

Nontransformational Syntax*

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Last year at the University of Wisconsin in Milwaukee there was a conference about the theory of syntax. Fourteen different theories of syntax were discussed at this conference. The aim of the conference was to see if there could be some measure of agreement about syntactic theory. I did not attend the conference but I am told that at the end of the conference there were still fourteen theories of syntax, maybe even more. I like to look behind disagreements about details and notice agreements that were sometimes apparent in the heat of discussions and battle. I don't know about Korean linguists but in America linguists love to argue, and it is very hard for many linguists to say, "Yes," when someone says, "Well, you really agree with me, don't you?" Then they usually say, "No." There is a story about Noam Chomsky, who, I think, is perhaps most argumentative of all. He was giving a lecture at a university in Canada, and someone in the audience got up and said, "Well, I agree with you because of this and this..." and Noam Chomsky immediately said, "No, you don't."

Anyway I would like to talk today about an area, I think, of agreement across a number of different linguistic theories, namely the idea that it is possible perhaps, given developments in syntactic theory since the early days of *Syntactic Structures* and the later days of *Aspects*, to think seriously about a serious syntax in which there are no transformations at all. Recently a British linguist, Gerald Gazdar, sent around a pair of papers to many people, one of which was called "Constituent Structures" and the other called "English as a Context-Free Language." That is just an example of one person who has tried to make a step. What I'd like to do today is the following. I'd like first to discuss three developments in the recent history of syntax and semantics which I think made it possible to consider nontransformational syntax in a serious way. The first of them is the development, more or less, within the Extended Standard Theory of Chomsky and his associates. I think here Joseph Emonds and John Bresnan are within that tradition.

The second thing that I'd like to say something about is the incorporation into linguistic theory of what I'd like to call serious semantics, more or less, along with the lines that Barbara Partee was discussing yesterday in perhaps Montague tradition but also other attempts to bring methods of formal semantics into linguistic theory.

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The third is the development that went almost unnoticed in the creation of the theory of *Aspects* of the serious syntax of the use of complex symbols and complex categories in syntax. When I say "almost unnoticed," I mean that it had been unnoticed until rather recently, and they will play a key role in the development of these nontransformational accounts of syntax of natural languages like English.

Then what I'd like to do is to explain to you by example just how some of these people proposed to account for the problems that were traditionally handled by transformational rules. Finally, I'd like to suggest that this sort of development makes it possible to think seriously about some alternative theories of syntax besides phrase structure syntax, namely the kind of categorial syntax, which was discussed early in the generative period of linguistics but then rather quickly dropped because it was shown that such grammars were equivalent to simple phrase structure grammars. But now that it is possible to think again about phrase structure grammars as providing all we need for the syntax, I think, it is interesting to look back at the categorial systems and perhaps solve some problems that remain in the recent attempts to provide simple phrase structure grammars with complex symbols for natural languages.

Before going on I'd like to make one thing clear. I believe that these recent advances would not have been possible at all without the development of transformational theory. That is—I want to repeat what I think Barbara Partee tried to make clear yesterday—I am not saying that the transformational grammar was wrong and has made no contribution. I am quite sure that the things that I am going to talk about today would have been impossible without the transformational developments of the decades from 1957 to 1977. There is a certain way, I think, in which we could look at the examples and ideas of some of the people who are proposing nontransformational analysis as being, in a sense, just a more restrictive theory of transformations. In other words, many of the insights of transformational analysis can be recast in this new form, and the thing that is important about it is that you can show that the different way of looking at these relationships is a very restrictive theory as compared to the full theory of transformations as defined by Chomsky. But in some sense, they are transformations redefined rather than something completely different.

The second thing I want to say before we launch into the rest of the talk is that generative grammar has always accepted—or generative grammarians have always accepted—the idea that if given two theories of natural language, one of which can be shown to be more powerful than the other and the second more restrictive, narrower than the first, you have to accept the more stringent but restrictive, weaker theory as long as there are no facts which show the inadequacy of it. In other words, presupposition is that if you need more power in a theory of language, you have to show that it is necessary, and conversely, if you can show that a more restricted theory will handle the facts, the data, and the problems of a more powerful theory, then you have to accept the stricter, less

powerful theory. Why? Because it projects a narrower class of human languages, a narrower class of grammars than the full theory. It has been known for a long time that transformational grammars as originally defined and redefined in *Aspects* are too powerful, and this is a mathematical result. Since that time, many linguists have been trying to construct more restrictive theories of transformational grammars.

The first part of the development I want to talk about goes back ultimately, I guess, to the work of Joseph Emonds, who I think was the first in the transformational tradition to make very clear a sharp contrast between two kinds of transformational rules. There were, on one hand, the local structure-preserving little transformations (let's call them.). By them I mean ones which work within a single clause, within a single sentence or perhaps from within two sentences, but strictly local in their effects. They are things like Equi, Passive, Dative Shift that is the transformation that was supposed to relate "I gave the book to John" and "I gave John the book." Equi would be a kind of rule of identical noun phrase deletion which would delete something but just one clause down strictly bounded in its application. Raising would be another example. In this theory, there were the clearly cyclic transformations that applied in a derivation from bottom up locally in such a way that whenever there was an unbounded relationship from something in a top sentence to something in a very deeply embedded sentence that involves only these rules that always happen by virtue of reapplication of small local things going on. For instance, Passive could apply and then it could be raising up into one sentence, and in next sentence Passive could again apply and then Raising would apply, so on, so that there were some connection between a subject here and a verb phrase way down there by virtue of small local movements. And these transformations were contrasted with the unbounded—let's call them—big transformations like Question Movement in English, Relative Clause Formation, Comparative Deletion and so on. Let me just say WH Movement for an example.

Now, there were two linguists within the Extended Standard Theory, a version of transformational grammar, and they are Joseph Emonds and John Bresnan.

Emonds, who tied the above fundamental distinction into the distinction between root and non-root transformations (I'm overlooking the details of Emonds' proposals here), was the first to say that many of the unbounded transformations were also structure-preserving. So you should be clear about the fact that when I say 'structure-preserving' I am not using it exactly the way Emonds did. It's obviously structure-preserving rules here that I am talking about. Emonds proposed new kinds of structures that would allow them to be structure-preserving, but if you just take a naive look at the actual sentences of English the results certainly don't seem to be structure-preserving in the sense of getting structures that are there in the base rules. So that there is just a warning about structure-preserving.

Bresnan and Brame among others noticed that for many of these local structure-

-preserving small transformations, it was possible to handle them in a quite different way given the developments of the lexicon and the development of subcategorization features and so on in the theory of *Aspects*. Let's take Dative Shift as an example. Dative Shift as a transformation was roughly of the form as follows:

- (1) NP-Dative-NP₁-to-NP₂ ⇒
 (2) NP-Dative-NP₂-NP₁

The sentence (1) would be changed into the sentence (2) in which the noun phrases switch their positions. I mean I am not claiming that it was accountable to see about which should be taken as basic and which was derived and so on, but just looking at this particular rule what Bresnan especially noticed was that what we have here really is a picture of the subcategorization frame for a certain verb, say *give* when we use it in a sentence like "I gave the book to John." Here we have another subcategorization frame for verbs like *owe* in "I owe him three dollars" or *envy* in "I envy him his good looks" and so on. One could think of these operations here as really not transformations in a literal sense, but rules for relating different lexical categories as expressed in the categorization features. So instead of having a transformation rule that would literally take a structure and change it into another structure, we could think simply in terms of way of relating two different categories of verbs and saying that verbs like *give* which perhaps would be categorized to occur in this sort of frame could also appear by some kind of lexical derivational rule in this kind of frame, and we could generate both structures directly without using a transformation. This stage within the Extended Standard Theory is the stage of paper by John Bresnan which was published in 1978 called "Towards Realistic Theory of Transformational Grammar." Bresnan proposed a theory of syntax in which all of the rules of this sort, all of these small little local rules, will be handled lexically in effect by making use of certain categorial information and then later certain information about the grammatical functions expressed by different verbs. And then the only transformations that would remain would be those of the big type that is the large unbounded transformations like WH Movement, Question Movement or Comparative Deletion, and so on.

The second development since *Aspects*—I am talking about the period roughly from 1965 to the present—was the adoption with the addition to the linguistic theory of formal semantics and the methods that have been developed by logicians and philosophers. I am not going to talk very much about this today but I want to just mention that in many cases where a transformation in the early days was the only device available to express some relation of importance between two kinds of sentences, the addition of a complex and powerful system of interpretation such as that used by Richard Montague received the attention of a linguistic public. A number of linguists and philosophers including Barbara Partee and Richard Thomason in many cases make it possible, because there are new

devices for relating the meanings of sentences, to express directly in semantics what had to be expressed by transformational terms in the earlier theory. The followings will be examples.

(3) John Pres try John to win

(4) John tries to win.

In the earlier theory of transformational grammar, one of the small rules called *Equi* must be used in order to account for the fact that in sentence (4) 'to win' has to be interpreted as somehow connected with subject 'John'. As I mentioned a minute ago, *Equi* noun phrase deletion derives sentence (4) from underlying structure (3) by deleting *John* under identity in "*John* tries for *John* to win." The relationship was expressed by transformational means. Given a theory of the meanings of sentences which makes use of devices like lambda abstraction, it is possible if we wish—I am not advocating this analysis, but it is possible if we wish—to express what was expressed by the transformational analysis directly in the interpretation of phrase like 'try to win.' That is, we can say that 'try to win' is property of being such that 'one tries for oneself to win' in effect, and that can be just stated directly as an interpretation of it. Again I want to say that I am not advocating this as a necessary way of handling it. It is not the only way but if one desires to imitate the effect of the Noun Phrase Deletion Rule, one can do it more directly and better in a sense by use of the semantics.

The third development, as I said before, was the adoption of complex categories in the syntax in the phrase structure part of syntax and it was introduced for rather different reasons in the theory of *Aspects*. It was adopted primarily to solve problems of subcategorization: that is, just as in phonology we want to be able to talk independently about different features associated with different segments. In syntax it is necessary to talk about different properties of syntactic items independently, and this was first done only at the level of lexical entry. The lexical entry has in *Aspects* complex symbols with lots of features and things like them, but later it was introduced into higher parts of the syntax, the phrase structure part of the syntax, for instance, in the development of X-bar theory of the last years since 1970 actually when Chomsky first proposed something like the current X-bar theory of MIT linguists.

A credit, I think, for the kind of use that I am going to illustrate today ought to be given to Gilbert Harman who a long time ago wrote a paper "Generative Grammar without Transformations" (*Language*, 1963) in defense of phrase structure. And there was a very quick reply to it by Chomsky, in which Chomsky argued against Harman. (Chomsky's reply appeared in 1966 I think, but was presented publicly earlier than that.) Let me first say what Harman proposed and then just make a brief comment on Chomsky's reply to it because it seems as if something was unnoticed at the time. Harman was trying to develop a system that is called phrase structure grammar, in which you will be

able to talk independently about a syntactic category, and then a certain subcategory that it might belong to. I can list some examples of the kind of distinction that Harman was trying to say as follows:

(5) S/Decl, S/Int S/X→A/X B/X (Harman, 1963)

Let's talk about the sentences in general, but let's talk about certain subclasses of sentences by adopting a notation in which we have an S to mean *major category sentence*, and then slash and then Declarative or Interrogative, for example. And let's adopt the system in which we are able to carry along the second part of each of the symbols in the phrase structure rule. So there might be a rule of the form S/X→A/X and B/X, in which we carry along the subcategories like Declarative, Interrogative, and so on, in the phrase structure derivation. Harman tried to show the above by actually writing a grammar for one of the Chomsky's fragments. How a system of this sort could be used to account for many of the things was accounted for transformationally without ever having any transformations as such; for example, the kind of relationship between "John is here." and "Is John here?" could be handled in such a way that there would be a set of rules that would be same both for the Declarative and Interrogative sentences, but then at some point the order would be changed but they would still carry along all the information we need to express the interrelationships between the categories that are same for Declarative and Interrogative sentences.

Now Chomsky's reply to Harman was to say, "...Well, what you have done is defined as a new kind of grammar which is not a phrase structure grammar, and the claim that English cannot be described by the phrase structure grammar still stands because obviously if you change the definition of phrase structure grammar and then show this new kind of grammar can do without transformations, you haven't said anything about phrase structure grammars because you are talking about a new theory of grammar." And in a somewhat corrosive kind of way, Chomsky said, for example, we could show that "Baboons can talk" in this way by redefining what baboons are, and then added the differences we have a fairly clear idea about what the baboons are but we have no very clear idea at all about what all Harman's grammars are like. That was, more or less, the end of that idea in that part of the development of the transformational theory.

Now what, I think, neither Chomsky nor Harman realized at that time (perhaps Chomsky did) is a little unclear when you read what he said about Harman whether he actually realized this or not, but what has become apparent, and this was a discovery or a realization that was made recently about grammars using complex symbols of this sort. It is that one can show that a grammar using only finitely many different categories for rules of this sort can always be replaced by a completely context-free grammar with many, many rules. That the abbreviations of this sort can be thought of as an abbreviatory device available for constructing grammars but one which doesn't change the power

of the theory at all because there will always be finite number of context free rules as long as we introduce more and more new categories which will generate the same language as before. So, look at the following.

(6) $A \rightarrow B \dots C$ (Gazdar, Saenz/Ross)

This is a schema for a kind of rule that has been proposed by various linguists including Gerald Gazdar, Saenz, and others at the University of Massachusetts. Suppose we take a phrase structure rule $A \rightarrow B$ and some number of symbols up to C , and we abbreviate by means of feature symbols various values like F_1 down to F_n , G_1 down to G_n , and so on down the line. We can think of this as a schema which would abbreviate the large number of rules that we could replace it by, in which we simply multiply the number of categories that are in the grammar. For instance, if we had a rule $A \rightarrow BC$ and certain features like gender, e.g. 1, 2, 3, or number, we can write as follows:

(7)	A	\longrightarrow	B	C	
	α gender		α gender	α gender	(α =masculine, feminine, neuter)
	β number		β number	β number	

If we compare this now to a context-free grammar for the same language, this schema could be replaced by a finite number of context-free rules that we get by just making new categories. So we could have a category A of masculine things of type $A^{M+Sing} \rightarrow B^{M+Sing} C^{M+Sing}$. This is now being considered as unanalyzed simple category of a context-free grammar, and another new category A choosing a different value for α like F , and maybe the same value for number. But if we keep doing this, we can replace the schema by a finite set, rather large, of context-free rules which will generate exactly the same language as this original schema did. That is the idea that Gazdar and others had.

If a credit should be given to anyone, I think probably one should mention Peters and Ritchie, who were the first trying to prove formal results about transformational grammars of the theory of *Aspects*. They ask themselves, "...well, does the use of complex lexical elements make any difference?", and they said, "No, we can safely ignore that because there are only finite number of complexes of finitely many features, and we won't change the power of grammar at all. We could simply replace each of those complexes by single symbol and we won't change the power of grammar at all." All that has happened here is that the same idea has been carried up into the higher parts of the syntax and the observation made that work also for complex symbols of categories, not just lexical elements.

I want to show how, in a system of this sort, one can handle two sorts of situations; one is the kind of agreement phenomenon or government phenomenon and the other as to how one can use complex symbols of this sort to express unbounded dependencies. Look at, for example, sentences as follows:

must be the same as the number of the first element to the right, that is the verb, and now we got everything set up so that we can choose a correct person and number form of a finite verb to go with what subject was. On one hand we can state directly dependencies over here and then by bringing in this sort of mechanism we can shuttle down or shuttle up the other way information about specific things that must carry the feature just mentioned.

The next thing then is to think about the problem presented by long distance, unbounded dependencies of the sort that were still handled by transformations in papers of Bresnan that were referred to before. There is no way in which we can show how far away the element of the gap, the missing element, is going to be from things—that is, to be associated with them—by unbounded movement or deletion rule. For example, consider the following sentence and we could add more things to it:

(13) Who did you think that Mary said that Bill thought...Sally saw?

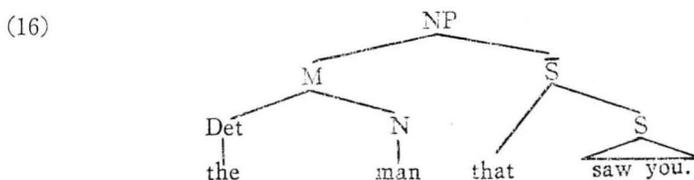
In sentence (13), we can finally get down to 'Sally saw' and we have to interpret this in such a way that *who* is the interpreter as the direct object of the verb *saw*. There is no way of characterizing this in a bounded way because there could be as many sentences as we want between them. And the facts that are accounted for by the transformational analysis are illustrated in (14) and (15).

(14) *Who did you think that Mary was seen by Bill?

(15) *Who did you think that Mary saw Bill?

That is, if we don't have a gap, then the sentences are ungrammatical. That cannot be interpreted because we have *who* which wants to find some place to be situated in a structure below it but everything is filled up. The trick device that one can use to express this kind of dependencies is very interesting because the fact that it works seems to be a result of the fact that there are certain constraints on kinds of deletion sites, that are islands in a language like English, and if there are no such islands then it would probably be impossible to do the kind of things that we are going to do here.

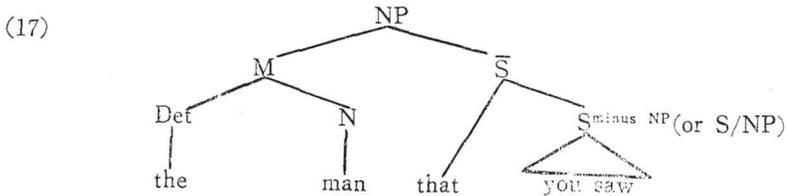
Let me introduce the idea in the following way. Suppose we have a noun phrase given the sort of analysis as follows:



An analysis of this sort says \bar{S} , then S, and then we have the big sentence. The problem is to figure out how, in a system of this sort, we can ensure that what we have here is going to have exactly one noun phrase missing somewhere. That is what the transforma-

tional analysis says. It says, "Take this form and delete under identity or move a relative pronoun up to the front (or one of those ways of doing it,)" (It doesn't matter which way you think about what the transformational analysis says.) In effect, a noun phrase with a relative clause in it is a structure that consists of a noun phrase followed by a sentence that has exactly one noun phrase missing somewhere we don't know.

Now we can use the kind of idea that ultimately goes back to Harman. I am now so giving basically the way Gerald Gazdar talks about to take a set of context-free rules and to create new derived rules that will get us just what we want in this relative clause. Let's ask ourselves here for a minute; What is the language of this kind of S that occurs over relative clause sentences? Well, it is a language that is exactly like English in this case except that every place where there could have been one place within the sentence, the set of the sentences, whether it could have been a noun phrase or had to be a noun phrase in ordinary English, we have to have one hole, one missing noun phrase. So let's do the following construction.



(17) is a language of relative clause of English in effect, a language which has all the structures of English sentences but somewhere there is a missing noun phrase. Let's call the new language S/NP or, if you like, "S minus NP."

Let's say we start with the context-free grammar for ordinary English, that is, English except for relative clauses. We introduce a new category for every category that we had before. So before we have S, now we have S/NP, before we have VP, now we have a new category VP/NP, and so on. Before we have PP, now we have PP/NP, and so on. This notation now means the set of things that are just like English sentences except their missing one noun phrase. This means the set of things that are just like English verb phrases except their missing exactly one noun phrase. This means the set of things that are just like English prepositional phrases except that they have no noun phrase and have just a preposition. And now we introduce for these new symbols following kinds of rules. For every rule we had before, which mentions one noun phrase, we introduce several new rules for a new category, S/NP, which is a new derived rule in this language of relative clauses.

These new rules that we introduce are of two sorts. One is what we can get by making a rule just like the above except we leave one noun phrase out, so it would be VP. The other is what we would get by leaving noun phrase and introducing the new category VP/NP. This means that we take care of things like "the man that saw you." It

would come from the first sort of the rules, and things like "the man you saw" would come from the second sort of the rule finding a verb phrase that has a missing noun phrase in it. Similarly, for all cases for which we had before $VP \rightarrow V NP PP$, we introduce VP/NP and then a series of rules, for example, $V PP$, for we left our just one noun phrase, $V/NP NP PP$, and $V NP PP/NP$. All these rules can be briefly shown as follows:

- (18) $S/NP \rightarrow VP$
 $S/NP \rightarrow VP/NP$
 $VP/NP \rightarrow V PP$ (for $VP \rightarrow V NP PP$)
 $VP/NP \rightarrow V NP PP/NP$ (for $VP \rightarrow V NP PP$)

These rules give us the constructions, for example, "the box that you put in the closet." where missing is the object, "the advantage which you took of Mary," "the box that you put the money into," where *into* is a prepositional phrase missing a noun phrase, and so on. Now notice that, because we are looking always for exactly one noun phrase and we do not carry this construction out again on these new categories we have added just a finite number of new context-free rules to the grammar, and the grammar still remains essentially a context-free grammar.

Before going to the last part of my talk today, let me just point out a couple of interesting things, some of which I already alluded to. Notice the way I did it, and this is not exactly Gazdar's way of doing it. I just excluded the noun phrase at each point which Gazdar included, and this difference now becomes really an empirical question. The construction that I have made says that a grammar of a natural language will always obey a certain kind of constraint that says that you can't ever have a XP whatever the XP is, removed from an XP of that sort. George Horn, several years ago, proposed for English what he called "Noun Phrase Constraint" that says that noun phrases can't be removed from noun phrases in English. The problem about what you have to do in reanalyzing that is construction, and so I am not concerned with the correctness of that or another constraint. I simply mean to show that one can handle certain kinds of island constraints rather directly. This constraint that I stated here says that you cannot ever remove a noun phrase by failing to include this category for the very category we were looking for already.

We have been introduced to the constraint in the system that says what is related to the A-over-A Constraint and also related to those things such as Horn's Noun Phrase Constraint, and probably it is too strong. But I just want to illustrate the fact that constraints on what were transformational rules cannot have the analogues in this sort of system in things of this sort where we can constrain the kinds of new categories we get.

Notice another example which shows that some of the constraints which have been proposed on transformations automatically fall out from the construction we have done so far. Relative clauses are islands in English. You can't question out of relative clause or

make relative out of relative clause. This comes out automatically from the construction which I have stated. Why? Because we don't take the new derived rules to make new categories like (S/NP)/NP. We always go back to the original set of basic rules to carry out the construction, and in order to get a question out of a relative clause we would have to introduce (18) which would allow us to look inside something that had a hole already for another hole. And it is apparent that something like the Island Constraints that transformational grammarians have been investigating with a great deal of detail and with very interesting results over years since Ross's Thesis 1967, plays a very essential role in the ability to construct an essentially context-free grammar for natural languages.

The last section of my talk today is going to be a suggestion that given results of the sort it might be interesting to look back upon the slightly different kind of essentially phrase structure sort of grammar. To think about problems of syntactic relationships, let me start this part by making an observation. The observation is the general scheme that I have given you for rules of the sort with various features down as follows:

$$(19) \quad A \rightarrow B \quad C$$

$$\quad \left[\quad \right] \quad \left[\quad \right] \quad \left[\quad \right]$$

What is still unsatisfactory is that we can express all kinds of dependencies that we probably would not want to express in a system of this sort; that is, there is no principle which tells us that a verb phrase ought agree with its subject or something like it. In other words, there is no principle that tells us what the categories and feature combinations we should be looking for when we look at natural languages. I mean to point out that (19) is just an arbitrary schema for a type of grammar and can be used to express things that probably should be ruled out from the theory of what natural languages are like in general. For example, we could perfectly well write within a grammar of this sort a series of rules that would ensure the determiner of the subject of a sentence agree in number and gender with the indirect object of a verb. Now I think we don't want to have grammars like this since we can always pass information down to whatever arbitrary piece of the next constituent. So we can always ensure that dependencies between different parts of sentence are there, and we could write a series of rules that would describe a language in which a determiner associated with subject agrees in gender and number with the noun of the indirect object, and that is the result that I think we want to exclude. So there are still within the system a lot of arbitrariness in which we want to seek out some general principles that will tell us from principles of universal grammar what sorts of things are allowable in the kind of feature dependencies that we expect to find in natural languages. I mean probably I would bet money on the claim that there is no language that has such particular dependencies. (It scares me to make the statement like this. Languages are always surprising me.) Actually I tried to think of an example of that sort. In Korean the plural marker sometimes tacks on the particle or things like that. It

is a very interesting kind of problems for the sort of consideration here.

Let me now just briefly and very speculatively say something about categorial grammar. I think it would be interesting to think about categorial grammar in this connection. Categorial grammar is associated with especially the names of a Polish logician, Ajdukiewicz; a real philosopher and linguist, Bar-Hillel; a mathematician linguist, Lambek; Lewis; John Lyons in a paper "Towards the Notional Theory of Parts of Speech"; more recently work by Montague himself and some of the followers of Montague, and recent works of Keenan and Faltz. All these people and some others have taken a view of language of something like categorial grammar rather than thinking of, for example, a noun phrase as consisting as phrase structure grammar of the sort of unanalyzed constituents.

In a categorial grammar, everything is thought of either as an argument or as a function or a functor. And syntactic categories are analyzed in terms of functions or functors which take certain kind of arguments to make certain other kinds of constituents. So the analogue to this picture of a noun phrase in English in categorial grammar would be something like (20) below.



Noun for our purpose here is not yet analyzed into function in argument but a determiner is something which takes a NOM on its right to make a noun phrase, and if we assign *the* and *man* respectively to the categories then by putting these two together the application of this function to this argument will give us the value of the things and we can get up here noun phrase. So the difference between the phrase structural way of looking at things and categorial sort of way of looking at is that we make some choice about these two things as to which is function and which the argument, and think of syntactic categories as being functions looking for arguments or, at most basic level, argument types of one kind or another. I give you, for example, the kind of categorial system used by Montague and his PTQ as follows:

- (21)
- i. e, t are in Cat .
 - ii. If a, b are in Cat , then a/b and b/a are in Cat .
 - iii. If $x \in P_{a/b}$ and $y \in P_b$, then $xy \in P_a$.
 - iv. If $x \in P_{b \setminus a}$ and $y \in P_b$, then $yx \in P_a$.

This starts off by saying that there are two basic categories, e and t , and this is actually an extension of Montague. For (ii), if a and b are in the category then both a/b and b/a are in the category. So I am using the idea that slash means something about direction, the side that you are looking for the argument on. Then we have two general rules,

which are shown above as (iii) and (iv), for a categorial grammar of this sort. These rules say that if x is in the category a/b and y in the category b , then xy is in the category a ; and similarly for the opposite way.

Keenan, in a number of papers and most recently in a fairly long monograph, has asked questions: How do we know in general how to analyze a certain construction? What makes us think certain things are functions rather than arguments? Why do we think that, for example, *man* is a basic category and it is at least the argument for something like determiner rather than thinking that maybe that *man* is a function looking for a determiner to make a noun phrase? Keenan suggested what he called "Functional Principle" and it is paraphrased below.

- (22) The Functional Principle: The form (and meaning) of a function depends on the form (and meaning) of its argument.

This says that there is a non-accidental relationship between function-argument relationship and dependency both in form and meaning of one element and another. Namely, the form and meaning of a function depends on the form and meaning of its argument. I listed examples under (23) below to show you how these categories work out for English, and this is again following Montague.

- (23) Some Categories of English:

Det: the, a, that, this,...NP/CNP

CNP: man, unicorn, woman who lives next door,...t/e

NP: the man, John he,...t/(t/e)

IVP: walks, is here, killed a fish,...t/NP

ADJ: big, red, intelligent,...CNP/CNP

The determiners like *the*, *a*, *that*, and *this* are assigned to the category that takes the common noun phrase what I called NOM up before and makes a noun phrase. To the extent English has any kind of formal differences reflected in its very rudimentary morphological system now, examples of *that*, *this*, *those* and *these* show that indeed the form of the function is determined by the form of the argument; that is, we get the form *this* if the argument common noun phrase is singular, and we get *these* if it is plural, and similarly for *that* and *those*.

Common noun phrase is a sign, the category t/e , i.e. a function from entities to truth values, a kind of predicate. The category e corresponds to the individuals in the domain of the interpretation, t corresponds to the two truth values $(0, 1)$, t/e corresponds to the set of functions from individuals to truth-values, and so on. NP is a sign, the kind of category, that I have already listed a set of their properties. Intransitive verb phrase is taken to be a function from noun phrase meanings to truth values. And adjective is taken to be something that takes common noun phrase (to make common noun phrase).

Now let's think for seconds about what we know about different languages. Com-

monly in natural languages, if the language has significant morphological differences and significant morphosyntactic categories like gender, number and so on, and if the morphology of the language has distinctive morphology for these kinds of the things, the form of adjective—let's read (23) from the bottom up—, typically an attributive adjective which is taken as a function here, will have the form determined by its argument. Similarly, typically in languages that have such distinctions, the form of intransitive verb phrase will be determined by the form of subject. This is one of the Keenan's arguments saying that the relationship between verb phrase and subject should be function-verb phrase and argument-subject rather than the other way around.

And typically—I haven't put this here, but—in languages that have object agreement against the form of the object noun phrase that determines the agreement on the transitive verb, there is such an agreement and so on down the line.

The last two things I want to say very briefly and again very speculatively seem to me that one can think in categorial terms about some ways of predicting which sorts of feature arrangements we should find in the analogues of context-free grammars of this sort. Namely, let's think about what it means to have government in a language, and let's think about what it means to have agreement.

First of all, to treat languages with case systems we can't get by just with this rudimentary sort of categories that used to talk English. That is, for German, when we talk about verb government, what that really means is that verbs have to be syntactically categorized according to the kind of arguments that take in terms of case systems. That is, some verbs take accusative, and so on. So I am suggesting that if we are going to seriously use categorial sorts of grammars for lots of different natural languages, we want to generalize our notion of just this positional relationship to a kind of categorial systems which will allow to say that a certain verb is categorized to look for something in a certain case, and that is my attempt to reconstruct the notion of government within the categorial system. Now notice that what is said in the context-free type rules of the sort which we were looking at before is that government is going to be a kind of situation where, if the language has the case systems, the verb is going to have a certain feature. It doesn't say that the verb has the feature of dative or whatsoever, but it says that the verb is looking for dative, accusative, or something like that. The function of government is given below.

(24) Government: A/B_c

$A \rightarrow A/B_c + B_c$ (e.g. German *mit*: PP/NP_{dat})

For agreement, what we want to say is that when we have functions of the sort given in (25) below, where an A is gotten by concatenating a function looking for B, agreement features can be predicted for rules for that sort according to the Keenan's Functional Principle.

- (25) Agreement: A/B + B
 A → A/B B
 [α F] [α F]

The idea that I am trying to put forward here is that if we think categorically about the development of non-transformational syntax about the problems of agreement, government, and so on, it seems like that we might be able to get somewhat closer to a good prediction, within the wider way of possible system that could be constructed in such grammars, somewhat closer to what we want to say a true expective systems available for natural languages, and that in some sense, it could be a universal grammar that would supply the feature arrangement. And all we have to say about certain languages are what the morphological categories are, what the constructions will be like, and then we could predict what some of the feature combinations would be like. I think I must stop at this point. Thank you very much.

Hong-Bae Lee: (Recapitulations in Korean.)

In-Seok Yang: Linguistics can't be a fashion. To make transformations survive, there are two factors to be considered: one is synonymy hypothesis, and the other transformational derivation in order to derive the correct surface structure from the deep structure. In the case of synonymy hypothesis, it seems that we can make transformation survive by incorporating the idea that we may allow that transformation may, to a certain extent, change meaning. There must be some devices to that effect. However, in the case of transformation derivation, there is no hope.

In his 1977 "Linguistic Gestalts" paper, Lakoff frankly confessed generative semantics succeeded on one hand *informally*, that means, by providing examples and argumentations. On the other hand, generative semantics failed *formally*, which means, in terms of PS rules and transformational derivations. It seems to me that generative semantics succeeded in terms of explanation and observation on one hand, but failed in terms of description and formalization on the other hand.

The very thing which deserves our real efforts in linguistic research seems to be to discover or capture the insightful observational generalities and, second, explanation-oriented principles. These two factors will preserve validity in the historical perspectives.

Let me here cite Sapir to the effect that all grammars leak and that there will be no complete grammar at all. One of our efforts is to formalize the least leaking grammar.

I hope your model of grammar will be one of the least leaking grammar.

Dong-Whee Yang: First, many of your arguments involve the so-called trading relation between syntax and semantics. In eliminating transformations from categorial grammar, you have to turn over many paths which had been done by transformations into semantics.

The second point is about the problem of psychological reality. Prof. Partee and you are talking over and over again that your theory is better even in psychological reality because children can learn the grammar better in your theory. That is, you mean

that your theory is more similar, more faithful to the children's internal grammar. But actually psychological reality problem should not be judged by such intuitive terms. You have to give a very explicit or falsifiable hypothesis on the psychological reality problem.

You are eliminating transformations completely. But as I said, there are some inherently syntactic phenomena such as scrambling or particle floating in Korean. As you mentioned, the plural particle in Korean is just floating around. That kind of phenomena seems to me purely syntactic. I think, for that kind of typically syntactic phenomena, you need some transformations.

(All the ensuing discussion has been omitted because of the unsatisfactory recording.)

Suk-Jin Chang: (Recapitulation and closing remarks.)