Sascha W. Felix/Henning Wode (eds.)

Language Development at the Crossroads

Papers from the Interdisciplinary Conference on Language Acquisition at Passau

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PRAGMATICS AND SYNTAX IN PSYCHOLINGUISTIC RESEARCH[1]

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The relationship between pragmatics and syntax is a matter of considerable controversy in linguistics and in psycholinguistics. One school views the two as *separate content areas,* i.e. two autonomous linguistic systems that interact but share few common principles. The other school views the relationship as one of *cause and effect within a single system,* wherein pragmatics is defined as the study of functional constraints on linguistic forms. We will refer to the first as the *autonomous syntax* approach, and to the second as *functionalist grammar* (Bates and MacWhinney 1979; 1982a).

Although the specific issues that are debated these days are new, the main points of the controversy are very old. In fact, like all of our best Western controversies, this one goes back to the Greek philosophers. The Anomalists, including the Stoics and the Skeptics, argued that language is an arbitrary system, given in nature, a system that should be described and studied in its own terms (Robins 1964). Since language is not the product of human reason, there is no reason to expect language to obey the rational laws of the human mind. This position is easier to understand if we consider the opposing view of the Analogists, including Aristotle. The Analogists insisted that language *is* a product of human reason. Hence we should expect language to be *regular* (at least at some underlying level) and *rational* (i.e. there are reasons why things look the way they do). To prove their point, the Analogists engaged in a kind of etymological research, to show how seemingly arbitrary forms emerged from simple and reasonable beginnings. For example, the name of the god Poseidon bears a resemblance to a conjunction of the Greek words for "foot" and "water", leading to the conclusion that the sea god was named for something like "He who has his foot in the water." Whatever the fate of the Analogist research program, the goals of their approach were clear: to *explain* linguistic forms in terms of prior materials (e.g. the first set of simple lexical roots) and external causal forces (i.e. the pattern-seeking properties of the human mind). By contrast, the goal of the Anomalists was simply to *describe* language faithfully.

The autonomous syntax position, as outlined by Chomsky (1965; 1975; 1978), can be viewed as a modern version of the Anomalist approach (with some modifications, discussed in Bates and MacWhinney 1982a). In elaborating his principles of *absolute autonomy* and *blind application of transformations,* Chomsky argues that no syntactic rule may be motivated by semantic concerns, nor may such a rule be formulated in terms of semantic-pragmatic structures. For example, it is a statistical fact of language use that the subject of a sentence is gen-

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erally a human agent and the topic of preceding discourse (e.g. Givon, 1979). However, for Chomsky this does not mean that the functional categories of "agent" or "topic" play a role in the grammar. The grammar is *interpreted* by outside processes, but it is not *structured* by them. In setting up formal barriers between grammar and meaning, Chomsky echoes the Anomalist claim that language is given in nature and cannot be reduced to or derived from general principles of human reason. There is no causal relationship between form and function.

The separation of grammar from meaning seems counter-intuitive to many non-linguistics, who cannot conceive of grammar as anything other than a tool for mapping meanings into sound. In trying to explain the concept of autonomous syntax (to myself as well as others), I have found a useful analogy with algebra. Algebra is a practical and beautiful system precisely because it does not take specific numerical content into account. We have rules for transforming and equating expressions that operate entirely on "x" and "y" without regard to what "x" and "y" stand for. Suppose that algebra were based on the "meaning" of those symbols, with rules written in the form "For every x, unless x is a prime number greater than 7, apply the following . . ." Such a system would be cumbersome, difficult to learn and difficult to use. Mathematical formulations are evaluated on the basis of their generality, coherence, and elegance. The same principles apply to the selection of competing formalisms in generative grammar. The less they are constrained by specific content or meaning, the better they are judged to be. In this respect, syntax is a kind of linguistic algebra.

Within such a framework, explanation consists of nothing other than a generalized level of system-internal description. A mathematician may discover that five seemingly disparate formulae can be restated as instances of a single, more general mathematical statement. There is a sense in which she has now "explained" the five lower-order formalisms by capturing their commonalities at a higher level. In a similar vein, Chomsky (1965) has discussed formalisms to describe Wh-movement in English (e.g. the transformation that relates "John told Mary that Peter would bring Susan to the party" to "Who did John tell Mary that Peter would bring? "). It is now suggested that English Wh-movement is a particular instance of the *subjacency principle,* a rule of Universal Grammar that constrains possible movements of noun phrases out of an embedded clause. The English rule has explanatory adequacy because it has been related to a more general set of universal rules. Note, however, that this Universal Grammar is described entirely in terms of an autonomous syntactic component. The subjacency principle itself is not explained or in any way related to causal principles in other cognitive domains, e.g. memory limitations, perceptual processing constraints, motor coordination. social functioning. Syntax is its own cause, a "mental organ". It is not shaped by other mental organs any more than the human heart is shaped by the human liver.

More recently, the modern Anomalists have extended their notions of autonomy, independence, "modularity" outside the boundaries of syntax, to the operation of other knowledge domains (e.g. Fodor 1975; Keil 1979; Osherson, 1978). Chomsky (1975; 1978) has speculated that syntax is only one of many mental organs, each with its own independent principles of development and self-regulation. These might include domains like knowledge of three-dimensional space, face recognition, classification of objects into natural kinds, and certainly several other aspects of linguistic knowledge (e.g. phonology).

From this point of view, pragmatics might also constitute a self-contained knowledge system. The content of such an autonomous pragmatic component has not been well defined, although some proposals are available (Jackendoff 1972; Gazdar 1977; Wilson 1975; Bierwisch 1980). In general, we would expect a pragmatic component to contain rules for relating well-formed strings to communicative contexts. This might include algorithms for locating the referents of anaphoric pronouns in time and space, rules governing speech acts and conversational interaction, perhaps a system for analyzing the status relations implied in levels of address (e.g. *tu* and *vous* in French). However, it is equally possible that the domain called pragmatics consists of several different, independent linguistic "modules". For example, take the statistical relationship noted above between the forms associated with sentence subject (e.g. sentence-initial position, agreement with the verbs, nominative case marking) and the twin functions of topic and agent. Within a modular theory, the set of subject-related surface forms are analyzed within the grammar. In Jackendoff (1972), case notions like "agent" belong within a semantic component, which is in turn separated from a "thematic" component that handles roles like topic. How do we decide where to draw the lines between linguistic domains? The boundaries and content of an hypothesized pragmatic or semantic component are determined on formal grounds, with the same criteria of internal logic and coherence that are used to evaluate grammars.

There are some important descriptive advantages to this modular approach to mind. That is, it is easier to describe a few (relatively) simple systems than one large complicated one. The appeal of the modern Anomalist position resides in the clarity and precision of formal descriptions that have been derived in generative grammar research (see Newmeyer 1980, for an historical review). Although much less can be said for formal descriptions of other modules, the "divide and conquer" method seems promising. In short, "linguistic algebra" fares well by its own mathematical criteria.

The major disadvantage of this approach lies in its biological implausibility. Although it may be easier to *describe* independent systems, it is far more difficult to learn them or to derive them phylogenetically. This problem is discussed at some length in Bates (1979), and in *Language and Learning,* a summary of the Piaget-Chomsky debates edited by Piatelli-Palmarini (1980). If mental organs are not derived from one another, nor from a shared set of simpler causal mechanisms, then where do they come from? Chomsky has been quite comfortable in responding that the separate systems are innate. However, as Piatelli-Palmarini notes, this particular brand of nativism finesses the question of origins by passing it out of ontogeny and into phylogeny. If grammar is innate, how did it get to be that way? It is a basic tenet of Darwinian theory that new forms have to be selected out of old ones. What does half of a subjacency principle look like on its way to becoming a mental organ? Unless our species underwent a chance mutation of unprecedented proportions, our language capacity must have been selected out of "old parts".

This does not mean that it was selected out of prior *communicative* abilities. Indeed, the developments that made language possible in our species may have been by-products of visual information processing (Gregory 1975), hand-eye co-

ordination and concommitant changes in the motor cortex, or any number of other "software" developments in the evolution of the human brain. The point is that innate grammar must have come from somewhere, involving some recombination of prelinguistic mechanisms. As Gould (1977) has noted, the construction routes used in phylogeny usually leave traces in the embryological development of individual members of the species. If this is the case, then it is likely that the lines between linguistic and non-linguistic mechanisms are still blurred in modern man. In other words, a position of interdependence between components is more likely in biological terms, even if such interdependence is more difficult to describe formally (Bates, 1979).

To summarize, there may be a trade-off between the descriptive advantage of the Anomalist approach and the biological feasibility of modern-day Analogism (i.e. functionalist grammar). However, before we go on to describe some advantages and disadvantages of functionalism, let us consider whether Chomsky's strong nativist position really is a necessary corollary of autonomous syntax.

It is true that the Greek Anomalists embraced *both* nativism (language is given in nature) and autonomy (language is not a product of human reason), in the same way that Chomsky ties together autonomous syntax and the theory of innate mental organs. However, modularity may not *logically* require nativist solutions. For example, Maratsos and Chalkley (1980) have argued that independent, formal grammatical categories could be learned by general inductive procedures, without assimilating those categories to any kind of pragmatic or semantic base. To illustrate, they cite the case of German gender. It is quite obvious to anyone who knows German that gender has little or nothing to do with a semantic notion of sex (e.g. the word for "bottle" is feminine, while the word for "little girl" is neuter). Yet children do acquire full gender paradigms before the age of six. The gender paradigms must be purely grammatical, since they have no basis in meaning. And yet it is most unlikely that the peculiarities of German gender are innate.

To solve the German gender problem, Maratsos and Chalkley argue that children can observe and acquire *correlations* among different surface forms (e.g. the relationship between the articles attached to nouns, and the way those articles change with case role). They attach implicit category labels to these sets of correlations (e.g. "+neuter"), and use the resulting formal categories to comprehend and produce well-formed utterances from that point on. This is a kind of anomalist position: children study and acquire forms *sui generis*, without imposing semantic or pragmatic interpretations. However, they accomplish this through the use of very general pattern-recognition mechanisms, learning principles that are not unique to language, and hence require no language-specific genetic equipment. In this second respect, Maratsos and Chalkley have more in common with the Analogists, stressing the role of human reason in pattern analysis. *Clearly we need a separation here, between assimilating grammar to non-linguistic categories, and acquiring grammar through non-linguistic processes.* Chomsky rejects both possibilities, and thus is forced to a nativist point of view. Maratsos and Chalkley reject only the first (and only for some grammatical categories). Hence their theory requires fewer innate assumptions, while at the same time permitting the acquisition of a "meaning-free" grammar.

Within the autonomous syntax school, a principled approach to the nativism issue has been proposed under the rubric "learnability theory" (Wexler and Culicover 1980; Pinker 1980). A learnability analysis is a mathematical method for assessing the number of trials (from zero to infinity) that would be required to learn a given grammar. It is important to keep in mind that learnability theory applies to all formally possible languages, and not just to the natural languages spoken by human communities. In fact, these mathematical methods bear a systematic relationship to the science of cryptography, as a formal means for determining the "breakability" of new, artificial codes.

As described by Pinker (1979), there are four components that must be specified in any learnability analysis: (1) the target grammar, (2) the learning device that must aquire that grammar, (3) the data input to the learner, and (4) the "starter set" of hypotheses or clues that the learner brings to bear on acquiring the target language. An adjustment in any of these parameters can change the number of trials required for learning to take place. If one of the components is weak, then the others must be correspondingly strong to keep the required trials from approaching infinity (in other words, to make learning possible).

In their learnability analysis, Wexler and Culicover first assume that the learner must acquire a transformational grammar (in this particular exercise, the kind of grammar outlined in Chomsky 1965). They further argue that the learning system is relatively weak. It is a simple inductive mechanism that tests whole grammars (or grammatical rules) one at a time against each incoming sentence. This means that one bad sentence could throw the learner off completely – unless he has some kind of "filter" for rejecting bad input (see below). With regard to the third parameter, Wexler and Culicover suggest that the input to human children is unsystematic. Its worst aspect is that it consists of "positive" rather than "negative" data. What this means is that adults talk to children (i.e. present them positive instances of sentences in the target language) but rarely if ever correct their grammar (i.e. telling them "This is not an English sentence" -- see Brown and Hanlon 1963). The absence of negative data is akin to a game of hot-and-cold in which the player is never told when he is getting cold; when nothing is eliminated, anything is possible.

Putting together their assumptions about the first three components (a complex target grammar, a weak learner, and an inadequate data base), Wexler and Culicover are forced to conclude that learning is possible *only* if children have a strong set of *a priori* assumptions about the target language. These innate clues are necessary to rule out an infinite set of weird or silly grammars that the child might otherwise consider, and to rule out bad sentences in the input. The only way a transformational grammar could be acquired in a reasonable amount of time is if the basic parameters of the system were there to begin with. If this analysis is correct, then Chomsky's theory does indeed require his strong nativist claims.

However, as Pinker (1979) and Braine (1978) have both noted, the Wexler and Culicover conclusion is not the only one that can be reached with learnability analyses. The strength of their fourth parameter (i.e. innate hypotheses) is required only because of the values they have assigned to the other three.

First, learnability theory does not apply exclusively to Chomskian grammars. Any target language could be analyzed to assess its learnability. This includes Pinker's (1980) analysis of Bresnan's lexical-functional grammar (Bresnan, 1978), or some more informal proposals by Braine (1978) concerning semantically-based case grammars. Indeed, Pinker (1979) has suggested that learnability analyses might ultimately be used to choose among grammars of equivalent descriptive power. All other things being equal, a grammar which requires fewer innate hypotheses would be preferable on grounds of parsimony.

Second, a wider array of grammars would prove to be learnable if the learning mechanism itself were more intelligent than the one proposed by Wexler and Culicover. For example, MacWhinney (1978) has proposed a learning device that takes probabilistic samples of the data, and formulates new hypotheses only when a criterion number of instances have accumulated in the system. Such a probabilistic learner needs fewer "filters" to keep bad sentences out of consideration, since ungrammatical input will be considered only if it is diabolically frequent. Another feature of MacWhinney's learner is its reliance on analogic processes; old hypotheses are "stretched" to deal with new input until an accumulation of "bad fit" forces reformulation of the theory. This conservative, bottom-up approach keeps the learner from considering a vast array of silly and/or ad hoc hypotheses that would seem quite plausible to the Wexler and Culicover learner. Maratsos' and Chalkley's proposals concerning correlational learning would also increase the heuristic power of the acquisition device.

These are just a few of many possible suggestions that would improve the sophistication of a language learning component. The kind of learning theory that Chomsky caricatures in many of his writings (e.g. Chomsky, 1975) bears very little resemblance to the more active and intelligent learning mechanisms proposed today in cognitive psychology and artificial intelligence (e.g. Anderson, 1975). For that matter, Chomsky and Wexler and Culicover might also want to consider Tolman's (1948) discussion of the hypothesis-forming abilities of rats, or Kohler's (1927) discussion of insight and problem-solving in chimpanzees. We may be a long way from a complete and coherent general learning theory, but at least a few advances have been made in the formulation of intelligent learning devices that are not easily fooled by a little bad data.

A third way out of the Wexler and Culicover conclusion is to consider some improvements in the data base. Some researchers have argued that language input to children is far more systematic than previously believed. Descriptions of motherese", or caretaker language to children (Snow and Ferguson, 1977; Newport, Gleitman and Gleitman, 1977; Furrow, 1979) suggest that adults typically use simple, complete and grammatical sentences that could provide a simplified model of the target language more in line with the child's current level of development. However, as Wexler and Culicover point out, such "prechewed" input only helps with the learning of very elementary aspects of grammar. If the child is to acquire the richer and more complex rules of his language, he will have to confront normal adult input sometime. Furthermore, the problem of "negative evidence" is not resolved by motherese. Children still need to find out what *non-sentences* in the language are like.

Some suggestions by MacWhinney (1978) are useful in this regard. There are several ways that children might realize "This is not a good sentence" without being told so explicitly. One way, of course, is by failure to communicate. Utterances which don't work in getting a point across will ultimately have to be analyzed to figure out what is wrong about them. However, a more explicit way to generate negative information is through on-line pattern matching: the adult produces a given form (e.g. "bought"), which reminds the child through the usual mechanisms of recognition memory of the way he himself usually produces that same form (e.g. "buyed"). This is a kind of analysis-by-synthesis, in which input is analyzed by retrieving a corresponding output. If this procedure results in a *mismatch* (i.e. "She didn't do that the way I would have done it"), then a kind of negative evidence has been generated (i.e. "I must not be doing it right.") Furthermore, this negative evidence is much more explicit than the relatively unfocussed feedback produced by a simple failure to communicate (i.e. "I must have done something wrong, but I don't know what it was.").

Such self-correcting mechanisms are not restricted to language learning. Anyone who has watched children engaged in symbolic play has seen such on-line corrections in a wide array of cultural behaviors: the child starts a pretend game of cooking, and corrects himself when the movements aren't quite right, perhaps against a model of Daddy's current cooking or a recent memory thereof. This kind of pattern-matching is not sufficient to account for all of the things children do in acquiring language, since many intermediate forms are produced that could not possibly be derived by imitation (e.g. Slobin and Welsh 1973). Nevertheless, it does provide one means for children to find negative evidence in the data base. As Wexler and Culicover have noted, one piece of negative evidence may be far more important than a vast set of positive instances.

Any of these changes in the first three parameters could reduce the number of innate hypotheses required to learn a language. However, there is another whole range of proposals that could be made concerning the nature of the fourth parameter itself, i.e. the "clues" children bring to bear on language learning. Wexler and Culicover describe a variety of constraints on the acquisition of rules, "filters" that keep bad input out of consideration and principles that delimit the kinds of rules that will be considered. Their constraints are all described in terms that seem to apply exclusively to language. For example, they argue that children will not consider a rule that requires prior application of another transformation (e.g. a rule that operates only after Wh-movement). On the surface, it is hard to see how this peculiar constraint could be derived from any kind of non-linguistic knowledge. Indeed, the same could be said for all of Wexler and Culicover's proposed constraints.

However, we may have to distinguish once again between clues based on non-linguistic *content*, and tendencies based on non-linguistic *processes*. As a case in point, consider the "operating principles" that Slobin (1978) has described in accounting for cross-linguistic patterns in language development. These include dictums like "Pay attention to the ends of words" (a principle that biases children to acquire suffixes) and "Avoid discontinuous morphemes" (a principle that helps children to locate morphological boundaries in most cases). All of Slobin's principles are stated in terms that seem to apply only to language. However, each

one of them could be *restated* as linguistic versions of well-known psychological laws. "Pay attention to the ends of words" results from serial order effects in memory. "Avoid discontinuous elements" fits with the Gestalt principles of continuity and closure. In fact, if a language learner were equipped with old-fashioned Gestalt figure-ground processes of a very general sort (proximity, continuity, closure, symmetry, assimilation and contrast), he might be well on his way toward an adequate parsing of any natural language -- without further, language specific clues. The apparent domain specificity of constraints on language learning may result from nothing other than our own inability to see the commonalities between domains.

To summarize so far, the modern Anomalists argue for the separation of grammar from other linguistic components, and from non-linguistic domains of knowledge. This "divide and conquer" approach is justified because of its descriptive advantages, permitting precise formal modelling of the grammar by reducing the number of elements that have to be accounted for. As the autonomous syntax position has been formulated by Chomsky, strong nativist claims are necessary to explain how children arrive at abstract syntactic principles. Learnability analyses by Wexler and Culicover seem to support Chomsky's intuitions about the innateness of grammar -- and yet it is difficult to explain how such innate ideas or "mental organs" could have evolved, given the limitations and imperfections of biological reality. If we want to explain language as well as describe it, it would seem useful to reduce the separation between components of mind and to find those points of continuity that make the emergence of language biologically plausible. How do we do that? Several suggestions have been made, within the framework of learnability analyses, for ways to reduce the explanatory power of the "fourth parameter", the starter set of hypotheses that children bring to bear on language learning. However, most of our proposals imply a different approach to the learning process than the one outlined by Chomsky, a functionalist approach that has more in common with Aristotle's Analogists. Let us now consider some advantages and disadvantages of modern day Analogy.

The term"functionalist grammar" applies to a heterogeneous set of linguistic and psycholinguistic theories that are united by one set of assumptions: *that the surface conventions of natural languages are created, governed, constrained, acquired and used in the service of communicative functions.* (Bates and MacWhinney, 1979, 1982a). Within linguistic theories that share these assumptions, the grammar is generally stated in terms that express a direct relationship to semantic and pragmatic constraints (e.g. Dik 1978; Kuno 1975; 1980; Givon 1979a und b; Li and Thompson 1976; Van Valin and Foley 1980; Lakoff and Thompson 1977). For example, the syntactic phenomena that revolve around a special surface role of "subject" are usually described with reference to their associated functional categories of "topic" and/or "agent". This descriptive approach is a direct violation of Chomsky's principles of absolute autonomy and blind application of transformations.

Indeed, it may be because of this violation that functionalist grammars remain fragmentary and in some cases, incoherent. In trying to capture interdependence and causal constraints among linguistic domains, functionalists promise a great de: more than they have been able to deliver so far. One can hear a classic American retort echoing in the ivy-covered halls: "If you're so smart, why ain't you rich?"

In short, the major disadvantage of the functionalist approach is the absence of a detailed grammatical theory; in seeking a theory with explanatory value, functionalists have not done well in description.

Of course, it should not surprise us that a more difficult task takes longer to accomplish. For one thing, most of the functionalist grammarians that we have cited here make considerable use of cross-linguistic, comparative research. A typical strategy is to concentrate on exotic languages that differ maximally from the characteristic structures of Indo-European languages (e.g. Van Valin and Foley 1980). From the functionalist point of view, a theory of Universal Grammar must account equally well for the full range of natural human languages (see also Keenan, 1976). Hence we should ascertain to the best of our ability what that full range is, before engaging in detailed exploration of one language. Some functionalists, including Givon (1979), extend their responsibilities to include facts about historical language change. In Givon's view, a theory of Universal Grammar should capture information about "structural weak points" and internal tensions in a language which force systematic changes over time. Finally, functionalists use a greater variety of data in their research, including analyses of spontaneous speech in conversations with two or more participants (e.g. Duranti and Ochs, 1979). In contrast with this emphasis on breadth, proponents of autonomous syntax have tended to opt for depth: detailed analysis of a handful of languages (in particular English), as they are used at one point in history, with particular reference to grammaticality judgments by trained native speakers. As a result, these investigators tend to know a great deal about a small set of phenomena; functionalist grammarians, instead, know very little about quite a few things. The advantages and disadvantages of both approaches should be obvious.

As psycholinguists, with a particular interest in language acquisition by children, we have been more attracted to the functionalist approach (Bates and MacWhinney, 1979; 1982a and b; MacWhinney, 1981, and in preparation). In their search for explanations, the functionalist grammarians are modern-day Analogists. They view language as a product of human reason, a phenomenon whose regularity and rationality will become clear to us if only we look carefully enough. The current interest in diachronic linguistics resembles in many respects the etymological research of the Analogists: explain how things got to be the way they are by examining their origins. Similarly, the interest in spontaneous speech also reflects these explanatory goals: describe language in its natural habitat, to uncover causal constraints on form. If an appropriate cover term for the autonomous syntax approach is "linguistic algebra", then a good nickname for the functionalist approach is "linguistic Darwinism".

From this point of view, the relationship between pragmatics and syntax is one of cause and effect. More precisely, pragmatics is the study of the contexts in which syntactic forms are used, the "ecology of grammar". There are, however, some very bad versions of this functionalist position. As far as we can tell, these are straw man theories that no one really believes; nevertheless, these oversimplified versions of the functionalist position have caused so much misunderstanding that they are worth considering in some detail, to clarify what the goals of a good functionalist theory really are.

The Straw Man version of functionalism suffers from the following failures and oversimplifications:

(1) A failure to account for the multiple, interacting constraints that determine linguistic forms;
(2) A simplistic view of causality that ignores the critical notion of *emergent form;*
(3) A failure to separate claims into distinct theoretical levels, each with its own empirical consequences;
(4) An inability to account for the learning of empty forms;
(5) A failure to account for automaticity in sentence processing.

Let us take these criticisms one at a time, to arrive at a more integrated theory of functional constraints on language.

(1) *Multiple Functional Constraints and their Interactions*

As described by some of its critics (e.g. Gleitman and Wanner, 1982), the functionalist position *equates* pragmatics and syntax. Functionalists view grammar as a reflex of communicative needs, a direct reflection of cognitive categories like "agent-action-object". The authors go on to show that such a position is necessarily in error, on several counts.

First, grammar could not bear a transparent relationship to communication. If it did, we could not explain the wide diversity among natural languages in expressing communicative functions. Slobin (1982) has made the same point, arguing that no theory of pragmatics can tell us why Japanese and Navajo have shape classifiers while English and French do not. In Slobin's terms, grammar is like a line drawing that bears an abstract relationship to the reality it represents. It is not a reflection of world knowledge, but rather a complex and often arbitrary process of selecting *which aspects* of reality to encode. Because this selection process varies so widely from one language to another, a direct relationship between pragmatics and grammar is impossible. Children must be equipped with a means of selecting the "codable" aspects of reality in their particular language. This should include biases to ignore those aspects of meaning that are never conventionalized in natural languages (e.g. pay attention to gender, number and perhaps shape, but not to color as a candidate for noun morphology).

A second point is raised by Bowerman (1981), in a paper entitled "Beyond communicative adequacy." Bowerman describes a period in which children make creative errors in semantics (e.g. confusing "put" and "give"), months or even years after using the same forms correctly. Clark (1982) provides similar examples of errors in assignment of form class to lexical items (e.g. saying "He broomed it" to refer to an act of sweeping, an incorrect use of a noun as a verb). Karmiloff-Smith (1981) makes a similar point with regard to the use of determiners by French children. If children were using forms correctly, in a fashion that insured successful communication, why should they go on to reanalyze the language and commit more errors as a consequence? Bowerman and Karmiloff-Smith both argue that children are interested in language for its own sake, as a problem to be solved above and beyond its communicative use. They will reorganize and "clean up" their set of rules without any external pressure, responding instead to internal pressures presented by points of inconsistency and/or a need for greater cohesiveness and symmetry in the system.

These are very fair criticisms -- if anyone had ever claimed otherwise. However, no one that we have read in the functionalist camp has ever proposed a one-to-one

relationship between form and function. Rather, grammars are viewed as solutions to an extremely complex set of interactions among many functional constraints. The procurement of goods and meeting of needs, i.e. the "cookie-getting" functions of language, can explain only a fraction of the many things that children do in acquiring grammar.

When a biologist examines the ecological niche for a given organism, he must consider many pressures toward survival and successful reproduction. Some of these pressures run directly counter to one another, e.g. the signalling value of bright colors in mating versus the dangers those same colors pose in vulnerability to predators. Organisms respond to these competing pressures in a wide variety of ways – all of them imperfect. Compromises are required that involve a cost-benefit analysis of incompatible needs, with solutions that depend on the genetic material available during adaptation. In the same fahsion, human languages present an array of possible solutions to a complex mapping problem, an interaction of functional constraints that often stand in direct conflict with one another. Slobin (1978) has discussed some of these competing charges to language, e.g. the need to remain perceptually clear while at the same time permitting rapid motor execution. A partial and cursory list of competing functional constraints on language would include the following:

(A) Communicative goals, or "classical pragmatics"
   1. Speech act functions at the level of individual utterances (requesting, promising, declaring, etc.)
   2. Discourse functions at the level of relations between utterances (e.g. topic maintenance, topic switching, disambiguation of reference)
   3. Social functions that cross utterance boundaries (e.g. establishment of status through levels of address, selection of formal versus informal lexical items in keeping with the context)

(B) Propositional content, or "classical semantics"
   1. Event-level content in relation to sentence-level grammar (e.g. agent, action, patient, location, instrument, time and space relationships)
   2. Schema-based content in relation to the inter-sentential discourse (e.g. the operation of a story grammar, or rules governing the formation of a good joke)

(C) Channel factors in on-line processing (i.e. functionalism that has nothing to do with semantic or pragmatic content)
   1. Perceptual constraints (salience, regularity, and continuity of forms – factors reflecting the operation of Gestalt principles of good form)
   2. Memory constraints in comprehension (serial order effects, constraints on the number of "chunks" available for segmenting event units)
   3. Memory constraints in motor planning (e.g. the accessibility of phrase structure units and individual lexical items in retrieval, as a function of frequency and uniqueness)
   4. Peripheral output constraints (phonological factors influencing speed and clarity of production)

(D) Constraints on long-term organization, e.g. "housekeeping" procedures that reorganize memory in the direction of symmetry and coherence.

It should be obvious from this partial list how many factors a complete functionalist theory has to account for. This should also make it obvious why no complete functionalist theories exist to date. Statistical procedures are available for modelling complex interactions in a "plurifunctional" or multifactorial causal space (see MacWhinney and O'Connell, in preparation, for one approach). But it is no easy matter to specify the values of all the parameters in such a problem-solving situation.

Our point for present purposes is that functionalist theories cannot and do not underestimate the complexity of form-function mappings in language. As Gleitman and Wanner (1982) point out, languages have indeed arrived at many solutions to the problem. And as Bowerman and Karmiloff-Smith note, children do try to solve the mapping problem and tinker with previous solutions, without any pressure from communication per se. There are so many psychological constraints that can account for variability in language that we need not invoke purely communicative functions to explain everything.

(2) *Formal causality and emergent form*

The issue of functional interaction brings us to a second problem with the Straw Man version of functionalism. Interactions of this magnitude are not additive, placing their constraints on grammar like so many raisins in a cookie. It is quite unlikely that any interactive solution will "look like" any of its inputs. Chomsky has frequently argued that grammars do not "look like" the rules of any other cognitive system, nor do they "look like" the impoverished data of spontaneous speech (e.g. Chomsky, 1975). Where, then, do the formal solutions of grammar originate? He concludes that they must exist in the child, as part of a genetic blueprint for language.

The problem with this argument is that it fails to take into account the notion of *emergent form,* a kind of causality that is best described in the classic Aristotelian format as "formal cause" (Bates 1979). To illustrate, take the example of hexagonal structures in the honeycombs created by bees. Where do the hexagons come from? They were certainly not in the wax itself, nor anywhere in the environment before the bees arrived. Must we conclude, then, that bees are innately prepared to build hexagons? In this case, no. There is a general law of form governing the interactions of circles and spheres, a geometric law according to which circular forms packaged together by random or even pressure form all sides will inevitably develop hexagonal structures at their interstices. This "packing principle" can account precisely for the geometry of beehives: bees with hemispheric heads pack in the wax with random pressure from all sides, resulting inevitably in the formation of hexagonal structures. The "innate" contribution of the bee to this process is quite simple and indirect; the actual solution is an emergent property of the interaction, dictated by general laws of form.

In the same fashion, the various grammars of natural languages can be viewed as emergent solutions to the problem of mapping non-linear meaning onto the linear properties of the acoustic-articulatory channel. Structures do not have to be innate if they will emerge inevitably anyway. The fact that we cannot find anything that looks like the solution in the inputs taken individually, does not mean that the solution is innate.

The concept of emergent form can help us to explain many of the creative errors that children make in language acquisition. Error data can be handled from two points of view. On the one hand, the creative errors that do occur suggest that children play a very active role in acquisition, constructing temporary theories that do not look like anything in their adult language input. Such phenomena provide strong evidence against passive, environmentally-driven theories of acquisition. On the other hand, Pinker (1979), among others, has argued that we should also attend to possible errors that do *not* occur in child language. By cataloguing these non-errors, and matching them up against possible grammatical rules that also fail to appear in the world's natural languages, we can construct a theory of natural constraints on grammar. Presumably these natural constraints are, in turn, part of the genetic make-up of the child, placing limits on the creative processes that are involved in positive errors. The problem with a genetic interpretation of error data is that we cannot account for truly bizarre errors when they do occur. First of all, it is dangerous in principle to base a theory on non-occurrences. More than once I have heard the non-error claim made in conference presentations, while a distraught parent next to me whispered that his child committed the same non-error that morning. Let us grant, however, that some errors are statistically less frequent than others. If we use a theory of innate knowledge to account for errors that do and do not occur, how do we deal with truly unique creations?

Let us offer one example of an off-beat, low-probability rule derived by one child. In Bates (1976), Italian children were given a series of paired requests uttered by identical puppets, e.g. "I want candy" versus "I would like a candy." The children were asked to decide which of the puppets was "nicer" or more polite, a task which presumably tapped sociolinguistic control over a variety of surface forms in their language. After each item, the Experimenter always asked the child to explain his or her response.

One of the items involved a contrast between formal and informal second person verbs, i.e. "Mi dai tu un dolce? " versus "Mi da Lei un dolce?" (Will you informal/formal give me a candy? "). From an adult point of view, the more formal level should be judged as more polite. However, one of the child responded with great conviction that "tu" was nicer, and explained that "Tu is nicer because it certainly isn't afternoon!" Upon further probing, it became clear that the child had formulated a rule in which verbs were assigned levels of address on the basis of time of day: *tu* in the morning, *Lei* in the afternoon. This rule did provide an indirect fit to the child's data base, since she attended preschool in the morning (where teachers and students all used informal address), but saw adults in public settings in the afternoon (e.g. grocery stores where parent and shopkeeper use formal address). However, the relationship between the child's conclusion and her environment is so indirect that no simple learning theory could explain it.

Do we have to invoke an innate bias to explain this odd and improbable rule? It seems to us that this event, like many other creative "errors" in child language development, reflects the operation of general but very powerful problem-solving mechanism interacting with a complex and variable data base. It may currently be the case that no natural language bases verb morphology on time of day (e.g. one form for the morning, one for the afternoon). However, it is apparently true that children are prepared to learn such a language if they encounter one. If some

errors are rare, or fail to occur at all, it may be because nothing in the child's current data base tempts her to a theory that would produce such errors. Alternatively, errors that seem plausible on semantic grounds (at least to the observing adult) may not occur because they are implausible or difficult to construct along any of the many other parameters that influence language learning (e.g. perceptual constraints on on-line processing, organizational constraints on long-term memory). The point is that nativist theories *underestimate* the creativity and plasticity of language learning. New solutions can emerge that bear little resemblance to their inputs, constrained only by general and universal laws of form. If we try to explain these solutions with a catalogue of innate ideas, that catalogue may become so large and unwieldy that it loses all explanatory appeal. Pushed far enough, the nativist position becomes a tautology.

(3) *Multiple levels of functionalist claims*

One source of confusion in functionalist theories is that many linguists and psycholinguists are working at completely different levels of analysis, with claims that require very different kinds of evidence. Since we have elaborated this point in two separate papers (Bates and MacWhinney 1979; 1982a), we will summarize it very briefly here, through the use of one example.

Take the relationship described earlier, a statistical correlation between the surface forms governed by *syntactic subject* (preverbal position, agreement with the verb in person and number, nominative case marking, etc.) and two distinct functional categories, *agent-of action* and *discourse topic*. This correlation can be analyzed at four different levels.

First, we can examine this relationship at the historical level, to determine where the correlation came from (e.g. Givon, 1979a and b). From this point of view, the triad of subject-agent-topic has a rather straightforward explanation: human beings like to talk about themselves and their activities. Hence it is not surprising that grammars have envolved mechanisms to take this high-frequency pattern into account. In fact, it is also true that subjects are statistically more likely to be first or second-person human agents, resulting in some implicational hierarchies that emerge in cross-linguistic research. That is, if a language has a surface role of subject, the following predictions hold: (a) if the language gives this role to non-agents, it will always give it to agents (and not vice-versa); (b) if the languages gives this role to third-person referents, it will always give it to first and second persons (and not vice-versa); (c) if the language gives this role to non-humans, it will always assign it to humans. And so forth. In sum, a wide array of interesting cross-linguistic facts can be explained if we assume that subject phenomena evolved to encode the overlap between agent and topic.

Historical claims do not require that the original causal relationship still operates in speakers of the language. Forms can evolve for a purpose that ultimately withers away, passed down through succeeding generations simply because they cause too little trouble to be selected out. Vestigial organs like our appendix or wisdom teeth illustrate the possibility of functional autonomy in evolution. At the second level of evidence, some functionalist theories claim that such functional autonomy is rare in language. Forms are maintained because they still actively serve some psychological function. The bulk of on-line psycholinguistic research involves claims at this level: certain functions are manipulated, to determine

whether a predicted form is used in their service. For example, MacWhinney and Bates (1978) manipulated givenness and newness in three languages, observing a series of predicted surface forms that were used in response to this function (e.g. word order variations in Hungarian, ellipsis of the subject on Italian, contrastive stress in English). A great deal of text analysis in linguistics also fits with these claims. For example, Duranti and Ochs (1979) have analyzed spontaneous conversations in Italian, showing how one particular word order variation (left dislocation of the object) occurs only when a particular kind of topicalization is needed.

Even if there is an on-line correlation between form and function in adult language use, this does not mean that children use the correlation in language acquisition. Some functional relationships might be so complex and obscure that children do not understand or need them at all in the early stages. To show that children use function as a guide to form, we need yet a third level of analysis, with its own kind of evidence. Specifically, we must show that children acquire a given grammatical form within a particular function "slot"; we may also want to describe the prior forms that children use while groping toward this solution. For example, McNew (1981) set up an experiment in which children were forced to use clausal information to identify referents ("No, not that one, it was the one that you bought!"). She successfully showed that children use far more relative clauses in this situation than in a related situation without functional pressure toward referent identification. At the same time, McNew was able to describe the prior forms that children use to solve the same problem before they have acquired relative clauses (e.g. pointing frantically while saying "that one!", even though the listener could not see the display). Similarly, Bates (1976) has shown how Italian children acquire the different surface forms associated with subject, in a piecemeal effort in which the forms are first separated into agent expressions (i.e. verb agreement) and topic expressions (i.e. word order). The same forms are put together to operate as a block at a later stage in development, mimicking the kind of historical course that Givon (1979a) has described for some natural languages.

The fourth level of functionalism pertains to the formulation of a competence theory, a grammar that takes facts at the prior three levels and builds them into a unified representation of linguistic knowledge. It is possible, in principle, to insist that the first three levels represent performance facts, which need not be accounted for in a more abstract and formal competence theory (e.g. Fodor, Bever and Garrett, 1974). The only way to decide between these competing approaches is to examine the kinds of grammars that can be written from each point of view (i.e. autonomous syntax versus functionalist grammar). If a good description can be offered that describes the workings of the grammar while at the same time accounting for functional constraints, then such a grammar would have to be preferable to a theory that accounts for fewer phenomena. Of course, it is entirely possible that this "supertheory" will prove to be impossible as Chomsky has suggested, and as the fragmented nature of current functionalist grammars may attest. It is an empirical question whether a unified theory of form and function can ever be attained.

In sum, to evaluate functionalist claims properly we should first ascertain what kind of claim is being made, and evaluate the theory by the kind of evidence

that is relevant at that level. Criticisms that mix these levels (e.g. attacking functional claims about acquisition, to defend formal grammar at the fourth level) will only add more turbulence to clouded waters.

(4) *The problem of empty forms*

Maratsos and Chalkley (1980) have raised a criticism of functionalism that is not easily handled by any of the above modifications. That is, how do children acquire arbitrary and empty forms in their language (e.g. German gender), forms that have no obvious functional value at all?

One way out of this cricticism is to point out that there may be "hidden" functions that maintain apparently empty forms. For example, it is true that German gender has little to do with sex. However, gender is helpful for disambiguating referents in a complex piece of discourse. Take the following example from English:

> John told Bill about the conversation because he was worried.
> John told Mary about the conversation because he was worried.

The first conversation is ambiguous: "he" might refer to John or to Bill. The second sentence is quite unambiguous, because the pronoun must agree in gender with its referent. The operation of gender is marginal in English, operating only within the pronoun system. However, it is a much more useful guide to co-reference in languages in which gender is built into the case system and/or marked on all noun modifiers. Indeed, Givon (1979a) has argued that many seemingly arbitrary aspects of morphological agreement can be traced back to number and gender marking on pronouns, pronominal morphemes which gradually erode into their co-occurring open class forms (nouns, verbs, adjectives) to become obligatory markers of sentence roles. Once such co-reference devices are built into the grammar, it may be difficult to get rid of them without inventing something else to take their place.

In short, many seemingly empty forms may have a communicative *function* even if they have no communicative *content*. Nevertheless, it is unlikely that children use the more arcane functions as a guide to acquisition. How do they acquire syntactic and morphological devices that seem to them to serve no purpose? The answer has to be that they do it by brute force, that is, by rote memory.

In Bates and MacWhinney (1982a), we discuss several aspects of "vestigial learning", mechanisms for acquiring functionless forms. These include evidence of rote processing, a greater role of imitation outside of direct communicative context, as well as a tendency to avoid empty forms or to acquire them more slowly than forms with a more transparent function (e.g. German children master case before they master gender). Maratsos and Chalkley's own proposals concerning the use of correlational data would be included in the "vestigial learning" devices discussed by Bates and MacWhinney (1982a). It is obvious not only in language acquisition, but in very general aspects of cultural transmission, that people can learn to do meaningless things, or things that have long since lost their meaning. In other words, the learning of empty forms is not specific to language. However, this concession in no way detracts from the main point, that simple functionalist theories fail to explain all aspects of language acquisition. Additional mechanisms are necessary to deal with the acquisition of functionally autonomous forms.

(5) *Autonomy versus automaticity*

In several psycholinguistic theories that incorporate principles of autonomous syntax, the claim is made that grammatical function words (i.e. "closed class items") and meaning-bearing words (i.e. "open class items") are processed in qualitatively different ways. For example, Healy (1980) has shown that typographical errors are more difficult to detect when they are embedded in closed class items. Bradley and Garrett (1979) have provided related evidence from lexical decision tasks, in which the reader must classify items as real words or nonsense items in his language. In such tasks, a complex nonsense word may contain a real lexical item in one of its syllables (e.g. "DOGIX"). When open class words are embedded within nonsense items, they yield clearcut frequency effects: nonsense words take longer to reject if they contain a high-frequency content word. However, the same frequency effects are not obtained if the hidden word is a closed class item (e.g. "WASIX"). The suggestion is made that closed class lexical items are stored in a separate lexical register, one that is used for grammatical rather than semantic processing. Further support for this position comes from the fact that closed class items seem to be selectively impaired in Broca's aphasics (Bradley, Garrett and Zurif, 1979).

In short, there is at least some evidence for a separation between grammatical and semantic processing. Does this require us to accept the concept of autonomous syntax? To some degree, it was already necessary to build meaning-free processing into our theory to account for the acquisition of vestigial or functionally autonomous forms (e.g. German gender). However, up to now we could argue that these were secondary strategies, supplements to a system in which form and function are well integrated. If all closed class items are registered separately and processed differently from open class items, then it might well be that a theory of autonomous syntax provides a more parsimonious account of the data.

There is, however, an alternative interpretation. Instead of "autonomy", we may want to describe the same phenomena in terms of "automaticity". Note that the closed class consists entirely of high-frequency items that are highly predictable in natural contexts. As such, the distinction between closed and open class items may be a special case of a much more general distinction in cognitive psychology between *automatic* and *controlled processing* (Posner and Snyder 1975; Shiffrin and Schneider, 1977). Although theories of automaticity vary somewhat, they all stress that frequent, predictable and "overlearned" behaviors require little or no conscious attention. At the same time, they are much less vulnerable to contextual factors that influence more deliberate activity. In fact, once a behavior becomes automatized, it is quite difficult to subject it to close scrutiny (e.g. thinking about how you respond to gravity while skiing). This does not mean that the behaviors are functionally autonomous, nor that initial acquisition of these skills required no deliberate effort. Instead, it is generally the case that automatic behaviors continue to be smoothly integrated with their "meanings" or functions. For example, we can change course smoothly as we maneuver through a crowd, with little attention to the mechanisms of walking or of obstacle-detection that are required to operate successfully. Automatic behaviors continue to be functionally-based and functionally-controlled; however, they run off in predictable ways that do not vary greatly from one context to another. In the same

fashion, grammatical forms are used to map meaning in clear and interpretable ways across rapid discourse. The fact that phrase structure "frames" and function words are accessed rapidly and automatically does not mean that they are "meaning free", nor that functions play no role in their acquisition. However, like so many automatic motor systems, these aspects of grammatical processing may be qualitatively different in accessing and execution when compared with more deliberate and variable linguistic activities (such as the selection of open class lexical items). The main point for present purposes is this: many of the apparent qualitative distinctions between closed and open class lexical items might reflect the operation of very general learning principles that distinguish automatized or overlearned behaviors from controlled processing. Within language, the empirical consequences of "autonomy" and "automaticity" might be close to identical. The difference between the two theories is that automaticity requires no new language-specific principles to account for the same phenomena. Hence it is a more parsimonious explanation.

It is an empirical question at this time whether modular or integrated theories will yield the most parsimonious description of language, nor do we know enough yet to reject a theory of innate mental organs. The functionalist approach appeals to use in part because of its biological plausibility. It is a theory that stresses continuity between linguistic and non-linguistic abilities, and integration of pragmatic and syntactic processing within language. As such, it makes it easier to envision how human beings might have evolved the capacity for grammar in the first place.

Ultimately, however, all scientists must answer to the mathematician's criteria of coherence and elegance of description. Ironically, we can invoke the mathematician's criteria to argue against modular theories of mind. Just as the mathematician tries to restate formulae in a way that permits generalization, so too the linguist might try to state Universal Grammar in terms that reveal the operation of more general mental principles. By avoiding the postulation of innate and/or language-specific mental entities, the functionalist tries to explain language with a smaller set of cognitive categories and processes. The resulting linguistic theory might be quite a bit less elegant. However, this loss may be compensated at higher levels, if it helps us to formulate a more cohesive theory of the biology of mind.

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