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CATEGORICAL GRAMMARS AS THEORIES OF LANGUAGE

0. GOALS

In recent years, there has been a growing interest in categorial grammar as a framework for formulating empirical theories about natural language. This conference bears witness to that revival of interest. How well does this framework fare when used in this way? And how well do particular theories in what we might call the family of categorial theories fare when they are put up against the test of natural language description and explanation? I say 'family' of theories, for there have been a number of different developments, all of which take off from the fundamental idea of a categorial grammar as it was first introduced by Ajdukiewicz and later modified and studied by Bar-Hillel, Curry, and Lambek. In this paper I would like to discuss these questions, considering a number of different hypotheses that have been put forward within the broad framework that we may call 'extended categorial grammar' and making a few comparisons with other theories. In my remarks, I will take as a general framework the program and set of assumptions that have been called 'extended Montague grammar' and in particular a slightly modified version of Montague's 'Universal Grammar' (UG: Paper 7 in Montague, 1974). From this point of view, the syntax of a language is looked at as a kind of algebra. Then, the empirical problem of categorial grammar can be seen as part of a general program that tries to answer these questions:

(A) What is the set of primitive and derived categories that we need to describe and explain natural languages in their syntax and semantics (and phonology, etc.)?

(B) What are the operations that we need to describe and explain natural languages (in the syntax, semantics, phonology, morphology, etc.)?

(C) What are the relations that we need in order to hook up with each other the various categories and operations mentioned or alluded to in (A) and (B)?

The brief outline just given provides us with a natural breakdown of
the kinds of questions I want to take up here: First, we want to ask about the categories of our grammars for natural languages; second, the operations that we allow; third, the relations between the categories and operations on various levels (syntactic, semantic, etc.). And as a second overarching kind of question we want to consider these questions from the classical points of view relating to descriptive and explanatory adequacy, as these terms have been used in the generative literature. We may put these last considerations as two further questions:

(D) Can our theories accommodate facts about natural languages that we know or think that we know?

(E) How well do our theories do in predicting facts about natural languages?

(By the way, I think we need a better term for the general framework in which I am working. 'Montague grammar' seems excessively particular, given the very fundamental changes that have been argued for both in the general outlines of the system, and in the particulars. The same goes for the term 'categorial grammar'. I have no good suggestions.)

There are two dimensions along which we can give a rough characterization of formal linguistic theories these days. By 'formal', I mean explicit, staying somewhat catholic about standards of explicitness. Being explicit is simply a matter of making sure that we can work out just what it is that a theory and the grammatical descriptions carried out within it claim or predict about the data of linguistics: judgments of various sorts by native speakers of the language, crosslinguistic variation, fit with experimental or other evidence about acquisition and processing, etc.

The two dimensions that I have in mind are these: (1) semantic accountability and (2) generativity.

Semantic Accountability

By 'semantic accountability' I mean only this: the acceptance as a reasonable goal to say something explicit about the interpretation or meaning of the abstract syntactic objects posited by your theory. The most prominent approaches to the goal of semantic accountability in the last decades have been model-theoretic in nature, and this is the general approach which I will assume here. But this is not the only possible approach: one may think of meanings as some sort of mental objects or concepts, or one may follow an axiomatic approach, and it may turn out in the end that all of these approaches have something to contribute to our understanding of language. Many linguists do not accept semantic accountability as one of their goals. I don’t see much point in arguing about this question. Ultimately, we just have to look at the fruitfulness of the results. I personally believe that the results of a decade and a half of work in the model-theoretic study of natural language semantics amply justify the expectation that continued pursuit of this program is worthwhile.

Generativity

By 'generativity' I mean only this: the goal of a grammatical description is to enumerate or define a set of structural descriptions of the expressions of a language. The acceptance of this goal was the primary turning point in the rise of the kind of linguistics that we associate especially with the work of Chomsky and his coworkers in the fifties.

Closely connected to the last contrast is a difference in style: what we might call the 'constitutional' approach and the 'architectural' approach (I intend 'constitutional' to be reminiscent of its political sense). In the former approach, description operates in a primary way by the statement of principles overlaid on some skeleton of a grammar (usually not explicitly given). The primary work goes into stating constraints or licenses. In the architectural approach, of which I consider various categorial theories to be prime examples, the attempt is made to build into the very structure of a grammar various properties from which will follow principles of the sort stipulated independently in the alternative sort of theory. A noncategorial example of this sort of approach is Kiparsky's model of Lexical Phonology, if I understand it correctly. I will bring up here a number of examples of situations where independently stipulated properties of grammars in other theories simply follow from the design of categorial systems of one sort or another.

Finally, in this initial discussion of the background of my remarks, let me mention a difference of research strategy, where, happily, there seems to me to be no substantive difference to argue about. Some people seem to like to work with systems that overgenerate and then try to figure out how to cut down on the superset of structures that their theories give them, while others like to work in the opposite direction,
being careful that their grammars just give what they are sure of. Arguments about these two general approaches often seem to degenerate into discussions of morality. Here, I would make a plea for tolerance. Hopefully, the two approaches will meet somewhere in the middle. More generally, I think it is good when linguists pursue lots of different theories. After all, the only way to argue for the NECESSITY of an empirical theory (strictly impossible) is to show that alternative theories fail, where the one in question succeeds. The reason that this is strictly impossible in an empirical discipline is that to argue for necessity we would have to rule out all possible alternatives. When we deal with the real world we can't do this, of course.

1. WHAT MAKES A GRAMMAR CATEGORICAL?

Let me paraphrase (simplifying somewhat) Montague's definition of a grammar (he calls it a 'language') in UG. We have the following:

A: a set of expressions, together with $\varphi: A \rightarrow \text{Pow}(C)$ which assigns each member of $A$ to a set of categories in $C$.

G: a family of sets of $n$-place operations, for $n = 1, 2, \ldots$

C: a set of syntactic categories,

R: a set of rules of the form:

$$\langle f; c_1, c_2, \ldots, c_k; c_{k+1} \rangle$$

where $f$ is a $k$-place operation, and $c_1, \ldots, c_{k+1}$ are syntactic categories

The meaning of such a rule is this: take expressions of category $c_1, c_2, \ldots c_k$, apply the operation $f$ to them and you will get an expression of category $c_{k+1}$.

If we look at a bidirectional categorial grammar of the familiar sort from the point of view of this general framework, we can see that it does two things: first, it specifies an infinite set of categories starting with a primitive set; second, it confines the operations to just two 2-place operations of concatenation. Moreover, in its fundamental way of dividing expressions into functors and arguments and implicit in the formulations of Bar-Hillel and Lambek, functor categories are distinguished according to the operations that are associated with them. So, for a start, we might set up the basic categorial definition as follows:

The following are examples of the usages:

GCAT is the smallest set such that:

1. $\{a_1, a_2, \ldots, a_n\} = \text{PCAT}$,
2. GCAT includes PCAT,
3. If $g$ is an $i$-place operation and $a_1, a_2, \ldots, a_i, b$ are in GCAT, then $\langle g; a_1, a_2, \ldots, a_i; b \rangle$ is in GCAT.

In a bidirectional sort of categorial grammar, the following choices are made:

in 3 we replace $i$ by 2;

$G$ includes just two operations:

LCON, RCON, where $\text{LCON}(x, y) = \text{RCON}(y, x) = x \cdot y$

These two operations both appeal to simple concatenation. The reason for distinguishing them is the hypothesis that functor expressions determine the syntactic operations that combine them with their arguments. This is what is implied by the more familiar notation where we write as follows:

$$a/b$$ for $\langle \text{RCON}, X, b, a \rangle$

$$b/a$$ for $\langle \text{LCON}, X, b, a \rangle$

(where $X$ stands for the functor expression in question). The usual rules for function/argument application may then be schematized as follows:

$$a/b \rightarrow a$$

$$b/a \rightarrow a$$

Here, the schemata mean this:

For the first, if $x$ is a member of the set of expressions categorized as $a/b$ and $y$ is a member of the class categorized as $b$ then $xy$ is a member of the set categorized as $a$; similarly for the second.

As I mentioned above, it has been well known for some time that the class of bidirectional categorial grammars is weakly equivalent to the class of context-free grammars. We will consider below some ways in which CG's are not like phrase-structure grammars but let us note for now that any arguments from weak generative capacity against CF grammars are, a fortiori, arguments against the weak adequacy of bidirectional categorial grammars, as used and interpreted in the way just outlined.

Let me point out right away that we encounter here one fundamental way in which various categorial theories differ: the treatment
of directionality. Recall that Ajdukiewicz’s original formalism made use of functor categories that could take arguments on either side (Ajdukiewicz, 1935). Two current theories retain the idea that functors are categorized in this order-free way at a basic level. Flynn (1983) reintroduces directionality for the functors by means of word-order conventions that capture generalizations for a language and across languages, thus offering an interesting set of parameters for Universal Grammar. His approach is quite compatible with the theory just sketched, which was introduced in Bach (1984) as a theoretical proposal in the spirit of Bar-Hillel (1953) and Lambek (1961). Ades and Steedman (1982) handle word-order facts by postulating a separate component of rules of combination for functional application and function composition (‘partial combination’) in either direction, and allow the possibility of restricting the operation of these rules to particular categories. Either of these approaches has to deal with the fact of exceptions: for example, there are languages that have both prepositions and postpositions (Dutch, German, English: notwithstanding, ago, etc.). In a theory like Flynn’s, I believe the most natural assumption would be that particular exceptional elements could be specified ad hoc, leaving the general conventions as the unmarked case. The approach of Ades and Steedman would have to incorporate something like exception features determining the applicability of the various rules of combination. In any case, there are interesting puzzles to face. Why, for example, do we have languages that have exceptional ‘adpositions’ but not, as far as I know, languages that have verbs that are exceptional in the position that their arguments must take? (More on word-order below, under OPERATIONS.)

Another important line of investigation has to do with the arity of our functors. The hypothesis of binarity (built into our definition above) is in no way necessary in a categorial approach. It seems to be a persistent idea in many different theories, however, that the canonical, if not the necessary form of linguistic constructions is binary. This was certainly Montague’s practice in PTQ and we see it in a Government-Binding guise in the recent work of Kayne (1984).

One crucial way in which categorial grammars differ from phrase-structure grammars is the fact that the former are built on the notion of function in the strict mathematical sense. Thus, we can carry over into our theories all the things that we know about the general theory of functions in a natural way, that is, without introducing any new concepts, extensions, or special devices. Many of the papers at this conference bear witness to this fundamental point, in particular, with regard to type-lifting, and function composition. Indeed, we seem to be faced here with a certain embarrassment of riches. What corresponds in a categorial system to transformational kinds of rules in other theories are the operations that compose functions and change categories, so it is quite expectable that we need to look precisely here for reasonable constraints in our search for explanatory adequacy.

Let me now take up each of the broad areas mentioned above: categories, operations, and relations among different ‘components’ of our theories.

2. CATEGORIES

There are two sorts of questions to ask here, when we consider the empirical import of our theories. The first has to do with the general structure of the categorial system, the second with the substantive content of the categories. Much recent work in syntax and morphology has been built upon X-bar theory. It is interesting to compare X-bar systems and their attendant concepts with categorial systems, and I shall do so in a moment, but let me first make a historical observation. The ultimate modern sources for the main ideas of X-bar theory are, I believe, the work of Harris in the fifties and an observation by John Lyons (1966; also 1968: pp. 330—332; see Bresnan, 1976, for one of the few explicit acknowledgements of this link), even though some of the ideas are closely related to traditional notions about exocentricity, endocentricity, and the like. Now, what is interesting to me is that Lyons referred explicitly to the categorial tradition and even suggested that it would make a better basis for a transformational system than the then universally used phrase-structure systems! Obviously, this idea was not picked up on (but see Lewis, 1972, and more recently Flynn, 1983).

The primary structural difference between a categorial system of syntactic classes and that of ‘some version’ of an X-bar system is easy to see. The recursive definitions of syntactic categories in the former projects an infinite set of possible syntactic classes, with a primary split between primitive categories and functor categories. X-bar systems take as primitive a small set of features, and, assuming a binary set of elements in the value space (or possibly ternary, if ‘unspecified’ is
admitted as a possible value), project a 'very finite' set of possible syntactic classes. (This statement is actually a little misleading. It is true that the number of 'major' syntactic classes such as \( N \), \( V \), and so on is very small but what we should compare to the categorial classes are really the conjunctions of the major class features and the subcategorization frames.) Now, lots of the categories that we get from the recursive definition are very nice categories, as we all know, allowing for example (should I say 'predicting'? the use of such things as generalized quantifiers in our theories. But a lot (infinitely many) of the projected categories seem quite useless if not perverse. For example, among the possible categories available to languages, and hence the little language-learner, are ones like these:

\[
(\mathcal{U}/(e\mathcal{U}/e) \quad \mathcal{U}/(\mathcal{U}/(\mathcal{U}/e)))
\]

The first corresponds semantically to a function that maps functions from individuals to functions from individuals to truth values (got it?). Moreover, could we not say that just by virtue of the finiteness property the \( X \)-bar system, being more restrictive, wins hands down here?

On the other side, an argument for the categorial approach, is that the categories have a clear content and a built-in semantic import. We have no idea what the import of such features as Noun or Verb (with + and - values) are (this is a little strong, see Jackendoff, 1977). In extended Montague grammar and categorial grammar we must commit ourselves on the semantic import of our syntactic categories. In other theories this is in a sense more a matter of choice than principle. One way in which we can view the categorial enterprise is as a contribution to an eventual substantive theory of syntactic and morphological categories.

There are two things to say about the problem of the overabundance of categories in categorial systems. The first is that we might appeal to some notion of markedness with respect to the projected categories (or some constraints on learning theory). In fact, this way of thinking has some attractions, as it might help us understand something about the crosslinguistic distribution of systems of 'parts of speech', perhaps also something about acquisition. Categorial systems have a built-in hierarchical structure that lends itself well to thinking about such matters. The second thing to remember is the difference in the roles played by the categories in a grammar, where, as noted already, systems of derived categories of various sorts, including especially composed categories, take over all or much of the work that is done in other theories by entirely separate sorts of rules. In any event, there are interesting empirical issues involved in these questions (see especially Dowty, this volume).

Let us now consider the actual substance of the categories themselves. It is apparent that an empirical theory must make some choice for the primitive categories on which the structure of the derived categories is built.

Montague followed Ajdukiewicz in taking the categories \( t \) and \( e \) as primitive (Ajdukiewicz: \( s \) and \( n \)), quite within the logical tradition, corresponding to the semantic notions of truth values and individuals as they appear in a standard model structure for a first-order language. Most of the initial work in Montague grammar followed him here. But in the last several years we have seen some basic modifications of this system. In particular, Gennaro Chierchia, in arguing for the inclusion of properties as primitive kinds of things in the model, has introduced a radically different kind of model structure which is almost type-free (see Chierchia, 1984). I am sure we will see a number of significant developments in this area in the coming years. I will not enter into a discussion of these possible modifications here, but rather couch my discussion within the more familiar scheme we inherit from Montague and Ajdukiewicz.

What kind of empirical claims are being made by a system of this sort? First of all, as always, we need to ask whether the categorial system is to be interpreted as part of Universal Grammar (in Chomsky's sense rather than Montague's), and if so, whether in a purely formal sense or in a substantive way as well. That is, is it simply claimed that each language will just have some list of primitive categories from which the functor categories will be constructed, or are specific categories stipulated as part of the innate human mechanism? It seems that we would want to shoot for the stronger claim and thus build the particular set of primitives into our universal theory. So in the case at hand, we would be claiming that \( t \) and \( e \) are the universally necessary primitive categories from which each language starts. In effect, then, our theory might be claiming that the syntax of every language contains a category for declarative sentences and for names. You will no doubt note that this is more than Montague claimed (if we make the counterfactual assumption that he was in fact making an empirical claim in his
analysis of English in his PTQ (i.e. Paper 8 in Montague, 1974)), for that paper contains no English syntactic category e. Questions about type-shifting become directly relevant here (Partee and Rooth, 1983). Notice that an essential part of testing such claims requires attention to the semantics as well as the syntax (Question C above) and this necessary connection must be treated as one of the most important empirical challenges to the theory and program as a whole.

There is no space here to enter into an extended discussion of this point (or of most of the other points that I will raise). Let me just give one illustration of the kind of question that we are facing here. It seems to me to be true that every language has at least the categories just mentioned, roughly, sentences and names. Further, a minimal syntax would need at least one further category in order to have sentences with any internal structure: te or et. Given type-lifting, we would then expect the category of generalized quantifiers (/{(t/e) etc.}, and it has been claimed (Barwise and Cooper, 1981) that this category is also universal. Although I feel in my bones that this is right, it is clearly incumbent on us to look very closely at languages such as Kwakw’ala in which all logical determiner meanings seem to be expressed by auxiliary-like verbal elements. Notice that from the point of view of the logic (and given Schoenfinkel/ Curry) we can understand a scheme like \[ DET(M) \langle N \rangle \] as exhibiting either one of two structures and in only one of them is there a semantic constituent corresponding to a generalized quantifier.

Before leaving this point, I would like to point to an entirely different kind of evidence that can bear on the question of the empirical support for our syntactic categorizations. Part of practically every syntactic theory is a system of features for dealing with matters of government, agreement, and percolation (i.e. the assignment of feature values to complex expressions as a function of the values of their parts). In Bach (1983b), I have shown one way in which such matters can be dealt with in extensions of categorial theories. Naturally, given the architectural approach of such theories, we would hope that the principles that guide such matters in the morphology/syntax interface would be derivable from independently motivated aspects of the theory, such as basic function/argument structure. The success of such subtheories should then be matched up with that of competing theories. In the case at hand, for example, if we compare the categorial theories to phrase-structure theories of various sorts, at the most general level we are dealing with the theoretical justification for such notions as head, modifier, specifier and the like in the latter type of theories and those of categorial grammar such as functor, argument, and directly derivable ones such as endocentric modifier (for interesting discussion of these points in the context of derivational morphology, see Hoeksema, 1984). I believe that results here are promising. They also throw some light on the question of the theoretical status of various sorts of type-lifting options. It would be a spectacular result, if in some language properties of government or agreement varied according to whether or not some type-lifting option occurred with an attendant flip of the function/argument structure. I’m pretty sure this never happens (so sure, that I would bet some money on it, say S50 — I may have just lost some money, see Moortgat, 1984, for an example that is claimed to illustrate just this point). I conclude that one would want to have a clear theoretical separation between something like a basic functional structure and those relationships that come through type-lifting (see Dowty, this volume, for discussion of this last question). An inspiration for much of the work on principles determining the way features work has been Keenan’s Functional Principle (Keenan, 1974; see Bach and Partee, 1980, for an application of the Functional Principle to problems of binding theory).

3. OPERATIONS

Let’s now look at some of the questions that arise about the possible operations that we want our theories to provide for natural language grammars. There are three main points that I want to take up here. The first is the question of one-place operations. The second has to do with the syntactic side of function compositions of various sorts. The third is the question of operations on categories.

One-Place Operations

From the point of view of Montague’s UG, it would be an artificial restriction to disallow one-place operations. Indeed, in the way in which I have laid out the skeleton of the theory above, we can put an upper bound on the arity of the operations, but not a lower bound. In classical categorial grammar, on the other hand, there is such a restriction. I believe that this is something of a historical accident which arises from the
point of view that we are given a set of expressions in a language and asked to assign pieces of the expressions to categories. This approach then leads naturally to the kind of ‘item-and-arrangement’ view of linguistic structure that has dominated much of American linguistics for the last four decades. As Schmerling (1983) has pointed out, Montague’s algebraic approach is much more consonant with the ‘item-and-process’ model of much earlier descriptive work (this terminology from Hockett, 1954). It is difficult to try to decide between these two approaches at such a general level, and for now, perhaps it must remain a matter of taste. It is obviously always possible, if we have no constraints on the number of different ‘inaudibilia’ we might posit, to set up an abstract zero morpheme to trigger off any conceivable one-place operation we might want to define. In the other direction, with no constraints on operations, any arbitrary element could be removed from the stock of ‘items’ and reintroduced by a suitable operation (compare Montague’s treatment of determiners in PTQ). I tentatively conclude that categorial and non-categorial theories are on about a par here, with much more research needed, especially on the interactions between morphology and syntax. (Note that Curry, 1961, includes one-place operations as one type of functor. See Bach, 1983c, for some arguments for one-place operations and see Hoeksema and Janda, this volume, for a discussion of the range of morphological operations actually found in natural languages.)

**Compositions in Syntax**

As I noted above, one of the most attractive features of categorial theories is the potential for composing functions that comes along with the functional basis of the theory. Indeed, the frequent appearance of fusions of categories in natural language can be taken as a kind of confirmation of the validity of the general approach (Bach, 1983a; Hoeksema, 1983). We know what the semantic constraints on compositions must be: given two functions \( f \) and \( g \), they can be composed into a new function \( h \) with domain \( A \) and range \( C \), just in case the domain of \( g \) is \( A \), the range of \( g \) and domain of \( f \) is some set \( B \), and the range of \( f \) is \( C \). But what about the syntactic side? Let us take the meaning of our operations of concatenation quite literally. A functor \( a/b \) takes expressions of category \( b \) to its right as its domain. It follows that ‘harmonic’ functors (that is, functors that are associated with the same concatenation operation) can be combined by composition. So we get free the syntactic compositions of the following two schemata:

\[
\begin{align*}
\text{a/b/c } \Rightarrow & \text{ a/c} \\
\text{c/b } \Rightarrow & \text{ c/a}
\end{align*}
\]

It also follows that only harmonic functors can be composed in a perfectly general way. The theory thus provides us with an interesting ‘architectural’ consequence. Following Ades and Steedman (1982), we can build on this possibility for a treatment of long distance dependencies: an indefinitely long composition of contiguous harmonizing functors will be available to act as arguments for special gap operators such as relative pronouns and particles and question words in languages that have these elements. For example, suppose English that is assigned to the category \((CN\backslash CN)/(S/\backslash NP)\), then it can take as arguments all of the following sorts of phrases:

- I saw
- I think that Bill saw
- I think that Bill said Sally saw

This treatment enforces the ‘adjacency corollary’ of Ades and Steedman but with the additional twist provided by the way in which operations are associated with functors rather than provided by independent combination rules. In Ades and Steedman, with no specifications of restrictions to particular categories on the rules of composition (forward and backward partial combination) any two properly related functors can be composed.

Again, this is not the place to enter into an extended discussion of the complicated issues touched on here. Rather I would just like to point out the way in which basic features of the theory can lead to interesting empirical consequences. Notice that there is a further consequence of the treatment of ‘gaps’ just illustrated. On the assumption that tensed verb-phrases in English are to be treated as members of the category \( NP\backslash S \), (internal) subject ‘gaps’ will have to be treated differently from all other gaps. More generally we will get various ‘left branch’ effects in English. Notice further that the treatment has built into it a kind of ‘empty category principle’ (Chomsky, 1982): gaps must be governed, that is, licensed by functor expressions. (It would be interesting to compare these results with approaches as those of Kayne (1983) and Koster (1984), both of which stipulate something
like our harmony principle, or rather its analogue in a phrase-structure system, as a constraint on long distance dependencies.)

**Operations on Categories**

So far, we have a very nicely constrained way of using syntactic compositions of functors. We might ask whether there are any ways in which we might milk the system for further sorts of operations to be derived from the basic set. In (Bach 1981), I suggested a way of looking at local 'reorderings' of constituents as the result of operations on complex categories. Suppose we have a complex category such as (a/b)/c. We can now consider the possibility that the same functor be reassigned to a category in which the order of the argument is reversed: (a/c)/b. As I show in the paper cited, this provides us with one way of looking at phenomena that have been treated under the heading of 'right-wrap' and the like. Another possible example, but of a slightly different sort, is offered by phenomena such as so-called 'subject-auxiliary inversion' in English (Bach, 1983a). In general, word order problems offer one of the most interesting areas for testing various extended categorial systems, and such systems offer an extremely interesting new perspective for looking at such problems. (Type-lifting operations fall under the general heading of this subsection. They are dealt with in detail in a number of other papers in this volume.)

**Long-Distance ‘Operations’**

A different approach to word-order variation is suggested in (Bach 1984). It is a generalization of the system of categories, in which categories are actually sequences of the original categories. Expressions are put together freely and the generalized categories are reduced by rules that make use of the notion of functors that look 'anywhere to the right', 'anywhere to the left', or 'anywhere'. We obtain in this way what I consider to be an interesting class of languages, namely the free permutations of arbitrary CF languages. Since the original note in which I discussed these languages has never been published, let me take this opportunity to present the result.

Let us call the class of grammars, scramble grammars. In the interesting case, where the languages of the grammars are finite we have the following:

PROPOSITION. The class of languages generated by scramble grammars is a proper subset of the CS languages.

Proof. Consider the language MIX = SCRAMBLE((abc)^+)(the names 'mix' and 'MIX' — pronounced 'little mix' and 'big mix' were the happy invention of Bill Marsh; 'little mix' is the scramble of (ab)^+). Intersect MIX with the regular language R = a^b^c*. The result is the non-CF language L = a^n b^n c^n. By the theorem that says that the intersection of a CFL and a regular language is CF we know that MIX cannot be CF. But there is no scramble grammar that will get exactly L. To see this, note that every scramble grammar of an infinite CFL has an infinite sublanguage which is CF. Clearly L has no such infinite sublanguage.

Q.E.D.

(geoff Pullum has pointed out to me that this result was obtained in the literature of computer science in the sixties: see Book, 1973, with reference to Sillars, 1968.) It is worth pointing out that we can get L if we add filters to the grammar. What this shows is that Partee's well-formedness constraint actually does have an effect on weak generative capacity.

Before leaving this section, I would like to point out a whole set of problems that have to do with the question of phrase structure. Obviously, phrase structure has an entirely different status within categorial theories than it has in traditional phrase-structure systems. It is sometimes assumed by critics that diagrams showing the way in which categorial resolutions can be carried out are to be interpreted in the same way as phrase-structure diagrams. In fact, they have much more the status of the analysis trees of classical Montague grammar as we see them in PTQ and much subsequent work. It seems to me to be a completely open question whether and how traditional phrase markers or tree diagrams are to play a role in categorial systems. It goes without saying, then, that all of the configurational notions, such as c-command and the like, used in Government and Binding theories and related approaches are up for grabs (see Bach and Partee, 1980, and Bach, 1983b, for some discussion).

4. RELATIONS AMONG 'COMPONENTS'

Probably the one most characteristic and essential feature of both Montague grammar and categorial grammar is the tight constraint on the relation between syntax and semantics. A priori this must count as

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THE LAMBEK CALCULUS

1. FLEXIBLE CATEGORIAL GRAMMAR

There is a noticeable revival of categorial grammar these days, as a vehicle for linguistic description. The systems used differ somewhat from the original calculus of Ajdukiewicz and Bar-Hillel, however. In particular, there is a component of rules for 'type change' of expressions, making for greater flexibility and elegance. One fundamental system of this kind is the so-called 'Lambek Calculus', whose type-change rules show a close analogy with the inference rules of constructive propositional logic. In this paper, we present one calculus of this kind, and survey its theoretical properties as a device in linguistic semantics. Our two main new contributions are a new and complete semantics for this calculus, as well as a modest study of its language-accepting capacity. In this way, we hope to provide a better understanding of the background theory of flexible categorial grammar, in tandem with its descriptive uses.

Before these results are presented, a look at the historical background will be useful. The main idea of Ajdukiewicz was to note the function-argument structure of expressions (following Frege), resulting in the following type-combination rules for adjacent expressions:

\[
\frac{B}{A} + A = B, \quad A + \frac{B}{A} = B.
\]

Here, the functional type \(\frac{B}{A}\) ('from A to B') is undirected, picking up arguments to either left or right. This allows for 'local permutations' in recognition. Later on, Bar-Hillel introduced directed variants, with the usual notations \(A \backslash B\) (left-searching), \(B/A\) (right-searching), creating additional descriptive power. Nevertheless, the well-known equivalence result for such categorial grammars with sets of context-free phrase-structure rules has led to a widespread belief in their inadequacy for linguistic purposes. (This belief has been challenged in recent years. For an up-to-date critical survey of the evidence, see Gazdar and Pullum, 1988 by D. Reidel Publishing Company.)
a point in its favor in comparison with many other theories. A theory with some constraint on the relationship — say, the homomorphism constraint of Montague's UG — is by definition making stronger claims about possible human languages than one with no constraints whatsoever. I have already touched on this point in the discussion of categories above. But the real empirical force of the theory must come by way of testing its explanatory power. We need to look for places where the theory provides a clear choice among several possible descriptions of some range of facts and then see how well it performs by asking for independent evidence for the correctness of its predictions as to descriptive adequacy. Given the flexibility offered by the possibilities of type-lifting and function composition it is fair to ask the question: isn't it always possible to come up with a compositional semantics for any arbitrary syntactic analysis? So, as always, we want to continue to look for constraints on the parts of the theory and their interconnections.

There has been a substantial amount of work on other parts of categorial theories, especially derivational morphology (see Moortgat, 1984, and especially Hoeksema, 1984, and the literature cited there, now also Hoeksema and Janda, this volume) and phonology (Wheeler, 1981), but relatively little on the interconnections among these subtheories (see Bach, 1983a, b, c for some suggestions about inflectional morphology). If we can take the relations between syntax and semantics as a guide, we would take a homomorphic relation to be the unmarked case, with apparent departures from it providing the most interesting challenges.

5. CONCLUSIONS

I believe that categorial grammar has come of age as a major and interesting approach to empirical linguistic work, again citing as evidence the papers at this conference and much other work presented and published elsewhere, primarily by participants here. Moreover, the influence of this work is beginning to be felt in other camps, especially among our closest cousins, those who are pursuing 'generalized phrase-structure' theories and related approaches (see especially Pollard, this volume, and Gazdar et al., 1985). We can expect the next few years to see much more work along these general lines. The distinguishing characteristic of this work and its most interesting feature is the centrality which it accords to the notion of functions and arguments, an idea that has formed an important core of thinking about language for more than a century.

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REFERENCES