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## What Shapes Children's Grammars?

**Melissa Bowerman**

*Max-Planck Institute for Psycholinguistics*

*Nijmegen, The Netherlands*

*and*

*University of Kansas*

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Crosslinguistic comparisons have been used systematically in the study of the human capacity for language acquisition for only a short time, less than 20 years. But research has been intensive in this period. The present volumes are a landmark in the continuing development of this essential research tool in part because they bring together a wealth of information about the acquisition of various languages, most of it quite recent. Beyond this, however, they are important because they represent the state-of-the-art in the vigorous program of crosslinguistic research and theorizing launched over 10 years ago by Dan Slobin in his seminal 1973 article, "Cognitive prerequisites for the development of grammar." It is a good time to take stock of how far we have come with this

approach, what its strengths and weaknesses are, and where we should go with it next. This chapter is devoted to these questions.

The discussion is organized as follows. First I try to place Slobin's approach in a historical perspective, since to appreciate the conceptual and methodological impact of "Cognitive prerequisites . . ." on research of the last decade it is necessary to recall what preceded it. In the next three sections I critically evaluate several important aspects of the approach as it is represented in these volumes and especially in Slobin's integrative chapter, "Crosslinguistic evidence for the Language Making Capacity": its methods of analysis, its overall coherence as a theory of language acquisition, and the viability of the characterizations of early child language to which it has given rise. I end the chapter with some brief suggestions for further crosslinguistic research.

A recurrent theme throughout these discussions is to find the right balance between the contribution of the child and the contribution of the linguistic input to the acquisition of language. In particular, I explore the problem of determining to what extent children's early grammars are shaped by universal, inherent preferences for certain ways of structuring meaning and mapping it into linguistic form, and to what extent by experience with the semantic and structural properties of the language being learned.

## 1. THE PROCESSING APPROACH IN HISTORICAL PERSPECTIVE

In the years immediately leading up to the 1960s, investigators typically viewed child language through a filter provided by adult language. Children's progress was described in terms of deficiencies or improvements with respect to the categories and rules of the full-fledged system. This changed in the early '60s when researchers, inspired in part by Chomsky's claim that language acquisition is a dynamic process of formulating abstract rules, began work aimed at characterizing child language in its own terms.

Empirical investigations into children's rule systems were embedded in a broader theoretical concern: the nature of the human capacity for language acquisition. Chomsky (1959, 1965) had drawn special attention to this problem with his compelling arguments that the behaviorist approach, accepted by many scholars at that time, was grossly inadequate, and by his strong counterproposal that children come to the language learning task equipped with inborn knowledge of both substantive and formal linguistic universals.

If children indeed have innate knowledge that guides and constrains their formulation of linguistic rules, it was reasonable to expect similarities in language acquisition across children learning different languages. Researchers therefore began to explore whether the regularities that had been documented primarily in the speech of children learning English would prove to be general

(e.g. Bowerman, 1973; Brown, 1973; Slobin, 1970). It was hoped that crosslinguistic commonalities, where found, would reveal general characteristics of the child's language acquisition capacity.

Although this approach to crosslinguistic research was methodologically sound, it had an important limitation. Testing whether certain properties are common to the speech of all children required hypotheses about potential universals of form, content, or function. Three such hypotheses of the 1960s were "telegraphic speech," the "pivot grammar," and "adherence to rigid word order." If crosslinguistic testing disconfirmed universality—as it in fact has done for these once-promising characterizations—it was unclear where to look next. Meanwhile, data that were language-specific—hence not relevant in any obvious way to the testing of universal patterns—went unexploited, since it was uncertain with which data from other languages they could be compared to yield interesting generalizations.

A crucial contribution of Slobin's "Cognitive prerequisites . . ." was to show a way beyond this sticking point. The fundamental insight was a new conception of what could serve as a basis for drawing inferences about the language acquisition capacity. Slobin proposed that, in addition to looking for universal stages of acquisition, researchers could use crosslinguistic data to determine the *relative difficulty* for children of the *formal linguistic devices* that languages employ to express meaning, such as prefixes, suffixes, and word order. Patterns of relative difficulty could then provide clues to how children approach the linguistic input in constructing a grammar.

Slobin phrased his preliminary conclusions about how children proceed in the form of "Operating Principles": children's self-instructions for perceiving and producing speech under short-term processing constraints and for organizing and storing linguistic rules. This notion had been foreshadowed much earlier by Slobin in a 1966 article. The discussion there concerned whether children should be credited with innate knowledge of categories and possible rules of grammar, as Chomsky (1965), McNeill (1966) and others had advocated. Slobin proposed that children may start out equipped not with "knowledge" but with a set of *procedures* for analyzing linguistic input. Language universals, in this view, would reflect the interaction of the analytical procedures with the input, but would not in themselves be innately represented. This approach (also suggested by Fodor, 1966) became known as the "process" approach to language universals. The orientation of Chomsky and McNeill, which emphasized substantive and formal universals, was termed the "content" approach.

Although a sharp distinction between process and content is no longer maintained (some researchers argue that these are merely notational variants of each other), modern descendants of the two approaches can still be identified. The work discussed in these volumes clearly carries on the "process" tradition, while work by scholars such as Goodluck (1981), Otsu (1981), Roeper (1982), and others (e.g. in Tavakolian, 1981) elaborates the "content" approach. An

important difference between the two orientations is in the methods typically used; these in turn reflect differences in underlying assumptions and in the primary method of reasoning.

"Content"-oriented researchers usually proceed deductively, starting from hypothesized universal constraints on the form of grammars, making predictions about how language-learning children should behave if these constraints are innate, and devising experiments to test these predictions. Comprehension is the performance modality usually studied, and research has only rarely involved crosslinguistic comparisons.

"Process"-minded researchers, in contrast, have typically been either skeptical about whether there are *a priori* constraints on the child's development of grammar or agnostic about what such constraints might be like. They have therefore proceeded inductively, examining the details of children's spontaneous speech at successive phases of development and trying to infer general principles that could give rise to the observed patterns. Although a researcher may work on only one language, crosslinguistic comparisons are considered essential to allow *general* procedures of the language-learning child to be disentangled from those that result from exposure to a language with particular structural properties (or transmitted in a particular social milieu, etc.). Experimental work is sometimes done, but the purpose is not usually to test predictions but rather to gather more controlled information on how children learning different languages proceed in a given structural domain (e.g. Johnston & Slobin, 1979), or to decide between alternative explanations for children's behavior by comparing children learning languages that differ structurally in key ways (e.g. Hakuta, 1981).

One of the most important consequences of Slobin's Operating Principles (OP) approach has been its stimulating effect on researchers around the world. The approach has both inspired further data collection and focussed attention on certain ways to interpret the data. When the immediate goal of crosslinguistic comparison is seen as to determine the relative difficulty for children of different formal devices for expressing meaning, the usable data base is much broader than when the goal is seen as to establish universal "stages" of development. Every piece of evidence may contain a clue; none is too small or too language-specific to take its place in the larger pattern of explanation.

In response to new information, further theoretical work, and suggestions from contributors to these volumes, Slobin (1985) has modified and extended the Operating Principles model. It has become more comprehensive both in the number of factors it considers relevant to acquisition and in the variety of linguistic structures whose acquisition it seeks to explain. There is also now an important new hypothesis about the initial outcome of the interaction between Operating Principles and linguistic input: the notion of "Basic Child Grammar."<sup>1</sup> We can anticipate further expansion and reformulation, of course, but the

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<sup>1</sup>This hypothesis credits the language-learning child not only with operating principles but also, as we discuss later, with the spontaneous tendency to classify the elements of experience into certain

approach has by now developed clear enough outlines to allow some serious evaluation.

In what follows, I focus on those aspects of the approach that I believe present the toughest challenges and will need the closest attention in the coming years if the OP approach is to realize its promise. I begin with some remarks about methodology, and then turn in more detail to questions about testing proposed OPs, about how the OP approach hangs together as a whole, and about the "Basic Child Grammar" hypothesis.

## 2. SOME METHODOLOGICAL PROBLEMS

### 2.1. The Time Lag Between Intention and Mastery

Within the OP framework, as it was conceived by Slobin in 1973, it is essential to be able to establish the relative acquisitional difficulty of various linguistic devices, since this is the primary basis on which inferences about the language acquisition capacity are drawn. Children's spontaneous speech offers a rich variety of clues, but it is no small task to bring this uncontrolled data source into some kind of order.

In "Cognitive prerequisites . . ." Slobin proposed an ingenious new technique for dealing with this problem. It depended on a "very strong developmental psycholinguistic universal . . . [that] the rate and order of development of the semantic notions expressed by language are fairly constant across children learning different languages, regardless of the formal means of expression employed" (Slobin, 1973, p. 187). If this universal is true, observed Slobin, and if the onset of the child's intention to communicate given meanings can be identified, then the time lag from intention to mastery of the conventional form can be taken as an index to how "complex"—i.e. how difficult on formal linguistic grounds those forms are for children. The *relative* complexity of different forms (e.g. prefixes vs. suffixes) can then be established by comparing how long it takes children learning different languages to move from first intention to communicate a meaning to mastery of the forms used to express that meaning.

2.1.1. *Problems in Measuring the Time Lag.* Despite the intuitive appeal of this technique, its primary contribution in the long run may turn out to have been conceptual rather than methodological: It has forced researchers to distinguish clearly between meanings—and the cognitive prerequisites for these meanings—and their somewhat arbitrary formal linguistic packaging. This clarification has

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categories of meaning for purposes of linguistic expression. Through this proposal "content" has re-entered the "process" approach. However, this type of content differs from what researchers previously had in mind in discussing prestructure in the inherent capacity for language acquisition, in that it concerns semantic organization rather than syntactic form.

led to progress in many domains. But applying the method in the way Slobin initially envisioned has proved difficult.

First, identifying the time of "first intention" to express a given meaning is often difficult or impossible (see Johnston, 1985). Second, establishing the time of mastery of a form is no easier. As Clark (1985) points out, measures of mastery based on children's production sometimes differ from those based on comprehension. Which measure should we then use? A further problem that affects our assessment of both intentions and the mastery of forms is the possibility of *reorganization* in the child's linguistic system. As recent research emphasizes (e.g. Bowerman, 1982b, 1982c; Karmiloff-Smith, 1979a, 1979b), children's initial appropriate uses of a form are often based on relatively superficial knowledge; "acquisition" beyond this point may be a drawn-out process in which the child discovers successively deeper levels of structure and regularity at the levels of both form and meaning. Where in the span of weeks or months beyond the first appearance of a form should the point of "acquisition" be set?

Given these difficulties in setting the lower and upper boundaries of the lag between intention and mastery, it is not surprising that the relative difficulty of linguistic forms has in practice often been estimated simply on the basis of the *relative ages* at which children learning different languages begin to use the conventional forms to express a given meaning. This substitute might seem harmless. However, it has an important drawback: It provides no check on the assumption that given communicative intentions arise in all children at about the same age regardless of their local language. If in fact properties of the language being learned systematically influence the time at which given intentions emerge (a possibility also recognized by Slobin, 1982), time of acquisition per se cannot provide an unambiguous guide to linguistic complexity.<sup>2</sup> This problem, especially when coupled with the common practice of estimating "average time of acquisition" for a whole population on the basis of data from a small number of children, makes "time of acquisition" information difficult to interpret.

2.1.2. *Confounded Determinants of Time of Acquisition.* As we have noted, the logic of the OP approach to assessing relative linguistic complexity depends critically on the assumption that meanings arise in the child independently of knowledge of the forms with which to express them. A further crucial assumption is that the time of acquisition of a form is influenced by *only two factors*: the difficulty of the meaning it expresses and its formal complexity for children. Given these assumptions, we can reason that if meaning is held constant and time of acquisition varies across languages, then difficulty of form must be the determining factor.<sup>3</sup>

<sup>2</sup>For instance, the relatively late acquisition of a linguistic device would be compatible with formal simplicity for the child as long as the underlying communicative intention *also* emerged relatively late, for whatever reason.

<sup>3</sup>Conversely, of course, if formal difficulty is held constant across a set of forms expressing related meanings, variations in time of acquisition can be attributed to differences in the difficulty of

The problem, of course, is that cognitive and linguistic complexity are not the only two factors that can influence time of acquisition, as research of recent years has made increasingly clear. For example, in "The Language-Making Capacity," Slobin (1985) discusses evidence for the role of two additional factors: the relative pragmatic usefulness for the child of different structures (see also Eisenberg, 1981) and differences in frequency of modeling.<sup>4</sup> The list may well become longer.

Although recognizing additional influences on time of acquisition is not objectionable on general theoretical grounds, it destroys the logic of the OP procedure for determining formal linguistic complexity. Unless we can figure out how to isolate the contribution of each determinant to the time at which a linguistic ability is acquired, then relative time of acquisition can serve as a guide *neither* to relative formal complexity *nor* to relative cognitive complexity.

## 2.2. Errors in Spontaneous Speech

Because of the difficulties of measuring and interpreting the time needed for acquiring a form, a second source of evidence for relative complexity has gradually increased in importance: errors in children's spontaneous speech. In fact, as Slobin points out, "error analysis forms the substance of most of our crosslinguistic developmental data" (1985, p. 206). The analysis of errors is accepted as an essential research technique by almost all researchers in child language, but it is sometimes criticized by those in neighboring fields (e.g. see Givón, 1985). Certain criticisms, I think, reflect a misunderstanding of the purpose of error analysis rather than real problems. On the other hand, there *are* some serious difficulties in analyzing children's errors that are rarely discussed. Let me try to distinguish between these.

**2.2.1. *Prescriptivism and Other Nonproblems.*** The most common charge against the study of children's errors is that it implies a prescriptive attitude: Adult speech is taken as "correct" and child speech is interpreted only in terms of its "deficiencies" with respect to this standard. This is seen as a failure to recognize or respect the integrity of the child's own linguistic system, in which departures from adult grammaticality may not be errors at all.

This complaint reflects a fundamental misconception. The motivation behind virtually all contemporary analyses of children's speech errors is *precisely* to understand the structure and functioning of the child's own system. Children's

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the meanings. This reasoning has been used in studies of the acquisition of locative markers (Johnston & Slobin, 1979) and connectives (Clancy, Jacobsen, & Silva, 1976), among others.

<sup>4</sup>The role of frequency is still controversial. However, even investigators who consider it relatively unimportant acknowledge that extremely frequent or infrequent modeling probably influences time of acquisition (e.g. Brown, 1973), and frequency also seems to play a significant role in determining which formal devices a child will learn first to express a given meaning when the language provides more than one option (Snow, 1977).

failures to meet adult standards are of no inherent interest; their value is rather that they are one of the most compelling sources of evidence we have about the workings of the system we are trying to understand. This is because, by virtue of their discrepancy from the input, they implicate children's *own efforts* to process and organize what they have heard.

When children's utterances are unremarkable by the norms of their speech community (regardless of whether the language being learned is a "standard" or "substandard" dialect), it is hard to infer the knowledge that underlies them. First, superficially correct usage can be compatible either with adultlike knowledge or with a more constrained or superficial understanding (e.g. a highly context-bound grasp of word meaning, or an unanalyzed or only partially analyzed representation of strings that for adults have complex internal structure). Second, even when children have demonstrably analyzed strings of a certain type (i.e. control all the morphemes in them as independent elements), more than one description of their structural knowledge is still almost always possible (just as in the case of adult speech). We cannot determine which description has psychological reality at a given stage of development—i.e. captures the categories and rules that are functional in the child's linguistic system—solely by looking at conventionally acceptable output. Errors, however, if carefully observed and fully exploited, can provide important clues.<sup>5</sup>

Givón (1985) warns that "conceiving of children's output as 'error' may lead to de-emphasizing what is systematic and universal about [their language], in favor of over-emphasizing what is idiosyncratic, language-specific or irregular" (p. 748). Although this might in principle seem a danger, in fact, the opposite is nearer the truth: Researchers have typically used error patterns within particular languages to infer universal predispositions concerning either the formal structure of language, its semantic/conceptual underpinnings, or both (e.g. see Berman, 1980; Bowerman, 1982b, 1982c, 1983a; Clark, 1977, 1981; Karmiloff-Smith, 1978, 1979a; MacWhinney, 1978, 1985; and Slobin, 1973, 1985, to name only a few; see also footnote 19).

**2.2.2. *Interpreting Children's Errors.*** What then are the more serious difficulties in using error data? One requirement is to become as sensitive to errors of omission as we are to errors of commission (see Brown, Cazden, & Bellugi, 1968, on these terms). Errors of commission are departures from adult norms

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<sup>5</sup>For example, with respect to the problem of unanalyzed forms, morphological overgeneralizations like *foots*, *comed*, and *uncapture* (=release) indicate that the child understands the internal structure of legitimate forms of the same pattern (Bowerman, 1982b; Ervin, 1964). Conversely, errors like *It's tell*, *It's have wheels*, *that a my book* (Brown, 1973) and *Lemme me do that*, *Show me me that* (from my unpublished records) indicate that complex strings (*it's*, *that a*, *let me*, *show me*) are unanalyzed by the speaker. For further evidence on how children's errors give clues to underlying categories and rules, see, e.g. Berman, 1980; Bowerman, 1982a, 1982b, 1982c, 1983a, 1983b; and Clark, 1981).



that, like *foots* and *goed*, are explicitly deviant. Errors of omission, in contrast, are restrictions in the distributional range of a form. Interesting examples discussed by Slobin (1985) include the initial limitation of markers of subject and direct object to highly transitive agentive events. Errors of commission are much easier to spot than errors of omission, and they have received far more attention in the literature. A child's apparent failure to use a form over its full range is not only unobtrusive but also hard to verify: The "missing" uses may simply be infrequent and therefore not sampled, rather than absent entirely. Nevertheless, information about restricted usage, if carefully evaluated, can contribute just as much as outright errors to our inferences about children's language-learning procedures.

It is just as essential to evaluate suspected errors of commission carefully. As Ochs (1985) and Smoczyńska (1985) point out, forms that our grammar books indicate are unacceptable in language X may in fact be common in the everyday speech children hear. If so, no special reference to children's own processing or organizational dispositions is required to explain them. Especially when dealing with languages or dialects not our own, we have to study the actual input, not our idea of it.

Even when we are confident that certain utterances really are deviant by the norms of the child's speech community, lack of information almost always hampers us in trying to interpret them. First, have errors of this type been documented repeatedly in the speech of an individual child and in other children learning the same language, or have they been observed only once or twice in all? Sometimes elaborate arguments are made on the basis of a couple of utterances. Almost anything can happen once or twice. For errors, just as for conventional utterances, we cannot base inferences on single exemplars; we need to know about *patterns* of usage, both within and across children.

Second, once it has been established that errors of a certain type occur repeatedly, we must ask how *frequent* they are relative to correct usage (see also Smoczyńska, 1985). When children are rarely or never correct in their use of a certain form (word, sentence pattern, etc.), and if their errors are consistent, it is reasonable to attribute the errors to characteristics of the grammatical rules the speaker has formulated. However, some types of errors, although recurrent, are infrequent relative to correct usage: Most of the time the child uses the forms involved correctly. Such "occasional" errors should not be taken as clues to the speaker's *grammar*; they are usually better accounted for by reference to *performance* factors (although the lines along which occasional errors are made can reveal important principles of underlying organization; we will come back to this later).

Third, is the error pattern observable from the child's *first* uses of the affected forms, or does it set in only after a period of correct usage? (See Bowerman, 1978, 1982a, 1982b, 1982c on the phenomenon of "late" errors.) Fourth, are there other forms in the child's repertoire that may compete with or otherwise

interact with a target form, causing errors? And fifth, if an explanation for an error type has been hypothesized, do other forms in the child's speech that are in principle vulnerable to the same source of trouble also show errors?

Answers to all these questions can be essential for deciding among alternative explanations for the errors under study, as will become clear from examples to be discussed. And how we interpret the errors, in turn, has far-reaching consequences for determining what the child brings to the language acquisition task. These topics are pursued in the following sections.

### 3. TESTING PROPOSED OPERATING PRINCIPLES

One of the most salient features of the OP approach is its capacity to assimilate and make sense of new data of a variety of types. This is an important source of its appeal, but it is also a danger. One drawback is that because of its "tolerance," the model tends to discourage testing: Researchers over the last years have often been content with pointing out "hits"—ways in which their data *accord* with one or another operating principle—and have failed to search as assiduously for "misses." More seriously, even when researchers do adopt a hypothesis testing attitude—and contributors to this volume were explicitly encouraged to do so<sup>6</sup>—the most basic claims of the model often turn out to be difficult to test. This is because many, perhaps even most, types of counterevidence can be readily dealt with either by reinterpreting the data to fit existing OPs, by simply expanding the OPs, or by adding in new ones, leaving the basic framework undisturbed.

This gives rise to a perplexing state of affairs. Naturally we want a theory that can account for all observed outcomes. And the suggested amendments and additions to the theory proposed in Slobin (1985) are generally quite plausible. But the overall effect is that the theory becomes increasingly enclosed and self-protecting, and it becomes harder to test whether observed outcomes really *confirm* the processes hypothesized to underlie them or simply are not incompatible with them.

A related problem is that of redundancy. As more OPs are added, both to account for counterinstances and to handle phenomena not previously dealt with, the OPs begin to encroach on each other's territories. As the system gets heavier, it sometimes becomes possible to account for a single phenomenon in more than one way. Again, this is often plausible: Certain outcomes may indeed be multi-determined. But this is probably not always so, and we need to become sensitive to redundancy and to develop principled methods for deciding among alternative explanations and for weeding out those that, however attractive, are superfluous.

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<sup>6</sup>This challenge was taken particularly seriously by Smoczyńska, with a number of insightful and provocative results.

The examples with which I illustrate these problems revolve around one of the most difficult, frustrating, and fascinating issues in the study of child language, the role of meaning in the child's construction of grammar.

### 3.1. Dealing with Findings Counter to Predictions

One of the central tenets of the OP approach since its 1973 inception is that children initially strive for a clear (one-to-one) mapping between units of meaning and units of form. The OPs in Slobin (1973) designed to reflect this tendency were E: "Underlying semantic relations should be marked overtly and clearly," and G: "The use of grammatical markers should make semantic sense." These two are missing from the current formulation of the OP approach (Slobin, 1985), but others designed to capture the same propensity have grown up in their place. It is with respect to children's assumed preference for clear mapping that the OP approach has become particularly well defended against disconfirmation.

3.1.1. *The Use of Grammatical Markers Should Make Semantic Sense.* In her chapter on the acquisition of Japanese, Clancy (1985) notes that young children initially add case particles after nouns in a semantically and pragmatically unmotivated way. She observes that this goes counter to the OP that "The use of grammatical markers should make semantic sense," and suggests that children may sometimes override this OP in their efforts to create a canonical surface structure with its characteristic intonation pattern.

On the basis of this and other similar evidence, Slobin proposes a new OP called UNINTERPRETED FORMS: "If a speech element is frequent and perceptually salient, but has no obvious semantic or pragmatic function, use it in its salient form and position until you discover its function; otherwise, do not use it" (1985, p. 246). This OP functions alongside OP:FUNCTIONS (a descendent of "The use of grammatical markers should make semantic sense"), which specifies (in brief) that the child tries to map uninterpretable speech segments (after extraction of content words) onto grammaticizable "Notions" (meanings).

These two OPs together form a closed circle: If the child uses a grammatical marker with a consistent meaning he is following OP:FUNCTIONS, and if he uses it without meaning or does not use it at all, he is following OP: UNINTERPRETED FORMS. The substantive claim made earlier in "The use of grammatical markers should make semantic sense" has become greatly attenuated. However, it survives at least in the implication that children will *first search* for a semantic or pragmatic basis for grammatical markers, and only switch to OP: UNINTERPRETED FORMS if they fail. This sequence is also proposed, in more explicit form, in OP: MORPHOLOGICAL PARADIGMS.

Do children in fact always first try to find semantic or pragmatic solutions and only use certain items meaninglessly if they cannot do so? There seems to be no

independent evidence for this.<sup>7</sup> Without such evidence we can just as well imagine an inverted OP that says "Use any (noncontent) forms that you have noticed (because they are salient, frequent, etc.) freely in their usual positions until it occurs to you that they have an associated meaning; after that, use them only in conjunction with this meaning." When we can so easily restate an OP as its converse, we must question whether it captures children's grammar-constructing procedures accurately.

3.1.2. *Plurifunctionality.* Another test of the assumption that children initially strive for a clear mapping between forms and meanings comes with plurifunctionality. If interpreted strictly, the one-to-one mapping hypothesis predicts (among other things) that children should not use forms that are plurifunctional—that express different meanings on different occasions. In fact, we know that they often do. Does this mean the hypothesis is wrong? Not necessarily. There are at least two ways to reinterpret counterinstances to be compatible with one-to-one mapping.

First, as Slobin points out, what for adults are two distinct meanings may for children be undifferentiated, hence, only "one meaning." He suggests that if we accept the principle of one-to-one mapping as given, it "guides us in discovering what constitutes 'one meaning' and 'one form' from the child's point of view" (1985, p. 207).

Second, as Karmiloff-Smith (1978, 1979a) has argued, children who in general adhere to one-to-one mapping could nevertheless use the same form for what they perceive as two distinct meanings as long as they do not yet recognize that the form is "the same" in these disparate uses.<sup>8</sup> In this case, when they *do* realize that one form is serving two functions, they should begin to mark these meanings with distinct, often ungrammatical forms. Karmiloff-Smith gives several examples of this phenomenon, and Slobin, guided in part by her arguments, proposes OP:UNIFUNCTIONALITY: "If you discover that a linguistic form expresses two closely related but distinguishable Notions, use available means in your language to distinctly mark the two Notions."

There is nothing inherently implausible about either of these reinterpretations of children's behavior to make it compatible with the principle of one-to-one mapping, but the argumentation is circular. We assume that children do not like plurifunctionality. When they do use forms plurifunctionally, however, we do not count it as evidence against this assumption. It just means that they do not yet

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<sup>7</sup>See Smoczyńska (1985) for a related argument that solutions based on meaning may enjoy no special privileges in initial child grammars. She proposes that *consistency* is what children look for, and a consistent relation between form and meaning is only one instance of this.

<sup>8</sup>Notice that if this is true, then, contrary to Slobin's proposal, we cannot safely take children's plurifunctional uses of a form as a guide to what they regard as a single meaning. It might be that they in fact regard the meanings as distinct but have not yet realized that the form is the same.

realize that they are using one form for two meanings. If and when they do discover this, they will try to mark the meanings distinctly. Clearly we need to break out of this circle by finding methods independent of how children use language for determining whether they differentiate certain meanings and whether they recognize that certain forms are "the same." This will not be easy, unfortunately, since the relevant children are extremely young, the meanings are usually subtle, and both the meanings and the forms, because they often belong to the "grammaticized" rather than the "content" portion of language, are particularly inaccessible to conscious reflection.

If we widen the data base for evaluating OP:UNIFUNCTIONALITY, we find other reasons for worry: on the one hand the absence of errors that *should* occur if the OP is really functioning, and on the other hand the presence of errors that *should not* occur.

With respect to nonoccurrence of predicted errors, it is important to recognize that every language is riddled with forms that are plurifunctional, at least by crosslinguistic standards. That is, they apply indiscriminately across meaning distinctions that other languages require speakers to make. If children were really concerned with eliminating plurifunctionality, we could expect many more errors than have in fact been reported.<sup>9</sup> Of course, even though errors are not reported, they may still occur. But when we set the tiny handful of error types of which we are aware against the vast number of structures in which children could be predicted to make errors according to OP:UNIFUNCTIONALITY, we must wonder why there are so few.

What about errors that would *not* be expected to occur? OP:UNIFUNCTIONALITY is designed to account for "late" errors in children's speech that, in effect, give separate marking to two closely related notions that previously were (correctly) expressed by the same form. Another genre of "late" errors is essentially the opposite: after a long period of using certain forms separately and accurately for distinct but related meanings, children begin to substitute these forms for each other occasionally in violation of the dividing line between their

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<sup>9</sup>Examples of common (American) English words that are clearly plurifunctional include *to know*, *to mean*, *to live*, and *can*. These words can apply across a semantic range that is divided and assigned to two words in a number of other languages. *To know* means both "to know something," e.g. a fact, as in "I know how old you are," and "to be acquainted with," as in "I know Mary" (Dutch *weten* versus *kennen*). *To mean* means both "to signify," as in "This word means . . ." and "to intend," as in "I mean . . ." (Dutch *betekenen* versus *bedoelen*). *To live* means both "to dwell," as in "John lives in New York" and "to be alive, exist," as in "How long do turtles live?" (Dutch *wonen* versus *leven*). Finally, *can* means both "be able to," as in "I can swim," and "have permission to" (=may), as in "Can I have a cookie?" (Dutch *kunnen* versus *mogen*). If English-speaking children strive to eliminate plurifunctionality, they should at some point try to disambiguate these meanings, but there is no evidence that they do; *can*, in fact, persists stubbornly with both meanings despite efforts by parents and teachers to persuade children to substitute *may* for one of its senses.

meanings (Bowerman, 1978, 1982c, 1983a). Examples in English include mutual substitutions of *make* and *let* (active versus permissive causation, respectively; e.g. *MAKE me watch TV* (=let; begging for permission) and *I don't want to go to bed yet. Don't LET me go to bed* (=make)); of *put* and *give* (cause-change-of-location to inanimate versus animate goal, respectively; e.g. *We're PUTTING our things to you* (=giving) and *GIVE some ice in here* (=put; into ice-crusher)); and of *put* and *make* (cause-change-of-location versus cause-change-of-state, respectively; e.g. *I PUT it brown* (=made; after child colors skunk's stripe brown), *MAKE them back up* (=PUT: request to have dolls that had fallen off table put back).

Errors like these are made at about the same age and with roughly the same frequency as errors that have been used in support of OP:UNIFUNCTIONALITY (both are infrequent, relative to correct usage). It is unclear why they should occur if children prefer unifunctionality and try to eliminate plurifunctionality, since they result in the blurring of meaning distinctions that previously were observed. A possible explanation is Slobin's (1985) suggestion that "Basic Notions" sometimes "persist": he uses the *put/give* confusions to illustrate the continuation of an earlier tendency to collapse the distinction between possessors and locative goals. One problem with this explanation is that it does not account for why the errors do not occur from the child's first uses of the forms involved. But more important for present purposes is the methodological problem it raises. Taken together, OP:UNIFUNCTIONALITY and "persistence" define a closed system: errors showing the "splitting" of semantic notions constitute evidence for the striving for one-to-one mapping, but comparable errors showing the "lumping" of notions do not constitute counterevidence because they can be explained in some other way. I return later to the possible causes of "splitting" and "lumping" errors.

### 3.2. Choosing Among Alternative Explanations

When every outcome can be explained but the explanatory principles buttress each other and so resist disconfirmation, it is time to look hard at whether the principles are the right ones or whether others would not do the job better. Efforts to home in on the right explanations become even more essential when there is more than one way to interpret given findings. Alternative explanations can in some cases be found within the set of OPs proposed by Slobin (1985), and the possibilities expand when we consider other researchers' efforts to account for the same data, e.g. the set of OPs offered by MacWhinney (1985).

Although the problem of multiple explanations is endemic to social science research, it is particularly serious for the OP approach, as it is developed by Slobin (1985), for the following reason. A pervasive feature of the approach, as noted earlier, is that children's errors are interpreted wherever possible in terms of the learner's *predispositions or preferences for how meanings should be*

*mapped into linguistic forms*. Slobin's hypotheses about these "mapping preferences," as I will call them, are interesting and generally plausible. However, because they postulate an intrinsic mode of organization, independent of children's experience with any actual language, they require particularly strong justification. In my view, the need for OPs specifying inherent mapping preferences has not yet been satisfactorily demonstrated because we have not yet thoroughly explored alternative, more everyday interpretations of the phenomena that such OPs are designed to explain—in particular, explanations that focus on children's *experience with the structural properties of the language being learned*. Forcing ourselves to pose alternatives to our favorite explanations is tedious, and we often lack the data to do careful evaluations. However, in making the effort we can pinpoint problem spots where data of a certain type are needed, and perhaps guide future research to come up with the critical information. Whatever conclusions we then reach about the nature of the language acquisition capacity will be correspondingly stronger.

In the previous section I discussed some difficulties with OP:UNIFUNCTIONALITY, which specifies that children try to mark separate meanings with separate forms. Let us now look closely at certain other OPs that are concerned with how meaning is expressed, paying special attention to alternative explanations of the phenomena on which they are based.

3.2.1. *Explanations that invoke "mapping preferences"*. Consider the following error types:

1. The use of inflections or derivational markers on words that already encode the desired meaning, e.g. *feets*, *ated* (Kuczaj, 1978), *to smoothen* (=to smooth), *to unopen* (=open), *to untake off* (=take off) (Bowerman, 1981, 1984); in Turkish, the use of the causative derivational suffix on verbs that are already inherently causative (Aksu-Koç & Slobin, 1985).
2. Constructions containing two or more bound or free forms that express essentially the same meaning but that in adult speech cannot be combined in a single sentence, e.g. *Mon mien de chapeau à moi* 'my mine of hat to me' (=my hat) (Clark, 1985), *He never won't scare me* (Bellugi, 1967).
3. Constructions that, although grammatical, are more analytic than adults would use in the same speech contexts, e.g. *make die* or *make dead* for *kill*, *make my shoe come on my foot* for *put my shoe on my foot* (Bowerman, 1982a, 1982c), *I will* for *I'll* (Bellugi, 1967).
4. The separate expression of functors that, when their meanings are juxtaposed, should be expressed with fused, portmanteau forms, e.g. *de le* 'of the' for *du* in French (Clark, 1985), and *al hu* 'on he' for *alav* 'on him' in Hebrew (Berman, 1985).
5. Constructions in which a functor is misordered or mentioned twice. The Polish conditional offers particularly nice examples of these (Smoczyńska,

1985). In the adult system, the personal ending is attached to the conditional particle *by*, which can either follow the verb, precede the verb as an enclitic, or constitute part of the connective *zeby* 'in order that'. Children make errors of the following types:

- a. Personal ending attached to verb instead of *BY*: *pisat-(e)m-BY* 'wrote-1SG-WOULD' or *ja BY pisat-em* 'I WOULD wrote-1SG' for *pisat-BY-m* 'wrote-WOULD-1SG' or *ja BY-m pisat* 'I WOULD-1SG wrote' (I would write).
- b. Personal ending redundantly marked on both verb and *BY*: *pisat-(e)m-BY-m* 'wrote-1SG-WOULD-1SG'.
- c. Conditional particle redundantly placed both before and after verb: *a moja mamusia tez BY miata-BY ładne włoski* 'and my mommy also WOULD have-WOULD pretty hair'.

Slobin (1985) explains errors types 1–4 and 5b,c by reference to children's striving for the explicit marking of meaning. The phenomenon can be covered by either OP:ANALYTIC FORM ('If you discover that a complex Notion can be expressed by a single unitary form (synthetic expression) or by a combination of several separate forms (analytic expression), prefer the analytic expression') or by OP:MAXIMAL SUBSTANCE ('While you are mastering the linguistic expression of a Notion, mark that Notion with as much acoustic substance as possible . . .'). Slobin posits both of these because, as he notes, there is ambiguity about whether redundant and overly explicit marking reflect how children try to construct their grammars (as in OP:ANALYTIC FORM) or is simply an "interim production strategy" that children employ in the early phases of getting productive control over a form or construction (as in OP:MAXIMAL SUBSTANCE).

For error type 5a and related constructions, appeal is made to a different mapping predisposition, the notion of *relevance*, which was inspired by Bybee's (1983, 1985) finding that there is strong crosslinguistic consistency in the relative ordering with respect to the verb stem of markers for aspect, tense, person, etc. The hypothesis is that children have inherent preferences for which kinds of meaning "belong together" and should be expressed by forms in close proximity to each other. Two OPs are designed to capture this, and either one can account for error type 5a. OP:RELEVANCE states that "If two or more functors apply to a content word, try to place them so that the more relevant the meaning of a functor is to the meaning of the content word, the closer it is placed to the content word. . . ." This OP is invoked to explain why Polish children often attach the personal ending to the verb rather than to the conditional particle: Markers of person are more relevant to verbs. OP:OPERATORS states that "If a functor operates on a whole structure (phrase or clause), try to place it external to that structure, leaving the structure itself unchanged." This one accounts for the same error in terms of children's preferences for handling the conditional particle rather than the personal ending: as a clause-level operator, it should not disturb



the rest of the clause (e.g. by receiving the personal ending, which in all nonconditional sentences goes on the verb), and it should ideally be clause-external. Of course, these two "relevance" principles do not conflict but in fact could work together to co-determine this particular error type.

3.2.2. *Competition.* All of these explanations are plausible, and they have a certain intuitive appeal to cognitively minded researchers. But the overall effect is somewhat piecemeal; for example, error types that seem to be closely related (e.g. 5a versus 5b and c) are sometimes explained with completely different principles. A more integrated proposal to account for errors of types 1–5 invokes a different kind of processing principle: *competition* among different methods for expressing the same or closely related meanings (Bowerman, 1978, 1981, 1984; MacWhinney, 1978, and especially his detailed treatment in this volume).

Competition is seen as a process that takes place at the moment of speech, when the speaker's intention to express a certain meaning activates more than one linguistic device associated with this meaning. The conflict is usually resolved by implicit pre-speech editing, but when it is not, errors occur.<sup>10</sup> The type of error depends both on what has been competing with what and on the relative activation strengths of the rival forms. Sometimes two or more linguistic devices are inappropriately combined within a single word (error type 1 above, e.g. plurality is double-marked in *feets*) or within a single sentence (error type 2, e.g. negation is double-marked in *He never won't scare me*). Second, analytic variants may win out in contexts where adults prefer synthetic variants or *vice versa* (error type 3, e.g. *I will . . . for I'll . . .*). Third, when two forms cannot combine with each other but must be replaced by a synthetic form (e.g. *du* for *de le* 'of the' in French), the two forms may nevertheless each have such high activation strength as the preferred method of expressing their particular meaning that they are inappropriately selected together (error type 4).

Competition accounts for errors 5a–c in terms of the activation of more than one possible *position* for a given morpheme (MacWhinney, 1985). In 5c the conditional particle is redundantly expressed in two separate positions. Both positions are activated because both are acceptable in Polish; errors that result from selecting them simultaneously are analogous to common errors of English

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<sup>10</sup>Competition is assumed to affect the speech of adults as well as children; see, e.g. Laver, 1973; Stemberger, 1982). An interesting difference between children and adults is that adults often self-correct their own competition errors and recognize them immediately as errors when asked to reflect on them; children, in contrast, correct at least certain kinds of errors very infrequently and may be unaware that they *are* errors (even though they may self-correct other kinds of errors) (Bowerman, 1978; see also Kuczaj, 1978, on children's explicit acceptance of forms like *ated*). This might seem an argument against interpreting the errors of children in terms of competition. However, it is not necessary within the competition approach to assume that the speaker has firm ideas about which of the competing alternatives is right, or knows that certain forms cannot be combined with each other; this may well be an independent kind of knowledge that develops with age.

speakers like *Pick up your socks up*. In 5a the personal ending is inappropriately attached to the verb rather than to the conditional particle, and in 5b it occurs on *both* the verb *and* the particle. Both positions are activated because, although in conditional sentences the ending should go on the particle, in all nonconditional sentences it goes on the verb. Notice that, in contrast to Slobin's account of error 5a with "relevance" OPs, the competition account makes no assumption that Polish children have an inherent preference, independent of experience with the structure of Polish, to put personal endings on verbs rather than elsewhere, or to keep conditional markers from interacting with the rest of the clause.

To researchers interested in the role of meaning in language acquisition, competition might at first appear to be a mechanical approach that "explains away" children's errors as uninteresting or irrelevant to the development of a theory of language acquisition without offering anything constructive in return. Fortunately, this is not true. It is true that competition is a very general cognitive principle, applicable to many behaviors in addition to language production (e.g. see Norman, 1981). However, when it is invoked in explaining phenomena in any particular behavioral domain, competition offers an unparalleled opportunity to discover underlying structuring principles that are difficult to get at in other ways. The important clues lie in *what competes with what*. Competing forms have been activated by the *same underlying intention*, whether the actor's purpose is linguistic expression or some other goal. By observing which linguistic forms compete and interfere with each other at successive points in children's development, and inferring the semantic and grammatical bases for this competition, we can achieve a deeper understanding of *how meaning is structured* for young children, how the organization of meaning changes and develops over time, and how children develop an implicit sense of the way the various parts of the linguistic system are interrelated.

3.2.3. "Mapping preferences" versus "competition": Which account to prefer? Neither Slobin nor MacWhinney regards "mapping preferences" and "competition" as mutually exclusive explanations. MacWhinney allows for the role of "relevance"-like predispositions, along with competition, in promoting certain ordering errors. Similarly, Slobin suggests that certain ordering and over-marking errors reflecting OP:RELEVANCE, OP:OPERATORS, or OP:MAXIMAL SUBSTANCE are co-determined by OP:MORPHEME PLACEMENT: "Mark a notion in the same place in the various constructions in which it can occur. . . ." This OP is a version of the "competition" principle according to which the position associated with a certain morpheme (or class of morphemes) in sentences of one type can have high enough activation strength to attract that morpheme even in sentences where another position is required.

Slobin and MacWhinney do not take up the problem of whether both accounts are really necessary, and, if so, which one applies where. In evaluating the two

approaches, it seems to me that "competition" has the following points in its favor:

1. It is more conservative in what it attributes to the prelinguistic child. According to "mapping preference" OPs, children have certain predispositions, independent of experience with a particular language, for relating meanings to forms in certain ways, and they draw on these predispositions in constructing their grammar. The competition account, in contrast, does not assume that the child has any particular prelinguistic expectations about form-meaning relationships. Instead, it looks to *children's experience with the structures of the language they are learning* for help in explaining errors of the types we have been considering. Any model of language acquisition must deal with the fact that children learn alternative ways to express similar meanings, and account for how they sort out which devices to use on which occasions. The competition account simply exploits this process in accounting for children's errors. I have argued earlier that, as a general procedure, we should avoid crediting children with inherent preferences in form-meaning mapping unless we are forced to. As long as explanations based on general learning principles in interaction with the properties of the input seem to be sufficient to explain the data, the burden of proof is on the advocates of inherent preferences to show that this more powerful explanation is really needed.

2. The competition interpretation is more parsimonious, accounting with a single principle for data that require several "mapping preference" OPs. The best model in the end may not be the most parsimonious one, but, just as we should not postulate intrinsic linguistic preferences unless the phenomena they are designed to handle cannot be satisfactorily explained with other principles that our model will clearly need anyway, we should not favor a less parsimonious model over a more parsimonious one unless it provides a better account of the available evidence. Competition is not only the more parsimonious explanation but at present it seems to account for several error types that are not currently covered by "mapping preference" OPs and that sometimes in fact violate mapping preference predictions. These include:

- a. The substitution errors described earlier as counterinstances to OP:UNIFUNCTIONALITY, E.G. *put* for *give* or *make* for *let* (see Bowerman, 1978, 1984, and MacWhinney, 1985, for an interpretation of these in terms of competition between semantically related forms).
- b. Errors in functor ordering where an appeal to "relevance" is difficult because two functors with closely related meanings show symmetrical rather than directional misordering with respect to their "closeness" to the nucleus: e.g. both *hook out* for *unhook* (a necklace) and *untake* for *take out* (stitches in a sewing project) (Bowerman, 1981).
- c. Errors in which a synthetic lexical form is used where its analytic counterpart is needed (the converse of error type 3 above), e.g. *It BRINGS your wishes true*

(=makes your wishes come true; said of a "magic" pebble); *water BLOOMED these flowers* (=made these flowers bloom) (Bowerman, 1981, 1982a). Notice that such errors run counter to OP:ANALYTIC FORM, just as substitutions like *put* for *give* run counter to OP:UNIFUNCTIONALITY.

3. The competition account is more comfortable with the low frequency of many key error types in children's speech, relative to correct usage. Prevailing correct usage or alternation between correct and incorrect forms is something of an embarrassment to the "mapping preferences" approach. If children strive for explicit marking in accordance with OPs UNIFUNCTIONALITY and ANALYTIC FORM, why do they not use unifunctional and analytic forms routinely? (See also Maratsos, 1979, who criticizes Slobin's (1973) earlier Operating Principle G, "Semantic relations should be marked overtly and clearly," on this basis.) Since the competition account does not ascribe errors to children's mapping preferences, infrequency of errors does not matter; the proportions of incorrect to correct versions of a construction would be explained purely by reference to the relative activation strengths of the competing alternatives.

4. The competition approach can deal with the absence in child speech of many errors that the "mapping preference" approach predicts. It was noted earlier that languages are peppered with plurifunctional forms where errors reflecting OP:UNIFUNCTIONALITY would be predicted but do not seem to occur. Similarly, there are countless opportunities for OP:ANALYTIC FORM to go into action, but in most cases children appear to be content with synthetic forms (see Bowerman, 1982c). This noncommission of errors is puzzling for the "mapping preference" approach, but unproblematic for the competition account, as long as it can plausibly be argued that only the correct form should get activated in connection with the relevant meaning or at least that it has much higher activation strength than more analytical competitors.

Despite these immediate advantages for the competition account, it would be premature to conclude that children's "mapping preferences" play no role in errors of the sort we have been considering. At least the following points should be considered:

1. Crediting children with notions of "relevance"—ideas about what meanings "belong together" and could be expressed by the same form or by forms in close proximity to each other—may seem gratuitously innatist if competition can account for the same error data. But if children have no intrinsic notions of relevance, we are left with a mystery: Why, as Bybee (1983, 1985) has shown, is there so much crosslinguistic consistency in the placement of various grammatical markers relative to content words?

Perhaps, then, children *do* have notions of relevance after all. The problem is that there is as yet no strong evidence for this. Earlier, we saw that when Polish children attach the personal ending to the verb rather than to the conditional marker where it belongs, there is no need to invoke notions of relevance. This is the

*normal position* for the ending in nonconditional sentences, so Polish children could be expected to favor it on general grounds of consistency and frequency (which contribute to greater activation strength), even if they have no *a priori* notions about relevance at all. The other examples given by Slobin (1985) in support of the two "relevance" OPs, RELEVANCE and OPERATORS, are not compelling for the same reason: The positions in which the morphemes incorrectly occur in accordance with "relevance" are positions that *in other common construction types are appropriate for these morphemes* (or, in some cases, their contextually-determined variants). Additionally, in the case of operators, the variant the language uses in the "more relevant" clause-external position tends to be freestanding and acoustically more salient than its bound, sentence-internal counterpart, so it might be acquired first simply because it is more easily picked out of the speech stream rather than because its position is favored on semantic grounds.<sup>11</sup>

To strengthen OP:RELEVANCE and OP:OPERATORS, we need additional evidence. There are two kinds that would help, and they may both eventually be forthcoming: (a) evidence that children disorder forms in accordance with "relevance" *even when there are no alternative construction patterns* in the language in which the forms are positioned in the same way as in the errors, and (b) evidence that when both a "relevant" and a "nonrelevant" position for a morpheme are modeled in the input, children prefer the "relevant" position *even when the language uses it in less frequent or far fewer construction patterns than it uses the "nonrelevant" position*.

2. Karmiloff-Smith (1979b) found that while doing an extended task that required devising a notational system for route directions, children sometimes spontaneously decomposed their own "synthetic" symbols. That is, they abandoned symbols that they had used earlier to convey two or more pieces of information simultaneously, replacing them with separate symbols that represented each meaning element more explicitly. Karmiloff-Smith argues that these decompositions are related to language errors in which children replace a synthetic morpheme with a sequence of morphemes in which each meaning element is spelled out separately: She attributes both to the child's efforts to mark separate meanings with separate forms until the meanings are thoroughly mastered. It is not clear how the competition account could be extended to cover cases of "decomposition" when there is no input system (whether language or something else) that offers the child *alternative ways* to express the same or similar mean-

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<sup>11</sup>For example, the clause-external *no* found in the speech of some children learning English may have its source in input sentences with anaphoric negatives like *No, don't cut two holes* (child version: *no (me) cut two holes*) (see deVilliers and deVilliers, 1979, for discussion) and *Kendall doesn't want to take a bath, no . . . oo . . . oo!* (child version: *Kendall take bath no*) (construction of this latter type were common in the speech of my subject Kendall and her parents; from my unpublished records).

ings. In sum, the “mapping preference” account can interpret a variety of one-to-one mapping behaviors within a common framework, whereas the competition account apparently cannot (Karmiloff-Smith, personal communication).

3. Even if we restrict our attention to language, competition seems at best to provide only a partial explanation for errors that have been cited in support of one-to-one mapping preferences. In particular, it gives no account of (a) why certain forms compete in children’s speech and other do not, (b) why errors that seem to reflect competition often set in late in children’s speech, long after the forms involved are well established and used productively and flexibly (Bowerman, 1978), and (c) why, at the same time that children begin to *produce* distinct markings for the meanings encoded by certain plurifunctional forms, they may also start to make *comprehension* errors that suggest that they have come to associate the form narrowly with only one of its meanings (Karmiloff-Smith, 1979a, 1979b).

As noted earlier, one important basis for competition among forms is *meaning*: Forms that, from the speaker’s point of view, express the same or closely similar meanings will tend to be activated simultaneously and compete for selection. This means that the most satisfying explanation for many of children’s errors may ultimately be one that combines the “on-line” notion of competition with an account of how children organize meaning and how their meaning structures change over time. I defer further discussion of this possibility until a later section in which I consider children’s early structuring of semantic space in connection with Slobin’s (1985) “Basic Child Grammar” hypothesis.

3.2.4. *Other Interpretations Based on Linguistic Experience.* Not all errors discussed by Slobin as evidence for “mapping preference” OPs can be interpreted alternatively in terms of competition. However, other interpretations for these errors often need to be considered which—like competition—invoke the child’s *experience with the distinct structure of the language being learned*, and correspondingly minimize the need to postulate intrinsic preferences for how meaning should be mapped. Consider the following example.

A characteristic error of children learning Hebrew is to insert a subject pronoun into relative clauses, as in *ha yeled še HU nafel ba mayim* ‘the boy that HE fell in the water’; *ha iša še HI ra’ata et ha naxaš* ‘the woman that SHE saw OBJ the snake.’ Slobin (1985) cites these examples in support of OP:CANONICAL CLAUSE FORM: “If a clause has to be reduced, rearranged, or otherwise deformed when not functioning as a canonical main clause, attempt to use or approximate the full or canonical form of the clause.” This OP reflects the more general “mapping preference” theme that children prefer a clear marking of meaning: “As in the discussions of OP:MAXIMAL SUBSTANCE and OP:EXTENSION . . . , overt marking of all sentence participants is an early and persistent characteristic of child language” (Slobin, 1985, p. 265). In this case, children avoid deletion of the nominal argument of a verb in an embedded clause.

If overt marking of all sentence participants is important to children, why do children learning English not make the same error, since—as the English glosses on the Hebrew sentences indicate—there is the same opportunity? And here a structural difference between Hebrew and English becomes relevant. In adult Hebrew, relative clauses often contain a resumptive pronoun (i.e. a pronoun that “copies” the relativized head into its underlying position in the relative clause). Such pronouns, which are required for oblique objects and optional for direct objects, are suggested by the following translation-equivalents: ‘Here’s the boy *that I gave the candies to-HIM*’; ‘Here are the candies *that the boy took THEM*’ (Berman, 1985 and personal communication). Resumptive pronouns do not occur in subject position, however. In English, resumptive pronouns are considered ungrammatical in any position in relative clauses (although they do occasionally occur with oblique objects).

These structural differences—a regular pattern of resumptive pronouns in Hebrew relative clauses to which subjects are an exception, versus no such pattern in English—coincides with making or not making the error of inserting a subject resumptive pronoun. This suggests that Hebrew-speaking children make the error not because of a language-independent preference to mark all nominal participants in a clause overtly (because then English-speaking children should make the same error), but because they try to extend a characteristic pattern of Hebrew to cases that are exceptions to it.<sup>12</sup>

This example provides a clear illustration of how accounting for errors requires careful study of the structure of the language being learned. Taken out of context, many errors might be interpreted as evidence for prelinguistic mapping preferences. Analyzed with reference to related construction patterns in the language, however, they often point instead to children’s close attention to the structural properties of the system to which they are exposed.

### 3.3 From a List of Operating Principles to a Theory of Language Acquisition

In the preceding sections I have discussed some specific examples of problems in the OP approach as it now stands, like the freedom with which new OPs can be inserted to deal with potential counterevidence, the way simple rewording can sometimes convert the preferences or procedures stated in OPs to their opposite, the explanation of apparently related error types with completely independent

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<sup>12</sup>This interpretation is adequately provided for within the OP system by OP:EXTENSION: “If you have discovered the linguistic means to mark a Notion in relation to a word class or configuration, try to mark the Notion on every member of the word class or every instance of the configuration, and try to use the same linguistic means to mark the notion.” This OP reflects a very general principle, not limited to language, such as “be consistent” or “avoid exceptions” (see also Smoczyńska, 1985). It does not say anything about what kinds of mappings children prefer, independently of the language they are learning, and it is therefore to be favored over OPs that do call on such preferences whenever both are applicable.

OPs, and the tolerance for alternative explanations of the same data. These difficulties all seem to reflect a larger problem, of which those who have worked within or been inspired by the OP approach are well aware: the lack of conceptual "glue" to bring the OPs into a compelling *relationship* with one another.

The need for such glue in the development of a theory is insightfully discussed by Kaplan (1964). Kaplan defines a theory as, most fundamentally, a system of laws. Of the two basic types of such systems described in his book, one stands out as immediately applicable to the OP approach:

A *concatenated* theory is one whose component laws enter into a network of relations so as to constitute an identifiable configuration or pattern. Most typically, they converge on some central point, each specifying one of the factors which plays a part in the phenomenon which the theory is to explain. . . . This is especially likely to be true of a theory consisting of tendency statements, which attain closure only in their joint application. (p. 298, emphasis in the original)

In the OP approach, language acquisition is of course the phenomenon to be explained. The OPs are the component laws that converge on this phenomenon, and they must operate jointly to obtain closure. But the "network of relations," "pattern," or "configuration" is still missing. Kaplan is rightly emphatic about the need for this:

The laws are altered by being brought into *systematic connection* with one another. . . . The theory is not the aggregate of the new laws but their connectedness, as a bridge consists of girders only in that the girders are joined together in a particular way. The theory explains the laws, not as something over and above them, *but by giving each the strength and purpose which derives from the others.* (p. 297, emphasis added)

Without connectedness, there are not enough constraints on what is possible. Every acquisitional phenomenon can in some way be accounted for, but the sense of compellingness or inevitability that would come from a more integrated system is still absent.

Where could the needed integration come from? One step, as Clark (1980, 1985) has urged, may be to establish how hypothesized OPs must be *ordered* and *weighted*. This would allow us to specify when each OP comes into play (e.g. as children move sequentially through possible solutions to a problem, or depending on what they have already learned about their language), and to state which procedure will take precedence in cases of conflict between OPs.<sup>13</sup>

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<sup>13</sup>One important problem with working out the notion of ordered OPs is the following. In the OP approaches of both Slobin (1985), and MacWhinney (1985), it is essential that children be able to distinguish between adequate and inadequate solutions to linguistic problems. When an adequate one is achieved the child is held to stop working on the problem and the strategi(es) used in finding it are strengthened; in contrast, when the solution is inadequate the child repeatedly comes back to the



Will this be enough to weld the girders into a bridge? I think that further work will be needed. In particular, I suspect that at present the OP approach does not provide sufficiently for how children home in on a grammar with certain "deep" structural properties and not others. The OP approach has done a great deal to increase our awareness of the importance for language acquisition of surface structural details like whether relative clauses look rather like main clauses placed after their head noun or are compacted into prenominal modifiers, and whether spatial location is expressed with prefixes or suffixes, prepositions or postpositions. But the OPs do not guide the child toward a grammar with more abstract universal syntactic constraints or parameters of the sort associated with the work of Chomsky (e.g. 1981) and his colleagues, nor are they intended to.

Nativist theorists would certainly urge that what is missing from the OP approach is a theory of grammar: a conception of how surface variability is constrained by deeper syntactic principles, and an account of how children's obedience to these principles guides their construction of a grammar for a particular language. I am reluctant to promote this solution too strongly because I am

problem, applying, by hypothesis, sequentially ordered strategies until it is solved (OP:PER-SISTENCE, Slobin, 1985).

How does the child know whether his solution is adequate or whether he needs to try the next OP on the list or the next clause in the same OP? According to Slobin and MacWhinney, the child's critical cue that a solution is inadequate is his detection, through monitoring, of *mismatches*, either (a) between what he says or would have said in particular contexts and what fluent speakers say, or (b) between the input and his ability to parse and make sense of it.

Mismatches are surely important, but they can by no means be the only factor that spurs the child to do further analysis. In particular, further work clearly takes place in situations of the following two kinds, even in the absence of mismatches:

1. Children sometimes construct overly general rules that account for all acceptable utterances of a given pattern (e.g. passives, shifted datives) and err only in that they also generate unacceptable ones (e.g. *she said me 'no' = she said 'no' to me*). In these situations, children can predict and fully account for all exemplars from other speakers, and they receive little or no negative feedback about their own productions. According to the "mismatch" account they should consider the problem solved. Nevertheless, they eventually identify words or semantic classes of words to which the rule does not apply, and adjust the rule accordingly (see Bowerman, 1983b, for discussion). What motivates their further work on the problem?
2. Children sometimes move from the correct, flexible, and adultlike use of certain construction patterns to incorrect, "mismatching" forms. This often seems to reflect the formulation of new, more abstract rules or relationships that link forms that were previously independent in the child's grammar; however, a grasp of these relationships is *not needed* for either normal production or comprehension (Bowerman, 1982b, 1982c; Karmiloff-Smith, 1978, 1979b). Again, what motivates the child to look for a new "solution" to what is not in any clear sense a "problem"?

An adequate theory of language acquisition must be able to account for cases like these where children find structure "because it is there," and not because they cannot resolve "mismatches" otherwise. Any attempt to order OPs in terms of when they will apply must come to grips with this phenomenon.

not yet convinced that we know which theory of grammar to install in the place of honor, and I think that at least some of the formal principles that nativist theorists have proposed crediting to the child's innate endowment can be learned on the basis of experience (see Bowerman, 1983b). It is probably true, however, that before the OP approach can develop into a truly satisfying theory of language acquisition it will have to deal more directly with the problem of how purely formal constraints on grammatical structure are incorporated into children's grammars.

To summarize, in this section I have considered the problem of testing and systematizing proposed OPs. I first argued that, with the expansion of the OP model, it has become difficult to test OPs directly because they often form closed systems that can interpret all observed outcomes. In addition, counterevidence can often be reinterpreted to be compatible with existing assumptions. A second problem is that multiple explanations for children's language behaviors have tended to proliferate within the OP approach. We need to test alternative explanations by systematically drawing out their predictions and determining whether these are met. In some cases we find absence of errors that should be committed if a particular OP is correct, whereas in others we find errors that should not be committed. Only by submitting candidate OPs to rigorous crosslinguistic analysis can we determine which ones fit the contours of the data and which are superfluous. Finally, I stressed the need to integrate OPs into a coherent system.

Woven through these methodological arguments has been a persistent substantive worry: that many of the OPs outlined in Slobin (1985) attribute to the child stronger inherent predispositions about the way meanings should map into linguistic forms than can be justified by the data currently available. In the following section we look more directly at the problem of how strongly language acquisition is directed by propensities that arise independently of experience with a particular language.

#### 4. BASIC CHILD GRAMMAR: IS ONLY ONE WAY OF STRUCTURING SEMANTIC SPACE "BASIC"?

Children who acquire language with Slobin's (1985) set of Operating Principles may not yet have been accorded much help from inborn ideas about formal syntactic structure, but when it comes to the organization of meaning they are richly endowed. Piecing together evidence from many languages, including most centrally those discussed in these volumes, Slobin (1985) has formulated a claim of great theoretical importance. He argues that children approach the language acquisition task with a prestructured "semantic space" in which meanings and

meaning clusters constitute a "privileged set of grammaticizable notions" (see p. 217) onto which functors and other grammatical constructions are initially mapped. The particular forms that get mapped vary from language to language, of course, but the *basic meanings are constant*, along with positioning constraints and certain other syntactic properties that result from the way OPs like RELEVANCE interact with the linguistic input, as I discussed in the preceding section. The outcome of this initial mapping process, according to Slobin, is a "universally specifiable '*Basic Child Grammar*' which reflects an *underlying ideal form of human language*" (p. 204, emphasis added).

The Basic Child Grammar (BCG) hypothesis is a bold and intriguing extension of what has long been a deep conviction of many child language scholars, that language-learning children do not simply passively accept the structures their language offers them, but actively strive to organize and make sense of the linguistic input in their own way. Beyond this, the claim has particular current importance because it accords closely with arguments from other fields that there is a universal cognitive/semantic substratum for language, most notably Bickerton's (1981) proposal, based on creole studies, for an innate language "bioprogram."

Because of its theoretical importance and because it attributes strong built-in dispositions to the language-learning child, the BCG hypothesis demands the closest scrutiny. How we evaluate it depends partly on how literally we take its claims. Slobin himself is in fact rather cautious. Reminding us that the available data are still sparse, he proposes the BCG hypothesis as a "broad first pass . . . at the mechanisms of the L[anguage] M[aking] C[apacity] that may be responsible for children's *preferences* to construct language in particular ways, knowing full well that such abstracted and generalized preferences cannot account in detail for the acquisition patterns of particular, individual children" (p. 206). While I recognize this qualification and appreciate the coherent theoretical perspective that such a "first pass" provides, I have chosen to interpret the notion of a universal basic child grammar rather strictly, for two reasons.

First, a strict interpretation is called for by the word "grammar." A "grammar" is not a loose collection of tendencies or preferences, to be sometimes observed and sometimes not, but a strong set of constraints. Although Slobin perhaps does not intend for the notion of a universal child "grammar" to be interpreted so literally, the word carries an impression of rigor that will encourage readers, especially those outside the field of child language, to assume that children's early grammars are much more uniform than they are.

Second, the only way we can make progress in understanding the conceptual underpinnings of children's early grammars is to test hypotheses about these underpinnings against data from individual children. Of course, hypotheses about "preferences" cannot be discarded immediately just because counterinstances are found. However, if it turns out that children frequently or con-

sistently fail to behave as the hypotheses predict, we will have to conclude that the proposed preferences are weak or nonexistent.<sup>14</sup>

The problem to be solved is a classic “nature-nurture” dilemma: To determine the relative contribution to children’s early grammars of, on the one hand, inherent, universal tendencies to structure conceptual material in certain ways and, on the other, experience with an input system that shapes meanings in a language-specific fashion.<sup>15</sup> In the following discussion I argue that the BCG hypothesis does contain a fundamental insight into early language development: that children’s starting semantic space is not a *tabula rasa*, passively awaiting the imprint of the language being learned before taking on structure. Rather, children are conceptually prepared for language learning: They can spontaneously categorize objects, events, situations, etc. for purposes of linguistic expression and, moreover, in doing so they use meaning distinctions that are *relevant for language*—i.e. distinctions of the sort that *often figure in the semantic systems of natural languages*. However, I will urge that these “candidate meaning distinctions” are far less rigid than the BCG hypothesis predicts. Specifically, they do not define a single, privileged set of semantic notions that strongly attracts the grammatical forms of the input and molds the initial usage of these forms to a uniform pattern. Rather, they are better understood as a system of *relatively accessible alternatives* for structuring semantic space within any particular conceptual domain—i.e. as a set of salient meaning distinctions that children will tend to try out first.

If, as I contend, the initial organization of semantic space is not fixed but flexible, there will be variation in the meanings children initially link to grammatical forms. One important factor that can influence the meanings children adopt is the *semantic structure of the input language*—i.e. the specific meaning categories associated with the grammatical forms of the language in the speech of fluent speakers. I argue that children are prepared from the beginning to accept linguistic guidance as to which distinctions—from among the set of distinctions

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<sup>14</sup>Here the lessons of the pivot grammar should not be forgotten. The pivot grammar was the reigning model in the 1960s of how children develop syntax. Like “Basic Child Grammar,” the model was a composite, built up from the findings of several researchers who had studied different children. When the predictions of the model were finally tested carefully against data from individual children, it turned out that *no* child observed its rules (Bowerman, 1973; Brown, 1973). The model’s failure to predict or describe what children’s utterances actually looked like, coupled with proposals for a “deeper” and more satisfying level of description (Bloom, 1970), quickly led to the model’s demise.

<sup>15</sup>This problem is related to, but not identical with, the issue of whether children have inherent mapping preference, which was discussed in the previous section. There the question was not what children’s meanings are, but whether children have intrinsic ideas about how meanings—whatever these may consist of—should be encoded in language (e.g. one form for each separate meaning element?). In the present section I am concerned with whether there is prestructuring, independent of linguistic experience, of the child’s *categories of meaning themselves*.

that are salient to them—they should rely on in organizing particular domains of meaning. In consequence, there is no single, universally shared “Basic Child Grammar.” Children begin with grammars that are slanted toward the semantic structure of the input language, even if not yet in perfect accordance with it.<sup>16</sup>

Like Slobin, I base the conclusion that there is structure in starting semantic space on children's errors—both errors of commission, in which a form is extended to meanings for which it has never been modeled in the input, and errors of omission, in which a form is used over a semantically restricted portion of its full adult range. Errors of both types are typically linguistically “sensible,” in that they reflect principles for categorizing meaning that play a specifiable role in the structure of one language or another. Sometimes errors reflect the child's reliance on a principle of semantic categorization that is relevant for the meanings of certain forms in the language being learned, but not the forms to which the child has applied it. Alternatively, the semantic principle may have no clear function in the child's language, but be important in the structure of another language, often even in connection with a form exactly parallel to the one with which the child has linked it.<sup>17</sup>

My conclusion that the structure of starting semantic space is *more flexible* than Slobin postulates in his Basic Child Grammar hypothesis is based on three types of evidence:

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<sup>16</sup>In a footnote (no. 13), Slobin (1985) provides what is in fact a very important qualification on how deterministically BCG imposes its semantic structure on the input. Slobin suggests that children may give separate treatment to semantic notions that BCG is in general prepared to conflate if their language distinguishes these notions with linguistic devices that, on formal grounds, are equally easy for children to learn, especially if these devices are highly differentiated (e.g. involve opposite word order patterns). If the implications of this footnote were carefully worked through, I think the BCG hypothesis would have to be weakened so much to allow influence from the semantic structure of the input language on the child's initial organization of semantic space that the position reached would be very similar to the one I am advocating.

<sup>17</sup>For example, Clark (1977) shows that the perceptual properties on the basis of which young children overextend words for objects (e.g. roundness, longness) are very similar to those that repeatedly turn up as important in the noun classifier systems of the languages of the world. (A noun classifier is a morpheme used obligatorily in constructions involving (depending on the language) counting, predicating motion or manipulation of, or locating objects, e.g. “Nine round-things gourds.”) Similarly, in Bowerman (1980) I proposed that one child's use of *night night* in connection with the horizontal position of objects that are normally extended vertically (e.g. a Christmas tree) reflected a sensitivity to features of extendedness and spatial orientation that play a relatively small role in the semantic system of English (e.g. *stand* versus *lie*) but that are of central importance to the classifier systems of many languages (e.g. Denny, 1979; Watkins, 1976). I also suggested that overextensions such as *kick* applied to an action of throwing, *sleeves* to pant legs, and *ankles* to wrists reflect an equation between upper and lower extremities that, although not formally encoded in English, is routine in many other languages (e.g. the word for “finger” is often also used for “toe”; Andersen, 1978). See also p. 336 of this chapter and Bowerman (1978, 1983a) for further examples and discussion of how children's incorrect form-meaning mappings often reflect linguistically sensible distinctions.

1. In some—and probably many or most—domains of meaning associated with grammatical forms, children adopt correct or near-correct meaning distinctions (i.e. those used by the local language and displayed in the input to the child) essentially from the start, even when these distinctions partition semantic space in strikingly different and language-specific ways.

2. When children do use forms in connection with nonmodeled meanings (i.e. meaning categories that are not displayed in the input), this deviation from adult norms is often not their *first* step. Errors that have been interpreted as reflecting a “basic” or “ideal” organization of semantic space sometimes emerge only *after* a period in which the forms involved are used flexibly and productively across correct and language-specific categories of meaning. Moreover, when “nonmodeled-meaning” errors arise relatively late, they often remain *infrequent* relative to correct use. They therefore should not be assumed to have direct implications for the properties of the *grammar* the child has formulated (see p. 309 above).

3. Children learning the same language vary to some extent in the meaning categories they first associate with grammatical forms. Some children first use a form in connection with a very narrow (underextended) category of meaning, whereas other children extend it across a broader category from the start. If even children with exposure to similar linguistic input make different form-meaning mappings, the initial organization of semantic space must be less rigid than the BCG hypothesis predicts.

The following discussion of flexibility in starting semantic space cannot cover all the evidence exhaustively. I focus therefore on evidence for initial language-specificity as in (1) and (2), and refer the reader to Bowerman (1976, 1982b) for some examples concerning (3).

Before considering language-specificity in the initial mapping of grammatical forms, let us take a quick look at the BCG claim about the child’s starting point. Slobin proposes that children’s starting semantic notions are language-neutral: “Semantic Space provides Basic Child Grammar with a level of organization that serves as an opening wedge to the acquisition of language-specific grammatical distinctions, *without at first biasing the child to any particular language*” (1985, p. 228), emphasis added). As children progress, however, they are led to the diverse grammars of their individual languages through linguistic experience: “Later in development, of course, the language-specific use of particular functors will train the child to conceive of grammaticizable notions in conformity with the speech community. At first, however, there is considerable evidence that children discover principles of grammatical marking according to their own categories—categories that are not yet tuned to the distinctions that are grammaticized in the parental language” (1985, p. 218).

Whether children can begin their mapping of a particular domain of semantic space in a universal, language-neutral way depends on whether an unbiased

"opening wedge" is logically possible.<sup>18</sup> In some domains it is difficult to imagine what such a wedge would be like. In others, a candidate wedge can be envisioned; the question is whether children indeed use it. I will look at some cases of each kind, using the distinction between domains that are or are not susceptible to an unbiased "opening wedge" as an organizing framework within which evidence for both claims (1) and (2) above can be presented.

#### 4.1. Unbiased "Opening Wedge" Often Impossible: Language-Specific Learning in Hierarchically Organized Domains

Languages often differ in how finely they subdivide particular categories of meaning. For example, English expresses the first person plural notion "we" with a single pronoun, *we*. In many languages, 'we' is subdivided into two categories, either on the basis of whether 'we' includes the listener (we-inclusive versus we-exclusive), as in Tamil, or on the basis of whether it includes two people or more than two (we-2 versus we-more than 2), as in West Greenland Eskimo. In still other languages, e.g. Hawaiian, both these criteria are applied at once, resulting in a four-way contrast: we-2-inclusive, we-2-exclusive, we-more than 2-inclusive, we-more than 2-exclusive. Finally, some languages, e.g. Nogogu, go even further, honoring not only all these distinctions but also breaking down 'more than 2' into '3' and 'more than 3' for a grand total of six categories, all marked with separate forms (see Ingram, 1978). As in this example, successive subdivisions in a semantic domain often form rough hierarchies or semantic trees, with some languages making no distinctions at all, or only one, others making an intermediate number, and still others making a great many. Other good examples of hierarchies involving the meaning categories associated with grammatical forms are discussed by Denny (1978, 1979).

When semantic distinctions are related hierarchically, there is often no language-neutral starting point for the child. No matter which level of distinction-making is compatible with the distinctions recognized in starting semantic space, it is likely that this level has already been chosen by one or more languages, so BCG would automatically be biased toward those languages in this corner of

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<sup>18</sup>To forestall possible confusion, let me distinguish immediately between two ways in which we can speak of starting semantic space as being "biased." If children have their own ideas, independent of linguistic input, about how to structure semantic space, then in one sense their initial organization of semantic space can be said to be "biased." Slobin has made a number of arguments about biases or preferences of this sort. However, he also hypothesizes that these initial preferences are not biased *toward any particular language*; they can develop equally easily in a variety of directions. In this sense, then, they are neutral or "unbiased." Since the following discussion is about whether children's early grammars show language specificity, I will be using the terms "biased" and "unbiased" in this latter sense.

semantic space. Biasing can at least be minimized by assuming that the child's preferred entry level in such cases is the *most superordinate*. This would allow children to proceed by gradually moving down the semantic hierarchy as required by their language, making successively finer subdivisions of meaning. If they start at the most superordinate level children will often *collapse meaning distinctions* that their language requires speakers to make. Slobin (1985) gives several examples of this phenomenon, including the tendency to conflate static locations and goals on the one hand, and locations and possessors on the other.

The hypothesis that children start at the top of semantic hierarchies may seem plausible at first glance, since, as is well known, they often cannot or do not discriminate between meanings that the adult language distinguishes. However, if we consider the problem from a general perspective, it is clear that children cannot be expected always to enter a hierarchy at the most undifferentiated level found in any language of the world. For example, studies of the acquisition of word meaning show that children typically enter a taxonomic hierarchy of lexical items at a middle level, and proceed beyond this point not only by learning more differentiated, subordinate words but also by learning more abstract, superordinate ones (e.g. Brown, 1958; Rosch et al., 1976). Unfortunately, however, if the notions the child will come to associate with grammatical markers are organized in starting semantic space at a level more finely differentiated than the uppermost, the idealization of an unbiased starting point is rather seriously violated: Children would start by making semantic distinctions that are *observed only in certain languages* and then have to erase them if it turns out that their local language is less finely differentiated in this semantic domain.

In summary, no solution is ideal for the assumption that starting semantic space is not biased toward any particular language, but a start at the top of the hierarchy is preferable on theoretical grounds. If some children start at a lower level, crosslinguistic comparisons become crucial to establish whether this is at least the *universally preferred* entry point. If it turns out that making certain distinctions is associated with learning a language for which this is appropriate (or varies across children and cannot be linked to language), then a key tenet of Basic Child Grammar is disconfirmed.

The available data are still quite inadequate for a thorough study of whether all children choose the same entry level for semantic domains that, seen in crosslinguistic perspective, form hierarchies of meaning distinctions. One problem is that children make no errors when they enter a hierarchy at the appropriate level for their language, so correct, language-specific learning tends to escape notice.<sup>19</sup> A second problem is that when errors are reported that reflect a child's

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<sup>19</sup>A drawback of reliance on error data, as Givón (1985) has pointed out, is that it draws attention to areas of grammar in which children have trouble at the expense of areas in which they learn quickly and well. Ironically, however, Givón sees this as leading to overemphasis on (among other things) the language-specific and neglect of the universal (see p. 308), whereas I am here suggesting the



failure to distinguish two meanings that are differentiated by the language, it is seldom clear whether these errors represent the child's *standard* usage or are a minor disturbance in what is basically a correct and language-specific pattern of usage. Despite these problems in performing a full-scale evaluation of children's initial entry-levels into semantic hierarchies, there is evidence that children are capable of observing certain language-specific semantic distinctions from the start, and no evidence as yet that their counterparts learning languages that do not make these distinctions also attempt to observe them. I offer two examples.

4.1.1. *IF and WHEN in Future Predictives.* The first example concerns that section of semantic space mapped into complex "future predictive" sentences of low hypotheticality. These constructions refer to two future events that might well occur, with the occurrence of the second contingent on the occurrence of the first: e.g. *If John comes home tonight, we'll go out.* A common pattern across languages is for the antecedent event to be mentioned in a subordinate clause introduced by a subordinating conjunction, and the consequent event to be expressed in the main clause. In formulating the subordinate clause, speakers of English must attend to a rather fine meaning distinction. If the antecedent event can be fully expected to take place, the conjunction of choice is *when*. If the antecedent is only a possibility, however, the speaker should use *if*.<sup>20</sup> The role of subjective certainty can be appreciated by comparing *IF John comes home tonight we'll go out* (John may or may not come) with *WHEN John comes home tonight we'll go out* (it is assumed that John will come). The certainty/uncertainty distinction is irrelevant for comparable constructions in many other languages: a single subordinating conjunction (e.g. *wenn* in German, *als* in Dutch) applies freely across the entire domain.<sup>21</sup>

Crosslinguistic studies of the acquisition of complex sentences (Bowerman, in press; Clancy et al., 1976) show that *when* emerges before *if* in children learning English, and is used appropriately to express future antecedents whose occurrence can be expected (hereafter, "certain antecedents"). Children learning languages with the generalized *if/when* conjunction also first use this form to express certain antecedents. This restricted starting point might reflect a cog-

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opposite: errors have often led to speculation about universals, with corresponding inattention to correct and language-specific learning.

<sup>20</sup>In sentences that do not refer to specific events but to generic or habitual events, both *when* and *if* (as well as *whenever*) are often acceptable, e.g. *When/if/whenever it rains I take my raincoat.* See Comrie (in press) for a crosslinguistic analysis of how the speaker's evaluation of the "degree of hypotheticality" of an antecedent event influences the choice of markers.

<sup>21</sup>Both German and Dutch have other conjunctions that do differentiate the meanings, but these are not colloquial, everyday forms comparable to English *if* and *when*; they occur in somewhat more formal speech and (especially) writing, and are learned later by children. Hypotheticality may also be indicated by the use of subjunctive (German) or conditional (German, Dutch) verb forms; however, these are also learned relatively late.

nitive developmental sequence by which contingencies involving certain future events are easier to conceptualize than those involving uncertain ones (Clancy et al., 1976). Whatever the explanation, it is the next step that is critical for our purposes: how are *uncertain* future antecedents expressed when they come in (which typically happens toward the end of the third year)?

Children learning languages with a single *if/when* form readily extend this form to cover uncertain antecedents as well as certain ones, and there is no evidence at present to suggest that they conceptualize the domain as involving two distinct meanings that just happen to share the same (plurifunctional) form.<sup>22</sup> This might tempt us to conclude that the meanings are conflated in the starting organization of semantic space. However, evidence from English-speaking children forces us to reconsider.

In studying the division of labor between *if* and *when* in three children for whom detailed longitudinal data were available (Bowerman, in press),<sup>23</sup> I found that the children were essentially perfect from the very beginning in their choice between these two forms: *When* was reserved for antecedent events that the child had excellent grounds for assuming would take place, like an anticipated instance of a recurrent daily event (*When Daddy comes home . . . , When I go to bed . . .*), an event being planned and about to be executed (*When I go outside . . . , . . . when you cook it*), the completion of a bounded, ongoing event (*When I'm through . . .*), and growing older (*When I get bigger . . . , When this house gets very old . . .*). *If*, in contrast, was used for antecedent events about which the child had no grounds for certainty, e.g. *If you lose that black thing . . . if that thing loses, then you won't have any key* (child looking at plastic top of mother's key), *If we go out there we haf wear hats* (spoken on a

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<sup>22</sup>"Lack of evidence" admittedly does not provide as strong a foundation as I would like for the claim that the form is not plurifunctional for these children. Let me therefore fortify my argument with a small but revealing set of observations. If *als* and *wenn* are plurifunctional forms (beyond a certain age) for Dutch and German children, respectively, and if they seek ways to differentiate the meanings, we would predict that in learning a second language like English that provides distinct forms, they should find it natural and easy to map the two forms onto their correct functions. However, the behavior of K, a Dutch child I have observed, suggests that this is *not* the case.

K began to learn English at age 4 in a natural setting. By age 5 she could readily produce complex future predictives of low hypotheticality. However, she made—and at age 6;8 continues to make—many errors in her choice between *if* and *when*, most typically extending *if* to subjectively certain antecedents as well as uncertain ones, e.g.: *IF I am bigger, I'm gonna do that too, what you're doing.* (watching older sister make a candle); *What are we going to do IF we are through?* (as family eats a meal); *I wanta wear a T-shirt IF I'm home.* (K riding in car on way home; is planning what she will do when she gets there). Such errors were nonexistent in the records of three monolingual English speaking children, as noted in the text. We can conclude from K's errors that she did not spontaneously make a categorical distinction between "certain" and "uncertain" future antecedent events, and this in turn indicates that when she used the Dutch form *als* 'if/when' in complex future predictive sentences in Dutch, it was not plurifunctional for her.

<sup>23</sup>I am grateful to Eve Clark for making data from one of these children available to me.

lazy rainy day), *If I get my graham cracker in the water, it'll get all soapy* (child in tub), *If somebody takes the newspaper I'll be sad* (after child has postponed bringing in the newspaper).

The apparent ease with which English-speaking children incorporate the meaning distinction signaled by the choice between *if* and *when* into their grammars—and do not, for example, just overextend *when* initially or persistently confuse the two forms—makes it unlikely that the two notions are conflated in starting semantic space. On the other hand, the ease with which children learning languages that do not make the distinction extend a single form across both meanings suggests that the conflation can readily be made. In short, *both organizations are apparently accessible to children from the start of their acquisition of grammatical markers for this semantic domain*. They can readily adopt whichever one is displayed in their language, at least assuming the meanings are clearly marked.

4.1.2. *Spatial and temporal meanings*. A second example of language-specific acquisition in a hierarchically organized semantic domain concerns spatial and temporal meanings. It has long been recognized that meanings of these two kinds have strong conceptual affinities. For instance, languages often extend words that are considered basically spatial to temporal meanings as well: e.g. in Hebrew and many other languages the words for *in front of* and *behind* also mean temporal *before* and *after*, respectively, and in English we can apply *long* both to objects extended in space (*a long stick*) and to events extended in time (*a long concert*). These and related phenomena have led many linguists to propose that spatial location is a basic organizing metaphor on which the temporal system of languages are built (e.g. Bennett, 1975; Traugott, 1978; see also Lyons, 1977, p. 718ff.).

The pervasive relationship across languages between spatial and temporal expressions makes it plausible to hypothesize that concepts of space and concepts of time are close neighbors in semantic space. Children might therefore be expected to show an initial tendency to conflate parallel space-time notions (see Traugott, 1978, for this hypothesis). Subsequent language-specific experience would teach them that separate marking is required, at least for certain meanings in certain languages.

The conflation of notions of space and time would be evidenced by errors in which a single form is incorrectly extended across both spatial and temporal meanings, or in which parallel spatial and temporal forms, if the child knows both, get confused. In accord with this prediction, my longitudinal records of the language acquisition of my two daughters are sprinkled with errors in which a spatial form is substituted for a temporal one. For example:

1. E 3;9 Can I have any reading *behind* the dinner? (= *after*. To M, who is fixing dinner; a request to be read aloud to.)

2. C 7;6 I don't remember *behind* those two. (=before. C has just been recalling her last two birthday parties; cannot remember any previous ones.)
3. E 3;9 (E telling that invited children failed to show up at a party):  
E: They didn't come (even) *at the back of* the birthday.  
M: (confused): At the back of the birthday? What is that?  
E: When the birthday is over.
4. C 4;5 (C telling sequence of activities at a birthday party she has just been to):  
The balloons is *on the other side*, after I ate. But there might have been more *on the first side*. ("on the other side" = *after* (the eating); "on the first side" = *first, before* (the eating).)
5. C 7;2 Do we have *room* before we go to bed for another reading? (=time. M has been reading aloud in the evening; has just finished book.)

Equipped only with the information that these errors occur, we might well conclude that children's initial organization of semantic space conflates notions of space and time. But one further piece of information is crucial: errors like these were never observed in the *first* period of the children's use of the relevant forms. They appeared quite late, months and—for some words—even a year or more after both the spatial and temporal members of a pair (e.g. *behind* and *after*) were well established—i.e. used frequently, in varied contexts, and *exclusively for either spatial or temporal meanings*. Even during the period when the errors occurred, they were always very infrequent relative to correct usage (see Bowerman, 1982c, 1983a, for discussion). These findings make it impossible to maintain that Basic Child Grammar's ideal or preferred organization of semantic space treats parallel spatial and temporal meanings as equivalent for purposes of grammatical marking. English-speaking children are quite willing to differentiate these meanings from the start.

Initial language-specific learning, followed later by occasional but recurrent substitution errors, also characterizes English-speaking children's approach to a variety of other semantic domains. For example, as mentioned earlier in the discussion of OP:UNIFUNCTIONALITY (p. 314), children make substitution errors involving *put* and *give* (locative versus possessive goal), *put* and *make* (cause-change-of-location versus cause-change-of-state), and *make* and *let* (active versus permissive causation) only after an extended period of using these forms flexibly and correctly. In every case the errors reflect an alternative semantic organization that is *linguistically well-motivated*: in many languages a single form is conventionally applied to both meanings in constructions that are otherwise parallel to the English ones. If the child had happened to be learning such a language, her ability to collapse the two meanings that English distinguishes—i.e. to appreciate their equivalence at a higher level of abstraction—would have fed directly into her construction of grammar. We will come back to the question

of why, once children have already learned the semantic organization required by their language, they still sometimes make errors suggestive of other possible organizations.

#### 4.2. Unbiased "Opening Wedge" Often Possible: Overlapping Semantic Categories

In the hierarchically organized semantic domains I have considered, there has been no plausible semantically neutral "opening wedge" for BCG to employ. The least biased starting point in such situations is the most superordinate, but the relatively few detailed analyses so far available of the development of the meanings associated with grammatical forms indicate that children may respect the more differentiated categories required by their language from their first productive use of the relevant forms.

Not all crosslinguistic differences in the semantic categories associated with grammatical forms can be described in terms of the number of discriminations made in a shared hierarchy of potential distinctions. In many grammatical systems languages make approximately the same number of basic cuts through semantic space, but they do so according to different criteria. When we compare the categories defined by these criteria crosslinguistically, we find that they overlap partially.<sup>24</sup> In some cases of overlap it is possible to imagine how children could start out in an unbiased way and wait for their language to lead them into the system required by their language. Do they do so? I explore this question with three examples involving major systems of grammatical marking.

4.2.1. *Subjects and Objects.* The three most basic grammatical roles associated with the noun arguments of predicates are subject of a transitive verb (transitive subject), object of a transitive verb (object) and subject of an intransitive verb (intransitive subject). Some languages (e.g. Takelma, an American Indian language) mark nouns in all three roles distinctly. However, most languages reduce the three categories to two by marking nouns in two of the roles

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<sup>24</sup>Explaining crosslinguistic differences in the partitioning of many, perhaps most, semantic domains requires reference to differences both in the number of cuts made and in the criteria used to make them. For example, there can be partially shared semantic hierarchies in which languages that agree on the first one or more basic cuts diverge at a certain level in the hierarchy, not because one goes on to make finer distinctions than the other but because both languages subdivide a given meaning category further on the basis of different criteria, or the same criteria applied in a different order (see Denny, 1978, 1979, for relevant discussion and examples). Alternatively, languages may differ in their first cuts, but languages that have adopted the same starting point may subdivide further according to a shared hierarchy. Although the discussion here is couched in terms of first cuts that differ, I intend it to apply more generally to what children could do at any point in the division or subdivision of a semantic domain where languages partition meaning into partially overlapping categories.

identically. Transitive subjects and objects are always distinguished in such systems. Where languages differ is in whether they treat intransitive subjects like transitive subjects or like objects. Languages that opt for the former solution, like English and Hungarian, are called “nominative-accusative” languages, while those that opt for the latter, like Eskimo and Hua (a language of Papua New Guinea), are called “ergative” (Dixon, 1979, Haiman, 1979). These differences are shown in Fig. 16.1.

The way in which languages handle the grammatical marking of subjects and objects is an instance of a more general pattern of crosslinguistic differences in categorization discussed insightfully by Andersen (1973) and Haiman (1978). When three (or presumably more) categories in a domain can be discriminated, languages have several options. Some may choose to make no linguistic distinctions at all in the domain, while others distinguish all three categories. However, many languages make only a two-way contrast. In this case they often agree that two of the original three categories constitute extremes that should be differentiated. Where they differ, however, is in how they treat an ambiguous “middle” category that shares properties with both extremes: Some languages choose to assimilate it to one extreme and some to the other. As Haiman remarks, “since both generalizations involve the suppression of significant differences, neither is more nor less arbitrary than the other” (1978, p. 582). The solutions can thus be regarded as equally “natural.”

Domains in which two categories stand out crosslinguistically as saliently different, but one or more additional categories share properties with both of them and are therefore ambiguous, lend themselves well to either of two hypothetical language-neutral learning strategies.

1. According to the first strategy, children would start by distinguishing the two extreme categories (e.g. using different markers for them, or marking one but not the other). This strategy would reflect the cognitive salience for children of the two extremes and the polar opposition between them. It would also be a safe beginning since every language will at least differentiate the extremes if it

	Transitive Subject	Intransitive Subject	Object
Nominative-accusative languages	/	/	
Ergative languages		/	/

FIG. 16.1. Differences between nominative-accusative languages and ergative languages in the grammatical treatment of subjects and objects. Hatching indicates the two constituents that the language treats alike with respect to grammatical marking.

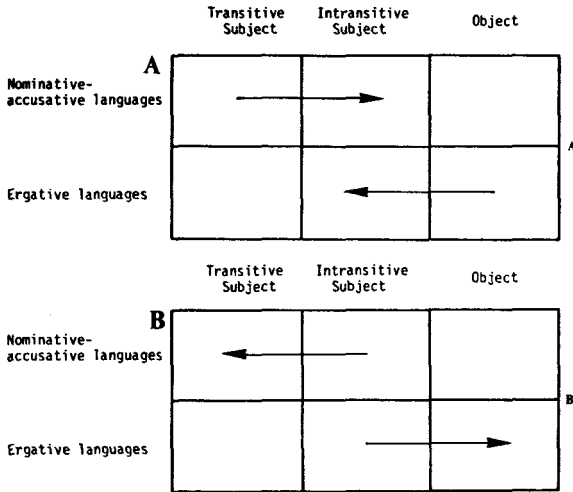


FIG. 16.2. (a) *Strategy one*: start by distinguishing the extreme categories; wait to see whether the middle category should be treated like one extreme or the other. (b) *Strategy two*: start by learning how to mark the middle category; wait to see to which extreme these markers should extend. (Arrows show the direction in which markers would be extended.)

makes any distinctions in the domain at all. Children would wait to see how to mark the middle category, since they would not yet know with which extreme their language groups it, or, alternatively, whether it requires still a third treatment.

Children following this strategy would at first associate markers of subject and object (e.g. case endings, word order patterns, control of verb agreement) only with *transitive* subjects and objects, leaving *intransitive* subjects unmarked. This means that if they are learning a nominative-accusative language they would underextend *subject* markers; conversely, if they are learning an ergative language they would underextend *object* markers. Eventually the markers from the appropriate extreme category would be extended to cover intransitive subjects. This hypothetical sequence is shown in Fig. 16.2a.

2. A second strategy, which intuitively seems less likely, would be to start with the ambiguous middle category, intransitive subjects in this case. Again, there would be underextension: children learning nominative-accusative languages would at first limit their use of *subject* markers to intransitive subjects, leaving transitive subjects unmarked; conversely, children learning ergative languages would limit their use of *object* markers to intransitive subjects, leaving objects unmarked. Later the treatment accorded to intransitive subjects would spread to the appropriate extreme—transitive subjects for speakers of nomi-

native-accusative languages and objects for speakers of ergative languages. At this point appropriate markers for the opposite extreme might also come in. This hypothetical sequence is illustrated in Fig. 16.2b.

Slobin (1985) indeed finds evidence for the initial underextension of markers for subjects and objects, and upon casual inspection it might appear that the marking pattern he identifies conforms to strategy one. However, a close look shows that this is not necessarily the case. According to Slobin, children's initial marking of subjects and objects is limited to utterances expressing a universally salient type of situation that he terms the "Manipulative Activity Scene." This scene is characterized by a cluster of interrelated properties like an agent intentionally and physically acting on an object, with the object often undergoing a change of state or location as a result of the action. The evidence for the importance of this scene in starting semantic space is that (1) children learning nominative-accusative languages tend to restrict their initial use of the accusative marker (i.e. the marker of objects) to direct objects that refer to objects acted upon (changed, etc.), instead of extending it to all direct objects, and (2) children learning ergative languages tend at first to limit their use of the ergative marker (i.e. the marker of transitive subjects) to subjects that refer to agents involved in manipulative activities, instead of extending it to all transitive subjects.

These patterns of underextension do suggest the cognitive salience to children of scenes with a strongly transitive relationship between agent and object, but they do not in themselves constitute evidence for a beginning that is neutral between the nominative-accusative and ergative systems. They are indeed compatible with an unbiased beginning, since transitive subjects and intransitive objects, whether conceived of too narrowly or appropriately broadly, are distinguished in all languages. But they are also compatible with a beginning that is biased in either a nominative-accusative or an ergative direction. The information that is critically needed to determine whether children begin in an unbiased way is what they initially do with *intransitive subjects*. If learners of nominative-accusative languages treat them like transitive subjects (e.g. by choosing nominative forms for both, positioning both in the same way relative to the verb, or allowing both to control verb agreement), then they have adopted a language-specific system from the start, even if they restrict their accusative marker to a subset of the direct objects for which it is appropriate. Conversely, if learners of ergative languages treat intransitive subjects like objects (e.g. by positioning or by using the absolutive marker for both), then they are also in tune with the structure of their local language, even if they limit their ergative marker to a subset of the transitive subjects for which it is appropriate.

A thorough investigation of whether children treat intransitive subjects like transitive subjects, objects, or neither, is far beyond the scope of this paper. Nevertheless, even a cursory look reveals that many children display language-specific patterns from the start.



The evidence is clearest from studies of word order, and I will restrict myself to this.<sup>25</sup> Braine (1976) has shown that children learning nominative-accusative languages often have a productive "actor-action" pattern in the two-word period. This pattern applies to words referring to both actors that initiate actions on objects (a subset of transitive subjects) and actors that initiate intransitive actions like self-movement (a subset of intransitive subjects); it positions constituents of both types identically with respect to the verb. In English, for example, transitive and intransitive actors are routinely placed before the verb, e.g. *Kendall break*, *Kendall bite* (i.e. Kendall breaks/bites something), and *Kimmy come*, *Mommy sleep*. This positioning remains stable for actors when three-term S-V-O strings become common (although some children do show uncertainty over the placement of nonactor intransitive subjects that undergo change, as in *thread break*; Braine, 1976).<sup>26</sup> Braine speculates that the agent category may differentiate out of the broader actor category when actors that are causes are distinguished from other objects that move (1976, p. 68).

The identical treatment of transitive and intransitive actors in the early sentences of children learning nominative-accusative languages seems so natural to adults who speak such a language that it hardly occurs to us to remark on it. But

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<sup>25</sup>Information about which constituents control verb agreement in children's early sentences and about the distribution of case markers and contrastive pronoun forms (e.g. *he* versus *him*) is scattered and frequently inadequate to answer our question; e.g. authors often do not distinguish between transitive and intransitive subjects in discussing the emergence of these grammatical markers. Case marking errors are common at least in the speech of children learning nominative-accusative languages, e.g. see Clancy (1985) on Japanese and Mills (1985) on German. However, these errors seem to involve transitive subjects and objects at least as often as intransitive subjects. It is not until children are reliably distinguishing between these two roles that it makes sense to ask whether they associate intransitive subjects with one, the other, or neither. In general, the absence of reports that children learning nominative languages at first mark only transitive subjects or only intransitive subjects with the nominative (or other subject markers) is compatible with the developmental picture obtained by looking at word order patterns.

<sup>26</sup>It may reasonably be argued that shared word order alone is an insufficient criterion for common category membership, for the following reason: since there are few possible sentence positions in two- and three-word utterances, words that from the child's point of view perform distinct semantic functions might nevertheless happen to share the same (plurifunctional) position (see Bowerman, 1975, p. 87ff). Fortunately, Braine (1976, pp. 59-61) gives careful attention to the problem of how we can determine whether two hypothetical order-based patterns—for example A (transitive actor-action, in this case) and B (intransitive actor-action)—are really two independent patterns or one. Briefly, Braine suggests examining how order of emergence varies among children: "If A appears before B in some children but B before A in others (or if A appears without B in one corpus and B without A in another), then we can infer that A and B are separate patterns that can be independently acquired. . . . If A and B appear simultaneously in all children (or if any corpus that has one always has the other), then the hypothesis that they are really one pattern is confirmed" (p. 59). By these criteria we must conclude that actor-action was a *unified pattern* for the children that Braine studied: children who produced patterned actor-action strings with intransitive actors also produced them with transitive actors, and vice versa.

it becomes striking by comparison with Schieffelin's (1985) finding that children learning Kaluli, an ergative language, never extend the ergative marker (i.e. the marker of transitive subjects) from transitive actors to intransitive actors. Taken together, these findings strongly suggest that children attend from the start to whether their language regards actors who perform an intransitive action like 'coming,' 'sleeping,' or 'sitting' as similar to actors who perform a transitive action like 'breaking,' 'biting,' or 'opening.'

Schieffelin's chapter does not give enough information about Kaluli children's handling of intransitive actors and other intransitive subjects to allow us to determine whether they treat these like objects, as ergative languages require, but the evidence from Samoan, another ergative language, is unequivocal: according to Ochs (1985), young Samoan children from the two-word stage on "reserve the location immediately following the verb for absolutive constituents—transitive patients [objects] and intransitive major arguments [including intransitive subjects]—but exclude ergative constituents—agents—from this position . . . *In this way they treat patients and intransitive arguments as a single category, distinct from agents*" (p. 831, emphasis altered).

In summary, there is good evidence that children show sensitivity even in their earliest sentences to the classification their language imposes on the constituent "intransitive subject," especially that subset of intransitive subjects comprising actors who perform actions described by verbs like *come*, *sleep*, *cry*, and *sit*. Children learning nominative-accusative languages typically position nouns in this role like transitive subjects, whereas children learning the only ergative language for which this information is available—Samoan—do not: Strikingly to the contrary, they position intransitive subjects like *objects*. The non-equivalence of intransitive subjects and transitive subjects for learners of ergative languages is also shown by their failure to extend the marker of transitive subjects to intransitive subjects.

4.2.2. *Noun categories.* Let us apply the line of reasoning built up in the last section to a second domain in which languages agree on which semantic end points should be distinguished but differ on what to do with an ambiguous middle category.

Lucy (1981) has described some fundamental crosslinguistic differences in how languages subdivide common nouns with their systems of noun markers. English divides nouns into two major classes, termed "count" and "mass." Count nouns, e.g. *pig*, can co-occur with the indefinite article *a*, plural *-s* (or its irregular equivalent), numerals, and certain other forms. Mass nouns, e.g. *mud*, cannot take the indefinite article or a plural marker (*\*a mud*, *\*muds*) and require a classifier (see footnote 17) to be able to co-occur with a numeral, e.g. *two PIECES OF mud*, not *\*two muds*. For nouns with concrete referents, the count-mass distinction correlates, although imperfectly, with perceptual differences among entities: Bounded objects that are not too small are typically referred to

with count nouns, e.g. *dog, book, tree*, whereas unbounded substances and collections of very small, undifferentiated objects are typically referred to with mass nouns, e.g. *water, mud, rice*.

The nouns of Yucatec Maya follow quite different rules for grammatical marking. First, only animate nouns may receive a plural suffix. Second, all nouns require a classifier when they are accompanied by numerals (comparable to *two PIECES OF mud*), but animate nouns are once again distinguished from inanimate nouns: they take the classifier *túul*, while all other nouns take *p'éeel*.

The count-mass and animate-inanimate distinctions make orthogonal cuts through semantic space. Simultaneous application of the two distinctions results in four noun categories, as shown in Fig. 16.3a: Type 1 [+count, +animate], e.g. *pig*; Type 2 [+count, -animate], e.g. *ball*; Type 3 [-count, -animate], e.g. *water*; Type 4 [-count, +animate], e.g. *poultry*. Type 4 is not discussed by Lucy and I will eliminate it too for the sake of brevity. Figure 16.3b shows the differences between English and Yucatec in the grammatical handling of the remaining three categories, Types 1–3.

Despite their differences, English and Yucatec are similar in one important respect, as Fig. 16.3b makes clear: Both distinguish grammatically between Type 1 nouns (*pig*) and Type 3 nouns (*mud*). Where they differ is in the handling

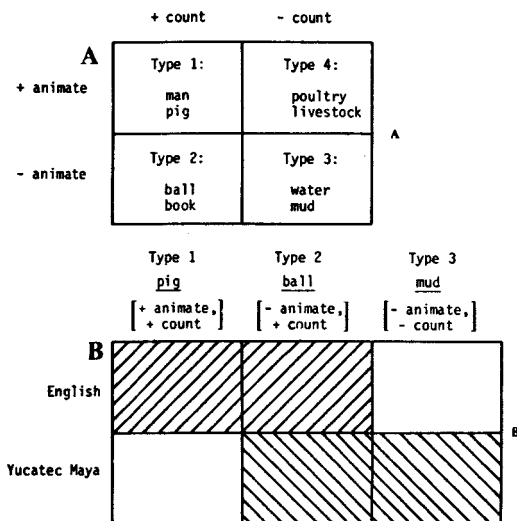


FIG. 16.3. (a) Four classes of nouns defined by the simultaneous application of the [ $\pm$  count] distinction and the [ $\pm$  animate] distinction. (b) Differences between English and Yucatec Maya in the co-occurrence of nouns of three types with plural markers and numerals. Hatching indicates the two categories that the language treats alike with respect to grammatical marking. (Adapted from Lucy, 1981.)

of Type 2 nouns (*ball*). English focuses on the "countability" of the referents of these nouns and groups them grammatically with other countable objects like *pig*, glossing over their difference in animacy. Yucatec, in contrast, concentrates on the inanimacy of the referents and groups them with other inanimate entities like *mud*, ignoring their difference in countability. Lucy (1981) argues that the opposing solutions to the grammatical handling of Type 2 nouns adopted by English and Yucatec are widespread in the languages of the world. He concludes that these solutions are "not accidental, local phenomena but . . . the genuine products of different ways of organizing experience in the categories of language" (p. 12).<sup>27</sup>

Children could approach the learning of noun markers in English and Yucatec in a way biased toward neither language with either of the two strategies described in the preceding section. Following strategy one, they would start by distinguishing grammatically between the two extreme categories, words for animate countable objects and words for inanimate substances. For example, English-speaking children would at first use the indefinite article and plural only with animate count nouns and wait for further evidence about whether this treatment is also appropriate for inanimate count nouns. Conversely, Yucatec-speaking children would at first use *p'éel*, the classifier for inanimate nouns, only with words for substances, extending it to words for countable inanimate entities like *ball* only after further linguistic experience.

Alternatively, following strategy two, children would begin by associating noun markers with the ambiguous "middle" category, words for inanimate countable objects. Children learning English, for example, would first use the indefinite article and the plural with members of this category, and children learning Yucatec would mark these nouns with the inanimate classifier *p'éel*. After further experience, they would extend the markers associated with the middle category to the appropriate extreme—animate count nouns for English speakers and inanimate mass nouns for Yucatec speakers.

Unfortunately, we do not have acquisition data from Yucatec-speaking children. However, it is easy to determine whether English-speaking children adopt one or the other of these two "language-neutral" strategies. We need only check whether they initially limit their use of the indefinite article *a* and plural *-s* either only to animate count nouns (strategy 1) or only to inanimate count nouns (strategy 2). I know of no study that explicitly looks at this, but many studies have given detailed accounts of the acquisition of the indefinite article and the plural in connection with other questions (e.g. Brown, 1973; Gathercole, 1983; Gordon, 1982; Macnamara, 1982; Maratsos, 1976). Since nouns referring to both animate and inanimate objects are common in children's speech from the one-word stage on, and since objects of both types have usually been included in

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<sup>27</sup>The solutions discussed by Lucy can presumably also be elaborated by further subdivision on the basis of gender, shape (as in classifier languages), etc.

testing comprehension or eliciting use of the markers, it is highly likely that if children initially used or understood *a* or the plural exclusively with either animate or inanimate nouns, this would have been noticed. It has not been remarked upon, however, so we can be reasonably confident that English-speaking children treat *dog*, *baby*, *ball*, and *cookie* alike when it comes to articles and plural markers. In doing so, they pass over an excellent language-neutral "opening wedge" and crack into the English system of noun subdivision with categories that are right for English but wrong for Yucatec and many other languages.<sup>28</sup>

4.2.3. *Temporality.* A third domain to which the general approach I have been discussing can be applied, although somewhat less precisely, is temporality. Slobin (1985) proposes that the initial organization of semantic space in Basic Child Grammar is oriented toward two major temporal perspectives: Result (punctual, complete) versus Process (nonpunctual, noncomplete, ongoing).

This distinction is marked early on by the perfective-imperfective form of verbs in Slavic languages, by the present (-*iyor*)—past(-*di*) forms in Turkish, by the progressive-past forms in English (-*ing* vs. -*ed*) and Japanese (-*te iru* vs. -*ta*), etc. In early transcripts of child speech it is difficult to decide if the child is marking a progressive-nonprogressive or an imperfective-perfective distinction, although the parental language may orient itself to only one of these major distinctions. I would suggest that the two temporal Perspectives of Basic Child Grammar are neutral and superordinate to these language-specific categories. (p. 227)

Beyond this beginning point, children "must learn the ways in which Result and Process interact with other distinctions to determine the use of the verb forms in the language" (p. 227). For example, the English-speaking child must learn to mark events in the past differently depending on whether he views them as progressive or not (*was fighting* versus *fought*) but not on the basis of whether they were complete, whereas the child learning Polish must learn to attend to completion but not to ongoing progress.

Notice that the developmental sequence Slobin proposes is similar to the one associated with the child's adoption of "strategy one" in the examples considered earlier. According to this sequence, children first use grammatical markers to differentiate "extreme" categories of meaning; later they discover which semantic properties of these extremes are critical for their language, and they determine how these properties interact with other properties to define (in this case) a language-specific tense-aspect system. Slobin uses this example to illus-

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<sup>28</sup>Working out the details of the system being acquired may take many years, as studies of the mass-count distinction in English have shown (Gathercole, 1983; Gordon, 1982). Nevertheless one of the basic parameters that distinguishes the English system from other possible systems seems to be in place essentially from the beginning: that nouns naming countable objects can be treated alike grammatically, regardless of whether these objects are animate or inanimate.

trate the notion of an "unbiased opening wedge" in Basic Child Grammar; the image is indeed apt.

There is no single ambiguous, middle category in the temporal system that children must learn to assimilate to one pole or the other. Most languages set up a variety of oppositions with their temporal markers, and many of the resulting categories are intermediate between Result and Process. Nevertheless, by simplifying a bit we can test whether children actually proceed in the hypothesized "strategy one" fashion.

According to the hypothesis, children first use past tense or perfective forms for verbs expressing *accomplishments in the past* (punctual, completive events with visible consequences like *The milk spilled, I fell down, The car broke*), and present, progressive, or imperfect forms for verbs expressing *activities in the present* (nonpunctual, noncompletive, ongoing events like *John's sleeping, The baby's crying*). An important question is what children do with the ambiguous category *activities in the past* (e.g. *John slept yesterday, The baby cried all night*). The hypothesis predicts that they do not at first mark verbs referring to such activities at all, since past activities do not conform to either of the poles that define the basic temporal opposition in starting semantic space, but rather share properties with both. Later, children learning English (for example) must relax the punctual, completive requirement for the use of *-ed* and extend this marker from the Result pole to past events in general, including activities. Conversely, children learning Polish must relax the present, ongoing requirement for the use of the imperfective form and extend it from the Process pole to noncompletive events in general, including past activities. This hypothesized sequence is shown in Fig. 16.4.

The hypothesis that children do not initially mark past activities like *cried* with their past tense or imperfective forms is based on several studies (e.g. Antinucci & Miller, 1976; Bloom, Lifter, & Hafitz, 1980; Stephany, 1981). However, a careful recent study by Weist and his colleagues (1984; summarized in Smoczyńska, 1985) shows that the use of past tense or imperfective forms for past activities was often simply infrequent in the corpora on which these studies were based, not entirely absent. These authors go on to provide compelling evidence that the development of Polish children does not fit the hypothesis. Their "single most important finding is that imperfective activity verb phrases with past-tense inflections were observed in all children starting from an early phase of tensed communication" (p. 353). Examples include the translation equivalents of *(She) was swimming* and *(I) was eating*. More generally, the data show that Polish children learn tense distinctions and the perfective-imperfective distinction simultaneously. There is no period in which they restrict their use of the imperfective to present activities or the perfective to past accomplishments.

The lack of errors in their Polish subjects' use of perfective and imperfective forms led Weist et al. to conclude that "the distinction between perfective and imperfective aspect appears to be primitive in child Polish" (p. 369). Comparing

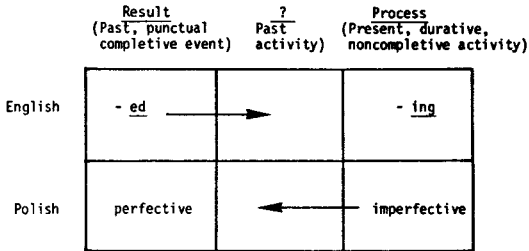


FIG. 16.4. Basic Child Grammar hypothesis about the development of verb markers in English and Polish, including the way in which markers would be extended to verbs expressing past activities.

this finding with Aksu's (1978) proposal that the initial aspectual split for children learning Turkish may be punctual versus durative, they make a proposal that accords completely with the view of the initial organization of semantic space that I have been urging throughout this discussion of the Basic Child Grammar hypothesis:

Children can take *different perspectives*. They can view situations internally and externally. When a situation is viewed internally, features like incomplete, durative, and continuous are salient and when viewing the situation externally, the salient features are completed, punctual, and discontinuous. *Depending on the language a child is learning*, one or more of these feature oppositions will characterize the fundamental aspectual distinction. In Polish, the perfective versus imperfective distinction is transparent in the morphology and children can readily process the relevant affixes. (Weist et al., 1984, p. 370, emphasis added)

In summary, children do not in the beginning inexorably conceptualize events in terms of the opposition between Result and Process, but are sensitive to—and capable of incorporating into their grammars—some of the specific semantic oppositions defined by the temporal system of their language, at least as long as these oppositions are clearly marked.

#### 4.3. Finding the Balance between Flexibility and Constraint in Starting Semantic Space

The various kinds of evidence I have presented suggest strongly that starting semantic space is flexible, not tightly structured. It does not limit the child to a fixed set of "Grammaticizable Notions" but rather makes available some alternative ways of distinguishing among objects, events, situations, and so on, and categorizing them for purposes of linguistic expression. Equipped with these options and guided by clues from the linguistic input as to which criteria for

classifying are important in the various grammatical subsystems of their language, children construct grammars that diverge semantically from the very beginning in the direction of the input language.

In arguing against the notion of a "Basic Child Grammar" I have repeatedly stressed that there is diversity in children's starting options. However, in rejecting the proposal that there is a single starting set of "Notions" I do not want to suggest that children are conceptually so flexible that all structure is provided by the input. Accordingly, I return in this section to the problem of constraint, with the goal of sketching out a hypothesis about starting semantic space that provides the latitude necessary to allow language-specific learning, at least within limits, but is still structured enough to account for the ways in which children depart from the semantic system displayed in the input.

At least two important sources of constraint in starting semantic space can be envisioned. One I have mentioned earlier (see p. 336 and footnote 17): that the meaning distinctions children bring to bear on the learning of linguistic forms are *linguistically sensible*, in that they are the kinds of meaning distinctions that figure in the grammatical systems of natural languages. The second source of constraint, to be pursued in this section, involves differences in the *relative accessibility* of alternative categorization schemes.

4.3.1. *Accessibility Hierarchies.* Up to now, I have treated accessibility in all-or-none terms, arguing that if children can learn different systems for partitioning meaning from the beginning, starting semantic space must provide more than one option for categorizing a given conceptual domain. In fact, however, it is likely that the accessibility of alternative schemes is a matter of degree: Several options may in principle be available, in the sense that all of them can be used by young children at least under some conditions, but some may be more obvious or congenial to children than others.

The idea that there is a hierarchy in the accessibility to children of various semantic notions is, of course, an important source of constraint in the "Basic Child Grammar" hypothesis. It might therefore appear that, having gone to much effort to show that children do not all start their construction of grammar in the same way, I am now returning to essentially the same hypothesis after all. This is not the case. The difference between the BCG hypothesis and what I am suggesting may be subtle, but it has important consequences for the conclusions we draw about how much the early stages of grammar acquisition are shaped by children's own organizing predispositions as opposed to by their experience with the structure of a particular language.

According to Slobin, alternative schemes for classifying meaning in a particular domain are not present from the beginning of the development of grammar, but only *become available over time* as the result of cognitive maturation. Those schemes that are present from the beginning—the initial set of "Grammaticaliza-



ble Notions"—are seen as the "most accessible"; they constitute the semantic basis for the "ideal form of human language" as it is reflected in Basic Child Grammar. Alternative, "less accessible" schemes emerge later; these consist, for example, of finer differentiations or higher-order groupings of the starting set of semantic categories. As these alternatives become available, they enable children gradually to modify their initial grammar in the direction of the specific semantic properties of the input language (see OP:REORGANIZATION, Slobin, 1985, p. 243.) The developmental unfolding of new ways of organizing meaning is indeed an important source of constraint, and must be recognized by any theory of language acquisition. However, our theory should also, I suggest, take account of differences in the relative accessibility to children of alternative schemes for partitioning meaning that are *available from the beginning of acquiring the markers in particular grammatical subsystems*.

My proposal is that children *can* partition given conceptual domains in more than one way, but that, all else being equal, they may favor certain schemes (not necessarily just *one* per domain). This hypothesis preserves what is most attractive about the Basic Child Grammar hypothesis, the idea that some methods of categorizing a domain may be particularly likely or natural for children. However, "all else" is often *not* equal, since, among other things, children are exposed to different languages. The hypothesis thus allows, as the BCG hypothesis does not, for *variation* in the categorizational principles children initially rely on in learning grammatical forms. In particular, it allows for the immediate influence on the child of the *semantic structure of the input language*.

Differences in the accessibility of co-existing options for categorizing can perhaps best be thought of as variability both in the probability that children will "try out" these options when they are learning the grammatical forms of their language and in their persistence in trying to apply them. Highly accessible meaning distinctions are those that children are likely to entertain early in the course of working out what governs the distribution of forms in the input. Such distinctions should also be relatively robust, in the sense that children will often be able to identify patterns in the data that conform to them even when these patterns are marked somewhat inconsistently or with acoustically unsalient forms. In some domains there may be several equally likely and equally robust principles, and in others perhaps only one or two.

Meaning distinctions of intermediate accessibility will have a lower priority and be less robust against noisy input, but if they are clearly and consistently marked in the language being learned, children will also be able to incorporate them into their grammars from the very beginning. Principles for classification that are not yet available because of the child's cognitive immaturity will not be used at all in the early stages of acquisition. Children faced with a language that uses such a principle for categorizing a given conceptual domain—or that uses a principle of intermediate accessibility but does not mark the meanings defined by

it clearly and consistently—may initially fall back on a more accessible principle, and make errors accordingly.

Invoking relative accessibility to explain the relative ease with which children learn alternative language-specific semantic systems is circular unless we can find some independent guideline to what is accessible (i.e. we first identify relative accessibility only on the basis of language acquisitional phenomena and then use it to *explain* these same phenomena). One intriguing possibility is that the relative accessibility for children of alternative schemes for partitioning meaning in a given conceptual domain is correlated with the *frequency with which these schemes are instantiated in the languages of the world*. Some categorization schemes turn up very frequently, others rarely. Given enough information, it is in principle possible to rank order alternative methods of categorizing a particular domain on the basis of their relative frequency across (unrelated) languages. It is plausible that relative frequency is correlated with “ease” or “naturalness” for the human mind; if so, it can serve as an index to the likelihood that *particular bases for categorizing will arise spontaneously in children even in the absence of guiding linguistic input*. Of course, even if relative frequency indeed reflects cognitive ease for humans, its power to predict what is easy for children is likely to be imperfect, since ways of categorizing that are easy and natural for adults may not yet be available to the very young child. However, predictions based on relative frequency can perhaps be adjusted and supplemented to some extent on the basis of our more general knowledge about the course of cognitive development.

Unfortunately, tentative rank orderings of the frequency of various classification schemes are available at present for very few semantic domains (for examples, see Ingram, 1978, on personal pronoun systems and Stassen, 1985, on comparatives). However, an important precedent for such analyses is found in studies of the relative frequency across languages of alternative word order patterns (e.g. Greenberg, 1966; Hawkins, 1982, 1983). These studies show that when several permutations of a word order pattern are possible, a few patterns account for the large majority of languages (e.g. SOV, SVO, VSO) while other patterns are rare or nonexistent (VOS, OVS, OSV). It is worth pointing out that, although it may be reasonable to call rare patterns “marked” or unusual in some way, there are no grounds for choosing just *one* of the remaining patterns as “most basic” (“unmarked,” “natural,” etc.). In the same way, information about the relative frequency of alternative schemes for classifying a semantic domain may suggest which schemes are cognitively “marked” or *less* natural, but they will not reveal any *single* “ideal” or “basic” system.

4.3.2. *Predicting Errors on the Basis of Relative Accessibility.* At last we are in a position to return to a question raised in an earlier section but postponed until the necessary background could be developed: is there a need for “mapping

preference'' OPs like UNIFUNCTIONALITY and ANALYTIC FORM in a theory of language acquisition?

As discussed earlier, OP:UNIFUNCTIONALITY specifies that when a language uses the same form to encode different meanings in different contexts, children will try to eliminate this plurifunctionality by giving each meaning a distinct marking. Similarly, OP:ANALYTIC FORM states that when a language uses a single form to encode more than one element of meaning simultaneously, children will try to spell out these meanings with separate morphemes. Some problems with these OPs, I argued, is that they predict that children should make many types of errors that have not been documented, and that children *do* make errors that they should not make if they are striving for unifunctionality and analyticity (for example, word substitutions that blur meaning distinctions previously observed, and the use of conflated, synthetic forms where analytic ones are needed). OPs that specify that children seek unifunctionality and analyticity are on shaky ground as long as we have no way of knowing when children will follow them and when they will not. Is there any principled way we can *predict* which forms in a language may give rise to errors, and what the direction of the errors will be?

An approach to prediction is suggested by the hypothesis that, although human cognition is flexible and can categorize the same set of referents in alternative ways, some of these alternatives have a higher probability of being used than others. Specifically, we can predict that the *likelihood and direction of errors in a given semantic domain is related to the position on the accessibility hierarchy of the categorization scheme employed by the child's language for that domain.*

If the language uses a scheme that is very common, we can hypothesize that it categorizes in a way congenial to children's spontaneous ideas about what meaning distinctions are important in the domain. Errors should therefore be infrequent or absent; if they do occur, they should reflect the transitory influence on performance of another categorization scheme that is also very common, not a rare one. In contrast, if the scheme is uncommon—hence, by hypothesis, further from children's spontaneous categorizational preferences—we can predict errors in the direction of a more common scheme. For example, if the child's language uses a single (plurifunctional) form for two meanings that language *typically distinguish*, we can expect the child at least occasionally to try to disambiguate. Conversely, if the child's language distinguishes certain meanings that languages equally or more often do *not* differentiate, the child may overextend one form or make substitution errors that suggest that the distinction is easily overlooked.

Sometimes the influence of a common scheme may show up in the very beginning, e.g. the child initially associates a certain grammatical marker with a semantic category that, although incorrect, is "easier" than the one required. In other cases the child learns the language-specific system from the start, but later makes occasional errors reflecting the passing influence of an alternative, highly

accessible method of categorizing the domain. Late errors might be especially likely to reflect the developmental emergence of categorization schemes that are inaccessible to very young children because of their cognitive immaturity, but that are powerful organizing principles for older children and adults.

The psychological process that I assume would mediate many errors reflecting the influence of alternative, highly accessible categorization schemes is *competition* among semantically related words or construction patterns. As noted earlier (p. 322), the principle of competition alone is insufficient to account for children's errors, since it does not tell us why certain forms regularly compete in a child's speech while others apparently do not. However, if we combine competition with some hypotheses about how children conceptualize things at successive points in development—e.g. which referents strike them as *similar*; which meanings they conceive of as single units and which they break down into subelements—we may be able to specify the conditions under which competition takes place.

For example, forms that distinguish between two meanings that are very often not differentiated in the world's languages (i.e. are encoded with the same form) may compete and recurrently substitute for each other because their meanings are so closely related in human cognition that the intention to encode one of them will frequently activate the forms associated with both. In addition, perhaps, the intentions are not distinct enough to allow the form for one of them to be unerringly discriminated from the form for its sister intention in the stage of prespeech editing. Conversely, forms that apply indiscriminately across meanings that languages commonly *do* distinguish are candidates for the process of differentiation that OP:UNIFUNCTIONALITY is designed to capture. In some cases this differentiation may be an incidental byproduct of competition: one of the meanings associated with a plurifunctional form is also associated with another form, and both of these forms may be activated and often incorrectly combined when the child attempts to encode this meaning, but not when he encodes the other. In other cases children may more actively attempt to differentiate meanings that seem distinct to them by constructing idiosyncratic forms, as Karmiloff-Smith (1979a, 1979b) has argued. Finally, if the forms of a language draw meaning contrasts in a given domain in a very common way, errors of substitution ("lumping") and differentiation ("splitting") may be relatively rare. For example, forms that differentiate meanings that languages usually do distinguish—even though some languages collapse them—may rarely substitute for each other, either because the intention to encode one meaning has little power to activate forms associated with the others or because its meaning is distinct enough to allow the appropriate form to be readily singled out from any competing alternatives.

With respect to synthetic versus analytic forms, we can hypothesize that forms that conflate meaning elements that languages rarely conflate will tend to compete with forms that express one or more of these meanings separately,

resulting in substitutions or blends (as in the error genres illustrated in 1, 2, and 4 on p. 315). This is because the elements have—by hypothesis—a high degree of conceptual independence for speakers, and each has its own power to activate the forms associated with it in other construction patterns. Conversely, forms that conflate elements that very often *are* conflated will cause less difficulty: since the cluster of meaning elements is apparently easily conceptualized as a single unit, only one form will tend to be activated.

These various predictions are based on the hypothesis that certain conceptual dispositions or “preferences” for the organization of meaning arise in the child *independently of the language being learned*. Although the overt behaviors—i.e. whether or not errors are made, direction of errors—may vary depending on the semantic structure of the language, the underlying dispositions themselves are assumed to be the same across children. It is possible that, in addition to any such universal categorizational biases, preferences develop differently in different children in response to certain abstract properties of the input language.

Crosslinguistic studies of syntax have shown that the various word order patterns of languages (e.g. the order of subject, verb, and object, of noun and modifier, and of noun and adposition) are not independent, but hang together in cognitively “harmonious” sets (Hawkins, 1982, 1983). Analogously, the particular semantic solutions languages have evolved for the partitioning of different conceptual domains may cluster together in cognitively coherent systems (see Stassen, 1985, for an interesting hypothesis along this line concerning comparative and temporal constructions).

If such interconnections exist between the way different conceptual domains are partitioned for encoding in language (presumably reflecting very “deep” principles of cognitive organization), children could develop expectations about what meaning distinctions will be important in one domain on the basis of what they have learned about other domains. This would facilitate rapid and error-free learning if the input language is harmonious. However, languages may be “dis-harmonious,” in the sense that the properties of one or more of their subsystems deviate from what would be expected, given the properties of the other subsystems. Children learning such a language might develop incorrect expectations that lead them to make errors; these errors would, in effect, bring a wayward subsystem into greater semantic harmony with the other subsystems. Such errors, if they occur, would probably not characterize the first stages of learning a discordant subsystem, but instead set in only later as learners developed a sense of how the various subsystems of their language hang together in a larger, semantically coherent pattern.

4.3.3. *Identifying Categorizational Predispositions: Some Problems.* In the preceding discussion I have concurred with Slobin that some of children’s language errors reflect the influence of categorizational principles that are especially “natural” or accessible to children. However, the effects of the learner’s pre-

dispositions for the organization of meaning will be subtle and extremely difficult to identify with confidence.

One important problem is simply that preferences for categorizing in certain ways will interact with, and often be obscured by, the formal complexity for children of the linguistic devices used to encode the relevant meanings. As Slobin (1973) has shown, some devices are easier for children than others, independent of the meanings they express. This means that a semantic categorization scheme that is in principle somewhat less favored might nevertheless be adopted faster or with fewer errors than a more favored scheme, if the markers through which it is manifested in a particular language are formally easier for children.

A second, even knottier problem is that when children use a linguistic form in connection with an inappropriate semantic category, either consistently or occasionally, the ultimate source of their error is often difficult to isolate: errors that at first seem to reflect a nonmodeled—hence, by inference, spontaneously generated—way of classifying can, on closer inspection, often be interpreted equally well as *extensions of a semantic pattern conventionally associated with one or more forms that the child already knows or is learning*.

For example, consider English-speaking children's use of spatial words like *behind* in contexts where temporal words like *after* are needed (see pp. 335–336). These errors might arise spontaneously simply because it is “natural” for humans to use a spatial framework for conceptualizing temporal notions. However, children in fact receive considerable linguistic encouragement for this conceptualization, since many English words *are* conventionally applied to both spatial and temporal meanings: e.g. *AT school, AT four o'clock; OVER the river, OVER the next few months; a SHORT boy, a SHORT nap*. In learning these words in both their spatial and temporal senses, children are in effect continually invited to see an analogy between space and time. If they pick up on this invitation, it would not be surprising if the (learned) analogy also began to affect the use of words for which it happens to be inappropriate (see Bowerman, 1982c, 1983a).

As a second example, take the following error by a German child, cited by Mills (1985): *Die Grossmama ZU den Affe* ‘The grandmother TO the monkey’ (= ‘The monkey’s grandmother’). Here a locative preposition, *zu* ‘to’, is extended to a possessive meaning; *von* ‘of’ would have been appropriate. Slobin (1985) interprets this error as stemming from—and, in turn, as evidence for—the conflation of location and possession in starting semantic space. This explanation might be correct. However, we should note that adult German in fact uses *zu* in many everyday contexts to mark a meaning better described as ‘belonging together with’ or ‘going with’ than as ‘locative goal’. In many such constructions *zu* and *von* are interchangeable: e.g. ‘the top ZU (VON) this bottle,’ ‘the cover ZU (VON) this book,’ ‘the hat ZU (VON) the boy’ (this last example would be appropriate in the context of assembling a jigsaw puzzle, for instance); ‘this belt PASST ZU (= goes with) this dress’; ‘this father GEHÖRT ZU (= belongs with)

these children' (e.g. pointing out pictures in a book). Since the error of using *zu* in place of possessive *von* occurred quite late in the speech of the child discussed by Mills (age 3;7), it is likely that he was already familiar with the 'belonging together' sense of *zu*. His error might therefore simply have reflected his (overly) productive application of a *pattern displayed in his language* rather than an inherent tendency to conflate possession and location.

The confounding of possible explanations seen in these two examples is pervasive, since principles of classification that children may prefer on a non-linguistic basis very often turn out to be functional somewhere in the input language, sometimes in connection with forms closely related to the ones on which children err and sometimes even in connection with specific usages of the *same* forms. Of course, in some of these cases linguistic patterns may simply *reinforce* children's inherent preferences; i.e. both could work together to co-determine particular error types. However, it is a serious methodological problem to determine when this is the case and when the semantic categories displayed in the input are in fact *critically responsible* for suggesting to learners what elements of their experience should be regarded as similar for purposes of linguistic expression.

Crosslinguistic comparisons are essential for resolving this confounding. For example, if errors reflecting the inappropriate conflation of time and space, possession and location, and so on, are made only by children learning languages in which these meanings *are* conventionally conflated in the use of certain common forms, we will have to give more credit to children's experience with the linguistic input and less to the intrinsic structure of starting semantic space in explaining the salience to children of these categorization schemes. On the other hand, of course, if errors of these types occur regardless of whether the relevant semantic organization is displayed in the input language, then the hypothesis that children start language acquisition with a prestructured semantic space is correspondingly strengthened.

#### 4.4. Priorities for Future Crosslinguistic Research

The previous section has ended with a theme to which I have repeatedly returned in this chapter: To what extent do the patterns we observe the speech of young children reflect their inherent dispositions for organizing linguistic material in certain ways, as opposed to their experience with the structural properties of a specific input language?

On the basis of the crosslinguistic research presented in these volumes and elsewhere, Slobin (1985) has concluded that children's early grammars show the same basic structure everywhere. From this he infers that predispositions for particular ways of partitioning meaning and mapping it into linguistic form play a critical role in the early stages of language acquisition. Although I have agreed with Slobin that an optimal theory of language acquisition will need to take into

account certain predispositions for the organization of meaning, I have argued that these predispositions are neither as strong nor as single-channeled as Slobin has proposed. For example, counter to the Basic Child Grammar hypothesis, children often show language-specific patterns from the outset of learning important grammatical subsystems. Further, although certain errors at first may seem to reflect organizing principles that have no model and hence must have arisen spontaneously in the child, closer examination often reveals plausible within-language sources for them: e.g. the child's growing grasp of syntactic regularities or principles of semantic categorization displayed in the linguistic input, or competition at the moment of speech between alternative forms the child has learned for expressing the same or similar meanings.

It is striking that even after almost two decades of intensive crosslinguistic research, possible explanations for many acquisitional phenomena are still so heavily confounded. This testifies not to the inutility of the crosslinguistic method as a technique for disentangling the roles of nature and nurture in language acquisition, but to the extraordinary complexity of the problem. Although many of the relatively global questions of an earlier era have been answered (e.g. do all children prefer fixed word order regardless of the rigidity of word order in the input language?), new and increasingly difficult sources of confounding keep coming into view. At present, the problem is particularly severe with respect to interpreting the causes and implications of errors in children's spontaneous speech, and this is why I have devoted so much attention to this issue.

Although the Operating Principles approach has not yet provided us with definitive answers, it has served as an invaluable framework within which critical issues can be conceptualized and investigated. As discussed in an earlier section, operating principles and other proposals within the Operating Principles approach have been developed inductively, primarily through comparison of the spontaneous speech of children learning different languages. This phase of inductive research has served several important functions. For example, it has allowed us to identify and explore a number of significant acquisitional phenomena that might well have been missed if research efforts had been narrowly directed toward hypothesis-testing. Additionally, the development of a framework that can accommodate and give meaning to scattered bits of data from many languages has been enormously important in fostering among child language scholars—many of whom have worked in relative isolation in their countries—a shared frame of reference and a sense of joint participation in a cooperative scientific enterprise.

Although we should not neglect inductive techniques in research of the coming years, we need now to become more rigorous, supplementing observation and inference with explicit hypothesis-testing. In particular, proposed operating principles and hypotheses about children's categorizational predispositions need to be subjected to critical evaluation. It is not enough to establish that there are



data that conform to the proposals. We need also to draw out the predictions of the proposals for the acquisition of specific languages and see how systematically they are met, whether apparent counterevidence can be explained in some reasonable way or should cause us to modify or reject a hypothesis, whether the phenomena cited in support of various proposals can be explained plausibly in other ways, and so on.

Testing the hypotheses of the Operating Principles approach will require crosslinguistic comparisons to be more carefully preplanned than has typically been necessary before. The languages to be used in testing a particular proposal will need to be selected so that they differ critically with respect to rather fine structural details. I would like to urge that the most pressing need in this respect is for studies that take crosslinguistic variation in *semantic structure* seriously.

In spite of the general interest in meaning shown by our field in recent years, variation among languages in the makeup and scope of grammatically-relevant semantic categories has been persistently neglected in crosslinguistic studies of grammatical development. In general, the way in which languages organize meaning has not been regarded as an integral part of their structure, equivalent in status to syntactic or morphological structure and comparable to these in its potential to influence rate of acquisition, likelihood of errors, and so on (Bowerman, 1985). For example, in the Operating Principles approach, *grammatical* variation is recognized as a critical independent variable, either to be controlled while the role of cognitive complexity is evaluated, or permitted so that its impact on acquisition can be studied (see p. 305 and footnote 3). In sharp contrast, variation in the *semantic* categories associated with the grammatical forms of languages has been virtually ignored. Thus, in studies designed to assess the effects on acquisition of crosslinguistic differences in grammatical form, "meaning" is typically controlled simply by choosing for comparison forms that are translative-equivalents across at least a portion of their usage range (e.g. *in*, *on*, and their counterparts in other languages). No attention is paid beyond this rough match to differences in the way these forms, together with other closely related forms, divide up the domain of meaning over which they operate into contrasting categories. Yet these differences may have important consequences for language acquisition. Further—and still more important—semantic organization has not been treated as an independent variable to be carefully contrasted while grammatical form and general conceptual domain is controlled.

The neglect of semantic variation in crosslinguistic studies of language acquisition reflects, I think, the difficulty our field has had in distinguishing meaning as it is structured by language from nonlinguistic, cognitive organization. In much recent theorizing about language acquisition, children are seen as possessing powerful cognitive capacities that enable them to organize and interpret their experiences independently of language. When language starts to come in, ac-

ording to this view, it does not introduce new meanings, but simply allows children to express those meanings they have already formulated. The emphasis in this model is on the cognitive understanding that all children are assumed to share; correspondingly, there is little attention to crosslinguistic variation in meaning structure, or to the process by which children learn to the specific way in which their language categorizes elements of experience (see Bowerman, 1985; Schlesinger, 1977; and Slobin, 1979 for discussion).

This model is not incorrect so much as it is seriously incomplete. Recognition of the role in language acquisition of conceptual development has been one of the important advances of our field in the last two decades, and I do not dispute that children's acquisition of linguistic forms is supported by their general cognitive understanding of the relevant concepts. Locative prepositions will not be learned until the child realizes that objects can be spatially related in various ways, and subordinate clauses with *when*, *if*, and so on will not come in until the child can conceive of contingent relations between events. Nevertheless, we must recognize that the child's ability to understand and interpret spatial configurations, contingent relations, etc. on a nonlinguistic basis is *not isomorphic with the ability to categorize these relations in the way required by the language being learned*. We therefore cannot be satisfied with a theory that stops with the observation that meanings in some sense precede the acquisition of the forms that encode them. We need to go beyond this to determine how children work out the principles of semantic categorization that are functional in their language.

Crosslinguistic comparisons will be essential to this effort. Only by studying how children approach language systems that differ in their organization of what is, at a deep level, the "same" conceptual material can we begin to discover how language-learners construct a highly structured and language-specific meaning system from their nonlinguistic understanding of daily experience. I hope research of the coming years will pay more attention to this central and fascinating problem of language acquisition.

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