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In recent work, a number of scholars (e.g. ^{common? see all} Bruner 1979; Snow 1979) have summarized the development of acquisition studies over the last two decades. In the mid-sixties, the field, which had previously been atheoretical and somewhat underdeveloped, came to be dominated by a type of innatist theory. This theory, derived largely from generative grammar, and in particular from works such as Chomsky (1962), held that the child acquired language through simple exposure to linguistic data, much of which was "degenerate" – i.e. ^{common? see all} consisted of sentence fragments, mid-sentence reformulations, and many types of performance error which would render natural speech a very unreliable mirror to mature native-speaker competence. Somehow the child had to sift the wheat from the chaff, and he could only do this, it was claimed, if he had some kind of inbuilt Language Acquisition Device (LAD). A LAD would contain a set of linguistic universals, presumed to be innate and genetically transmitted. These universals would not, however, precisely specify a particular potential language, as in the theory described at the end of the last chapter; rather, they would define somewhat narrowly the limits on the forms which human language might take, thereby drastically reducing the number of hypotheses that the child could make about the structure of his future native tongue and rendering it correspondingly easy for him to select the correct hypothesis.

Since it is well known that children, whatever else they may do, do not in fact instantly and unerringly make correct hypotheses about adult structures, but rather approximate to those structures by means of a fairly regular and well-defined series of stages, the innocent observer might have expected the next step to consist of an examination of the initial (and often incorrect) "hypotheses" ^{see all} made by the child, to determine why it was that that particular hypothesis, rather than any other, was originally selected. Further steps might have consisted of determining in what ways the child discovered the falsity of his original hypothesis and how he subsequently modified it (or selected an alternative) in order to approximate more closely to the linguistic models available to him.

Unfortunately, nothing of the kind was done. The founders of generative theory remained grandly aloof from the hare they had started, claiming that real-

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world acquisition processes were still too chaotic and ill understood to constitute a legitimate object of study and taking refuge in the "idealization of instantaneity" described in Chomsky & Halle (1968: Chapter 7). Workers in the field were not simply left to their own devices; they were continually harassed by endless revisions of the theory. Doing acquisition work along Chomskyan lines became rather like playing a game which few minutes the umpires revise the rules.

Bearing this in mind-and bearing in mind too that workers in the field not only had no training in the analysis of variability and dynamic process generally but also had been given no reason even to think that such training might be necessary – it is not surprising that their results were somewhat unrevealing. In general, as shown, for example, in Brown & Hanlon (1970), Brown (1973), Bowerman (1973), etc., the predictions that generative theory seemed to make about acquisition were simply not borne out: young children did not show conclusive evidence that they knew $S \rightarrow NP VP$ or other basic PS rules; syntactic structures were not acquired in the order that was dictated by their relative complexity, and so on.

At the same time, and inspired at least in part by the meager results of generative-oriented work, many scholars began to question the assumptions on which this work was based. Was the input really degenerate? Was learning as rapid as had been claimed? Did it take place in the cognitive vacuum that at least seemed to be implied, if not actually asserted, in most generative writing? Upon examination, a number of these assumptions appeared to be partly or even wholly incorrect. Thus, there came about in the early seventies a very rapid and extreme swing of the pendulum, leading to an all but universal consensus among those working directly on acquisition which persists, with relatively minor variations, up to the present.

This consensus, while not ruling out entirely the possibility that some kinds of innate mechanisms may be involved in acquisition, systematically plays down and degrades the role of such mechanisms, often regarding them as constituting no more than a "predisposition" to acquire language, whatever that might mean (they never do say). The consensus holds, however, that prelinguistic communication and extralinguistic knowledge (acquired, naturally, through experience) play crucially important roles in acquisition, but that perhaps the most critical role of all is that of the interaction, paralinguistic as well as linguistic, which takes place between the child and the mother (or other caregiver). The mother, it is claimed, models language for the child, adapting her outputs to his linguistic level at every stage. Far from being degenerate, the data she provides are highly preadapted, highly contextualized, and patiently repeated. "Mothers *teach* their children to speak," Bruner (1979) states. When all these factors are taken fully

into account, the consensus claims, the need to posit an innate component in language acquisition shrinks to near zero or even disappears altogether.

Unfortunately, the whole position of this consensus is based on a fallacy – a fallacy that should be readily apparent to all readers of the two previous chapters. That fallacy is perhaps most concisely expressed by Snow (1979: 367) when she remarks that “Chomsky’s position regarding the unimportance of the linguistic input was unproven, since *all children*, in addition to possessing an innate linguistic ability, *also receive a simplified, well-formed and redundant corpus*” (emphasis added). This is quite simply untrue. The input that the first creole generation in Hawaii received was over-simplified rather than simplified, and was as far from being well formed as anyone could imagine; and we can assume that in other areas where creoles formed the same state of affairs must have existed. Mother could not teach *these* children to speak, for the simple and inescapable reason that Mother herself did not know the language – the language didn’t exist yet. But even so, without Mother, those children learned how to speak.

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In addition to this fallacy of fact, the Bruner-Snow position is based on a simple logical fallacy. If we accept that in the vast majority of circumstances mothers do teach and children do learn, it by no means follows that children learn BECAUSE mothers teach. It would be logically quite possible to argue that there is no connection whatsoever between mothers’ teaching and children’s learning, any more than there is between children’s walking and uncles’ dragging them around the room by their fingertips. If it could be shown that without well-formed input from the mother the child could not learn to speak then we might indeed assume a causal connection. In fact, we have shown the reverse: well-formed input from the mother cannot constitute even a necessary condition for children to acquire language; for, otherwise, creoles could not exist.

But our argument, though logically correct, need not be pushed to its logical extreme. I am perfectly willing to accept that if mother did not teach her child English, that child might have a much harder time learning it – even that the child might never acquire a perfected form of the language, but might significantly distort it in the direction of the kind of pattern we reviewed in the last chapter. All I want to claim is that if we persist in believing that the child must have input in order to learn, we shall continue to misunderstand completely the way in which he does learn a developed, natural language. Just as the child does not need mother in order to learn, so he could not learn even with a myriad of mothers if he did not have the genetic program that alone enables him to take advantage of her teaching.

In fact, the evidence we reviewed in the first two chapters of this book has

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simply never been taken into account in studies of child language acquisition. The vast majority of scholars in the field evince no awareness whatsoever of the existence, let alone the possible significance, of pidgins and creoles; an honorable exception is Slobin (especially Slobin 1977). Unfortunately, the data available to Slobin at the time were by no means as ample as those given in the present volume; moreover, he makes the common mistake of supposing Tok Pisin to be paradigmatic of normal pidgin-creole development. Still, even limited access to pidgin-creole data is better for acquisitionists than none, and in consequence we shall find the work of Slobin and his associates illuminating on a number of points in the pages that follow.

Meanwhile, in the absence of the insights provided by creolization, the current paradigm has provided us with much information that we lacked before – on the nature of input to the child and of child-caregiver interaction; on the acquisition of turn-taking, conversational routines, and the kind of social appropriateness summed up under Hymes's concept of "communicative competence"; "on acquisition strategies" based on contextualization, semantic and pragmatic clues to the function of novel structures, etc., etc. – and yet, as more and more thoughtful scholars are realizing, the gathering of this information has merely served to conceal the fact that the central question of acquisition, the question with which the early generativists did at least struggle, however unsuccessfully, is simply not being answered:

How can the child acquire syntactic and semantic patterns of great arbitrariness and complexity in such a way that they can be used creatively without making mistakes?

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> Cromer (1976: 353), for instance, observes that the concept of "acquisition strategy" has made us aware of some of the ways by which the child may possibly "get into" the linguistic system. It has shown us the importance of perceptual mechanisms for interpreting utterances, and how as adult speakers with full linguistic competence we nevertheless rely on a number of short cuts to understanding ... The concept of language acquisition strategies has told us much – except how the child acquires language." Bowerman (1979), who cites this passage with approval, further points out that while such strategies may enable children to understand utterances which still lie outside their developing grammars, those strategies do not and indeed cannot, in and of themselves, assign structural descriptions to these novel utterances. Yet children must achieve this kind of structural knowledge if they are subsequently to use such utterances themselves in a productive and creative way – understanding something is miles away

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tions zigzags up and down like a malaria victim's temperature chart, before finally leveling off at or near the 100 percent mark. The innocent observer might think that the most interesting thing you could do in acquisition study would be to figure out *why* this happens, but as usual, he would be disappointed. Fashion and expediency dictate that order must be imposed on disorder: to determine the order of acquisition – a “need” dictated merely by current theory – Brown (1973) established a purely arbitrary “criterion” for acquisition, i.e., a 90 percent production rate in appropriate environments, maintained over three consecutive recording sessions. The reign of the criterion merely reinforced what has always been a trend in acquisition studies, and a deplorable one: to look to the goal rather than the path, to ask “What has the child acquired?” rather than “How has he acquired it?” In consequence, masses of potentially valuable data, which would be required by any interesting acquisition theory, were simply flushed down the drain.¹

In addition to deficiencies of this nature, we have to remember that all the data collected to date were collected for very different purposes than the present one. It is a general law applicable to all research that one tends to find what one is looking for, and not to find what one is not looking for. Hence, it would be unrealistic if we expected to find massive quantities of unambiguous evidence pointing toward the truth of our theory, which had yet somehow been missed by previous observers. The most that one can ever hope for from data collected under other assumptions and for other purposes than one's own are oblique hints, gaps that one's own hypotheses might fill, puzzles set aside that might begin to make sense in the context of a different framework. However, if one finds any of these at all, it is a reasonable assumption that a purposeful search of raw data sources would reveal much more-something comparable to the invisible eight-ninths of the iceberg.

With regard to the second word of caution, I can lay claim to no special expertise in the field of child language. In creoles, I have fourteen years' experience,

¹ Even today, I know of no study of child language acquisition in any language which follows the simple and obvious procedure of noting the very first emergence of a given form or structure in a child's speech, then following the development of that feature until Brown's “criterion” is reached – meanwhile noting what that form or structure alternated with in those contexts where it was inappropriate, as well as those where it was appropriate, with the aim of figuring out why variation occurred and what the form or structure might mean to the child. Normally, second-language acquisition trots along obediently in the footsteps of first-language acquisition, but here roles are reversed, as my student, Tom Huebner, is about to complete a dissertation which applies the above approach to the acquisition of English by an immigrant Hmong speaker (see also Huebner 1979). The field is wide open for similar first-language studies, which should help to revolutionize our understanding of acquisition.

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simply demonstrates a failure to understand how complex language really is.²

With these preliminaries disposed of, we can begin our review. The evidence we shall consider will fall into two quite separate classes. One class will consist of the "incorrect hypotheses" which, in the course of language acquisition, children often make, yet which often seem to have no simple explanation either in the structure of the input (th) child receives or in any general theory of acquisition. The similarity between such "hypotheses" and the structures which actually emerge as part of the grammars of creole languages is often quite striking, and when I first contemplated writing this chapter, I felt certain that examples drawn from this class would constitute by far the strongest evidence in favor of the bioprogram theory. After writing the first draft of this chapter, however, I became much less certain, not so much because of the weakness of the original evidence – although there are some phenomena, as I shall show, which may allow alternative explanations – but because of the growing impression that a much subtler and less obvious class of evidence made on me.

As the "incorrect hypotheses" suggested, there were many things in language which children seemed to find quite difficult to learn, often spending years before they acquired full control over the structures concerned. On the other hand, there were certain other things which seemed to give them no trouble at all, which they learned very early in the acquisition process and/or without any of the "mistakes" which arose so frequently in other areas. On principle, one might suppose that these differences correlated with some kind of scale of relative difficulty, and yet it was extremely difficult to see exactly what objective factors might constitute such a scale. Indeed, from a commonsense linguistic viewpoint, some of the things that were easily and effortlessly acquired looked a lot more difficult to learn than some of the things that gave so much trouble.

But obviously, to talk about things being "difficult" or "easy" from an adult standpoint is totally irrelevant in an acquisition context. What is "difficult" or "easy" for the child is all that is of interest and one might therefore conclude

² In fact, rather than such a conflict, the present theory entails a division of labor. The innate component is necessary in order to get the child into a position where he can learn any human language, for as Fodor (1975) argues (see below), it is impossible to learn a language unless you already know a language. Some other kind of component is necessary to get the child from the innate creole – like grammar to the idiosyncratic grammars of Italian, Yoruba, Akawaio, Walbiri, or whatever language that particular child is going to have to learn as part of his socialization. Because I have not discussed this second component in the present volume, the reader should not conclude that I deny its importance. My failure to say anything about it is, as I said, strategic; until we know where the innate component stops, we cannot know where any other devices start.

- (10) Bill wanted to buy *a cat* and *a dog*, but he couldn't find *a dog* that he really liked.

Maratsos constructed an ingenious set of stories which his child subjects were asked to complete. In some of the stories, reference was made to a specific entity; in others, to a nonspecific entity; in both cases, naturally, the entity was introduced into the story as *a NP*. However, the completion task required the child to produce *a NP* just in case the entity was nonspecific, and *the NP* just in case the entity was specific, in accordance with the rule illustrated in (9) and (10) above (for full texts of the stories and a more complete description of the experiments, see Maratsos 1976).

The success rate in this experiment was almost 90 percent for three-year-olds and over 90 percent for four-year-olds. In order to maintain these high rates, the children had to determine that out of some NPs identically marked, half had specific real-world referents and half had not. The stories were original and contained no contextual clues as to the status of the referents. How did the children succeed so often?

Maratsos himself was surprised and impressed by his subjects' capacities, and he discusses the implications of his experiments at some length and with great insight. He notes that the high frequency of articles in adult speech is often regarded as an adequate explanation of the relative earliness and lack of error shown in the acquisition of articles. He points out, however, that "although the frequency of [articles'] use may somehow serve to bring them to the child's attention and provide data for him, he must still select and attach to the articles just those abstract differences in the circumstances of their use that correspond to the specific-nonspecific distinction. One clear requirement is that he have available some conceptual understanding of such matters as the difference between the notion of any member (or no member) of a class and that of a particular class member. This understanding must be sufficiently well articulated for the child to perceive just this difference in the circumstances of use of the definite and indefinite morphemes and construct the meaning of the terms accordingly" (Maratsos 1974: 453).

Let us try to reconstruct the process or processes by which the child might arrive at this perception. We will ignore the problems that arise from the child's original isolation and recognition of articles, although these are far from trivial (especially with *a*, so frequently reduced to an unstressed schwa and so closely linked to its following NP that morpheme boundary perception becomes quite difficult), and deal solely with how, having recognized them, he determines their functions. If the conventional accounts are correct, the child can do this in only