ON THE FAILURE OF GENERATIVE GRAMMAR

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An attempt to construct a generative grammar of French with a coverage comparable to that of traditional grammars has failed. A description has been arrived at in the course of this work, however; it is much more complex than expected, and turns out to be entirely taxonomic. This result calls into question the validity of the so-called theory of generative grammar.[*]

The methods of transformational (generative) grammar have been available for more than twenty years. It is believed that, thanks to them, syntax has become a natural science. It was convincingly demonstrated at an early date that transformational models imposed on the description a precision and a coherence never reached before.

But one may wonder why no linguist has been able to construct a transformational grammar with the type of coverage that traditional grammars used to provide.[1] It seems significant that such powerful methods have not been systematically applied to a language like English—and surprising, because linguists who base their work on the study of English are so numerous that a substantial part of an English grammar should have been constructed by now. I will attempt to analyse this omission and to show how new research directions may be defined.

The following observations are not the result of my epistemological reflections on the foundations of generative grammar (GG): rather, they are conclusions reached after attempting to construct a transformational-generative grammar of French. I and my co-workers have built a formal grammar encompassing a significant portion of French, but we were unable to accomplish this without considerably modifying the theoretical framework. This grammar contains about 600 rules and conditions of application (we do not distinguish these two notions). We attempted to verify systematically the applicability of these rules to more than 12,000 lexical items. I am unaware of any other attempt to verify the scope of applicability of such a formal grammar in English or in any other language. These descriptions have been published and can be tested by the community of linguists (Boons, Guillet & Leclère 1976a,b; Giry-Schneider 1978; Gross 1975; Labelle 1974; Meunier 1977;

¹ Jespersen 1909, Poutsma 1904–29, etc. The only recent step in this direction (Stockwell et al. 1973) is not a grammar; it is an attempt to integrate partial data of heterogeneous origins, and a study in consistency of rules. It is the only compilation of transformational constraints ever attempted, and is now obsolete—mainly because new theoretical developments have, it seems, entirely modified the situation. Let us also mention the efforts by Chapin 1967 and Householder et al. 1964–65, who have classified non-trivial numbers of lexical elements.

Négroni-Peyre 1978). The linguistic data are represented in the form of binary matrices which could not be stored, retrieved, and modified in a reasonable time period without recourse to computer techniques. More precisely, the descriptions constitute a classification of the simple predicates of French, in which each class is a submatrix of a 12,000 × 600 binary matrix. Even their most elementary properties raise new questions which appear to have psycholinguistic significance (Gross 1974). If we compare, two-by-two, the rows of the matrix—i.e., if we compare the syntactic properties of any two lexical items—it is observed that no two lexical items have identical syntactic properties. If we compare the columns two-by-two, i.e. the domains of the rules, the result is the same. A sample of the material is given as Table 1, and I will argue in §3 that such conditions of formal irregularity have serious consequences for the problem of learning.

N=V C		Auxiliaire avoir	Auxiliaire être	N_0 est V_pp Ω	N_0 V Prép N_1	Compléments directs ou indirects										Comp. indirect					
						Complétives						Noms				N_0 V Prép N_2	Noms				N_0 v V Ω
						que P	que Psubj	(Qc z i)	V_0	ce (ci + là)	ppv	N_hum	ppv	le fait Qu P	ppv		N_hum	ppv	N_hum	ppv	
+	s'agir	–	+	–	+ de	–	+	+	+	–	+	–	+	+	–	– pour	+	–	–	–	–
+	apparaître	–	+	–	+ 0	+	–	–	–	+	+	–	+	–	+	à	+	+	–	–	–
+	apparoir	–	–	–	0	–	+	–	–	–	–	–	–	–	–	0	–	–	–	–	–
+	s'avérer	–	+	–	+ 0	+	–	–	–	+	+	–	+	–	+	à	+	–	–	+	+
+	y avoir avantage	+	–	–	+ à	–	+	–	+	+	–	–	–	–	+	pour	+	–	–	–	–
+	y avoir lieu	+	–	–	+ de	–	+	+	+	–	–	–	–	–	–	pour	+	–	–	–	–
+	n'empêcher	+	–	–	+ 0	+	–	–	–	–	–	–	–	–	–	0	–	–	–	–	–
+	être besoin	+	–	–	de	–	+	+	+	+	+	–	+	–	+	pour	+	–	+	–	–
+	être l'heure	+	–	–	de	–	+	+	+	+	+	–	+	–	+	pour	+	–	–	–	–
+	être question	+	–	+	de	–	+	+	+	+	+	+	–	+	+	+ pour	+	–	+	–	–
+	être temps	+	–	+	de	–	+	+	+	+	+	–	+	–	+	pour	+	+	–	–	–
+	faire bon	+	–	+	0	–	–	+	–	–	–	–	–	–	–	pour	+	–	–	–	–
+	falloir	+	–	+	0	–	+	–	+	+	+	+	+	+	–	à	+	+	–	–	–
+	s'en falloir	–	+	–	pour	–	+	–	–	–	–	–	–	–	–	de	–	–	+	–	–
+	paraftre	+	–	+	0	+	–	–	–	+	+	–	+	–	+	à	+	+	–	–	+
+	paraître	–	–	+	0	+	–	–	–	–	+	+	–	+	–	0	–	–	–	–	–
+	sembler	+	–	+	0	+	–	–	–	+	+	–	+	–	+	à	+	+	–	–	+
+	souvenir	+	–	+	de	+	–	+	+	+	+	+	–	+	+	à	+	+	–	–	+

TABLE 1 (from Gross 1975).

We faced all the problems to be discussed below when we attempted to construct and study abstract mechanisms based on a grammar of French. We studied Equi-NP Deletion (Gross 1968, 1975) at a time when the same phenomenon in English was undergoing major theorization; but we always felt unable to do the same for French, because each time we introduced a new example, the rule had to be applied in a way different from that used in all previously studied cases. Variations were minor most of the time: prepositions could appear or not, a special tense or mood was involved, etc. We were forced to conclude that we could obtain no generalization without a reasonably complete study of the lexical items of the language and their syntactic uses. The next step was to build a classification for the data collected in this systematic fashion.[2] After more than ten years of investigation, our objectives—which were initially those of GG—have been entirely shifted to numerous new problems, raised by these large-scale experiments and by theoretical elaboration of the resulting data.

Important differences between this work and standard generative studies need to be made explicit. The present remarks lead to a clear conclusion: GG could have been demonstrated to be a descriptive method far superior to all previous traditional and structural attempts. But the insistence on an experimental paradigm which depends entirely on introspection to provide the linguistic examples, and which is explicitly motivated by a desire to treat linguistics at an abstract level of argumentation, has caused the field to evolve toward some surprising philosophical speculations. Work based on sentences demonstrably acceptable to all but a few speakers of English—at least as it would be construed by linguists outside this philosophical school—has almost entirely vanished. Academic discussions on forms of Universal Grammar have appeared instead.

1. LINGUISTIC FACTS. The notion of linguistic fact has evolved considerably over the last century. Although never made explicit in any linguistic framework, it can be reconstructed from the practice of specialists:

(a) Comparative linguistics has for its object of study the evolution and/or parenthood of language. As a consequence, a fact will be a linguistic form relating two languages or two 'states' (in time) of the same language.

(b) In traditional grammar, the notion of fact is especially variable (Chevalier 1968). Facts have been linked primarily to pedagogy of native languages, with special attention given to spelling (mainly agreement rules) and to literary stylistics.

[2] In particular (Gross 1975), we studied the distribution of the sentential complements (ce) que S—and, independently, that of the infinitive complements V-INF Ω (S without its subject). In each case, more than 2,600 V's are involved. We observed strict coincidence between the two distributions, with essentially three classes of exceptions:
  (a) about 70 verbs, modals, and aspectuals have only the infinitive construction (Table 1 in Gross 1975);
  (b) 180 others that take only infinitives are semantically characterizable by a notion of 'displacement' (Tables 2 and 3);
  (c) about 200 others have only sentential complements; they are semantically characterized by the notion 'logical deduction'.
All other V's, i.e. about 2,400, have both constructions. Under such conditions, the abstract notion of complementizer, which appears in all works on Equi, has no meaning. Nonetheless, lack of an analogous study for English led to its generalized use.

Teaching foreign languages brings out other grammatical facts, such as the use of prepositions, word order etc.

(c) In structural linguistics, additional facts are the strings of categories, i.e. syntactic forms (Bloomfield 1933, Harris 1946).

(d) In transformational grammar, Harris 1952 changed the notion of linguistic fact by making RELATIONS between syntactic forms the central point. Chomsky 1956 defined a set of hypotheses about the form of these relations. For him, a new category of facts is any construction or putative relation between constructions that will restrict hypotheses; these are facts about the metalanguage he has proposed.

An example will illustrate some of these transformational concepts. Study of similar syntactic forms invariably leads to the following situation: given a syntactic property, some forms have it, and others don't. Consider:

> (1) a. King John launched an attack against the city.
> b. King John watched an attack against the city.

In both sentences, *King John* is the subject, and *an attack against the city* is the direct object, which in turn is analysed as containing a head (*an attack*) and a noun complement (*against the city*). From another point of view, the sentences are different: with *to launch*, but not with *to watch*, the phrase *against the city* is also a complement of the main verb. This conclusion is a consequence of the differences in acceptability observed in the following pairs:

> (2) a. It is against the city that King John launched an attack.
>   *It is against the city that King John watched an attack.
> b. An attack was launched against the city by King John.
>   *An attack was watched against the city by King John.

This is a type of fact which is looked for in GG, because it reveals unconscious constraints by speakers of a language.

In traditional grammar, this example would be construed as being typical of the situation of rule and exception. The construction in which *against the city* has two roles presumably would be exceptional, since it is limited to verbs like *to launch*, which seem less numerous than the others.[3] GG has inherited these empirical features from traditional grammars; cf. Chomsky (1972:126–7):

'In intention, at least, traditional scholarly grammars are generative grammars, although they fall short of achieving the goal of determining how sentences are formed or interpreted. A good traditional grammar gives a full exposition of exceptions to rules, but it provides only hints and examples to illustrate regular structures ...'

The real difference between GG and traditional grammar appears to lie in the nature of the metalanguage which is formalized in GG; in traditional studies, concepts are imperfectly defined and fluctuate with intuitions.

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[3] The data are in fact more complex, since the phenomenon depends on the main V, the N, and their respective positions. There are examples in subject positions:

An agreement between them should occur.

An agreement between them would displease Max.

Only in the first example can *between them* be extraposed.

Simple use of any formalism entails important differences in the practice of linguists. Since grammatical categories and rules are represented by fixed symbols, their consistent use has introduced into the description of independent linguistic facts a coherence among syntactic assertions that is largely absent from traditional linguistics, though not from structural linguistics. To some linguists, application of formal techniques requires examination of ALL details of the constructions (determiners, prepositions,[4] and more generally, grammatical words otherwise neglected). As an immediate consequence, new classes of facts are uncovered.

However, certain fundamental criticisms against traditional grammar also apply to GG. In a domain where constant suspicion of intuition should have developed, specialists satisfy themselves with data of traditional grammar, only providing more details. In the best cases, they secure new constraints—but of a non-systematic nature, and on narrow families of examples.[5] More generally, practically no GG specialist seems conscious of the fact that DEMONSTRATING THE EXISTENCE of phenomena is a prime necessity. Even if a clear difference of behavior has been observed in some sentences, the observation may constitute an artifact. Only comparison with a large number of cases can bring plausibility as to the presence of significant data. It is bizarre that the rules of GG have been formalized in great detail by means of the most powerful devices of Logic and of Computer Science, before the range of the corresponding phenomena has been shown to extend beyond exception.

I will now illustrate these points with four types of example which have been the subject of a number of classical studies.

1.1. For the first example, Passive, there are fundamental problems concerning its existence; the severity of these renders surprising the tenacity put into formalizing it. Two sentences like *Max ate my soup* and *My soup was eaten by Max* are considered to be related by some transformational process. The justification for this passive relation is the fact that pairs of NP's like {*Max, my soup*}, {*The boy, a cake*} preserve the synonymy relation under fixed formal conditions. First, I am unaware of the existence of studies where pairs of NP's are SYSTEMATICALLY compared in active and passive forms. Such studies would involve either large numbers of lexical items, or a semantic classification of nouns, at the least. Thus an unjustified step is taken between (a) verifying a few obvious pairs brought in by pure intuition, and (b) asserting that a formal relation holds independently of the lexical content of the NP's. Second, even if a careful study had been made with *to eat*, it would not allow generalization of Passive to any other verb. As a matter of fact, examination of a few verbs demonstrates the necessity of a careful approach. Consider the verb *to inhabit*, which has no passive in

    (3)   Max inhabits Manhattan.
           *Manhattan is inhabited by Max.

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[4] Generative linguists seem, however, to believe that distribution of prepositions is not an interesting fact. They think they have solved the problem once and for all by using an ad hoc contextual rule of the type V NP → V PREP NP, which introduces the right preposition in the right context.

[5] For example, adverbial complements, poorly studied in traditional grammar, have been largely ignored in GG.

But when the subject is plural or 'collective', we have

(4)   Rich politicians inhabit Manhattan.
      Manhattan is inhabited by rich politicians.

Consider the verb *to receive*, with this paradigm:

(5) a.   Max received our parcels.
         Our parcels were received by Max.
    b.   Max received all possible guarantees.
         All possible guarantees were received by Max.
    c.   This question will receive all our attention.
         *All our attention will be received by this question.

Such pairs show that, for a given verb, existence of passive forms depends on use, metaphorical or not, of the verb—a use linked to particular pairs of NP's. It also appears that the notion of metaphor is hard to formalize; but it has an unmistakable syntactic basis (Boons 1971).

The generality of such situations brings seriously into question the very notion of transformation, and draws attention to the concept of distribution, which has never been investigated.

The following is another active–passive pair:

(6) a.   The symbol ψ represents this function.
    b.   This function is represented by the symbol ψ.

But this case is far from clear, for sentences like 7a exist, with passive 7b:

(7) a.   Physicists represent this function by the symbol ψ.
    b.   This function is represented by physicists by the symbol ψ.

Now, when a passive form occurs with agent (e.g. 7b), a similar form occurs without agent (e.g. *My soup was eaten*). Therefore 6b has two descriptions: (a) passive of 6a, and (b) passive of 7a without agent.

Note that no difficulty arises with this pair:

(8) a.   This reply astonished Max.
    b.   Max was astonished at this reply.

This is because we can directly verify that *at this reply* is not an oblique complement of the verb *to astonish*:

(9) *NP astonished Max at this reply.

We can now see that the problem of distinguishing agents from prepositional complements of the verb varies from verb to verb; each verb should then have been studied from this point of view. But no generative linguist has proposed to use a dictionary to sort out those verbs with Passive from those without. Application of such a simple-minded but fundamental procedure would quickly lead, in almost any language, to the discovery of examples like

(10) a.   Sharp intuitions underlie his discourse.
     b.   His discourse is underlain by sharp intuitions.

Here the passive has, out of context, an obligatory agent:

(11) *His discourse is underlain.

The existence of such forms complicates the determination of the agent in the analysis of examples such as 6b.

It is of little use to suggest a passive of a general kind without testing acceptability for a significant number of sentences composed from lists of relevant verbs and contexts. One does not see how the manipulation of computational devices like the dummy symbol Δ or the trace marker *t* can contribute importantly to the empirical description of the passive.

Neither generative nor traditional grammars contain any notion of enumeration of linguistic instances. Not a single attempt to enumerate regular and exceptional cases has ever been made by a syntactician. Chomsky's position on exceptions is strange, as in the following quotation (cf. also the second sentence of the quotation given above):

'There are in fact exceptions to many of the transformational rules given above, perhaps all. These will have to be separately listed, unless some more general formulation can be found to account for them as well. The discovery of such exceptions is in itself of little interest or importance (although the discovery of an alternative formulation in which the exceptions disappear would be highly important) ... But discovery of exceptions to grammatical generalizations is of no consequence in itself, except when it leads to an alternative, more comprehensive generalization' (Chomsky 1962:244–5).

Under the most favorable interpretation, Chomsky appears to regard the exception to a linguistic rule as a physical scientist might regard an experimental result incompatible with his theory as being caused by some unperceived error in the experimental apparatus. Certainly a physical scientist who has not explicitly designed an 'experimentum crucis' is reluctant to abandon his theory on the basis of one experimental failure. Nevertheless, it is his responsibility to DEMONSTRATE, either by experimental repetition under better conditions or by an analysis, that the given experimental result inconsistent with his hypothesis is in fact erroneous by reason of experimental error. It will not do to dismiss a sentence acceptable by those competent in a language, when theory suggests it should be unacceptable (or conversely), simply by noting that it is an exception. Of course, any natural language will have exceptions, i.e. special usages; of course, one may not be able to demonstrate at once that each is idiosyncratic; but at the least, one must note all the exceptions, even if one defers their consideration for later study. Discussion of Passive over more than twenty years is a striking example of this methodological error, which consists basically in denying the requirement that relevant linguistic instances be enumerated.[6]

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[6] In GG, NP's such as *the solution of the equation by our teacher* are supposed to involve the passive of *to solve*, and complex problems of notational formalization can thus be raised (Chomsky 1975:106–17). No generativist has ever thought of avoiding them by using the solution of Harris 1968, which directly relates the two sentences:

    (a) Our teacher solved the equation.
    Our teacher effected the solution of the equation.

The relation is based on an auxiliary verb, here *to effect*. This verb has a passive:

    (b) The solution of the equation was effected by our teacher.

This passive form enters by relativization in the NP:

    (c) the solution of the equation that was effected by our teacher.

Now, by deletion of *that was effected*, we obtain the NP under analysis, and in a way that

It will be hard for the specialist in a natural science to believe that such investigations have never been undertaken for questions of English syntax. Traditional grammarians did not construct syntactic inventories either. But today we know they did not possess methods and motivations that might have enabled them to succeed. Transformational methods made this form of research possible; and for practically all problems of syntax, accumulating data appears to be at least as necessary as it is for Passive. Since new insights into the nature of syntactic phenomena are likely to arise from systematic exploration with the aid of a dictionary, it is all the more surprising that GG has ignored this aspect of linguistics.

Accumulating data is obviously not an aim in itself. But in all natural sciences it is a fundamental activity, a necessary condition for evaluating the generality of phenomena. Such a concept of generality or of importance of facts is totally absent from GG, where sentences acquire significance only with respect to formalism. In GG, a linguistic example appears to be significant only if it allows one to choose between competing theories. In the last few years, the consequences of this view have become caricatural. Linguists, now well-trained in formalistic manipulations, know how to invent new theories at will, and don't restrain themselves. Under these conditions, well-worn sentences are sufficient for all theoretical discussions: any motivation for looking into new phenomena has been lost. In this way, GG has settled into a state of closed-circuit functioning. I will return later to these aspects of generative theories.

The anecdotal character of data in GG is emphasized by the importance given to certain formal details. Many phenomena, traditionally treated in a few lines, are inflated by their formalization; this should be contrasted with the considerable number of easily discoverable sentences left unstudied.

1.2. Consider a second example: Raising, a problem that generated polemic

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accounts for the following difficulties:

NP's whose N has no associated V, such as *the painting by Klee,* will be obtained, by deletion of *that was made,* from *the painting that was made by Klee.*

NP's without a corresponding transitive V, e.g. *the appeal to their conscience by the police,* follow from derivations like this:

(d) The police appealed to their conscience →

The police made an appeal to their conscience →

An appeal to their conscience was made by the police.

Various restrictions on the determiners of these NP's are also accounted for (Giry-Schneider 1978).

Moreover, examples that bring into direct question the existence of the passive relation have never been discussed. Consider this:

(e) Max has an annoyed tone of voice.

His manner of speaking is always annoyed.

In these sentences *annoyed* appears as a past participle in a passive construction, but the active forms are unacceptable:

(f) *(One + this) always annoys his manner of speaking.

*(One + this) annoys his tone of voice.

The mere mention of this fact, which affects hundreds of verbs, should be revealing to the specialist, whereas elaborate formalization leads him to problems without any basis in reality.

discussions not long ago (Chomsky 1971, Postal 1974). The two main situations are illustrated by pairs like these:

(12) a. It seems to me that Max is stupid.
Max seems to me (to be) stupid.
b. I believe that Max is stupid.
I believe Max to be stupid.

Chomsky considers he has shown that only the first pair involves a transformation, and that the second pair should undergo a different formal treatment; but Postal believes there is no significant difference between the two cases. Neither indicates that enumeration of the verbs entering into each of the two pairs is relevant to his theoretical position, since determining the lexical extension of linguistic forms has no status in GG. When this determination was made for French, where the problem is essentially the same, we found three verbs like *to seem*, and more than 600 like *to believe*. (The three verbs are marked with plusses in the last column of Table 1.) One might well claim that this statistical information has nothing to do with the formal problem.[7] But it shows that the type *to seem* is quite limited—frozen, in a sense—while the other type is productive, in that it may affect new verbs and new constructions of verbs. It thus appears that the second process might well be considered more important than the first. It should therefore be studied carefully, and close attention paid to the formalism it requires. Our conclusions are thus different from those of Chomsky and Postal; the concept of fact on which they are based has an empirical foundation quite different from that used by the practitioners of GG.

1.3. Relativization is my third example. Linguists have always proceeded as if any noun (subject, object etc.) could accept a relative clause; e.g.,

(13) a. The book that Max bought is poorly written.
b. Max bought the book which is on the table.

Moreover, when the relative has an attribute, as in 14a, it is used as the source of the corresponding adjective of 14b:

(14) a. Max bought the book which is red.
b. Max bought the red book.

Aside from a remark by Kuroda (1968:265), no grammarian has discussed the acceptability of 14a, which is quite different from that of 13b—so much so that, as a first approximation, 14a seems unacceptable to me. Sentence 14b, which is putatively derived from 14a, is accepted only with contrastive interpretation involving the existence (contextual or extra-linguistic) of other books. This observation does not hold for 13b, or for its reduced form. Now, there are syntactic positions where nouns cannot accept relative clauses; this is the case with *ressort* in the French example *Ce travail est du ressort de Max*, whose meaning and form is close to 'This

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[7] Nonetheless, this type of data is used implicitly. For example, the fact that all verbs can have a subject is a statistical observation on the lexicon (of English, French etc.), since it cannot be made on a corpus. This remark is particularly important, being the empirical basis of the 'fundamental' structural dichotomy of the sentence: subject–predicate. Exceptions like Fr. *voici, voilà* are rare indeed.

work is within his competence', where *competence* accepts only relative clauses with auxiliary verbs of the type discussed in fn. 6:

(15) This work is within the competence which he has.
   *This work is within the competence which we all appreciate.

The last sequence is rejected, despite selectional compatibility. From my point of view, lack of the most elementary description of these restrictions precludes the possibility of constructing any theory that would go further than the description of data taken from a high school grammar. One may then question whether applications of varied mathematical logics to the problem makes any sense.

The various examples I have given indicate how a data base largely independent of theoretical apriorisms can be built. It is virtually an absolute rule in all natural sciences that accumulation of consistent data precedes theoretical advances. Accumulating and classifying facts, and constructing a global image of the language under study, is the major experimental process, all the more in that syntactic problems have never been approached in this way. Thus it appears, from what has been observed in the last few years, that the bulk of linguistic phenomena exhibits great irregularity, and that in many cases the source of the irregularity lies in historical and cultural accidents. When a property is studied, one should attempt to evaluate whether it is general, or whether it is an accident inherited from special circumstances. Diachronic discussions are then fundamental, but there is no room for them in GG (Lightner 1975, Stéfanini 1973). Consequently, problems are selected in accordance with the special tastes of the linguist, and the importance attributed to them depends entirely on fluctuations of linguistic fashion. Numerous examples show clearly this absence of concern for syntactic exploration, but they are never recognized as such.

1.4. I will now discuss such an example: the treatment of the so-called French 'aspirated *h*' in generative phonology. This question is important, because it can supposedly motivate (or refute) the existence of a cycle in French. Consider two cases of liaison:

(16) *les haricots*:   /leariko/,   */lezariko/   'the beans'
   les animaux: */leanimo/,   /lezanimo/   'the animals'

These are orthographically distinguished by means of aspirated *h*. But it can be easily verified that this distinction is entirely artificial, and has been explicitly imposed by the French educational system. Only so-called educated persons possess the *h*, while most French speakers struggle in vain to pronounce /leariko/, ending invariably with /lezariko/. Furthermore, children never have *h* at the age when they master the complete phonological system of French, i.e. before they enter school. Teaching *h* is difficult, as can be heard daily in the classroom and in the street. The non-existence of the linguistic problem is confirmed by the lack of internal coherence of data: *héros* 'hero' has *h*, but not fem. *héroïne*; *héron* 'heron' has *h*, and fem. *héronne* does too. The verb *harnacher* 'to harness' is supposed to have *h*, implying that 1sg. *je harnacherai* is pronounced with schwa. However, in 3pl. *ils harnacheront*, the form with *h* is not accepted: */ilarnašrɔ̃/. There are numerous similar cases. Moreover, school teachers do not 'correct' liaisons of pupils beyond the commonest syntactic positions, between article and noun and

between subject pronoun and verb. Hence, in constructions not taught at school, all liaisons are made in the natural way, i.e. following the dominant consonant–vowel rule:

(17) a. *Les chefs ont combattu par héros interposés.*
    'The leaders fought through interposed heros.'
  b. *Max porte des vêtements pour héros.*
    'Max wears clothes for heros.'
  c. *Tout héros qu'il soit, Max a peur.*
    'For all the hero that he is, Max is afraid.'

(The consonant that immediately precedes *héros* must here constitute a syllable with the *é* of *héros*.) These aspects of the use of *h* are artifacts of pedagogy, and have nothing to do with the way in which the phonological and syntactic system of French is learned. Generative linguists, unaware of such considerations, have argued about this phenomenon as if it were illuminating for the structure of language (for a detailed review of these questions, see Gaatone 1978).

Numerous phenomena are of this type—i.e., they are irrelevant to the theory envisaged.[8] But in most cases, evaluation of the meaning of a phenomenon cannot be made from an external point of view, as in the case of *h*.[9] It then appears necessary to resort to systematic coverage of the language under study. Such basic coverage has not been available, since GG has undertaken no empirical tasks of significant size. This absence of system or of criticism in choosing the collection of linguistic examples has an immediate correlate: practically no empirical justification of the choice of a given phenomenon as a legitimate object of study has ever been given by generative linguists.

I now discuss the nature of theories that have been proposed under these conditions.

2. THE THEORY. In GG, the importance accorded to theory is easily estimated by perusal of the literature. Very few publications do not pretend to contribute to a theory or to a change of theory. Discussions invariably refer to linguistic theory, though the term 'experimental syntax' may not have been used in twenty years. This is surprising, because in general the design of experiments is inseparable from the construction of theories. A grammar of a language is a theory of the language (Chomsky 1972:26–7). From a more technical point of view (Chomsky 1956, Harris 1951:372–3), a grammar is a generative device (rewriting system, algebraic system,

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[8] It is interesting to note that Harris' models (1970, 1976) have not been elaborated by the standard GG methods. One of the remarkable features of his theory is that accidental constraints are isolated from general ones in a special component of the grammar: the extended morphophonemics. The generative opposition between lexical and transformational phenomena has never been discussed from this point of view, and any resemblance between Harris' morphophonemics and the lexicon of GG can only be superficial. The rare examples presented in GG cannot be interpreted as relevant to the empirical problem of determining the significant facts.

[9] There are other examples of linguistic 'phenomena' where teaching intervenes, thus modifying their nature: French has liaisons and elisions, alternation of *-al* and *-aux* in the plural, etc. In order to take these data into account, it is important to isolate their systematic aspects from the data of performance, which are imposed by the social context in a variable and non-linguistic way. At present, the systematic and the performance aspects are mixed.

system of equations etc.) enabling one to compute the shape and the meaning of sentences. The homolog of a grammar, e.g. in quantum physics, will then be a statement like Schrödinger's equation, which permits computation of waves and trajectories of elementary particles. From an epistemological point of view, one should not distinguish construction of a grammar from construction of a theory in any domain of science. Directions for research are then immediately defined: ordinarily, a process of systematic data accumulation is undertaken, which is constantly compared with hypotheses about rules. Today this kind of practice is easy to develop. Accumulation can be extended to all accessible languages, since this will allow us to introduce in orderly fashion more and more general hypotheses on the form of grammar, i.e. on human linguistic capacities:

'... the most crucial problem for linguistic theory seems to be to abstract statements and generalizations from particular descriptively adequate grammars, and wherever possible to attribute them to the general theory of linguistic structure ...' (Chomsky 1965:64).

In syntax, the fundamental type of experiment consists in constructing and evaluating sequences of words whose structure varies with three basic combinatorial deformations: permutation, insertion, deletion. These experiments must be reproducible, at least among linguists; but no 'theoretician' accepts this elementary requirement, or the necessity of first enumerating the facts as completely as possible. Theoreticians content themselves with a few examples, often of doubtful acceptability, drawn purely from intuition—though thousands of linguistic instances could and should be assembled, compared, and classified. Taking into account these two impressive gaps, the volume of abstract discussion is out of all proportion with the arbitrary selection of the data, a selection principle which has been explicitly advocated by Chomsky (1972:165):

'I believe that modern linguistics has real achievements to its credit ... But it must be kept in mind that these achievements owe little to modern science and less to modern technology. The gathering of data is informal; there has been very little use of experimental approaches (outside of phonetics) or of complex techniques of data collection and data analysis of a sort that can easily be devised, and that are widely used in the behavioral sciences. The arguments in favor of this informal procedure seem to me quite compelling; basically, they turn on the realization that, for the theoretical problems that seem most critical today, it is not at all difficult to obtain a mass of crucial data without use of such techniques. Consequently, linguistic work, at what I believe to be its best, lacks many of the features of the behavioral sciences.'

The first attempts to formalize rules of syntax (the equations in Harris 1946, the context rules in Chomsky 1956 and Chomsky & Schützenberger 1963, and the transformations) led Chomsky to formulate a general hypothesis: grammars belong to the class of formal systems (i.e. rewriting systems). Since then, research has been directed toward the discussion of facts that restrict the form of these general systems, the aim being to render as specific as possible the form of grammars of natural languages. This approach may seem legitimate at first, but it presents a peculiarity to be examined later: all arguments have been made with respect to CLASSES OF FORMAL GRAMMARS (i.e. sets of grammars defined a-priori), and not with respect to PARTICULAR GRAMMARS OF LANGUAGES. We will see in §5 that this program is related to that of mathematical linguistics.

But constructing grammars of particular languages must still be an integral part

of the program of linguistics, since a grammar is a model of morpho-syntactic knowledge acquired by native speakers (Lakatos 1978). There is then a fundamental behavioral inconsistency between (a) the idea that GG provides the basis upon which to discover a theory of language and (b) the total lack of a program for constructing grammars of particular languages. The generative approach, initially justified by reason of its contributions to making precise a variety of grammatical procedures, has arrived at a state in which linguistic research based on systematic empirical work has been dismissed as irrelevant. Obtaining as complete and detailed a picture as possible of any language is no longer a task for this 'linguistics'. Thus generative syntax has become a new rhetoric whose vocabulary takes its inspiration from logic, syntax, and computer sciences. Its purpose appears to be limited to the construction of abstract representations for a small body of artificially concocted (mainly English) sentences.

The evolution of the notion 'transformation' illustrates the way empirical problems have disappeared, to be replaced by abstract speculation. Consider the original notion introduced by Harris 1952. It has approximately the form

(18) $S_1 \rightarrow S_2$

This allows one to derive sentence form S_2 (e.g. a passive) from another S_1 (e.g. an active). Chomsky 1965 renounced this formulation entirely and insisted on the necessity of a level of ABSTRACT structure, the deep or basic structure DS from which transformations derive surface structures, namely sentence forms like S_1, S_2. We then have

$$DS$$
$$\swarrow \quad \searrow$$
$$S_1 \qquad S_2$$
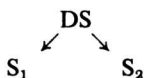
This attitude eliminates the empirical basis of transformations which was made explicit by direct relationship between S_1 and S_2, and creates at a purely abstract level a complex geometry for tree structures. However, before trying to determine the tree shape of DS, the question whether a linguistically-based relationship exists between S_1 and S_2 must be answered. This question is mainly empirical, and does not prejudge the FORMAL NATURE of relations. But in GG, these sorts of facts are never discussed as such; they vanish behind formal devices and abstract calculi that then necessarily become the object of linguistics.

Clear separation between questions of existence of relations among sentences and geometric problems of sentence structure would provide greatly improved understanding of both theoretical and empirical questions. In fact, problems of existence, such as those mentioned about Passive, arise in the same terms in any theoretical framework. Their discussion is identical, whether in Chomsky's generative framework or in Harris' algebraic system. However, theoretical arguments and problems are quite different in these two formal approaches. Chomsky has attempted to construct a geometry for the deformations of trees, and his main purpose seems to be a search for abstract conditions on the deformations. Harris has minimized the amount of formalization needed to relate sentences to each other, and has defined an algebraic structure on classes of sentences practically independent of the geometry of sentences. There are still other ways to formalize these phenomena (e.g. Postal 1977).

The hypothetical character of all these formalisms should now be clear. While most linguists deeply believe that a grammar must be a formal system, I consider that the validity of the notion of a geometry for constituent structure has not yet been demonstrated or even made plausible. Moreover, it is equally clear that, in the present state of knowledge, a linguistically significant correspondence between sentences can be accommodated within a wide range of formalisms, among which rewriting systems are only one example.

Formal exploration of theories has proved extremely fruitful in physics. But in linguistics, this mode of thinking has had some negative effects. One may even suppose that the reason many new and important phenomena have not been subjects for study is their incompatibility with generative theories. Thus rewriting rules (e.g. $S \rightarrow NP\ VP$, $VP \rightarrow V\ NP$) describe only LOCAL dependencies. For example, while the N head of an NP depends on (is selected by) a V, other elements internal to the NP (e.g. determiner, modifiers) depend on N and not on V. But there are numerous syntactic situations that involve non-local constraints. Belief in generality of generative models, all local, has caused paradigms like the following to be over-looked:

(19) a. *Max drives at the speed.
         *Max drives at a speed.
         *Max drives at the astonishing speed.
     b.   Max drives at an astonishing speed.
          Max drives at the legal speed.
         ?*Max drives at a legal speed.

This paradigm concerns numerous adverbials, and in this sense has considerable importance (cf. fn. 5). In the same way, consider the following pairs, made up of sentences closely related in meaning:

(20) a. Mary's lucidity surprised Max.
         Mary surprised Max with her lucidity.
     b. Max liked Mary's lucidity.
         Max liked Mary for her lucidity.

In the (b) sentences, there is a special relationship between *Mary* and the complements (*with* + *for*) *her lucidity*, which behave like common indirect complements. Consider the pairs:

(21) a. The totality of the books will go to Max.
         The books will go to Max in their totality.
     b. ?Max read the entirety of those books.
          Max read those books in their entirety.

The relation between sentences of each pair involves a constraint between the predeterminer *the totality*, *the entirety*, and the noun *books*. This relation was also observed in the preceding pairs; in subject position, it takes the form

(22) $[_{NP}\ N_0\ of\ N_1]\ V\ X = [_{NP}\ N_1]\ V\ X\ [_{PP}\ PREP\ N_0]$

Here N_0 is the NP head of the subject, N_1 its noun complement; the relation 'restructures' the whole subject and extraposes its head. The relation is analogous in object position.

Note that this problem, the localization of quantifiers, is more general, for it also bears on the analysis of

(23) Max read the books (entirely + in their entirety).

Here the adverbials derived from the predeterminer *entirety* affect the noun and not the verb:

(24) *Max read (entirely + in their entirety).

All these pairs are quite general, and are observed with almost all verbs (Gross 1977). They are important, because existence of these relations brings into question the way nouns and determiners are introduced with respect to verbs. But these phenomena are never mentioned in GG. It is difficult to relate such sentences in GG, since they would require operations more complex than those of Raising; one may then suppose it is such difficulties of fitting them into the generative framework that have caused them to be overlooked by linguists.

I mention now another category of facts never investigated in GG—perhaps because they are too precise! Syntactic rules are always limited to certain lexical items; e.g., Raising is limited to certain verbs. Postal 1974 provides substantial lists of verbs for English; and as I indicated, recent work on French grammar put us also in a position to provide lists. But there is an important difference between the two enumerations: for French, we can assert that there are NO VERBS other than the three mentioned. In this way, we provide complete lexical localization of the phenomenon, a notion which is not thought to be relevant in GG. Only the fact that systematic classification is available allows us to formulate statements of this form. Moreover, study of French has led us to the following observation: the phenomena that would be called lexical by Chomsky are the rule, while the ones he termed transformational are quite rare. In other words, when one possesses an extensive picture of a language, i.e. a categorization of the great bulk of the lexical elements and their local constraints, one sees that the formal notion of transformation, as promulgated by GG, is of marginal importance. GG bears only on insignificant and arbitrary parts of the materials. Moreover, it has never developed the means to verify its limitations.

The considerable efforts invested in creation of a theoretical level are explained in great part by the GG linguists' adherence to certain philosophical principles, e.g. the ever-present idea that an adequate linguistic theory must have a form yielding to mathematical analysis, and hence producing formal results of deep linguistic meaning. Within linguistics, it is hard to find empirical support for this faith. Moreover, §5 below shows that the results of mathematical linguistics do not encourage such hopes. Nonetheless, the quest for abstract constraints that restrict the class of possible formal grammars is practically the only current activity of GG. From a point of view external to linguistics, one can explain this search only by a narrow concept of science. It is true that, in physics, analysis of fundamental equations has led to spectacular predictions; but this situation is rather unique. Biology, botany, chemistry (until recently), and geology do not have this character. The special status of physics has been made popular since Kant by philosophy textbooks which still, too often, neglect the discussion of other natural sciences.

Numerous linguists seem to have been victimized by the necessity of reaching, at all cost, the so-called scientific level of mathematical prediction.

It is worth mentioning that a whole domain of linguistics exists which adheres to the standard procedures of empirical science: Indo-European comparative grammar. Observing the similarity between Sanskrit and European languages was the initial step, made in the 16th century, and more adequately in the 18th century by Sir William Jones. Since then, observations have been accumulated; they were of an etymological nature at first, but became more and more comparative up to the beginning of the 19th century. At that point, Bopp assembled and classified data in such a way that he was able to abstract general features shared by the languages studied. Schleicher then made a further abstraction: he hypothesized their common origin, the Indo-European language. Since then, there have been numerous advances, and many applications of the theory have been made. The history of the field should have served as a model for present-day linguistics.

Analogy between physical theory (or, more generally, any theory in the hard sciences) and linguistic theory is revealing of the attitudes I am discussing. The most striking difference is that the abstract level (even when not mathematical) of seriously founded theories has been reached only after decades or even centuries of work, during which facts have been accumulated, classified, and made coherent by locally applicable theories with constant concern for systematic investigation— conditions that are all necessary to any important generalization. In contrast, generative linguistics has grown into a field of abstract discussion of formal notations that undergo rapid and extensive variation with no sign of convergence; in its haste to generalize episodic observation, GG has left no room for the possibility of accumulating systematic data.

This philosophy has confined GG to a level of abstraction that is by now independent of the great body of linguistic data. But so far, the proposed theories present no interest in themselves. In fact, the formal mechanisms used by theoreticians are simply (within terminological changes) those used by professional programmers who specialize in the treatment of non-numerical data. For example, the dummy symbol Δ is essentially a reserved memory whose content is specified by program; the trace symbol t is an address pointer; the bar notation is an indexing device for the number of times a loop is entered, etc. Arguments about these mechanisms of abstract grammar are then isomorphic to those involved in optimization of the programming of any algorithm. The choice between two theories, e.g. between 'generative' and 'interpretative', is analogous to the choice between SNOBOL and PL/I for a given program—with the operational difference that a programmer for whom the result would be sufficiently important can always program his algorithm in both languages, and choose according to the performance of the program in each language. In the same situation, generative linguists have not succeeded in exhibiting any experimental clues favoring the superiority of one system over another. One more difference between linguists and programmers is that the latter are of necessity more rigorous because they are limited by convention to certain well-defined languages. Linguists, on the contrary, tend to believe that introducing new formal devices constitutes an original and creative contribution to the field. Lack of scientific culture prevents them from seeing that this activity is in

_general trivial, and that numerous mechanisms (together with variants) can often be proposed by professional programmers.[10] Such mechanisms would be those that linguists pompously call 'alternative theories', and which they praise for 'empirical adequacy' and 'explanatory power'.

Other signs of this limitation to abstract levels can be pointed out. Thus the distinction between competence and performance is the one made by physicists between LEGITIMATE and ACTUAL behavior: legitimately, a particle should behave according to an equation; actually, the environment always determines discrepancies. The psychological terminology corresponds to the same double point of view on the acceptability of strings of words, i.e. on syntactic experiments: strings may be grammatical—i.e., they may be well-formed with respect to some theoretical competence or grammar; and they may be acceptable, i.e. observable (by direct intuition, or by a panel of speakers of the language). It is significant that, during the last few years, this distinction has been submerged by many authors who use only the term 'grammatical', thus merging two independent points of view. This shift must be interpreted as another indication of the loss of all consciousness of experimental work, a loss that has reduced GG to purely abstract activity with unclear intellectual content.

3. LEARNING. The abstract character of discussions on learning, a cornerstone of generative construction, must also be pointed out as a consequence of the same ideology. It will be enough here to observe the level at which Chomsky (1975:14–35, 156–8) discusses the problem. He represents, for the sake of clarity, a hypothetical learning theory as a function $LT(O,D)$ of two variables: O for organism and D for domain of knowledge. In the course of the argument, he sets $O = Human$ and $D = Language$ for the linguistic case—a special case of a general psychobiological situation, another example being $O = Rat$ and $D = Maze$. It is important to realize that GG can deal only with such metaphysical material,[11] since it has never constructed any actual model for a given language. Nonetheless, many questions exist that can be submitted to theorization and corresponding experimentation, as soon as one has access to a meaningfully formalized description of a language. Given the structure of the lexicon-grammar presented above, how is it possible for a native speaker to acquire such a pattern of data? More particularly, how can acceptabil-

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[10] In this respect, I need only quote Sussmann & Zahler's 1978 discussion of Catastrophe Theory, with minor paraphrasing: 'The claim is sometimes made that devices such as the A over A principle, trace theory etc. are the first step toward the development of scientific linguistics. Whatever its shortcomings, it is said it is the only tool we have got.' Defending GG in this way is like defending the proposal to use chicken soup as a cure for appendicitis, by means of the argument that 'It may not be a very good method, but we have nothing better.' Of course, (a) we do have something better; and (b) even if we did not, that does not suffice to establish that chicken soup is any good as a cure for appendicitis.

[11] Chomsky indulges in a philosophical discussion of a function $LT(O,D)$ that has no specificity. At the same time, GG offers astonishingly concrete evaluation procedures for grammars (Chomsky 1964:24–47). For example, the number of binary features in the rules of a grammar is one of the parameters used to measure the appropriateness of a theory (Chomsky & Halle 1968:392–3). This mechanistic way of choosing belongs, in my opinion, either to numerology, or to the most behaviorist concept one could entertain about learning (unless it has a technological motivation carefully kept secret).

ities be learned? and more cogently, non-acceptabilities, most of which are never heard? Such problems, for which precise numerical data could be given, deserve at least as much attention as the hypothetical search for universal constraints on the formal nature of language, in a context where not a single example of grammar has yet been constructed.

Choosing the explanation of learning as the central task for linguistics has resulted in a paradox: generative linguistics has become a behaviorist activity, a defect repeatedly claimed to have been eliminated by the abstract level of theorization finally reached. Generative linguists have indeed founded their doctrine on the rudimentary observation that all restrictions of some particular language are acquired by native speakers. This remark is, first of all, not very revealing; moreover, it makes no distinction among the various types of acquired devices. Some are, e.g., residues of formerly productive processes, or literary experiments of a limited nature which have been accidentally preserved. In the generative approach, accidental facts must be considered as belonging to the language, hence to the grammar. However, one might imagine that, apart from the mysterious way in which children learn the basic mechanisms of sentence structure (some of which might even be innate), there may be an important amount of rote learning that accounts for accidental constraints. But since GG has to explain learning, it cannot admit that distinct processes may be at work. This position is clearly revealed by Chomsky (1964:7-8):

> 'It is evident that rote recall is a factor of minute importance in ordinary use of language, that "a minimum of the sentences which we utter is learnt by heart as such—that most of them, on the contrary, are composed on the spur of the moment", and that "one of the fundamental errors of the old science of language was to deal with all human utterances, as long as they remain constant to the common usage, as with something merely reproduced by memory" (Paul 1886:97-8).'

I must point out that the only available 'evidence' for this statement is the citation from Paul. No studies are available that would provide the slightest indication about the amount of rote learning involved. The consequences of this simplistic belief have not yet been perceived by generative linguists: facts of quite different kinds have been amalgamated as if they had something in common and were of equal importance (cf. Raising). From the point of view of learning, all facts are treated equally; and it is lack of a proper linguistic system of reference that prevents the GG linguists from detecting priorities in the treatment of facts. Descriptions then become identical to the same behavioristic gathering of data which is so strongly criticized.

Founding linguistics on direct explanation of learning also results in neglect of historical and dialectal factors in synchronic grammars. But only study of these parameters may lead to a characterization of a synchronic kernel, i.e. of the set of general phenomena. The fallacy of arguing that a child has no access to the structures of Old and Middle English precludes the use of the powerful methods of comparative linguistics. Since there are survivals that have been transmitted through generations, how can one then show that they do not belong to a system (though learned)? The only method consists in studying historical factors; e.g. one examines the way a given form has evolved from Indo-European—or, using a greater fund of documents, from Latin to the family of Romance languages.

Numerous observations across languages are claimed to have no validity. Many interesting studies have been discarded for the same reason,[12] and many blatant errors have been made. Consider the case of the determiner in English and French. In both languages, articles are often ambiguous, with simultaneous interpretations as 'definite', 'generic', 'specific' etc. that depend on the tense-aspect of sentences where they appear. These ambiguities introduce experimental difficulties in evaluating the acceptability of many test strings. There is no doubt that an elementary comparison of determiner distribution in both languages would clarify various problems. But a form of reasoning that involves several languages is forbidden in GG, despite the enormous amount of convincing work accumulated in traditional contrastive linguistics. One of the reasons invoked is that languages are generally learned (by children) independently of each other, and thus have no structural influence on each other. Comparative methods have been neglected at even the most elementary level of word-for-word translation between related languages, e.g. by Chomsky (1975:97–8) when he discusses the active-passive relation:

(25) Beavers build dams.   (dams = some dams)
Dams are built by beavers.   (dams = all dams).

The interpretations he selects for discussion are found in French:

(26) *Les castors construisent des barrages.*
Les barrages sont construits par les castors.

The English examples define a problem for Chomsky, since the meanings of the sentences, associated by Passive, are different. Chomsky draws an argument from this observation that supports a certain logical theory of the phenomenon, and that even justifies the so-called trace theory. But a simple look at the translations is enough to verify that the phenomenon does not exist;[13] the first sentence has a

[12] The description of English raises a classical problem of comparison. There is a double vocabulary: French and Germanic. A preliminary question is to determine to what extent this remark is correct. In order to answer, lists of pairs of words or of sentence forms as complete as possible should be given; but such lists do not seem to exist (cf. Buck 1929). Another question is syntactic. Often, words with similar meaning have similar syntactic properties; this observation is the basis for attribution by traditional grammar of elements of meaning (e.g. object) to properties of form (e.g. direct). One can then ask to what extent the two lexical sets have the same syntactic properties. The answer presupposes that systematic enumeration and comparison should be made, from which one could expect surprising results, especially if analogous properties and lexicons of French and German are also studied. One sees clearly why this problem has no meaning in GG. On the one hand, generative methods do not provide for systematic surveys; on the other hand, questions and answers do not bear on formalism. Since such studies neither confirm nor refute the use of any so-called theoretical device, there is no reason for GG to take it into consideration. A first step in the study of Romance languages along these lines has provided unexpected results (Elia 1978).

[13] Even without reading through dictionaries to study the distribution of phenomena, Chomsky might have found pairs like

(a) Cosmologists build cosmological theories.
Cosmological theories are built by cosmologists.

These show that the interpretation under discussion depends on extralinguistic relations between subject and object. We also have (b) and (c):

(b) Beavers appreciate dams.
Dams are appreciated by beavers.

regular passive:

(27) '*Des barrages sont construits par les castors.*

The active form of the second sentence is, just as banally,

(28) *Les castors construisent les barrages.*

I.e., the meaning is preserved in both instances. Two facts, more obvious in French, explain the pseudo-phenomenon of English quite simply:

(a) A 'zero' determiner in English corresponds to two distinct forms in French: the generic definite article *les* and the indefinite article (partitive *du, de la*, or plural *des*); hence the English sentences are a-priori ambiguous. This fact is well-known to pedagogues.

(b) Subjects with indefinite determiners are practically unacceptable for most French verbs. This fact holds for English too (Jespersen 1924:154–5), but is less apparent in the subject position, since it favors generic interpretations. This constraint may limit the degree of ambiguity to three.

These English sentences should thus be considered as degenerate forms which raise, before anything else, problems of perception for their three or four interpretations. This difficulty is literally multiplied when active and passive interpretations are put into correspondence. There exists, however, a classical question: how do languages without determiners express semantic notions such as 'definite', 'generic' etc.? The case of English may be interesting as an intermediate step between Russian, with no articles, and French with obligatory articles; but the metalanguage of GG does not allow the statement of such a problem.

4. ATTITUDES. Chomsky has chosen to develop a modern philosophy of mind in the context of cybernetics and automata, i.e. in the technological environment of electronic computers. He had to base his speculations on some new theory of language, since no conventional theory was available to support his views. In this way, generative theory has developed in a way that converges toward a demonstration of Chomsky's views about mind. This mode of thinking about language and mind has well-known antecedents in the Middle Ages, when emblematic concepts were already subject to sophisticated reasoning. A passage in Dreyer ([1906] 1953:234) about Roger Bacon is quite pertinent to our discussion:

'The scholastic doctors also, after the manner of the ancients, talked finely about experience as the only safe guide in the visible world. But it began and ended in talk; they did not find a single fact in natural philosophy, they did not determine a single value of any astronomical constant.'

This attitude begins to affect phonological studies as well. These differ sharply from syntactic studies because of a simple combinatorial argument: when a natural language is looked upon as a set of sequences of phonemes, the number of its

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(c) Beavers need dams.
   Dams are needed by beavers.

Here Chomsky's interpretation problem has either disappeared or changed entirely. I maintain that, given such lexical variation, the 'problem' discussed by Chomsky does not exist. One should at best consider it a phenomenon of non-local dependency between a verb and the determiner of its object.

generators is of the order of magnitude of 50 (phonemes); but when the same language is considered as a set of sequences of words, the generators are of the order of 10^5 (words). As a consequence, lists of combinations of phonemes from many languages have been regularly compiled and analysed, but syntacticians have never thought it possible to accumulate significant data in the form of large lists of combinations of words, i.e. lists of sentences or of sentence types. This hesitancy in the face of large amounts of data is unjustified; note that the size of such lists would be considerably smaller than the number of pictures taken daily from bubble chambers and analysed by physicists (Gross 1975). Chomsky & Halle 1968 and Lightner 1972 advocated recourse to systematic data; but the progressive introduction by their students of unlimited variations on the formalism may detach phonology from the empirical studies that should be triggered by the new theories. Today there is no longer any distinction between generative syntax and a substantial part of generative phonology; the sole object of both is symbolic manipulation of a few well-known facts, intended to show that the human mind can be reduced to a formal class of abstract automata. The universities which have built linguistic departments with the aim of increasing knowledge about language now find themselves equipped with philosophy departments of a strange specialization, perhaps not altogether desirable. Linguistics has vanished.

There are other reasons, more sociological than philosophical or technical, that have led to this state of affairs in the domain of linguistics. Emphasis on abstraction, i.e. on a purely abstract activity, is an organized reaction against behavioral attitudes which, mainly in the United States, long kept psychological (but not linguistic) studies at a superficial level. Today it is clear that Chomsky's reaction has had devastating effect and that he underestimated the effect of his criticism, perhaps more polemic than scientific. His criticism, blindly accepted, overwhelmed the field, and set up generative linguistics as the dominant school. A particular result is that any attempt or even proposal to collect systematic data is instantly qualified as anti-theoretical, and eliminated from what has been institutionalized as linguistics. Such work would be criticized as having no explanatory value for any significant problem in linguistics.

At the same time, linguists have acquired a degree of snobbery that leads them to prefer handling a prestige vocabulary to painstaking experimental work. Brilliant dissertations, sprinkled with decorative symbols and equations, can be composed on such deep themes as a determination of theoretical and empirical conditions that should be met by Universal Grammar. Meanwhile, the ingenuity and concentration of efforts necessary to classify large numbers of structures do not lend themselves to the practices developed by pure theoreticians. Concrete effects of this attitude are visible. Normally, a specialist who invents some abstract mechanism should propose some way to verify its adequacy, or verify it himself; this can and should be done by applying the mechanism to all relevant parts of well-studied languages. This elementary rule is almost never followed. The justification of this system is supposed to be identical to the division found in physics between theoretical and applied or experimental research. To the extent that this view is meaningful, it might be justified by the enormous dimensions of the domain, but it is in no way thinkable for a field as narrow as English syntax or as ephemeral as trace theory; it

takes only a few hours to extract from a dictionary the verbs that have no passive. An experimental scientist is perfectly willing to spend a few weeks or more at such an elementary but essential task. Given this incredible rejection of experimental work, the majority of studies published so far in generative linguistics would never have had access to international journals, if specialists in natural sciences had evaluated them.

To conclude this discussion of dogma in GG, let us note that generative linguists seem trapped in the dilemma of the chicken and the egg (Popper 1963:47). They insist constantly on the truism that accumulating data without prior ideas or theories is a senseless activity, hence the necessity to elaborate theories first. This type of criticism has been applied to the taxonomy of linguistic forms; but if it had been applied to biology or physics, Hooke would have been forbidden to look into his microscope, or nuclear physicists to use particle accelerators. Fortunately, such questions are raised only rarely by working scientists. A scientist who accepts the theories of electromagnetism and of bubble nucleation will nevertheless search literally millions of images in order to find particles for which he has no theory. Nobody will deny that theories are necessary in science, not even firm behaviorists. But the result of adherence to the pseudo-Cartesianism that gives anteriority or innateness to theories has been that none of the many generative theories proposed so far has been used to explore a given language (English, for example) as completely as possible. Linguists have been totally unaware of the converse of their rule, a truly Cartesian rule: there is no possible theory without concomitant accumulation of data.

5. MATHEMATICAL LINGUISTICS. Studies in mathematical linguistics constitute an important ideological background for GG, for they permit linguistics to be placed on the same rank as physics on some evolutionary scale that values theories with respect to their level of abstraction. Once the relevance of such studies is accepted, they indirectly justify recourse to an unlimited range of formal devices. Thus their evaluation will complete my discussion of GG.

Since Chomsky's studies on the classification of formal languages, the power of formal grammars has been largely identified with their adequacy in descriptive syntax. Various questions can be raised about this now common way of thinking.[14] In particular, various applications of undecidability theorems were carried out in the early days of mathematical linguistics. Their interpretation has been questioned by Chomsky himself (1965:60–62). Nonetheless, the notion of undecidability has permeated the reasoning of many generative linguists, and is still influencing them by encouraging a search for formal developments.

One of the main uses of undecidability theorems in GG concerns the deletion of strings, as commonly used in syntactic analysis. There have always been discussions about the validity of analysis by ellipsis in grammar. Deleted strings cannot always

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[14] For the correctness of adequacy proofs, see Gross (1972:125–8). Insofar as automatic syntactic analysis is concerned, analysers have always used variants of context-free (CF) grammars with a certain degree of success (from the syntactic point of view, but not in planned applications). In the same way, the notational variant of CF grammars proposed by Harman 1963 seems adequate at the descriptive level, at least for English; for French, the grammar effectively constructed by Salkoff 1973 is a convincing proof of the potentialities of CF grammars.

be defined as certain sequences of well-determined words; reconstruction is possible, but only up to a paraphrase. For example, sentence 29a must sometimes be analysed as a reduction of 29b:

> (29) a. Max loves wine.
>   b. Max loves to drink wine.

But then the question is: why delete *to drink* rather than *to degustate, to swallow,* or *to gulp,* or even *to drink often, to consume regularly* etc.? In fact, there is no upper limit on the length of the strings that can be reconstructed to make explicit the usual meaning of 29a.[15]

Because of deletion rules, transformations have the form $x \rightarrow y$, with no length condition on x and y. The class of transformational grammars is thus equivalent to the class of semi-Thue systems. In other terms, natural languages are recursively enumerable languages of the most general kind. Whence an important idea for GG: if analyses by ellipsis could be restricted, perhaps even eliminated altogether, natural languages would be mathematically more specific.[16]

From an empirical point of view, the only deletions that have been accepted by all linguists are deletions of grammatical morphemes (prepositions, articles etc.); ellipsis of complex strings remains controversial. But it is quite difficult to draw a dividing line between the two types of deletions. At the same time, allowing even small amounts of deletion in a class of recursive grammars changes the CLASS to the general class of recursively enumerable grammars. Thus the belief in the validity of such arguments, namely the belief in the relevance of the general notion of recursivity to the description of natural language, has led various authors to abandon all analyses by deletion.[17]

This state of affairs appears to have led Chomsky and his followers to give up practically all transformational descriptions, and to replace them by phrase-structure analyses, in which relations of interpretation are defined that specify the meaning of the forms. An example of this move is the following: sentence 30a is usually derived transformationally from a source like 30b:

> (30) a. Max told Leo to leave.
>   b. Max told Leo that he should leave.

A rule called Equi-NP Deletion reduces 30b to 30a, if the two NP's (*Leo* and *he*) are coreferential. But it now seems that, in order to eliminate the deletion process from the grammar of English, 30a will be generated by phrase-structure rules, and an interpretation rule will state that *Leo* and not *Max* must be understood as

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[15] In different contexts, the meaning of 29a may change considerably. If Max is a waiter who gets higher tips when he serves wine, it will mean 'Max loves to (sell + serve) wine.'

[16] Detailed studies have been performed by Peters & Ritchie 1973 on the definition of formal constraints on deletions. Particular constraints allow certain analyses by ellipsis, and at the same time restrict the class of natural languages. Nonetheless, deletions of a general kind seem unavoidable, and the amount of material to be deleted then goes well beyond the limitations that would render strictly recursive the grammars for natural languages (Gouet 1976).

[17] It might be feasible to organize the reconstructible sequence into an ordered graph in which only minimal elements could be erased; or one could attempt to define equivalence relations on the sets of reconstructed sequences. Such constructions might solve the difficulty, but no attention has ever been paid to these possible solutions.

subject of *to leave*. The advantages or inconveniences of this new type of description are not known. As is generally the case with every theoretical 'advance' in GG, only a severely limited number of examples have been worked out. This precludes any significant comparison or discussion that would go beyond the sterile and by now routine controversies. The only clear point is that a deletion has been eliminated, which should bring the CLASS OF FORMAL GRAMMARS FOR NATURAL LANGUAGES closer to the strictly recursive processes.

The following formal example shows clearly the difference that I wish to point out, between arguments made on CLASSES OF GRAMMARS and arguments made on SPECIFIC GRAMMARS. Consider the specific grammar which contains context-sensitive (CS) rules that generate the language $\{xcx : x \in \{a, b\}^*\}$, on the one hand, and the following transformational rule, on the other:

(31) $xcx \to xcv$; $x = uv$, $v \neq E$ (E is the null string).

This rule deletes initial u of the second occurrence of x, but not the entire x. Adding this particular rule to the CS grammar does not change it into an undecidable semi-Thue system. Also, the (unbounded) deleted string u is trivially reconstructible. Note that this example is not linguistically unreal, since it comes close to formalizing certain conjunction reductions. Thus no problem arises in terms of the specific grammar, whereas allowing this type of deletion for the whole class of CS grammars will certainly entail undecidability in the general case.

I emphasize that I am not trying to show that various notions of formal grammars are irrelevant to linguistics. On the contrary, I think such concepts (and others belonging to the algebraic theory of languages) should be mastered by every linguist. In the same way that trigonometry is studied by future navigation officers, differential equations by bridge engineers, etc., formal languages should constitute the main abstract background for linguists.

What I have tried to show in this section is that, in mathematical linguistics as well as in language studies, linguists have not directed their efforts at building and studying particular grammars, but at looking for abstract constraints on whole classes of grammars.

6. CONCLUSION. There is no doubt that this critique will, for certain readers, take on the appearance of a principled attack against new ideas; but the basic tenets of GG are now twenty years old. I wish only to remind the reader that respect for some older fundamental principles could have avoided many difficulties.

There is a whole tradition that considers linguistics an activity that should result in discovery of new epistemological frameworks or of illuminating revolutionary programs; this activity is closely related to construction of universal languages and of symbolic codes that would represent all languages with marvelous compactness. One can name many linguists who invested a lot of energy in such attempts, and who belonged to various philosophical traditions. Our most remarkable contemporaries in this respect are Marr with his reductionism and his four elements, and the numerous linguists who set up Saussure as a savior as a result of his apocryphal *Course*. Although one of the greatest Indo-Europeanists, Saussure was credited with the decisive 'discovery' of the arbitrariness of signs (analysed much more clearly in the Port Royal *Logic*, by the way); another component of his fame is the

celebrated dichotomy between synchrony and diachrony, which acted like pure magic: not the faintest arguments have been adduced to support it. Finally, let us recall Hjelmslev and his glossematics, whose simple-minded formalism (the re-discovery of Boolean algebra for kindergarten) permitted speculation about language quite independently of any data. It appears that much generative work is imprinted with these mystical aspects. It is well known that manipulating formulas of logical or programming languages triggers, in the minds of professionals, a compulsive feeling of satisfaction. Among linguists, this unhealthy feeling is reinforced by a belief that such mechanisms explain, in some deep (and as yet unfathomable) fashion, the functioning of human thought. And this belief is supported by the materialistic nature of the explanation: it is suggested that the formulas have a neuro-psychological translation, although there is not even the beginning of any plausible argument to support this new metaphysics (Blakemore 1977:137–41).

In this way, some old respectable domains of linguistic research have died such a death that it is not clear, at present, in what form modern studies can be resuscitated. If most of the intellectual investment made in formalistic thinking were to be abandoned, then many empirical problems of significant size could be approached and solved. I have mentioned a few of them that have deep roots in language phenomenology. As in all sciences, linguistic investigation must generate newer questions, suggesting less and less sketchy theories. Only such an approach permits progress in knowledge about language; it clearly has no relation to current exercises in formal logic.

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[Received 11 August 1978.]

★ = "Descriptions" of grammar & applicability to French — p. 859, bottom.