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On Syntactic Categories

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On syntactic categories

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A traditional concern of grammarians has been the question of whether the members of given pairs of expressions belong to the same or different syntactic categories. Consider the following example sentences.

(*a*) I think *Fido destroyed the kennel.*
(*b*) The kennel, I think *Fido destroyed.*

Are the two underlined expressions members of the same syntactic category or not? The generative grammarians of the last quarter century have, almost without exception, taken the answer to be affirmative. In the present paper I explore the implications of taking the answer to be negative. The changes consequent upon this negative answer turn out to be very far-reaching: (i) it becomes as simple to state rules for constructions of the general type exemplified in (*b*) as it is for the canonical NP VP construction in (*a*); (ii) we immediately derive an explanation for a range of coordination facts that have remained quite mysterious since they were discovered by J. R. Ross some 15 years ago; (iii) our grammars can entirely dispense with the class of rules known as transformations; (iv) our grammars can be shown to be formally equivalent to what are known as the context-free phrase structure grammars; (v) this latter consequence has the effect of making potentially relevant to natural language grammars a whole literature of mathematical results on the parsability and learnability of context-free phrase structure grammars.

In this paper I shall begin by attending to what may look like a very narrow technical syntactic issue. However, I shall show that by choosing to resolve that issue one way rather than another, we obtain explanations for classes of linguistic facts that until now have been unexplained or inadequately explained. Furthermore, we effect a considerable simplification of the theory of grammar – in particular we remove a major motivation for the class of rules known as transformations, and for the two-levels-of-structure view that goes with them. We end up with a single level theory of generative grammar, a theory that suggests various avenues of research with respect to both syntax acquisition and syntactic processing.†

1. Categorial identity

A traditional and continuing concern of grammarians has been the nature of the set of syntactic categories or 'parts of speech' employed in a grammar, and the manner in which that set of categories divides up the constituent expressions of a language. Here the crucial issue for given pairs of constituents is whether or not they belong to the same syntactic category.

† I am grateful to J. Lyons, F.B.A., to Julie Rutkowska for useful comments on the first draft of this paper, and to L. J. Cohen for helping me improve on one of the examples. The type of grammar presented informally below is developed in considerable technical detail in Gazdar (1981*a*, *b*). This research was supported by grant HR 5767 from the S.S.R.C.

For example, we might ask whether the two constituents shown in (1) and (2) belong to the same syntactic category or not.

(1) Sandy's rabbit.
(2) The cat that Kim feeds.

The most familiar heuristic for getting an answer to such a question involves comparing the distributions of the two constituents by slotting them into a range of particular syntactic positions and seeing if the resultant larger expressions are grammatical. If it is never true that the occurrence of one leads to grammaticality but the other does not, then we have reasonable grounds, *prima facie*, for assuming that they do indeed belong to the same category. This kind of distributional analysis reached its zenith in the work of the American structuralists in the 1940s and 1950s, notably Harris (1951) and Fries (1952). The modern generative grammarian no longer treats distributional analysis as an end in itself, but sameness of distribution nevertheless remains an important criterion of categorial identity in contemporary syntactic argumentation.

Thus, it is currently uncontroversial to assume that (1) and (2) are constituents that belong to the same syntactic category, despite the fact that, superficially, they would appear to have little in common. If the assumption were to be challenged, then one could point to distributional facts such as those charted in (3).

(3)	*a.*	____ chased Fido.	[1,2]
	b.	Fido chased ____.	[1,2]
	c.	Fido gave a bone to ____.	[1,2]
	d.	The best behaved animal is ____.	[1,2]
	e.	Kim showed Fido ____.	[1,2]
	f.	Kim chased Fido ____.	[*1,*2]
	g.	Kim ____ Fido into the garden.	[*1,*2]
	h.	Fido is very ____ to please.	[*1,*2]

Both (1) and (2) fit happily into the slots in (3*a*)–(3*e*), but, as the asterisks indicate, neither will fit into the slots in (3*f*)–(3*h*). The category that (1) and (2) belong to is, of course, that known as noun phrase or 'NP'.

Now I shall turn to a question that linguists have not considered, much less addressed, in recent years. The question is whether the two constituents shown in (4) and (5) are instances of the same syntactic category.

(4) Felix destroyed the kennel.
(5) Felix destroyed.

Notice that the string of words *Felix destroyed* as it occurs in (4) is not a syntactic constituent at all and so, according to assumptions commonly made by generative grammarians, it does not make sense to ask to what category that particular occurrence of *Felix destroyed* belongs. We are thus concerned only with the category of *Felix destroyed* in sentences in which it does form a constituent, for example as in (6) and (7).

(6) That kennel, I think Felix destroyed.
(7) Felix destroyed, and Sandy repaired, the kennel in the orchard.

Let us look at the distributional facts to see if they suggest and answer to this question.

(8)	*a.*	Sandy rebuilt the hutch and ____.	[4,*5]
	b.	If ____ then Fido will be homeless.	[4,*5]
	c.	The fact that ____ distresses Fido.	[4,*5]
	d.	Fido suspects that ____.	[4,*5]
	e.	Sandy rebuilt the hutch which ____.	[*4,5]
	f.	Sandy rebuilt more kennels than ____.	[*4,5]
	g.	Whichever kennels ____ will be rebuilt.	[*4,5]
	h.	Kim wondered which kennel ____.	[*4,5]

What we find is that (4) will fit into (8*a*)–(8*d*) quite happily, but leads to ungrammaticality if inserted into (8*e*)–(8*h*). The constituent in (5), on the other hand, will not fit into (8*a*)–(8*d*), but will produce well-formed sentences if inserted into any of (8*e*)–(8*h*). We may choose to infer from this data that the constituents in (4) and (5) are indeed members of distinct syntactic categories. Such a conclusion runs counter to an assumption made, more or less explicitly, by generative grammarians over the last two decades, to the effect that both (4) and (5) are instances of the same syntactic category, the category of sentence or 'S'. This assumption is part and parcel of a syntactic framework that arrives at constitents like (5) by deriving them from constituents like (4). The operations that achieve this derivation are not permitted to change syntactic categories.

Nevertheless, the assumption is of relatively recent origin. Z. Harris, for example, writing in 1962, maintained that constituents such as those in (4) and (5) did not belong to the same syntactic category (see Harris (1962), especially pages 11 and 37). For him, as for everyone since, (4) was of category S, but, unlike his successors, he maintained that (5) belonged to a different category of sentence-with-a-noun-phrase-missing. It is the ramifications of this idea of Harris's that I shall be exploring in the following sections of this paper.

2. Derived categories

Before we can proceed further, I need to introduce some of the notation and formalism of generative grammar. In (9) are given the abbreviatory names of the basic syntactic categories employed.

(9) *a.* V, VP, P, PP, N, NP, Det, S.
b. verb, verb phrase, preposition, prepositional phrase, noun, noun phrase, determiner, sentence.

In (10) and (11) the format of what are known as phrase structure rules is shown.

(10) *a.* S → NP VP
b. 'A sentence can consist of a noun phrase followed by a verb phrase.'

(11) *a.* N → Det N
b. 'A noun phrase can consist of a determiner followed by a noun.'

Here, (10*b*) and (11*b*) gloss in words what is expressed formally by the rules in (10*a*) and (11*a*).

For our present enterprise, we need also a notation for 'an A with a B missing', and I shall adopt the notation 'A/B' for this purpose. S/NP, then, represents the category of being a sentence that has a noun phrase missing. I shall refer to these A/B things as 'derived' or 'slash' categories. The set of derived categories may be defined as in (12).

(12) The set of derived categories is {A/B: where A and B are basic categories}.

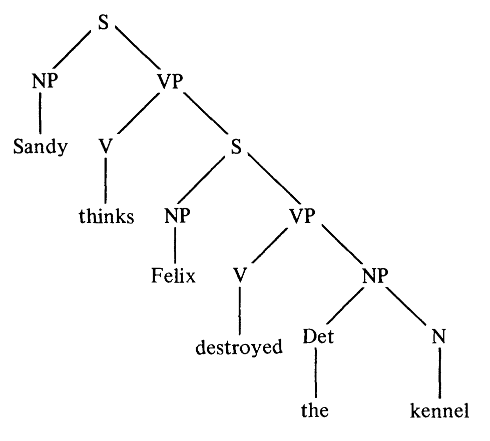
Actually, we probably want to restrict this definition a little to exclude useless categories like V/NP and NP/N, but that is a detail that I propose to ignore in the present context.

Clearly, our interpretation of these derived categories is crucially dependent on the interpretation of the basic categories. The interpretation of the latter is wholly determined by the rules that tell us what constituents of such and such a basic category can consist of, namely phrase structure rules like those shown in (10) and (11). For the sake of exemplification, here is a subset of the basic phrase structure rules that would be needed for a very modest fragment of English.

(13) *a.* S → NP VP
b. VP → V NP
c. VP → V VP
d. VP → V S
e. VP → V NP PP
f. PP → P NP
g. NP → Det N

These rules, taken together with rules for introducing lexical items – and the nature of those rules need not concern us here – will, for example, attribute the hierarchical structure shown in (14) to the sentence *Sandy thinks Felix destroyed the kennel.*

(14)



As can be seen, (14) contains the constituent shown in (4) and assigns it to the category S. However, the rules in (13), as they stand, will not provide us with structural analyses for sentences containing the constituent shown in (5), such as the examples in (15).

(15) *a.* That kennel, Sandy thinks Felix destroyed.
b. Sandy wondered which kennels Felix destroyed.

These sentences contain a constituent of category S/NP and we have, as yet, no rules that make mention of such derived categories. Fortunately, it turns out to be extremely straightforward to characterize the relevant set of rules. Consider S/NP, for example. An S consists of an NP followed by VP. So an S/NP must consist either of an NP-missing-an-NP followed by an ordinary VP, or of an ordinary NP followed by a VP-missing-an-NP. Those are the only two possibilities. Thus, corresponding to the basic rule in (16), we must have the two derived rules shown in (17).

(16) S → NP VP
(17) *a.* S/NP → NP/NP VP
b. S/NP → NP VP/NP

A single definition will provide us with all the derived rules that our grammar will need.

(18) The set of derived rules with respect to a set of basic rules R and a category B is {A/B → ...C/B...: where A → ...C... is a rule in R, and C is a category that can dominate B, given R}.

The requirement that C be able to dominate B in (18) serves to exclude useless rules like VP/NP → V/NP NP. For the sake of illustration, suppose we apply (18) to the set of basic rules in (13) with respect to the category NP. This will give us the set of rules in (19).

(19) *a.* S/NP → NP/NP VP
S/NP → NP VP/NP
b. VP/NP → V NP/NP
c. VP/NP → V VP/NP
d. VP/NP → V S/NP
e. VP/NP → V NP/NP PP
VP/NP → V NP PP/NP
f. PP/NP → P NP/NP

Our grammar does not need to list or separately specify such derived rules since everything about them is predictable from (18) taken together with a set of basic rules.

In addition to the derived rules, we also need linking rules to introduce and eliminate derived categories. The most general linking rule, that for elimination, can be expressed in the following schema:

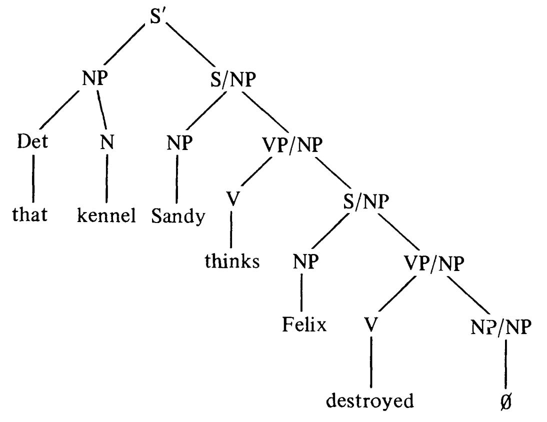
(20) *a.* A/A → $\emptyset$
b. For any category A, an A/A can consist of nothing (i.e. the empty string).

The linking rules that introduce derived categories are more diverse and the details of the rules vary with the construction involved. I shall considerably oversimplify matters here and pretend that there is just one such introductory linking rule, that shown in (21).

(21) S′ → NP S/NP

This rule now allows us to provide an analysis for (15*a*) as shown in (22)

(22)



From this illustration, it should be clear that the derived rules have the effect of carrying information about the hole down the tree, and of ensuring that there is at least and at most one hole in constituents of the A/B type.

3. Linguistic results

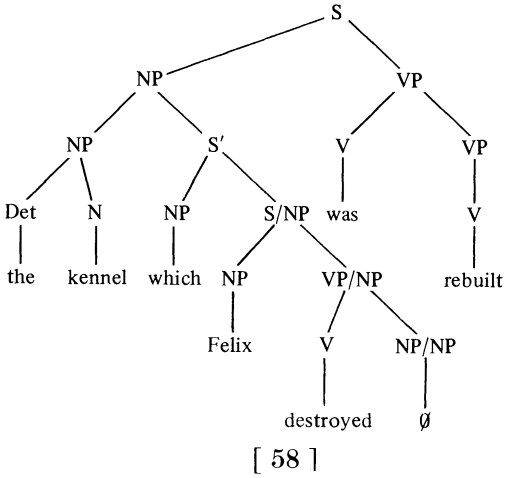
We have provided a formal reconstruction of Harris's notion of categories for constituents that have things missing. An immediate consequence of this reconstruction is that we now have a categorial explanation for the pattern of distributional facts noted in (8). The slots in (8*a*)–(8*d*) require a constituent of category S and will not permit the category S/NP, whereas the slots in (8*e*)–(8*h*) require a constituent of category S/NP and will not permit the category S.

The grammatical apparatus sketched above makes it as simple to state rules for constructions of the kind exemplified in (8*e*)–(8*h*) as it is to state rules for sentences with canonical structure, such as that shown in (14), for example. We have already seen how the linking rule in (21) provides us with an analysis for sentences containing topicalized constituents such as *that kennel* in (15*a*). And rules along the lines of (23) and (24) will provide for relative clauses and embedded questions, respectively.

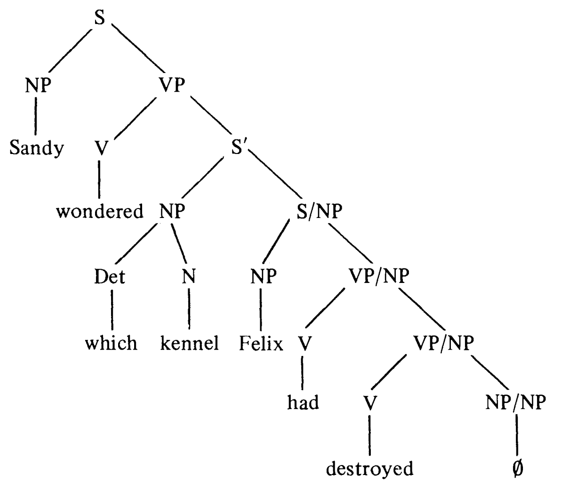
(23) NP → NP S′
(24) VP → V S′

The structures that these two rules contribute to are illustrated in (25) and (26).

(25)



(26)



Let us turn our attention now to a class of facts discovered by J. R. Ross some 15 years ago, facts that have remained more or less mysterious ever since. Ross (1967) noted examples like the (27) and (28).

(27) *a.* *The kennel which Sandy built the hutch and Felix destroyed was rebuilt.
b. *The kennel which Sandy built and Felix destroyed the hutch was rebuilt.
c. The kennel which Sandy built and Felix destroyed was rebuilt.

(28) *a.* *Kim wondered which kennel Sandy built the hutch and Felix destroyed.
b. *Kim wondered which kennel (Sandy built and Felix destroyed the hutch).
c. Kim wondered which kennel Sandy built and Felix destroyed.

Ross was assuming analyses of relative clauses and questions that involved moving constituents containing *wh* words out of canonical NP positions, so leaving a gap. The data in (27) and (28) are puzzling since it seems to indicate that this movement derivation must be constrained by a stipulation that forbids movement out of coordinate constructions unless every conjunct has the same thing moved out of it. This stipulation is known as the coordinate structure constraint.

Permissibility of coordination has traditionally been taken as evidence for sameness of syntactic category, and impermissibility as evidence of a categorial distinction. Thus the grammaticality of (29) might be used to defend the claim of categorial identity made in respect of the constituents in (1) and (2).

(29) Sandy's rabbit and the cat that Kim feeds chased Fido.

And the ungrammaticality of (30) might be used to argue that *Sandy's cat* and *frightened by mice* are constituents belonging to different syntactic categories:

(30) *Felix became Sandy's cat and frightened by mice.

Familiar facts of this kind have led many linguists to propose that the properties of coordination follow from a phrase structure rule schema, a simplified version of which is shown in (31).

(31) *a.* A → A *and* A

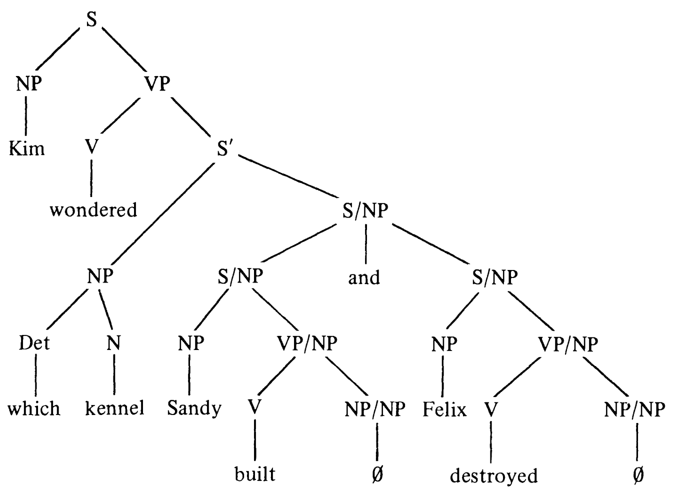
b. For any category A, an A can consist of an A, followed by *and*, followed by an A.

This has the effect of requiring categorial identity among conjuncts, thus permitting (29) [= NP *and* NP] but not permitting (30) [= NP *and* AP] ('AP abbreviates 'adjective phrase').

However, the observations just made have not been seen to be of any relevance to the Ross facts noted in (27) and (28). This is because of the assumption noted in the first section of this paper, that constituents such as *Sandy built the kennel* and *Felix destroyed* were of the same syntactic category, namely S, and thus that the categorial identity of conjuncts required by (31) was met. Given this assumption, Ross's observations become a puzzle in search of an explanation.

But we are not making the assumption. On the contrary, we are taking such constituents to belong to the distinct syntactic categories S and S/NP. And since these categories are distinct, schema (31) will not permit us to conjoin them. Thus the ungrammaticality of (27*a*), 27*b*), (28*a*) and (28*b*) just follows from our rules without the need for any additional constraints. And the grammaticality of (27*c*) and (28*c*) follows similarly: *Sandy built* is an S/NP and *Fido destroyed* is as well, so (31) will provide for their coordination. Example (28*c*) will have assigned to it the structure shown in (32).

(32)



Another puzzle involving coordination concerns sentences such as that shown in (33).

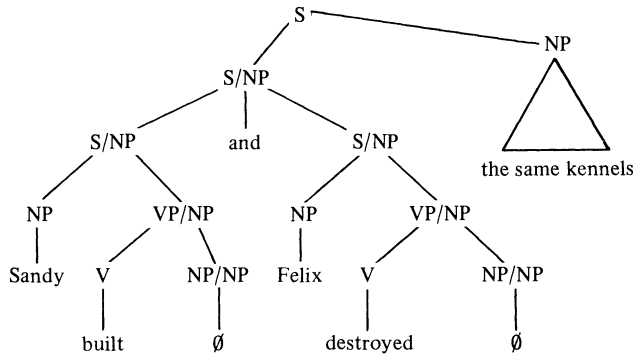
(33) Sandy built, and Felix destroyed, the same kennels.

Most linguists have assumed that sentences of this form derive via a movement rule known as 'Right Node Raising'. Unfortunately, this rule amounts to little more than a name for the problem created by sentences like (33), since as Jackendoff (1977, p. 33) has recently pointed out, there are 'no remotely coherent formalizations of Right Node Raising'. Suppose that we add the following rule to our grammar.

(34) S → S/NP NP

This rule, taken together with the coordination schema in (31), immediately provides us with an analysis, as shown in (35), for examples like (33).

(35)



If we generalize rule (34) to range over categories other than NP, then we end up with a schema that provides an entirely coherent formalization of the grammatical facts that would have been handled by a rule of Right Node Raising, had the latter ever been defined.

4. Theoretical implications

As recently as 3 years ago, Joan Bresnan claimed that unbounded syntactic dependencies, such as those involved in relative clauses, *wh* questions and the topicalization construction, could not be adequately described by phrase structure rules (Bresnan 1978, p. 38). That claim, when she made it, was hardly controversial. However, lack of controversy is no guarantee of truth, and we can now see that her claim was false.

Although the sort of grammar that I have described employs both complex symbols like 'S/NP' and metagrammatical definitions of sets of rules, as in (18), it is nevertheless provably equivalent in all relevant respects to the sort of grammar known to computer scientists and mathematical linguists as context-free phrase structure grammar. This class of grammars, and the class of languages that they can generate, namely the context-free or type 2 languages, constitute perhaps the most researched and best understood area of formal language theory (see Hopcroft & Ullman (1979) and references therein).

At present, it seems to me to be both empirically plausible and methodologically interesting to suppose that the natural languages constitute a propert subset of the type 2 languages, and that the grammars appropriate to their description are formally equivalent to some proper subset of the context-free phrase structure grammars. In concluding, let me draw attention to two instances of the methodological interest of the supposition as it affects one's research strategy.

The first concerns syntax acquisition. This topic has been enormously discussed in the last 15 or 16 years, but that discussion has either been at an extremely high level of abstraction and generality, or else has been of an essentially descriptive nature. What has been almost, though not entirely (see Anderson 1977; Power & Longuet-Higgins 1978; Wolff 1980), lacking are detailed, mathematically precise theories about the syntax acquisition process. However, in the formal language theory literature, extensive work has been done on what is known there as grammatical inference (see Fu & Booth 1975; Levine 1979). Much of this work has been concerned with inferring phrase structure grammars for type 2 languages from information about finite samples of the language. Now, it would be foolhardy to pretend that this literature contains a ready-made theory of the acquisition of the syntaxes of natural languages. However, it would also be foolhardy to simply dismiss the relevance of this research on the basis of an assumption that natural languages are not type 2, and do not have phrase structure grammars.

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On the contrary, it seems to me that if we want to develop a formal theory of the acquisition of natural language syntax, then this literature is the place that we should start (cf. Pinker 1979).

My second and final point concerns language processing. The sentences of a natural language can be parsed. We do it all the time. Furthermore, we do it very fast (see Marslen-Wilson 1973) for relevant psycholinguistic evidence). The ready parsability of natural languages is a property that has been almost entirely ignored by linguists until very recently (see Fodor 1978). And yet it is a property that stands in need of explanation: there are many mathematically conceivable classes of languages that can be shown to be unparsable for all practical purposes. However, if natural languages are type 2 languages with phrase structure grammars, then we have the beginnings of an explanation. Extensive work has been done by computational linguists on characterizing parsing algorithms for various classes of phrase structure grammars (see Aho & Ullman 1972). Algorithms exist, as do complexity results holding for the languages accepted by the algorithms. For example, many type 2 languages, including ambiguous ones, are provably parsable in linear time (Earley 1970, p. 99). Again, it seems to me that if we want to develop a formal theory to explain natural language parsability, then this literature is the place that we should start.

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Discussion

H. S. Thompson (*Department of Artificial Intelligence, Edinburgh, U.K.*). I should like to pursue the claim that the approach presented by Dr Gazdar is a 'notational variant' of Professor Chomsky's own approach. This claim raises two issues, a particular and a general. The particular point is how Professor Chomsky's claim can be understood in this instance. My problem with this claim is as follows.

1. The power of a theory involving transformations is of a different order than one involving only context-free phrase structure rules.

2. So I suppose that it is impossible to assert any direct equivalence of any significance between the one and the other in the general case.

3. Thus I can only interpret Professor Chomsky's claim as relating to context-free phrase structure grammar on the one hand and a strongly constrained particular transformational grammar on the other.

4. In which case it seems to me the burden is on Professor Chomsky to show that the constraints are in fact sufficient to effect the reduction in formal power which I take it is necessary to allow a well founded comparison.

5. And in any case, surely Professor Chomsky is stipulating in his theory certain things (principles and constraints) that are not subject to choice in Dr Gazdar's theory, but rather are necessary consequences therein of the vehicle he has chosen, namely context-free structure grammars with complex category symbols.

This brings me to the more general point, which is about the status of claims of notational equivalence in general. The term has been often used by way of rebutting counter-proposals over the years, but its status has never been clear. What does it take to establish notational equivalence, and what follows from it?

It is illustrative to consider in this regard the case of *X*-bar theory. The claim that this is but a notational variant of the old standard S VP NP V N... notation has been rightly resisted. One obvious ground for denying any equivalence, despite the fact that there is clear overlap, and that many people go on using the old symbols while declaring their allegiance to the *X*-bar approach, is that the *X*-bar theory *embodies in its notation* constraints that must be stipulated in theories employing the ordinary notation. In general it seems to me that by and large different notations in fact embody different constraints, and to the extent that the equation of explanatory power with restricted expository power is upheld, then one cannot equate two notations, one of which embodies constraints and the other does not.

N. Chomsky. Consider first Dr Thompson's points (1)–(4). As noted in my paper and in earlier publications, the theory of transformational grammar (t.g.) that I have been investigating for about the past 10 years permits only a finite number of grammars in principle; the same may be true of alternative theories that merit serious consideration. A core grammar is determined by settling values for finitely many finite-valued parameters; we may think of the grammar as a sequence of such values. Nothing changes in principle when we consider marked periphery, or lexicon. Therefore, there is no question of proving that these theories can be *reduced* in power (i.e., weak generative capacity) to the theory of context-free phrase structure grammar (c.f.g.), which permits an infinite number of grammars. No doubt it is possible to add constraints to the theory of c.f.g. that will restrict it too to a finite class of grammars, in which case the meaninglessness of the question of generative capacity will be fully evident. The theory of c.f.g. is vastly too powerful to take seriously, in that there surely are infinitely many context-free grammars that are not possible gramars for human languages. When we turn attention to the substantive question of determining the conditions that characterize possible human languages, the question of generative power disappears.

Furthermore, even when we consider theories that permit an infinite class of grammars (say, the version of t.g. studied by Peters & Ritchie (1973)), the question of weak generative capacity ('power', in Thompson's sense) is virtually without empirical significance, for reasons discussed in Chomsky (1965, pp. 60f.), and repeatedly elsewhere. This is an example of the misunderstanding of mathematical results that I mentioned in my paper. There, I gave the example of parsing results that show (if anything) that there should be a preference for 'short grammars'. Furthermore, more powerful theories permit shorter grammars for given languages; e.g. there are finite languages that require very long finite state grammars (essentially, lists) though they may be described with quite short context-sensitive grammars. Combining these results, we conclude that universal grammar should be extremely powerful, thus permitting short grammars for given languages, facilitating parsing. I do not, incidentally, intend this conclusion (which is precisely contrary to Thompson's assumption) to be taken seriously; it is no more serious than most of what appears in the literature with regard to the empirical significance of results in mathematical linguistics.

I will put aside Thompson's point (5) for the moment, and turn to his 'general point'. The term 'notational equivalence' has a precise meaning, of course, but in more informal discussion (as in this context) it has commonly been used more loosely with reference to theories that share the same basic ideas and principles but use different formal devices to express them. To illustrate, consider the sentence (d1), represented in its surface structure (S-structure) form in the theory that I briefly outlined.

(d1) I wonder who$_i$ [$_S$ John$_j$ seems [$_S$ t$_j$ to like t$_i$]]
('I wonder who John seems to like')

The notation expresses the fact that the interpretation is as in (d2), where *John* is assigned the semantic role of subject of 'like *x*' and *seem* is predicated of 'John likes *x*'.

(d2) I wonder for which person *x*, [$_S$ seems [$_S$ John to like *x*]]

Now consider the theories T_1 and T_2. T_1 generates the S-structure (d1) in the following way: generate the deep structure (D-structure), represented schematically as (d3), which is then mapped into the S-structure (d1) by the rule *R* (= Move-α), where NP* is an empty category and β is an abstract representation of *who*.

(d3) I wonder [$_S$ NP* seems [John to like β]]

The structure (d3) is a pure representation of the semantically relevant grammatical functions, in the obvious sense that is made clear in the literature. T_2, in contrast, first generates (d4), which is mapped into (d1) by a rule *R′*, which happens to have exactly the properties of *R*.

(d4) I wonder who [$_S$ John seems [$_S$ t to like t]]

In both (d1) and (d4), the trace *t* stands for a category NP with no phonetic content. T_1 and T_2 share other rules, for example the rules of construal that relate overt anaphors such as *each other* to their antecedents. These rules crucially have quite different properties from those of *R*, *R′*. The properties of *R* and *R′* hold for a certain class of constructions, namely, precisely those called 'movement constructions' in t.g., for example (d5).

(d5) *a.* [$_{NP}$ a man t] was here [$_S$ who you met] (extraposition)
b. who did you meet t (*wh*-movement)
c. John seems [$_S$ t to like Bill] (NP-movement)

In (d5*a*), the extraposed clause is understood in the position of its trace; in (d5*b*), the *wh*-phrase is a quasi-quantifier binding a variable in the position of its trace; in (d5*c*), *John* is assigned the semantic role associated with the position occupied by its trace. Both T_1 and T_2 express directly the important generalization that the processes of (d5) have a distinctive set of properties, different from those of other constructions.

At this level of discussion, theories T_1 and T_2 express the same ideas in a different form. In this sense, we may loosely describe T_1 and T_2 as 'notational variants'. They may differ in empirical consequences, but to determine whether this is a real question, or if so, what the answer is, is far from a trivial matter.

One might assume that T_1 and T_2 differ in empirical consequences in that T_1 blocks the generation of such S-structures as (d6) while T_2 permits (d6) to be generated by the rules that form (d4).

(d6) I wonder who [$_S$ John seems [$_S$ t to like Bill]]

Examples (d4) and (d6) are generated in the same way in T_2, with different choices for NP; but T_1 does not permit (d6) to be generated at all. Clearly, (d6) is ungrammatical, but failure to bar (d6) by base-rules (e.g. by a c.f.g. generating (d4) but not (d6)) is no defect of T_2 because (d6) is marked ungrammatical by another property shared by both systems, in fact, every system of grammar: namely, it yields no well-formed LF-representation (no representation of its logical form), no semantic interpretation, because it contains a quantifier binding no variable, a general property of LF in natural language. Therefore, it is unnecessary to use syntactic devices to exclude (d6).

The fact that T_1 and T_2 are virtually equivalent has been noted since the earliest work on trace theory some 10 years ago, and has frequently been discussed in the literature (see, for example, Chomsky 1977, pp. 206–207). To the extent that they are equivalent (which may be complete), it is possible, if one wishes, to describe TG as a 'one-level' theory, with base-generation of S-structures, the transformational rule (or rules) being understood as a distinctive part of the mapping to LF-representation. The crucial substantive question is whether the rule (or rules) $R = R' =$ Move-α has distinctive formal properties; the facts seem to me to show that it does (see Chomsky (1981) and earlier publications). It is an intriguing, but much more difficult, question to determine whether T_1 and T_2 are different theories in any significant sense.

We could describe the shared content of T_1 and T_2 in various other ways. For example, consider T_3, which generates (d1) directly along with the ungrammatical structure with the indices on the *t*'s reversed, then using the rule R'' to check which of the two structures is grammatical; or T_4, which is like T_3 except that it also uses the rule R'' to map S-structure to D-structure, independently of the mapping to LF-representation, thus in effect factoring S-structure into its two components: D-structure and R'', where D-structure is a representation of semantically relevant grammatical functions. Since the properties of R'' are just those of R, R', we again have another version of t.g.

Note further that we can virtually dispense with the whole complex system of context-free phrase structure rules for base-generation in all of these theories of t.g., relying on the projection principle and the general principles of X-bar theory (see Jackendoff 1977), which are not part of any particular grammar.

The significant questions in linguistic theory relate to the properties of general principles such as the projection principle, or to the clustering of properties associated with various rules (e.g. the rule $R =$ Move-$\alpha = R' = R''$, as distinct from other rules associating overt anaphors or non-trace gaps with antecedents), and the principles that determine this clustering. In contrast, the choice of one or another format to express these basic principles is a secondary issue, if it is an issue at all.

In my paper, I pointed out that in the 1950s there had been a shift of focus from language to grammar, and that in the past few years there has been a further shift from rule systems to systems of principles, at least in the work that I am discussing. The first shift relates to Thompson's 'particular point'; it renders the question of 'power' (i.e. weak generative capacity) more or less irrelevant, a fact that is obscured by a historical accident, namely that the question of power was raised (primarily with regard to finite state grammars, where the issue has some empirical significance) at the same time that the shift of focus took place that marginalized this issue, sometimes in the very same work. The second shift of focus relates to Thompson's 'general point'; more and more, the substantive questions appear to be those relating to systems of principles such as those briefly mentioned in my paper, the question of the format for rule systems declining in significance accordingly.

Now consider the thory proposed by Gazdar to which Thompson refers. This is simply a needlessly complex variant of T_2, the latter supplemented by a class of superfluous devices (derived categories, additional subcategorization rules of the base for derived categories, devices to carry through a derivation the information that some category must be phonetically unrealized, etc.), all designed to ensure that such sentences as (d6) cannot be generated. But as noted, there is no need for any such device. Therefore, we can replace this theory by the far simpler variant T_2, in which case it becomes (in the loose sense) a notational variant of t.g. This of course assumes that Gazdar's theory is supplemented with constraints to exclude the vast class of c.f.gs that cannot serve as possible grammars for human languages, presumably reducing it to finite generative capacity, so that Thompson's points (1)–(4) are not pertinent.

There is, however, an important sense in which Gazdar's theory differs from the several variants of t.g. mentioned above. It rejects the generalization expressed in R, R', R'' (the variants of the transformational rule Move-α), and uses quite different rules to deal with (d5*c*) on the one hand, and (d5*b*) (and perhaps (d5*a*)) on the other. Here, there appears to be a substantive difference between Gazdar's theory and the several variants of t.g., in that the latter capture a general property of constructions such as (d5) that is unexpressed in Gazdar's theory. I think that this is an important advantage of t.g., but that is a separate matter. The relevant point here is that apart from its failure to capture the generalization underlying (d5) – namely the fact that these constructions exhibit the cluster of properties associated with R, R', R'' – Gazdar's theory is simply a needlessly complex variant of T_2. Further differences would arise if we were to move beyond this level of discussion and pursue additional properties of the systems, but this is not the place to consider these questions.

Now let us turn to the second of Thompson's 'particular points', namely, his point (5). Thompson asserts that I am stipulating principles and constraints that are necessary consequences of Gazdar's theory. Since I am unaware of any such examples, I cannot comment on this claim. To my knowledge, it is untrue. Perhaps Thompson has in mind the example that Gazdar discussed in his paper, namely the phenomenon of parallelism in certain constructions. In fact, the phenomenon can be partly described in the notation of derived categories, but this does not constitute an explanation since the devices used have no independent justification, but are simply superfluous additions to T_2. Therefore no explanation is offered, but merely a description of (a subcase of) the phenomenon to be explained by using devices that have no independent empirical or conceptual justification. Furthermore, I believe the description to be inadequate in that a general account of the parallelism phenomenon in question will subsume the special case that can be described in terms of derived categories (see George (1890) for some relevant discussion). But again, this is not the place to pursue the matter.

As for the case of *X*-bar theory, Thompson is surely right to deny that this is (even in the loose sense) a notational variant of c.f.g. It permits only a highly restricted (in fact, finite) class of grammars, and these incorporate properties (feature structure, projection) that are not part of the theory of c.f.g. at all. Again, one should not be misled by the fact that each *X*-bar grammar is weakly equivalent to some c.f.g., a property of no significance so far as is known.

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G. Gazdar. When one realizes that the syntactic theory that Chomsky has been developing over the last ten years has embraced phrase structure rules, complex symbols, a level of S-structure, a level of D-structure, a level of 'Logical Form', filters, transformations, interpretive rules, stylistic rules, coindexing conventions, and abstract cases, among other things, it is a little surprising to hear him castigating as 'needlessly complex' an alternative syntactic theory that employs only phrase structure rules, complex symbols and a level of surface structure.

Chomsky's rhetoric embodies a serious misunderstanding. He asserts that the theory outlined in my paper (T_0, henceforth) is a 'variant of T_2, ...supplemented by a class of superfluous devices (derived categories, additional subcategorization rules of the base for derived categories, devices to carry through a derivation the information that some category must be phonetically unrealized, etc.)'. This assertion is clearly false, as Chomsky would presumably have realized had he been able to consult the more technical papers upon which my relatively informal presentation was based (Gazdar 1981 *a*, *b*). The derived category/rule apparatus allows one to dispense with many of the devices assumed by T_2; in particular it has no need for interpretive rules like R′, or for the level of structure that they map into. Chomsky calls T_2 a 'one-level' theory, but in fact it has at least two levels: S-structure and 'LF'. In a theory like T_2 semantic interpretation applies to 'LF', whereas in T_0 semantic interpretation applies directly to the (surface) structure generated by the grammar (see Gazdar (1981 *b*) for discussion of this point).

Chomsky's remarks, quoted above, misleadingly suggest that the derived categories and 'devices to carry through a derivation the information that some category must be phonetically unrealized' are distinct types of entity whereas they are, of course, one and the same thing. And his claim that T_0 requires 'additional subcategorization rules of the base for derived categories' might have been true had T_0 employed the context-sensitive approach to lexical insertion presented in Chomsky (1965). However, as things stand, it is not true: the context-free rules responsible for introducing lexical items in T_0 (see Gazdar 1981 *b*) make no reference to derived categories, nor could they make such reference. Consequently, permitting derived categories makes no difference to the number of subcategorization rules in the grammar.

Chomsky's misconstrual of T_0 as some kind of augmented T_2 leads him to conclude that the analysis of Ross's facts in my paper 'does not constitute an explanation since the devices used have no independent justification, but are simply superfluous additions to T_2. Therefore no explanation is offered, but merely a description of (a subcase of) the phenomenon to be explained by using devices that have no independent empirical or conceptual justification'. Again, his claim that the derived category notation has no motivation independent of the Ross facts is just false: Gazdar (1981*a*) motivates that notation on the basis of the structure of English relative clauses and constituent questions, the distribution of the complementizer *that*, the extraposition construction, the 'pied-piping' facts, left branch phenomena, 'right-roof' violations, and the c-command position of *wh*-phrases, among other things.

Chomsky correctly notes that T_0 uses quite different rules to deal with the constructions in (d 7).

(d 7) *a.*	Who did you meet?	= Chomsky's (d 5*b*)
b.	John seems to like Bill.	= Chomsky's (d 5*c*)

In doing so, T_0 denies the existence of the generalization that Chomsky thinks holds across these constructions. Detailed argumentation showing the failure of the putative generalization, by authors of diverse theoretical persuasions, can be found in Bach & Horn (1976), Brame (1976), Bresnan (1978), Kimball (1972), Pullum (1979) and Riemsdijk & Williams (1981), among others.

Consider the following strings of English words.

(d 8) *a.* *I wonder who John seems to like Bill.
b. *I wonder who John seems to like her.
c. I wonder who John seems to like.

Chomsky maintains that it does not matter if one's syntax generates (d 8*a*) since 'it yields no well-formed LF-representation, no semantic interpretation, because it contains a quantifier binding no variable. Therefore, it is unnecessary to use syntactic devices to exclude (d 6) [(d 8*a*)].' Chomsky seems to be implying that the unacceptability of (d 8*a*) follows from some independently given property of logic. But this is not true, as a glance through a few logic textbooks soon reveals. Most definitions of the syntax of predicate calculus do not exclude the possibility of vacuous quantification, and for good reason. Excluding vacuous quantification makes the syntax more complex but does not pay any compensating semantic dividend. The standard rules for the semantic interpretation of predicate calculus will interpret expressions like $\exists x\phi$, $\forall x\phi$ and $\lambda x\phi$ quite happily whether or not there is a free occurrence of x in ϕ. Chomsky is at liberty, of course, to define the syntax of his LF level in any way he chooses (although he has not yet chosen to define it at all). In particular, he could define it in such a way as to exclude vacuous quantification. But defining it that way in order to provide for the deviance of (d 8*a*) does not give us much of an explanation for that deviance. Moreover, semantic analyses of Japanese relative clauses and the English *such that* construction arguably require the possibility of vacuous quantification if they are to be maximally general. Note also, in this connection, the unacceptability of (d 8*b*) in English. Since pronouns like *her* are standardly taken to be variables in semantic terms, Chomsky needs an explanation for why this variable does not get bound by the interrogative expression *who*. This explanation cannot be couched in terms of putatively universal syntactic properties of Chomsky's LF level, since word-for-word translations of (d 8*b*) into some other languages give rise to grammatical sentences.

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Chomsky's opinion that questions of generative capacity are meaningless and irrelevant is based on two related claims about finiteness. I shall conclude by commenting on each of these claims.

(i) He claims that the syntactic framework to which he currently subscribes 'permits only a finite number of grammars in principle'. This technical claim is programmatic at best. No proof of it has been given, nor even sketched. Moreover, the notions necessary to such a proof, i.e. 'longest possible base rule', 'longest possible lexical entry', 'longest possible filter', etc., remain undefined and without values.

(ii) He claims that there only finitely many possible grammars for natural languages. This putatively empirical claim is unfalsifiable. To see this, consider a Chomskyan theory of universal grammar U that specifies finitely many finitely valued parameters. Suppose that U stipulates that the maximum value of the ith parameter is k. Suppose further that we investigate some natural language L and discover that the best grammar for L requires the ith parameter to be set at $k+1$. We have falsified U, but we have not falsified the claim that there are a finite number of possible natural language grammars since the proponent of that position can simply shift to a new theory U′, which is just like U except that the maximum value of the ith parameter is stipulated to be $k+1$. No fact or set of facts about natural languages can ever refute or support the finiteness claim.

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H. C. Longuet-Higgins, F.R.S. (*Laboratory of Experimental Psychology, University of Sussex, U.K.*). Would Dr Gazdar regard the following sentence as grammatical or not?

I met the girl who jilted John and Harry loves.

If not, does Dr Gazdar's theory of English grammar exclude it as ungrammatical?

G. Gazdar. I would regard the sentence as ungrammatical. A related example, due to Williams (1978, p. 34), and shown below,

* *I know a man who Bill saw and likes Mary*

is discussed in some detail in Gazdar (1981).