copies of my writings
to be kept in a file
somewhere accessible
to you.

In Jay L. Garfield (ed.) Modularity in Knowledge Representation and Natural-Language Understanding. MIT Press/Bradford, 1987

8

Categorial Grammar and the Domain Specificity of Universal Grammar

Michael Flynn

It is a commonplace that judgments about an object's properties are influenced by the point of view from which it is considered. For example, differences between theories of grammar that sometimes loom so large in the daily lives of linguists are for the most part invisible to just about everyone else, often even to colleagues in closely related disciplines. The observation of the distinction between a star and a planet, crucial for some purposes, on other occasions would be quite impertinent or downright rude.

The object I want to consider in this chapter is Universal Grammar, in Noam Chomsky's sense. What I am going to suggest is that if assignment of structure to sentences of natural languages is done with a categorial grammar along certain lines, then a widely adopted attitude about a property of Universal Grammar namely its domain specificity, shifts a bit. Before I turn to that, though, I want to add a little perspective to the discussion and outline the instantiation of categorial grammars I have in mind.

Once upon a time, the hierarchical organization of a sentence's constituents and their relative ordering were simultaneously specified by a context-free phrase-structure grammar, that is, by rules of the form $A \rightarrow \psi$, where A is a single nonterminal symbol and ψ is a finite ordered sequence of symbols of any sort. The resulting derivations can be associated with trees in the familiar way. As far as I know, these approaches were rarely contrasted with alternative procedures (at least not in print and with explanatory adequacy in mind; however, see McCawley 1968 for an exception). Besides, phrase-structure rules were straw men anyway. Chomsky forcefully argued that the important and influential work of many American Structuralists presupposed these sorts of rules, and that they (I mean the rules) don't do what they are intended to do, at least not by themselves. By a venerable strategy that dates back at least as far as the time of Ptolemy, new technical devices were grafted onto the old, improving, it came to be widely agreed, the empirical adequacy of the theory.

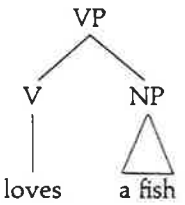
Later on, as the perception of linguistics as a branch of human psychology

began to reemerge in a new form,[1] the question of how a child abducts structure-assigning rules from data that underdetermine them became prominent. The story here is complex and, I think, fascinating, even (maybe especially) from a sociological point of view. (For more details, see Newmeyer 1980.) For example, one could regard these rules as universal and hence innate, thus shifting the explanation for their complexity (or, at any rate, their character) to another science, such as psychobiology. What I'd like to emphasize now, though, is the very important work in which Jackendoff (1977) gave an explicit instantation of the $\bar{X}$ theory, some version of which (as we say) is today widely assumed. Much of the technical apparatus of Jackendoff's deployment, such as the symbol counting evaluation metric, has now, it appears, faded from the scene, but aspects of the spirit of the theory live on, particularly in the theory of government and binding. I'll display this by giving a simple example of how several components of the grammar interact to assign structure to phrases. These principles are under intensive investigation and are continuously being modified and elaborated, but I will overlook much of this here, as my goal is to highlight general features of the approach.

Consider how the principles in (1) assign the structure to the VP *loves a fish* in (2).

(1) a. X-bar theory: Predictable phrasal categories are projected from lexical categories.
b. θ criterion: Each argument bears one and only one θ-role, and each θ-role is assigned to one and only one argument, under (perhaps proper) government.
c. Head Parameter: English is head initial.
d. *Loves*, a verb, assigns a θ-role to an NP.

(2)



That V is dominated by VP follows from the $\bar{X}$ theory. That VP also dominates NP follows from the θ criterion, given (1d).[2] That the NP appears to the right of the verb follows from the setting on the Head Parameter.

The $\bar{X}$ theory and the θ criterion are a part of Universal Grammar (UG). The setting of the Head Parameter requires some input data. Notice that dominance and order, once inseparable, are now specified by independent principles. Other components may be involved as well, for example the theory of abstract case (Stowell 1981; Pesetsky 1983). And the head-initial setting may actually be a parameter on the directionality of case and θ-role assignment (Koopman 1984; Travis 1984). I won't elaborate on any of this here; the main point is that in the Government and Binding framework (or, for that matter, in just about any theory of grammar that comes to mind these days[3]) hierarchical organization and left-right order are specified by independent principles. Innate principles of UG, given minimal lexical information, determine dominance. Ordering principles, be they settings on the position of the head or parameter settings on the directionality of case and θ-role assignment, require some data to determine. Dominance, then, is largely universal, whereas order is language-particular (but not rule-particular, as in Montague 1973 and Chomsky 1965). Ordering principles, once they are determined, are presumably intended to apply language-wide.[4] It is exactly this way of slicing up the pie that I will now sketch by using a categorial grammar.

I take the following characterization of categorial grammars to be essential: In categorial grammar, dominance relations are explicitly encoded in the assignment of lexical items to categories, there is a functional correspondence between the syntactic categories and the logical type expressions in that category denote, and primitive syntactic categories correspond to primitive semantic entities whereas fractional categories correspond to functions. (For a particularly clear exposition of the basic idea, see Lewis 1970.) What I'm going to suggest is that the $\bar{X}$ theory and the θ criterion can be replaced by a categorial grammar. Now, what I'd like to do is simply surgically excise these two principles and the categories they presuppose, put in the replacement part, stitch the edges back together, and see how the patient gets along. The trouble is that things go badly, as Williams (1984) has pointed out.

Rather than go into this here, I will embed a revised version of an idea I had about categorial grammars a few years ago (part of which appears in Flynn 1983) in a slightly modified form of the approach taken by Chierchia (1984, 1985) as influenced by the work of McConnell-Ginet (1982). The modifications I'll make are intended to factor our responsibilities in the grammar so that the organization will come as closely as possible to what I take to be the Government and Binding framework. (This is in part a rhetorical strategy. Since it is Chomsky's view of UG that I want to examine, I want to minimize the damage done to the framework that prompts the view. I also want to leave open the possibility of embracing the other insights articulated within the framework.) Then I'll be in a position to make the observations I promised about the domain specificity of UG.

One important innovation in Chierchia's work is the adoption of Nino

Cocchiarella's theory of properties, deployed by means of the syntax and semantics of a family of second-order systems, prominently including Chierchia's IL_*. I can't do justice to this system here, but I will try to outline aspects of it that are important for my purpose.

The ontology of a natural model for IL_* consists of two basic kinds of entities: propositions (the nature of which is intentionally left open) and individuals. Individuals are of many a spot and stripe. They include not only ordinary things, such as Bertrand Russell's favorite necktie, but also what Chierchia (following the lead of many others) calls kinds, qualities, locations, periods, eventualities, and other things, many of which correspond to properties by means of what Cocchiarella calls a Fregean embedding (a map from properties to individuals). Properties themselves are *n*-place functions from individuals to properties. Propositions can be regarded as 0-place properties.

Expressions of natural languages fall into three broad classes, depending on the sorts of things they denote. First, there are the *basic* categories, as in (3).

(3) S, NP, CN, Pred_{pp}, Pred_{ADJ}

Expressions of these basic categories denote basic entities. The type-assignment function (τ) for them is as in (4).

(4) $\tau(\mathbf{S}) = \text{p}$; $\tau(\alpha) = \text{e}$ for all primitive categories α, where $\alpha \neq \mathbf{S}$

p is the type of propositions, e the type of individuals.

There are two kinds of expressions assigned to fractional categories: predicates and functors. Predicates denote properties. Examples of these are given in (5).

(5) $\mathbf{IV}\left(\frac{\mathbf{S}}{\mathbf{NP}}\right)$ $\mathbf{TV}\left(\frac{\frac{\mathbf{S}}{\mathbf{NP}}}{\mathbf{NP}}\right)$ $\frac{\mathbf{IV}}{\mathbf{S}}$ $\left(\frac{\frac{\mathbf{S}}{\mathbf{NP}}}{\mathbf{S}}\right)$

The "official" specifications for these commonly used abbreviations appear in parentheses. The type assignment for these is given in (6).

(6) $\tau\left(\frac{\mathbf{A}}{\mathbf{B}}\right) = \langle \mathbf{e}, \tau(\mathbf{A}) \rangle$

For example, the type assignment for transitive verbs is $\langle \mathbf{e}, \langle \mathbf{e}, \text{p} \rangle \rangle$, i.e., a two-place predicate interpreted as a two-place property. Also to be included among the predicates in (5) are what McConnell-Ginet calls an admissible argumentation of an *n*-place predicate. Basically, McConnell-Ginet argued that certain restrictive modifiers, instead of being functions in, say, **IV/IV**, are actually arguments of the verb. She writes: "If $\langle \mathbf{X}, \mathbf{V} \rangle$ is a cover symbol

denoting any category whose lexical members are verbs ... then I will propose a category of the form $\langle \mathbf{X}, \mathbf{V} \rangle$/**AD-V**." (1982, p. 164) In our terms, if *runs* is in category **S/NP** then an admissible argumentation for *runs* is a member of

$$\frac{\frac{\mathbf{S}}{\mathbf{NP}}}{\mathbf{AD\text{-}V}}.$$

The type assignment here will be $\langle \mathbf{e}, \langle \mathbf{e}, \mathbf{p} \rangle \rangle$, and the first argument of the function which the admissible argumentation of *runs* denotes will be the individual corresponding to the predicate denoted by the **AD-V**.

Chierchia also admits a third broad category (the second class of fractional categories), which he calls functors. Functors, like prediates, correspond to functions, but they are unlike predicates in that the objects they denote are not in the domain of the Fregean embedding, (i.e., they do not correspond to individuals in the ontology). Chierchia indicates this difference with a double slash, as in (7).

(7) $\frac{\mathbf{IV}}{\mathbf{IV}}\left(\frac{\frac{\mathbf{S}}{\mathbf{NP}}}{\frac{\mathbf{S}}{\mathbf{NP}}}\right)$ $\mathbf{DET}\left(\frac{\mathbf{NP}}{\mathbf{CN}}\right)$ $\mathbf{Prep}\left(\frac{\mathbf{PRED_{PP}}}{\mathbf{NP}}\right)$

The type assignment for functors is as in (8).

(8) $\tau\left(\frac{\mathbf{A}}{\mathbf{B}}\right) = \langle \tau(\mathbf{B}), \tau(\mathbf{A}) \rangle$

The important point for my purpose about all this is that this system conforms to what I said earlier about the essential characteristics of categorial grammar. There is a functional correspondence between the syntactic category an expression belongs to and the logical type of thing the expression denotes, and primitive categories correspond to primitive semantic entities (individuals or propositions) whereas fractional categories correspond to functions. (Chierchia calls this the Principle of Functional Correspondence.) I'll now turn to the claim that dominance relations are encoded in the assignment of lexical items to categories.

Chierchia specifies the core combinatorial part of the syntax of the object language by means of a parametrized template he calls $\mathbf{AFFIX_n}(\alpha, \beta)$.

(9) $\mathbf{AFFIX_n}(\alpha, \beta)$: Affix α after the $\mathbf{n^{th}}$ constituent of β.

He writes: "The parameter *n* and the level of constituenthood that $\mathbf{AFFIX_n}(\alpha, \beta)$ has access to are severely restricted by an independent set of

principles determining how such parameters may covary with a particular categorial base, the configurational characteristics of the language, etc. [Here Chierchia cites Ades and Steedman 1982 and Bach 1980, 1983 as examples.—M.F.] Right concatenation and Bach's 'Rightwrap' ('Affix α after the head of β') are the only instances of the core operation that have been argued to be active in the grammar of English." (Chierchia 1985, p. 427)

In line with my goal here, I'd like to reformulate this a bit. Suppose instead that we regard the grammar as a system of principles that structures assigned to sentences must satisfy in order to be generated by the grammar. So, corresponding to the items in (1a)–(1d), I'll reformulate our grammar so that one aspect of UG takes over the role played by the $\bar{X}$ theory and the θ criterion, and a language-particular setting on a parameter plays the role of the head-initial property of English.

Let us think of this in terms of node-admissibility conditions. The grammar will contain, say, functions from (sub)trees to $\{\surd, *\}$ ($\surd$ for admitted, $*$ for rejected), and a tree is admitted only if every nonterminal node in the tree is admitted. I'll take the lexicon to be a set of ordered pairs of the form $\langle \alpha, \mathbf{W} \rangle$, where α is a lexical item and **W** is a category, and regard such specifications as licensing trees of the form

W
|
α

I confine myself here to those functions whose role is to check dominance and order. All these will conform to the provisional principle in (10), which I'll call Connection after the title of Ajdukiewicz's seminal paper (1935).[5]

(10) Connection
A node **W** is admitted only if it immediately and exhaustively dominates a node labeled with a fractional category of the form **W/Y** (or **W//Y**) and a node labeled with the category **Y**.

Connection will play a role corresponding to the $\bar{X}$ theory and the θ criterion in a way that will become obvious momentarily. The role of the Head Parameter will be played by what I call an Ordering Convention. Here, too, it is anticipated that UG will make available a number of options from which languages select, and, as with the setting of the head parameter, it is intended to apply language-wide. (See Flynn 1983 for some discussion of this in slightly different setting.)

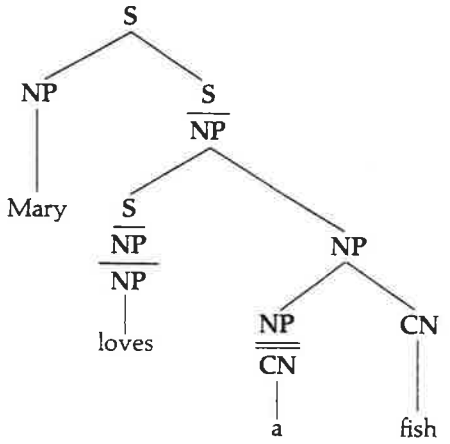
For the purpose of illustration, I propose the principle in (11) as the language-particular principle to which functions in the grammar of English conform.

(11) Ordering Convention for English
An S node is admitted only if its main functor is to the right of its argument. For all other nodes, the main functor must be to the left of the argument.

The term "main functor" here is from Ajdukiewicz (1935), who attributes it to Leśniewski. Basically, the main functor is that unique expression (perhaps I should say "tree" here) which can be analyzed as the fractional category **W/Y** in Connection. Before giving an example of how this works, let me be clear about its status. The convention in (11) is simpler that the one I had proposed for English in previous work because restrictive modification is handled differently in the Chierchia-McConnell-Ginet system than it was in the framework (i.e. Montague's) that I was assuming before. I regard this new convention as quite programmatic. It does, however, handle correctly all the cases displayed in (5) and (7). Whether or not it or something like it will work in general, especially when "wrapping" constructions are considered in detail, will I hope become clear as the investigation proceeds. The condition on wrapping proposed in Flynn (1983) will work to get the correct relative ordering between object **NP**s and restrictive modifiers under McConnell-Ginet's revision, but I still regard these cases as far from closed.

At any rate, for now I will serenely presuppose that (11) works perfectly. To see how it works, consider the tree in (12).

(12)



As the reader can easily verify, each node satisfies both Connection and the ordering convention for English. Among the consequences of the ordering convention are that English is SVO and that it is prepositional.

Though further details remain somewhat murky, especially to those a bit unfamiliar with the work I am relying on, the general approach is, I hope, fairly clear. I regard it as quite reasonable to suppose that a framework

roughly isomorphic to the Government and Binding theory can be erected using a categorial grammar. Connection plays the role of the $\bar{X}$ theory and the θ criterion. The ordering convention plays the role of the setting on the Head Parameter (or, perhaps, the directionality of Case and θ-role assignment).[6] Conditions on the Wrap function will play the role of the Case Adjacency Principle. I said "roughly isomorphic" because it is to be hoped, and it appears, that there are empirical considerations that will choose between the two frameworks. (See, for example, Chierchia 1985.)

I now turn to the central conditional of this paper: If the approach outlined here can be sustained, what happens to our judgment about the domain specificity of properties in UG? This topic is a terminological minefield, and I will try to avoid lengthy textual exegesis. But I do want to be clear about what I think the empirical questions are, and about how the approach just outlined bears on one of them.

There are a number of attitudes one can adopt toward the lay of the mental land, approximations of some of which can be seen by considering the alternatives that arise in (13) by choosing one from column A and one from column B.

(13) Most things of interest to us about the mind are $\left\{\begin{array}{c}\text{A}\\ \text{innate}\\ \text{acquired}\end{array}\right\}$ and $\left\{\begin{array}{c}\text{B}\\ \text{domain-specific}\\ \text{general}\end{array}\right\}$.

The Procrustean beds induced here do not exactly characterize anyone's position that I know of, but I do think it fair to say that the dominant view in generative circles tends toward some variation of the top line selection in (13).

Chomsky's view of the matter is, I think, pretty clear, at least as far as language goes. During the famous debate with Jean Piaget, he put it this way:

> I would say, then, that a rational approach would be to assume that in the domain where we have some nontrivial results concerning the structure of language,[7] the principles of organization that determine the specific structures of language are simply part of the initial state of the organism [i.e., they are innate—M.F.]. So far as we know, these principles don't generalize, that is, there are no known analogues in other domains of intelligence (it is possible that such analogues are partially there, but this remains to be seen). (Piattelli-Palmarini 1980, p. 172)

Let me elaborate here by saying how I'm going to take the prevailing terminology. First, let's take the "domain of language" or "language domain." As Chomsky has noted (1975, pp. 15–16), pretheoretically we make a guess about the domains. Further investigation may indicate that our first approximations of domain boundaries (chosen usually on teleological grounds; e.g., face recognition, chess-playing) have to be altered somewhat to achieve a tolerable degree of coherence. But we do not want to *define*, say, the domain of language in terms of specific properties; if we do, Chomsky's claim becomes a tautology. After all, if the only properties a domain can (rather than happens to) have are those that are domain specific, then it is a trivial and uninteresting observation that properties that domains have are domain specific. In what follows, then, I will regard the domain of language to be the strictly formal (syntactic) properties; though this is still rather fuzzy, nothing really hangs on it. Thus, I take it that it is at least logically possible that factors determining grammaticality, no matter how refined we take this notion to be, are not specific to the domain of language.[8]

I will construe Universal Grammar in what I take to be the standard sense—namely, that system of principles, conditions, rules, and whatnot that are elements of properties of all human languages by biological, not logical, necessity (Chomsky 1975, p. 29; Chomsky 1980a, p. 28).

Now consider what Chomsky has called "the language organ." I'm not completely sure how he wants to define this notion, but I will take it as follows: The language organ is that part of UG that is domain specific. So UG is a set of principles or whatever, $P_1, \ldots, P_n$, that determine the formal properties of languages and are determined by human biology. The language organ (LO) is the collection of those principles P_i that are (contingently) domain specific.

I will assume that UG is non-empty, by the standard poverty-of-the-stimulus argument. The possibilities for LO, as I see it, are those given in (14).

(14) a. LO = UG
b. LO $\subset$ UG (proper inclusion)
c. LO = $\varnothing$

I take Chomsky's view to be that expressed in (14a). Piaget, on the other hand, I regard as having endorsed the view expressed in (14c). (I read him as agreeing with Chomsky that UG is non-empty.) The view I want to urge here is the one in (14b); that is to say, I want to suggest that there is at least one property in UG that is not specific to the domain of language. (Notice that 14b does not preclude 14c, but I don't want to argue for the stronger 14c.)

As the reader can probably guess, the property I have in mind is Connection and the categories it presupposes. Another domain in which I think it plays an important role is what I sometimes think Chomsky (1975,

pp. 48 ff.; 1980a, pp. 55, 92) is referring to when he speaks of "common sense understanding." In other words, our inventory of category indices and a principle that regulates admissible combinations of expressions assigned to these categories are in part determined by our semantics, and this is determined by our ontology (i.e., the way we categorize, to borrow Quine's charming phrase, "the passing scene"). Chierchia also writes in this vein. At one point he says: "If, as many philosophers seem to think, the role of a logical grammar should be to display somehow the structure of the world, then one of the theses of HST* [one of the systems that instantiate Chocchiarella's theory of properties—M.F.] seem to be that properties may be looked at from two points of view. *Qua* predicable entities they appear to be essentially incomplete or 'unsaturated' structures.... Properties however, can also be 'nominalized' and nominalized predicative expression can be subjects in predication acts." (Chierchia 1984, p. 53) (This is the role of the Fregean embedding I mentioned earlier.) Later Chierchia writes: "There is a certain hypothesis about possible semantic universes built into IL_*, namely, the three layers hypothesis ... syntactic categories fall into three natural classes, determined by the semantic type associated with them: arguments, predicates, and functors." (1984, p. 151)

Thus, the view is that the categories that play such a major role in the formal syntax of languages are determined, broadly speaking, nonlinguistically, as a reflex of how we categorize the world, i.e., our ability to analyze and manipulate abstract objects. By the way, we would probably not want to say that this ability is inherited from or attributable solely to natural language. As Fodor puts it (1975, p. 56), "either we abandon such preverbal and infrahuman psychology as we have so far pieced together, or we admit that some thinking, at least, isn't done in English." Like Fodor, I regard the latter as far more attractive. During the Chomsky-Piaget debate (Piattelli-Palmarini 1980, p. 173) Fodor remarks: "I take for granted that thinking is a domain that is quite different from language, even though language is used for the expression of thought, and for a good deal of thinking we really need the mediation of language." (See also Fodor 1975, pp. 83 ff., where it is argued that there must be some mental representation, independent of natural language, the terms of which are coextensive or perhaps synonymous with predicates of natural language.)

The view I am arguing for here may have been shared by the young Wittgenstein. For some discussion see Küng 1966,[9] chapters 4 and 6. Küng writes: "It is important to notice how for Wittgenstein the so-called logical form is not merely the syntactical form of sentences but at the same time the ontological form of the world of facts. He calls it explicitly 'the form of reality' (Wittgenstein 1922:2.18)."

I don't think that this sort of view comes as any news to those who have have been working on categorial grammars, especially those working in the

very rich tradition of Polish philosophy. For example, Roman Suszko, in a paper about categorial grammars, writes: "It may be a reasonable conjecture that the content of this paper is connected with structural inquiries in linguistics and with some problems of the philosophy of language and thinking. But we do not discuss here these connections."[10] (Suszko 1958)

Recapitulating, I will now try to express the fundamental point here as succinctly as I can. In categorial grammar, the combinatorial possibilities of expressions in a language are explicitly encoded in the labels of the expressions' categories. The empirical claim is that the categories are functionally related to what one might call classes of objects in the language user's semantic universe—that primitive categories correspond to primitive objects and that fractional categories correspond to functions. These classes of objects are determined by the language user's classification of the world. Thus, the way we organize the world and the way we hierarchically organize the expressions we use to speak of the world are, in a sense, two sides of the same coin.

I see this not simply as a matter of two components "interacting" (Chomsky 1980, pp. 55, 123). It is, rather, a case of a principle and the objects the principle refers to in linguistic theory being homomorphic to a subsystem of a quite distinct domain. For a discussion along similar lines, see the transcript of the discussion between Chomsky and David Premack in Piattelli-Palmarini 1980.[11]

To conclude, I'd like to briefly articulate the view of the mind I think is suggested by (14b), generalizing now a bit. First, consider the view, suggested by (14a), that all properties of Universal Grammar are domain specific. Each species (humans, for example) has a set of domains in each of which (or at least in many of which) normal members of that species achieve a system of knowledge and/or belief. The boundaries of each of these domains are clarified as research proceeds, and are determined on grounds of coherency, not specificity. Suppose that what (14a) says for language is true for all domains. Without question, this would be an empirical discovery. In each case, the character of the domain is determined by domain-specific principles in the organism's initial state. Each of these principles has a biological explanation (in principle, at least) but no independent psychological explanation. The messy, fuzzy, difficult-to-classify activity we observe in the organism in question results from the interaction of the organ/domains. I believe this is the view that Chomsky has in mind when he writes of the "modularity of mind" in *Rules and Representation* (1980). (The sense of the phrase here is quite distinct from that discussed in Fodor 1983 and presupposed through much of the present volume.)

The picture painted by (14b) is somewhat different. Here we have the same domains, their interaction is responsible for the unwieldly behavior we observe, and we have what might be called a modular mind. The

difference is that the modules don't correspond so closely to the domains. In other words, there need not be a unique module for every domain—or, indeed, for any domain.

Looking at things this way takes the bite out of what I think is a rather vexed question: the determination of domain. For example, is the ability to swat a fly in the same domain as the ability to hit a curve ball? A lot seems to hang on what abstractions we wish to make. Either way seems to lead to conceptual confusion when it comes to discussing the organ(s) on which the domain(s) is (are) based. But if we think of an organ, or module, as contributing to but not determining a domain, the number of modules (which, I am assuming, is biologically determined, i.e. an empirical question) can stay pretty small—say, under a thousand. But the number of domains we might be concerned with can be very much higher, and can be determined by what we take the organism to be doing and what we are interested in. (This is, I gather, a familiar strategy in ethology; see, for example, Morris 1958.)

Thus, (14b) suggests that the mind does consist of genetically determined modules, but that these do not match the domains in a one-to-one fashion. Each domain makes what might be called a selection, either ontogenetically or phylogenetically,[12] from the modules in constructing itself, perhaps adding genetically determined special-purpose properties. In particular, the language domain shares its categories with the independent domain that characterizes the world.

However, the language domain may also have special-purpose genetically determined properties. For example, suppose that Principle A in (15) is true and domain specific.[13]

(15) An anaphor is bound in its governing category

It is indeed difficult to imagine another domain that has this property. If there is no other such domain, the resulting picture we get is something like figure 1. (The leftmost compartment may well by empty.)

The way I have set things up here runs the (empirical) risk of termino-

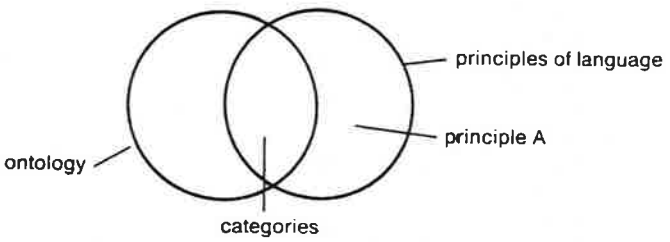


Figure 1

logical misrepresentation. That is, suppose it turns out, as more and more features of the description of the formal properties of language become known, that in fact many of these features are shared by other domains or are selected from more general, more abstract, genetically determined modules. The situation I'm envisioning here is one in which several distinct, higher-order modules contribute formal properties to the language domain. It would begin to seem very odd to speak of "the language organ." It would perhaps be better to speak of "the language organs" much as we speak of the speech organs (which in this case would be quite analogous), adding that one of these organs is domain specific. I need hardly mention that what I have said in this paragraph is rather speculative, yet it seems worth keeping the possibility in mind.

To sum up: I have suggested that the $\overline{X}$ theory and the θ criterion can be replaced by a categorial grammar embedded within a very promising general theory of language. I have argued that if this view is correct, our view of the impact of linguistic theory on the organization of the human mind is altered a bit. However, there should be heavy emphasis on the *if* in the last sentence. I hope I've contributed to the conceptual issues involved. Many intriguing and quite difficult empirical issues remain.

Acknowledgments

I would like to express thanks to Dan Finer, who gave me patient advice concerning several topics in the first section, to Gennaro Chierchia for checking parts of the second section for accuracy, and to the Max Planck Institute for Psycholinguistics in Nijmegen, The Netherlands, for providing me with the opportunity, in the fall of 1982, to begin thinking about some of the topics in the third section. Thanks also to Richard Oehrle for reading an earlier draft and making several suggestions.

Notes

1. In particular, the goals of general-process learning theory were abandoned. For a clear discussion of this, though not with reference to linguistics, see the introduction to Seligman and Hager 1972. I'd like to thank Randy Cornelius for recommending this book to me.
2. Perhaps not quite. The NP must also be strictly subcategorized by *love*, so that the position is obligatory. For discussion of this, see Chomsky 1981, pp. 40–41. Of course the argument remains an enthymeme, but the additional premises, including the one mentioned in this note, don't matter for the point I wish to make.
3. See also Gazdar and Pullum 1981.
4. Somewhat less strict requirements have been proposed by Huang (1982) and others.
5. This statement of Connection is provisional because it will no doubt have to be revised when wrapping operations and functional composition are added to the grammar. Though people working with categorial grammars have a clear intuition of what Connection will have to do in these cases, I have found an elegant articulation of the principle oddly elusive.

6. Curry (1963; written much earlier) writes that "we may conceive of the grammatical structure of a language as something independent of the way it is represented in terms of expressions." The grammatical structure, or what Curry calls the *tectogrammatics*, is the hierarchical organization of the phrases without the terminal elements; the *phenogrammatics* are the terminal elements plus their lieft-eight ordering. For further discussion of this distinction see Dowty 1982.
7. A nontrivial result for Chomsky is "one that has some explanatory force over a range of empirical facts and that can be refuted." It has been widely appreciated, at least since Quine 1951, that unpacking the notions of explanatory force and refutability is not entirely straightforward (see for example the introduction to Piattelli-Palmarini 1980.) But I will assume, as I think Chomsky intends, that if the principles at the "hard core" have general applicability and if it is reasonably clear when these in conjunction with "protective belt" hypotheses would make false predictions, then the principles at the core are nontrivial. For some further discussion of this, see Koster 1978, pp. 8–11.
8. I am avoiding the phrase "the language faculty." Chomsky sometimes seems to identify this notion with UG, as in the following passage:

> Suppose further that the operation of rules of grammar is in part determined by semantic properties of lexical items; ... These are by no means implausible ideas. If they are correct, the language faculty does not fix a grammar in isolation, even in principle. The theory of UG remains as a component of the theory of the mind, but as an abstraction. Note that this conclusion, if correct, does not imply that the language faculty does not exist as an autonomous component of mental structure. Rather, the position we are now considering postulates that this faculty does exist, with a physical realization yet to be discovered, and places it within the system of mental faculties in a fixed way. (Chomsky 1975, pp. 42–43)

   However in the paragraph following the one just quoted, Chomsky writes: "My own, quite tentative, belief is that there is an autonomous system of formal grammar, determined in principle by the language faculty and its *component* UG" (footnote supressed, emphasis added). My own inclination runs to a broad and nontechnical use of the phrase "language faculty" (broader even than that of "language domain"); however, in order to minimize confusion, I will not use the term. I think Chomsky is using "UG" here in a sense different from and narrower than the one articulated in the next paragraph of the present chapter. In the interests of clarity I should remark that, were the paragraph quoted in this footnote to be rewritten using the terminology as it is employed in the present chapter, I could make an empirical claim of it only by replacing "language faculty" and "UG" with "the language organ" (a phrase to be introduced momentarily). To slightly modify a plea Jerry Fodor made (in a different context) at the conference at which the papers in this volume were presented: I will be happy to give away the words if I can keep the distinctions. Readers may have alternative (and equally, if not more, reasonable) appelative proclivities, and I hope that the sympathetic among them will make the required glosses, something I will henceforth eschew.
9. I'd like to thank Ron van Zonneveld for bringing this book to my attention.
10. This paper is translated, somewhat awkwardly in spots, from Polish. My quotation is altered slightly to improve readability. The original reads: "It may be a reasonable conjecture the content of this paper to be connected ...," with the rest as above.
11. See also Papert's (1980) and Putnam's (1980) discussions in the same volume.
12. To say "ontogenetically" here would be to begin moving a bit closer (but not much closer, I think) to Piaget's view; see Papert 1980. To say "phylogenetically" is, it seems, to move closer to the work of Konrad Lorenz; see Bischof 1980.
13. This principle is from Chomsky 1982.