmental acceleration begins suggests that, pace adherents of the 'continuity assumption', some qualitative change in competence takes place at or shortly after the end of the second year.

However, the acceleration phase is much clearer in Seth's case than it is in Hildegard's. Seth goes from predicate-only utterances through very primitive monoclausals to his first complex sentences in a bare month, while Hildegard has five months of monoclausals, some of them quite complex in form (e.g. *I want drink of water out my glass* at 2;2.1) prior to the emergence of biclausals.

What are we to make of such differences? Are we to assume that some shift in syntactic capacity is tied to the emergence of biclausals? Or is it the case that a full syntactic capacity matures within the monoclausal period but that, for reasons yet to be determined, the appearance of biclausals is still delayed? Could the whole apparent phenomenon be one which simply dissolves when one looks at it more closely? The record in Leopold (1939-49) is not rich enough to answer these questions, while Seth's record, though rich enough, shows such rapid development that the relevant questions cannot be posed, let alone answered, on the basis of such data. To answer them, we require an adequately rich record of a child who, like Hildegard, spent a relatively long period developing monoclausal sentences before the first clear cases of biclausals made their appearance.

Fortunately such a record became available in the unpublished diary kept during the late seventies by Richard Brislin on his daughter Cheryl as part of a larger investigation organized by Susan Braunwald[1]. Brislin, an internationally known scholar in the field of cross-cultural relations, was primarily interested in the development of Cheryl's communicative competence rather than in syntax, but his practice of noting at least one, and often three or more, of Cheryl's utterances each day yields between 70 and 100 utterances per month, a total large enough to carry out a quantitative analysis with some degree of reliability.

Cheryl Brislin had the further advantage, from a research standpoint, of being free from factors that (it could be argued) might have affected the development of Hildegard and Seth in ways that would make them unrepresentative of children as a whole. Hildegarde was bilingual in English and German (although, as Table 1 indicates, she was heavily English dominant during the period in question); Seth was totally blind at birth and remained heavily sight-impaired despite the achievement of some degree of peripheral vision. Bilingualism and blindness could perhaps have served in some way to sharpen their attention to linguistic form and thus led them to an earlier-than-normal development. But Cheryl Brislin was a normal, healthy monolingual. Moreover, like Hildegard, she was one who developed monoclausal sentences over quite a long period before the emergence of biclausals (around 2;5-2;6). Prior to that period, there do emerge apparent biclausal sentences with want, but it is far from clear that *want* is analyzed as a full verb, rather tahn a modal, and there are none of the other types of bicaluseal that appear in Hildegards's and Seth's developmental bursts.

Accordingly it seemed desirable to apply to Cheryl's utterances some measure(s) that, in the absence of biclausals, would indicate whether or not there was any significant change in the complexity of those utterances over the months

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[1]I am extremely grateful to Dr. Brislin, presently of the East West Center, Hawaii, for making this diary available to me.

that preceded the emergence of biclausals. For reasons given, MLU was an inappropriate measure, but it is of interest to record the development of Cheryl's MLU if only as an object of comparison with the results of other measures.

1;10	1.77
1;11	1.83 + 3.4%
2;0	2.15 + 17.5%
2;1	2.63 + 22.3%
2;2	2.94 + 11.8%
2:3	2.93 - 0.03%

Table 3: MLU for Cheryl, by month (plus monthly percentage gain)

Table 3 gives MLU figures for a six-month period together with the increase in MLU from one month to the next as a percentage of the previous month's MLU. As we see, there is nothing astonishing here: Cheryl's MLU climbs quickly, more so in the middle of the period than at either end, but without any clearly marked change in pace.

However, one alternative measure would be to look at whether there is anywhere any significant increase in the number of structural types that Cheryl produces. If indeed some dramatic increment in syntactic capacity occurs early in the third year. one would predict that an initially small number of syntactic types would increase quite suddenly. For the purpose of this and indeed all the other measures decribed in this paper, types of syntactic structure were analyzed in the most theory-neutral way possible. Each utterance was treated simply as a combination of Noun (N), Verb (V), Adverb (Adv), Adjective (Adj), Determiner (Det), Deictic (Deic -- *here*, *there* etc.), Negative (Neg), Copula (Cop) and so on. Thus, for instance, *see daddy('s) car* would be V/Poss/N, *I want drink* would be N/V/N, *there('s) dolly* would be Deic/N, *no want more* would be Neg/V/Adv and so on, each being taken as a representative of a different syntactic type.

Type inventory	1;10	1;11	2;0	2;1	2;1	2;3
2-w	8	9	9	12	17	20
3-w	4	4	7	17	22	29
4-w	2	2	6	17	28	40
5+				8	24	30
Total	14	15	22	54	91	119
Percentage Change		+7%	+47%	+146%	+69%	31%

Table4: Cheryl's range of structural types

The results of analyzing Cheryl's utterances in this way are given in Table 4. A basic assumption in this table is that when Cheryl has produced a form, she has mastered it, and if it does not reappear in subsequent months, this is merely an accident of the sample. The assumption is probably too strong insofar as some forms may be deliberately excluded; in other words, if a subjectless form used at 1;10 is not repeated at 2;3, this is quite likely because Cheryl has recognised that

such forms are not in English. However, totals of structures that are actually used in any month, to be found in Table 6a, surely underestimate the range of structures that Cheryl commands at any given time. Since any estimate of the structures that Cheryl may have 'discarded' in this way is unavoidably subjective, we can only assume that the number of structures commanded in any given month lies somewhere between the totals of Table 4 and Table 6a, probably lying much closer to those of Table 4 (since Cheryl's output yields very few unEnglish structures).

As will be seen from the table, the structures available to Cheryl, which change little over the first three months of the record, increase rapidly in the fourth month and continue to increase, if at a slightly slower pace, over the next two months. Equally striking are the figures based on the ratio of structural types to structural tokens. If Cheryl is undergoing some dramatic change in syntactic competence around age 2;1, one would expect to see this reflected, not only by the overall number of structural types that she produces, but also by a growing flexibility in her choice of structures. This is a more conservative measure than raw structural-type increase, since only those structural types actually occuring in each month's data.

1;10;0	1-w	35	na	1;11;0	1-w	24	na
	2-w	31/8	26%		2-w	37/8	22%
	3-w	13/4	31%		3-w	8/4	50%
	4-w	2/2	100%		4-w	2/1	50%
Total		81/14	17%	Total		71/13	18%
				Increase			+6%
2;0;0	1-w	24	na	2;1;0	1-w	20	na
	2-w	39/8	20%		2-w	24/10	42%
	3-w	12/6	50%		3-w	28/13	46%
	4-w	11/5	45%		4-w	16/13	81%
	5+	1/1	na		5+	9/8	89%
Total		87/20	23%	Total		97/42	45%
Increase			+28%	Increase			+96%
2;0;0	1-w	10	na	2;3;0	1-w	13	na
	2-w	18/12	66%		2-w	17/11	61%
	3-w	19/11	58%		3-w	32/17	53%
	4-w	22/17	77%		4-w	19/15	79%
	5+	17/17	100%		5+	8/7	56%
Total		86/57	66%	Total		89/50	56%
Increase			+47%	Increase			-15%

Table 5: Type-token ratios for Cheryl (by month and sentence length: 1-w = one-word utterances; 5+ = utterances of five or more words)

As will be seen, the type-token ratio, which remains below the 20% level for the first two months, barely passes it in the third month, but in the fourth month rises to 45%. In other words, at 2;1 Cheryl shifts from using a very limited number of structures over and over again to using a variety of structures which are relatively seldom repeated.

In Figure 1 (next page), we can compare these two measures -- raw type increase and type-token ratio -- with the two conventional measures of syntactic development, MLU and maximum length of sentence. The two conventional measures show a slow and relatively even increase over the six-month period; the two novel measures give a very different picture of near stasis followed by explosive growth. What is perhaps most interesting about the table, however, is a slightly more subtle relationship that exists between the two sets of measures. Over the first three months, all the measures give very similar, indeed almost identical, results: whatever the two sets are measuring in this period, it would appear to be in some sense 'the same thing'. However, the sharp divergence of the two sets at and after 2;1 strongly suggests the entrance at this point of some new factor(s) which the novel measures record but which the conventional measures do not.

	1;11	2;0	2:1	2;2	2;3
types used in previous month(s)	10	14	12	16	21
new types (current month)	5	6	32	37	28
new types as a proportion of old	50%	44%	267%	231%	133%

Table 6a: Rate of introduction of new structural types by Cheryl

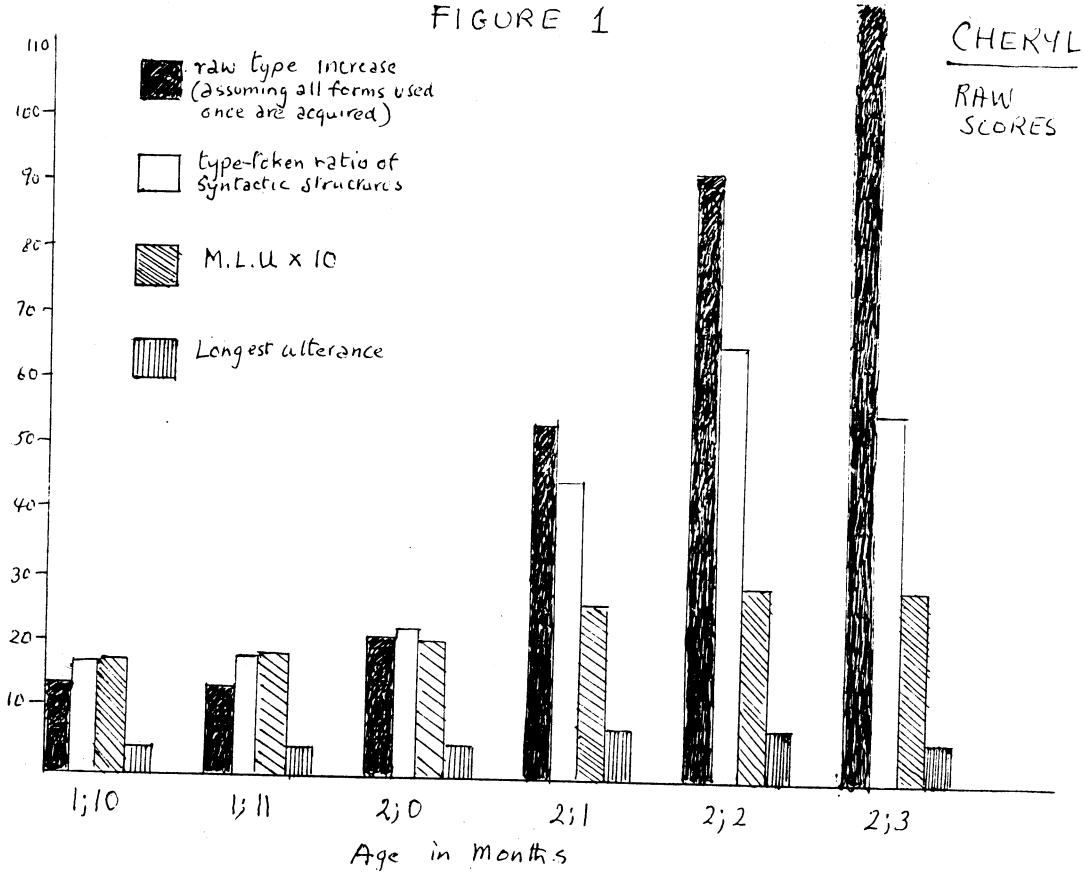
1;11 and 2;0	.01	no significance
2;0 and 2;1	10.49	p = .005
2;1 and 2;2	.09	no significacne
2;2 and 2;3	1.48	no significance

Table 6b: Chi-square comparison of Table 6a by month

This impression is strengthened by a third measure which is shown in Table 6a. This measure is obtained by taking, for each month, the number of syntactic structures used in previous months, and comparing it with the number of novel structures produced in that month. If we divide the second figure by the first, we obtain a figure indicative of the rate at which structural innovations are introduced. It then becomes possible to compare the rate of type innovation in consecutive pairs of months by the chi-squared method; resulting probabiliity figures are given in Table 6b. As will be seen, differences between all but one pair of months are not significant. However, the difference between months 2;0 and 2;1 is significant at a probability level of .005; that is, there is less than one chance in a thousand that the difference between these two months is attributable to chance. 2;1, of course, is the month that our other measures have already pinpointed as that in which, for Cheryl, some dramatic change in syntactic capacity took place.

Indeed, perhaps the most accurate (and certainly the most dramatic) picture of the changes taking place in Cheryl's output over this period is given if we use, for several measures, the criterion of percentage change per month. Such a measure is obviously highly sensitive to changes, more so than a mere comparison on the basis of raw figures: for example, it can reveal a quickening or a slowing of

FIGURE 1


CHERYL
RAW SCORES
raw type increase (assuming all forms used once are acquired)
type-token ratio of syntactic structures
M.L.U x 10
Longest utterance
110
100
90
80
70
60
50
40
30
20
10
1;10
1;11
2;0
2;1
2;2
2;3
Age in months

the pace of acquisition much more clearly than raw figures can. This sensitivity is illustrated in Figure 2, which compares monthly percentage changes for four measures.

It is worthy of note that in Figure 2 (next page) all four measures show a similar pattern -- even MLU, although as always this measure minimizes the extent of the changes taking place. All four measures show a peak in month 2;1 and a decline thereafter (most marked in the rate of type innovation). What this means is that while the motor of syntactic acquisition accelerates abruptly in month 2;1, it does not maintain that rate of acceleration, but rather falls back towards some kind of 'normal cruising speed'. It is hard to resist the conclusion that, at 2;1, some qualitative change took place in the child's syntactic capacity.

The change surely exists as a phenomenon in all of the only three children to be examined so far. It remains to be seen whether the same or different measures applied to the syntactic development of other children will provide similar results. If they do, then the question of explanation arises.

A simple and straightforward hypothesis is that, in the period of acceleration, the child might switch from what Gleitman (1981) termed a 'tadpole' to a 'frog' stage of development. In the 'tadpole' stage, the child would be simply putting words together like beads on a string, whereas in the 'frog' stage, the child would somehow have acquired the capacity to arrange them in hierarchical structures. The 'tadpole' phase might result from the fact that the brain mechanisms that generate syntax are not yet matured, hence the child is obliged to learn language in the same way that eating with a spoon or interacting with relatives are learned -- that is, by observational and inductive learning mechanisms. The 'frog' phase would commence as soon as the appropriate brain mechanism had matured; we know that the child's brain does not complete its growth processes until age two or later, and it has been suggested that around two, events occur in and around Broca's area that might well have the result of initiating the 'frog' phase (Greenfield 1991).

For a variety of reasons, many linguists will be reluctant to accept this hypothesis. It might be argued that while the tables and figures discussed above may reveal Cheryl's productive competence, they tell us nothing whatsoever about her receptive competence. It might then be the case that she knew and understood at 1;10 many of the structures that, for some reason, she was unable to produce until 2;1 or later. It might even be the case that such passive competence was a prerequisite for the subsequent explosion of forms -- a period in which she was somehow able to work out for herself the nature of syntactic structure, a problem the solution of which precipitated a cascade of novel forms.

Appealing though such a picture may seem to many, there is as yet little substance to back it. While there can be no doubt that comprehension usually goes well in advance of production, it may be able to do this only because it does not depend on explicitly syntactic knowledge. All those who have tried to learn a foreign language by the immersion method know that a great many structures are comprehensible to them which they could not themselves either produce or analyze. Semantics and pragmatics supply numberless clues to the meaning of sentences that can be exploited by their hearer. It would be difficult to devise, for two-year-olds, a test that would filter out clues of this nature, forcing an interpretation on syntactic grounds; however, some such test should undoubtedly be attempted.

There is also the question of what mechanisms would undertake such tacit learning. As noted above, there appear to be mechanisms in the brain that might automatically mediate a change from serially-ordered to hierarchically-ordered

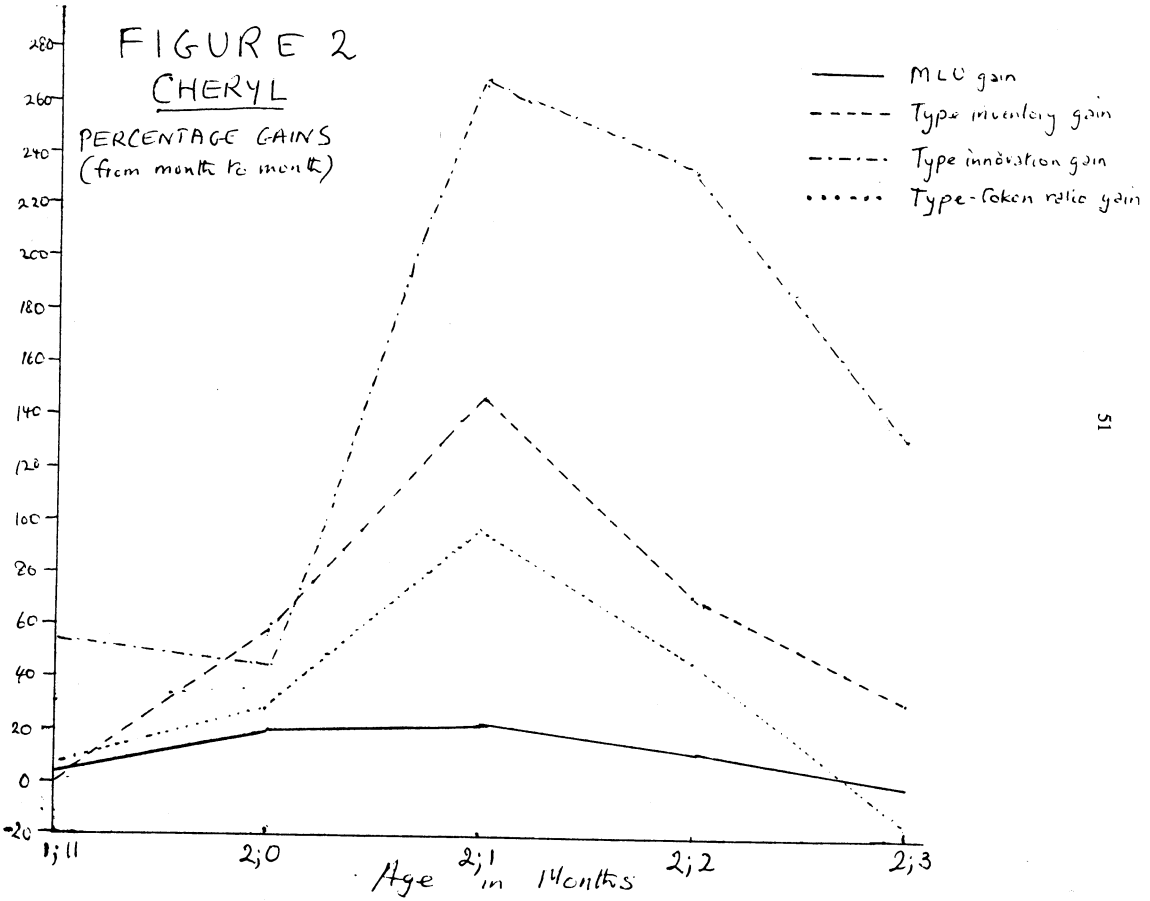


FIGURE 2
CHERYL
PERCENTAGE GAINS
(from month to month)
MLU gain
Type inventory gain
Type innovation gain
Type-token ratio gain
280
260
240
220
200
180
160
140
120
100
80
60
40
20
0
-20
1;11
2;0
2;1
2;2
2;3
Age in Months

structures. However, a mechanism that would form inductions over sets of sentences passively (but not actively) acquired prior to age two has not so far been identified, and one may legitimately doubt that it ever will be: it sounds too much like the 'general problem-solving device' beloved of behaviorists to have much credibility in terms of neurology.

Thus the data surveyed above lead one in the direction of some form of maturation theory, with a marked change in the capacity to produce syntactic structures occurring (in normals) early in the third year. Such a maturation theory, predicting as it does only a single maturation event, would be proof against the argument advanced by Ingram (1989) and others that a maturationist theory places no constraints on the number of times the nature of the child's underlying capacity may change. Since a variety of phenomena not considered in this paper point in a similar direction (see Bickerton 1990, MS for full discussion), the onus lies on those with other approaches to develop alternative explanations of the data surveyed above.

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