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The Central Question in Comparative Syntactic Metatheory

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Abstract: Two kinds of theoretical framework for syntax are encountered in current linguistics. One emerged from the mathematization of proof theory, and is referred to here as generative-enumerative syntax (GES). A less explored alternative stems from the semantic side of logic, and is here called model-theoretic syntax (MTS). I sketch the outlines of each, and give a capsule summary of some mathematical results pertaining to the latter. I then briefly survey some diverse types of evidence suggesting that in some ways MTS seems better suited to theorizing about the relevant linguistic phenomena.

1. Preliminaries

Syntactic metatheory is the metascientific evaluation of competing frameworks for theorizing about human language syntax. The question I regard as most central, in that it arises at a level more abstract than any comparison between rival theories, concerns the choice between two ways of conceptualizing grammars. One has its roots in the mathematicization of logical proof as string manipulation in the early 20th century, and the other springs from a somewhat later development in logic, namely model theory. In this article I sketch the outlines of each, and sketch some reasons for believing that most current syntactic theorists have (unwittingly) made the wrong choice.

The question I explore here emerges most clearly when we consider how syntactic theory is to be formalized. Since explicitness has no enemies, it is puzzling that formalization should have so few friends. But some of the bad press that formalization has attracted (see Ludlow, 2011, pp. 162–70, for example) is due to people confusing it with at least three distractors: (i) Hilbert's programme for reducing mathematical truth to truth in a decidable formal logic, (ii) Carnap's programme for eliminating dubious appeals to meaning by building proper uses of terms into the syntax of a formal language, or (iii) the mindless translation of claims

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into the symbolic hair shirt of some hard-to-read proprietary notation. None of these have any relevance here.

All that I mean by formalization is the use of appropriate tools from mathematics and logic to enhance explicitness of theories. This is how most scientists use the term today (see, e.g., Grafen, 2009 on evolutionary theory). Any theoretical framework stands to benefit from having its content formalized. This is true not only for those linguists who call their work 'formal linguistics' but just as much for the linguists who claim to deprecate it.

2. Generative-Enumerative Syntax

The approach to the formalization of grammars that has dominated linguistics for half a century originates with the mathematical logician Emil Leon Post. In his 1920 dissertation (published in 1921), Post set out to mathematicize logical proof, i.e. syntactic derivation of formulae from other formulae, in terms of meaning-blind systematic manipulation of symbol strings. (Consistency of theories was still provable, but not in terms of existence of a model: because of the familiar fact that from a contradiction you can derive everything, Post was able to define a consistent set of formulae as one for which there exists a formula that is not derivable from it.)

Post viewed inference rules as operations licencing the composition of a new string from parts of a finite set of given ones, the given strings being either initially given or already derived. These string-manipulation operations are called **productions**. A production consists of a set of one or more patterns to be matched by old strings plus a specification of how to make a new string from their parts. Post developed the idea very abstractly, but it can be readily understood by looking at a an example such as how to state Modus Ponens. The production expressing Modus Ponens would say this (where the W_i symbols are free variables over substrings):

(1) Given a string that matches $\vdash (W_1) \rightarrow (W_2)$
and a string that matches $\vdash (W_1)$,
add a new string that matches $\vdash (W_2)$.

If '$\vdash (p \vee q) \rightarrow (\neg(r))$' were either an axiom or already derived, and likewise '$\vdash (p \vee q)$', we could let W_1 cover '$p \vee q$' and let W_2 cover '$\neg(r)$' and thus licence the generation of the new string '$\vdash (\neg(r))$'.

Post understood that a set of productions in his sense could define any collection of strings that had a finite membership definition, and he knew that the collections defined would not necessarily have a decidable membership problem. In other words, he had already provided, in 1920, a system for defining arbitrary sets of the type later characterized by Turing (1936) and now called **computably enumerable** (henceforth CE). Such systems would become known 35 years later as generative grammars.

He also proved the first two theorems about expressive power of such grammars. If we allow the total set of symbols available to productions to be a finite set $V = V_T \cup V_N$, where V_T contains the symbols appearing in generated strings (now called **terminal** symbols) and V_N is an additional disjoint set (now called **non-terminals**) that can appear in intermediate strings but not in the final generated strings, a radical simplification of productions is possible. Post (1943) proved that any CE set over V_T can be generated by some finite set of productions over V each of which has this form:

(2) Given a string that matches $x\ W$, add a new one that matches $W\ y$.

Thus productions do not need to be able to do anything more than simply erase a specified substring x from the beginning of the input string and tack a specified string y on the end. Later (1947) Post proved that the format in (3) has the same expressive power:

(3) Given a string that matches $W_1\ x\ W_2$, add a new one that matches $W_1\ y\ W_2$.

This is the rule format that Chomsky (1959, P.143) calls 'type 0'.

Chomsky cites Post only in connection with the use (in a rather different context) of the term 'generate' (Chomsky, 1959, p. 137n). However, later Chomsky proposed reinterpreting the word 'generative' to mean simply 'explicit' (Chomsky, 1966, P. 11), implying nothing more than going 'beyond traditional grammar in a fundamental way' so that no 'essential appeal to the intelligence of the reader' is made. This confusing move shifts the focus from a specific type of rule system, which has been the subject of controversy, to the anodyne methodological virtue of exactness and completeness, which has not. To avoid any confusion, from now on I will follow Pullum and Scholz (2001) in using the term **generative-enumerative syntax** or **GES** for syntactic systems involving generative rules in the style of Post.

Chomsky's own contribution to the study of production systems, and it was an important one, was to show that by placing tighter restrictions on the form of GES rules it was possible to restrict the generable sets to interesting proper subsets of the CE stringsets. For example, requiring y in (3) to be no shorter than x ensures that the stringset generated will be decidable (in fact, that it will fall within a proper subset of the decidable stringsets now called **context-sensitive**); requiring x to be a single member of V_N yields the extremely important class that came to be known as the **context-free** stringsets; and requiring also that y be a single member of V_T with or without a single following member V_N yields the **finite-state** (or **regular**) stringsets defined by Kleene (1956).

In the kinds of GES grammar studied in Chomsky (1959) a derivation starts with a single nonterminal — a specified symbol in V_N — and expand it repeatedly until the result contains only members of V_T. I will call grammars of this sort **expansion-oriented**. All of the work executed or inspired by Chomsky up until the end of the 1980s assumed expansion-oriented grammars. But there is an alternative kind of grammar in which a derivation starts with a selection of elements of the vocabulary V_T and combines them to produce larger and larger complex objects until eventually a string of a goal category representing a completed sentence is produced. I will call these **composition-oriented**.

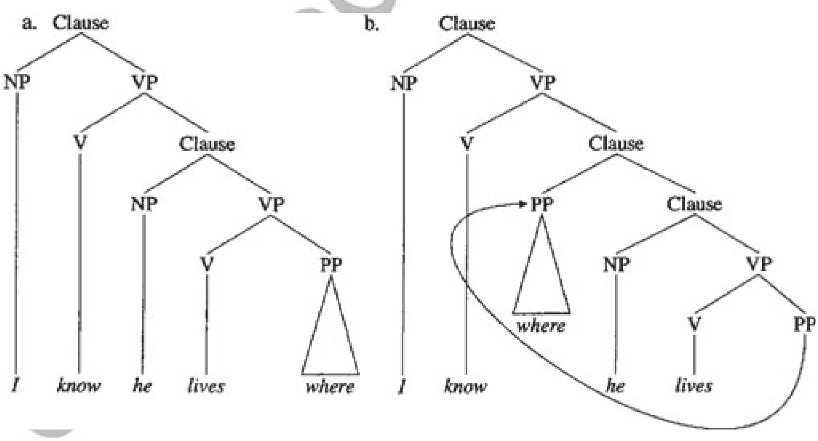
Post's production systems are general enough to encompass these too. They are represented in modern linguistics by categorial grammar (as exploited in Montague (1973)) and the more expressive combinatory categorial grammar of Steedman (2000). In these frameworks derivations start with with some lexical items labelled with categories that determine for each item what it can combine with, and the derivation proceeds through a series of combininations of words with other words, or with units already formed, until a unit of the designated sentential type has been built.

Chomsky made an abrupt switch from expansion-oriented to composition-oriented generative grammars when he introduced his 'Minimalist program' (Chomsky, 1993). But if the formalization of Chomsky's proposals by Stabler (1997) is true to his intent, his grammars are far more expressive than standard categorial grammars or Steedman's combinatory ones (this point too will be relevant below).

GES frameworks formalize grammars as nondeterministic set enumerators. That is, a GES grammar provides an implicit functional definition of a set *L* of symbolic objects, and does it by providing a method or programme for random construction such that for every object in *L* there is a way of constructing it, and everything that can be constructed is in *L*. The finite sequence of symbolic objects obtained in the course of constructing a member of *L* is referred to as its **derivation**.

The fact that derivational steps come in a sequence has encouraged the practice of talking about them in procedural terms. Although this is merely a metaphor, it has come to have a firm grip on linguists' thinking about syntax. The structure seen in (4b) is supposed to be derived from an underlying structure more like (4a). The derivation is conceived of dynamically, as if 'first' there is (4a) and only 'later' is (4b) produced from it by the 'movement' suggested by the curved line (which is purely expository, not a part of the structure).

(4)



Even those who (like Chomsky) believe GES grammars exist as mental inscriptions do not assume that a representation of (4a) pops up in the mind to be replaced a few nanoseconds later by a representation of (4b). The idea is, rather, that a full structural description of the sentence *I know where he lives* must include the information represented in (4a) as well as that represented in (4b). Nonetheless, linguists maintaining GES assumptions constantly speak about derivations in temporal terms: a constituent originates over here, and then later in the derivation gets moved past this and up to here . . . The pervasive dynamic metaphor is deleterious to clear thinking about what the phenomena are and what our explanation of them is supposed to be.

The boundary defining membership in a set is sharp. An object is either in the set defined by some GES grammar G (if G provides a derivation that constructs it) or out (if there is not). The grammar does not define any vague penumbra of objects that are nearly but not quite in the set. And L has an exact cardinality: it contains either some finite number of objects or a countable infinity of them.

A finite bound on expression size is entailed for members of L: every generated object must be finite in size, because derivations have to complete in a finite number of steps. No evidence supports the claim that English couldn't have an infinitely long sentence, or could tell against it; it is simply stipulated.

A GES system attributes syntactic properties only to generated objects. An object that is not generated is not characterized in any way. The grammar gives it no derivation, so it has no phrasal structure, no phonological or phonetic properties, no semantic properties, and no lexical items contained in it. So consider a question once posed by former president George W. Bush:

(5) **Is our children learning*?

No correct grammar for Standard English would treat this as well formed, so a GES grammar for English would not generate it. Such a grammar would therefore claim that is not an interrogative clause, does not contain a plural noun phrase, does not begin with a vowel, and does not make any reference to immature human beings. The grammar cannot make any claims about (5), because it is not generated.

A key property of the GES conception of grammars (an undesirable one, I will argue later) is that it makes syntactic properties depend crucially on lexical ones. Only in virtue of the existence of certain lexical items can there be any syntactic (or phonological or semantic) properties of an object. In order to be generated, an object must have a completed derivation, and it must result in a sequence of items that are present in the lexicon. This is true both of expansion-oriented and composition-oriented GES grammars: for the former, a derivation has to complete by introducing terminals so as to complete a string entirely composed of symbols in V_T. And for the latter, composition can only start with items selected from V_T.

First language acquisition is naturally represented in GES terms as successful identification of a particular GES grammar: the infant's task is to take as input a bounded and essentially random sequence of observed utterances and to produce as output a GES grammar generating a specific unbounded set of expressions corresponding to the right generalization from the data. This is the conception argued for in Chomsky (1965, Chapter 1), and it suggested to E. Mark Gold

(1967) the mathematical problem of determining the conditions under which this is possible in principle. Gold's results were extremely pessimistic, which led some (e.g. Matthews, 1984, 2006) to conclude that language acquisition is impossible without innate knowledge of grammatical principles. (It was assumed that such innate knowledge would render the task possible; ultimately, of course, that would need to be demonstrated.)

With this very brief review of some characteristic properties of GES grammars, let us turn to an alternative way of conceptualizing grammars.

3. The Model-Theoretic Alternative

There are alternatives to the idea of formalizing grammars in GES terms. Here I discuss just the one that is the most thoroughly articulated and, in my view, the most promising.

Since the 1950s there has emerged a mathematical theory connecting logical languages to what they talk about. The original idea was to endow logical formulas with meanings in an exact way, but we can deploy the same mathematical tools in a different way: we can describe sets of structures, by taking a set of formulas to be a description of all the structures (within some predefined class) that satisfy all the formulas. A description of English would be given by a set of formulas iff (i) all of the formulas are true of every grammatical represention for a well-formed sentence of English, and (ii) any representation for a non-well-formed sentence of English fails to satisfy at least one of the formulas.

The grammatical expressions of a human language such as English, together with their syntactic structure (and recall that I regard them as actually existing objects with inherent structure), can be idealized mathematically as **relational structures**. Each expression will correspond to some mathematical object consisting of a set of points called the **domain** and a collection of relations defined on the domain. For example, the tree in (4a) is a relational structure where the 14 nodes are the domain, the category labels (Clause, NP, VP, V, PP) are unary relations (properties of nodes), and the binary relations that linguists usually call dominance and precedence (in addition to identity) are defined. There is a node labelled Clause that dominates all nodes in the tree; it immediately dominates a node on the left labelled NP and a node on the right labelled VP; and so on.

We could ask: What is the simplest and most elegant set of axioms that is satisfied by those structures that are appropriate representations for grammatical English sentences, and thus in effect characterizes grammatical well-formedness for English?

This question is the basis of the **model-theoretic** approach to syntax, henceforth **MTS**. In essence it is defined by the acceptance of three simple propositions:

(6) a. All rules are **constraints** — **truth**-eligible statements about the internal structures of expressions.

b. Grammars are **unordered sets** of such constraints (theories, in the logician's sense).

c. Well-formedness of a structure is determined by **satisfaction** of the constraints.

3.1 Description Languages

To define an MTS framework we must (inter alia) fix a **description language**: a logic whose formulas will be used to state constraints on structures. Separately from that we must define a general type of structure for the logic to be interpreted on. Obviously, if we were to allow the equivalent of constraints saying ‘The structure is one of those that the GES grammar $G = \langle \cdots \rangle$ generates,’ (where ‘$\langle \cdots \rangle$’ is a full spelling out of some GES grammar G), we would have a one-clause MTS description of any CE set, though satisfiability might be undecidable (because the problem of whether an arbitrary GES grammar generates anything is undecidable), and there would be no differentiation at all between GES and MTS.

But MTS descriptions can be given in terms that are much more restrictive than this, in interesting and promising ways. We can define description languages that look at syntactic structures internally rather than externally: instead of quantifying over whole structures, they quantify over a domain contained within a structure, and make reference only to certain specified relations in it (compare the conception of modal logic advocated by Blackburn *et al.*(2001): that it is a description language for relational structures as seen from the inside).

Some remarkable results have been obtained in recent years concerning the expressive power of different description languages on various types of relational structure. A survey is provided in Pullum *et al.* (to appear). Here I merely note that as we move to more powerful and expressive description languages we find that larger and larger ranges of structures become describable. In the most basic and primitive description languages worth considering, the statements are simply atomic propositions stipulating bans on local configurations On strings, a statement might say something like ‘An *a* following a *b* is not allowed’. On trees, we might have statements like ‘No *A* node is allowed to be the parent of two nodes both labelled *B*’. Allowing boolean connectives (‘and’, ‘not’, ‘or’, ‘if’, etc.) adds power, and makes new sets of strings or trees describable. Adding first-order quantifiers increases expressiveness yet more. And allowing second-order quantification over finite sets yields a description language about as powerful as any that are commonly studied: it is known as weak monadic second-order logic, henceforth **MSO**.

A couple of specific results about MSO are worth noting. Around 1960 it was proved independently by Richard Büchi, Calvin Elgot, and Boris Trakhtenbrot that MSO on string-like structures characterizes precisely the finite-state stringsets (the proof of this theorem is now a standard topic in textbooks on finite model theory, e.g. Libkin, 2004). That is, a set of strings can be recognized by a strictly finite string-recognizing automaton if and only if it is the set of all and only the finite string models that satisfy some statement in MSO. This gives us a characterization of the finite-state stringsets in purely logical terms, without reference to GES grammars or automata.

Doner (1970) extended this to 2-dimensional trees, proving that MSO on trees is equivalent to finite-state tree automata. That is, a set of trees can be recognized by a strictly finite machine that crawls through it inspecting the nodes if and only if it is the set of all and only the finite trees that satisfy some statement in MSO. This gives us a characterization of the recognizable tree-sets in purely logical terms. And since it has been known since the late 1960s that the string yield of a recognizable tree-set is context-free, we also have a purely logical characterization of the stringsets generated by context-free grammars.

The range of theories that have been shown to be (weakly or strongly) equivalent to context-free grammars is wider than most linguists realized. Generalized phrase structure grammar (GPSG) as introduced by Gazdar (1981) was strongly context-free, and was intended to be. Marcus (1980) devised a kind of parser intended to permit the computer implementation of transformational grammars as it existed in the 1970s, and assumed at the time that it had greater expressive power, but Nozohoor-Farsi (1986, 1987) showed that it could parse only context-free stringsets. The government-binding theory of the 1980s (stemming from Chomsky, 1981) was likewise thought to be of greater expressive power than GPSG, but Rogers (1998) obtains a truly remarkable result: by reducing the principles of the theory to constraints on trees stated in MSO, he shows that it was strongly context-free, assuming the relativized minimality principle of Rizzi (1990) and the locality theory of Manzini (1992).

Nothing greater than the power of context-free grammars seems to be needed for English (see Pullum and Rawlins, 2007) for a response to one argument that formerly looked convincing). But Zurich Swiss German has been argued fairly convincingly by Shieber (1985) to fall outside the context-free class. We therefore need to ask what might be done to find a description language that would permit description of non-context-free stringsets. The answer is that we do not need to change description languages: we can simply generalize the class of models.

Rogers (2003) shows how to generalize the progression from 0-dimensional structures (expressions taken as atomic) through 1-dimensional (strings) and 2-dimensional (trees). The three can all be seen as singly-rooted tree-like structures: trivial one-node trees, unary-branching trees, and standard planar trees. For strings we need (in addition to identity) a single binary relation symbol $<_1$ corresponding to the one dimension in which a pair of nodes can be adjacent. For 2-dimensional trees we need two, $<_1$ (immediately precedes) and $<_2$ (parent-of). Rogers generalizes to all positive *n*.

Describing *n* -dimensional tree-like models calls for *n* binary relations. Just as in a 2-dimensional tree a node may bear the $<_2$ (parent-of) relation to an entire 1-dimensional object (the string consisting of all its child nodes), so in a 3-dimensional tree a node may bear the $<_3$ relation to an entire 2-dimensional tree. Rogers shows how a 1-dimensional string of terminals may be obtained from a structure of any dimensionality $n \geq 1$. He further proves that this yields a strict hierarchy containing infinitely many MSO-characterizable stringsets. It is essentially the same one discovered much earlier by Khabbaz (1974), recaptured in a different form by Weir (1992) as the ‘control hierarchy’.

The ultimate source for Weir's work is Carl Pollard's dissertation (Pollard, 1984). Starting from the insight that context-free grammars compose constituents by juxtaposition of pieces, Pollard generalized it by allowing composition of other sorts. For example, assuming strings each having a designated element H called the head, a string *wHx* with head *H* might be combined with a string **yHz** with head **H** not just by concatenating, to get '**yHz***wHx*' or alternatively '*wHx***yHz**', but by splitting *wHx* adjacent to its head and wrapping it around **yHz** to get '*w***yHz***Hx*' or alternatively '*wH***yHz***x*', or by wrapping **yHz** around *wHx* to get '**y***wHx***Hz**' or alternatively '**yH***wHx***z**'. Weir develops Pollard's idea in full generality to get what he calls the control hierarchy. Rogers (2003) then shows how to capture it model-theoretically.

A particularly interesting correspondence for linguists is the one that Rogers obtains for $n = 3$: on 3-dimensional tree-like graphs, MSO yields equivalence with an interesting class of grammars called the **tree adjoining** grammars Joshi (1985), which turn out to have the same generative capacity as Steedman's combinatorial category grammars. This class has many of the desirable properties of context-free grammars (e.g. deterministic-polynomial recognition, i.e. tractable parsing), but sufficient additional expressive power to embrace Zurich Swiss German, or to describe unbounded phrasal reduplication. The equivalence involves not just the word strings but the tree structures, since 3-d tree-like graphs can be related to derivation trees of a tree adjoining grammar.

Further results have been obtained concerning the much more elaborate class of stringsets characterized in GES terms by Stabler (1997), whose goal is to render precise the ideas adumbrated in Chomsky (1995) and subsequent work under the banner of 'the minimalist program' (Graf, 2010, p. 74 gives a terse summary). The class of grammars Stabler defines turns out to capture what in GES terms would be the entire infinite union of the control hierarchy. That is, we get all of the stringset classes that are string yields of MSO-characterizable classes of finite n -dimensional tree-like graphs. This, as shown by Michaelis (2001), coincides with an independently defined class of powerful GES grammars known as **multiple context-free grammars** (MCFGs, introduced by Seki and Fujii, 1991). So the hierarchy in terms of model dimensionality looks like this:

(7)

DIMENSION	MODEL-THEORETIC	STRINGSET-THEORETIC
0	MSO on points	finite
1	MSO on strings	finite-state
2	MSO on trees	context-free
3	MSO on 3-d tree-like graphs	tree adjoining = combinatory categorial
4	MSO on 4-d tree-like graphs	(see Rogers, 2004)
⋮	⋮	⋮
n	MSO on n -d tree-like graphs	minimalist (Stabler, 1997) = MCFG (Seki and Fujii, 1991)

The point of this highly compressed summary of some MTS-relevant mathematics is to note that we have a large set of results concerning the results of using various description languages to give model-theoretic characterizations of sets of structures of different sorts, and there is ample descriptive power to permit coverage of anything in human languages that we know about. We have suitable machinery with which to construct theories of syntax that make no use of GES mechanisms whatever.

3.2 MTS Frameworks in Recent Linguistics

Some theoretical frameworks in linguistics that are already in use clearly instantiate MTS principles. Early attempts at sketching something like MTS included Lakoff's botched reformulation of generative semantics (1971; see Pullum, 2007) and the relational grammar of Perlmutter and Postal (1977). Arc Pair Grammar (Johnson and Postal, (1980) originated as a formalized version of the latter, and Chapter 14 of Johnson and Postal (1980) sets out the earliest serious discussion of some of the consequences of MTS, and is an important source for the discussion in the next section, where I will discuss it more.

Lexical Functional Grammar (Bresnan, 1982) has been described by one of its co-developers, Ronald Kaplan, in clearly MTS terms (see especially Kaplan, 1995), but one aspect of the framework appears to remain inexpressible in MTS terms: the device of constraining equations. Blackburn and Gardent, 1995 provides some illuminating discussion about formalizing the framework in MTS terms, but leaves the matter of constraining equations unresolved.

HPSG ('head-driven phrase structure grammar') in its more recent variants (see e.g. Pollard, 1999 and Ginzburg and Sag, 2000) adopts the model-theoretic approach quite clearly. Construction grammar, as described in fairly precise terms by Kay (2002), appears to do likewise: although Kay uses the term 'overgeneration' (2002, fn. 13), this appears to mean nothing more than allowing for objects to satisfy the grammar when they should not.

Optimality Theory is often assumed to fall into the MTS class, but I think it does not. There are constraints; but the constraints are ranked, and putatively universal, with the ranking doing all the work of distinguishing one grammar from another (apart from what is done by the lexicon), and crucially the constraints are not taken to be simultaneously satisfiable. Indeed, it is standard for them to constitute an unsatisfiable set. Instead, a pool of candidates is defined by a 'Gen' function (which in effect is a GES grammar, as its name might suggest), and on the basis of a kind of tournament between candidates to see which come closest to satisfying the more highly ranked constraints, a set of winning 'optimal' candidates is skimmed off, and the entire edifice functions exactly as if the set of winners was generated by a GES grammar.

4. The Natural Consequences of MTS

There are certain natural consequences of MTS, in the sense that they seem to follow naturally from MTS assumptions if no tweaking is done to avoid them (though such counterproductive tweaking would of course be possible). Among these natural consequences are some very interesting theoretical claims that I want to argue are just the right ones.

4.1 Sets and Boundaries

An MTS grammar is a logical theory: a set of truth-eligible statements. A structure is defined as well-formed by a grammar iff it satisfies the statments that make up the grammar. If we take those statements to be separate and independent, we get a picture that is radically different from the GES one. This is primarily because an MTS grammar is not an implicit functional definition of a single set with sharp edges and a defined cardinal number (though we can stipulate definitions of such sets as we may need for theoretical purposes, e.g. to prove the correspondences listed in (7)). It is perfectly possible for a structure to satisfy all the constraints except one, or to satisfy all the constraints except at one node where a single constraint is violated. Thus the notion 'almost grammatical but not quite' is fully coherent (whereas the notion 'almost generated but not quite' is not).

Furthermore, nothing requires us to pick one of the definable sets of structures and equate it with the human language we seek to describe. Given a class M of eligible structures to be models for a set of constraints Γ, there will be a unique set containing all and only the structures in M that satisfy Γ; but which class we choose as our M is quite a different matter from determining the formulation of the constraints in Γ. The work of the grammarian is the latter, not the former. Stipulating a model class will be a separate matter determined by the theoretical goal at hand. One might want to set M to be a finite class large enough to include structures for all of the sentences that have ever appeared in *The New Yorker*, so that it would be possible to use the arithmetic of finite sets on the entire class in some statistical corpus investigation. For a different purpose one might want to set M to be the set of all finite trees, or perhaps even all trees whether finite or not. It would depend on the purpose at hand, and a lot of the time it would not even matter.

A typical MTS grammar would therefore not say anything about how big expressions can be. (One could stipulate, if one wished, that there are not more than 37 nodes; a tedious and lengthy (but straightforward) first-order formula could do it. But absolutely no one thinks that would be a good idea, for 37 or any other number.) There is no need to ask whether we should regard billion-word sentences as grammatical but impossible for a human being to process: the grammar makes statements about what structural conditions must be respected, but it can be entirely agnostic on size.

A typical MTS grammar will therefore not even stipulate that expressions are all finite. In fact if the description language is first-order or weaker, such a stipulation is impossible: by an easy corollary of the Compactness Theorem, any first-order theory that has finite models of arbitrary size has at least some infinite models. Using MSO it would be possible to assert finiteness of models, but that doesn't mean such a stipulation is needed. No syntactic evidence bears on the question of infinite sentences, in either direction, and none ever could. MTS grammars leave it open, which is just as it should be.[1]

Language size similarly becomes a non-issue. A term like 'the set of all and only the expressions in the language' does not receive any definition in terms of the statements in the grammar. If a finite class of candidate structures is stipulated, there will be only finitely many expression structures that satisfy the grammar, whereas if an infinite class of candidate structures is chosen, there may be infinitely many models, but that too is a non-issue. A grammar will neither entail that the set of all expressions is infinite nor entail that it is not.

This gives us a different view of a strange dispute that has come to the fore in recent years concerning whether infinitely many expressions are grammatical in human languages. It has become standard to argue from lengthenability: because a clause like *That was very nice* can be lengthened to make *That was very very nice*, or *That was very very very nice*, and so on, therefore the set of all expressions must be infinite. Otherwise we have to assert the existence of a number *m* such that although *very*^{m} *nice* is grammatical, *very*$^{m+1}$ *nice* is not, and that seems absurd. MTS opens up a third way, discussed in more detail by Pullum and Scholz (2010): the grammar entails neither consequence. The model class can be chosen for particular theoretical goals, and can be set large enough to allow *very*$^{m+1}$ *nice* or not, with no consequences for the content of the grammar.

The issue of infinitude connects with Daniel Everett's controversial claim (2005) that Pirahã (a language of the Amazon basin) has no subordinate clauses, no coordination, and no iterated modifiers (as in *his brother's neighbour's wife's dog*). Some linguists have treated this claim as a calumny, as if the Pirahã people would be demeaned if it were true. But it has no implications for the ability of the Pirahã people to think or express propositions, because the expression of a proposition can always be broken up into separate simple clauses.

Even in English, spontaneous subordinate clause construction in conversation on everyday topics is rare. Frozen formulae predominate (*He said that*___; *You know*___ ; *It wouldn't surprise me if*___ ; etc.). Only fairly expert speakers go significantly beyond one-at-a-time construction of simple, fairly short clauses Pawley and Syder (2000). In Pirahã, or any other language spoken by a small and compact preliterate community, *all* language use is spontaneous conversation on everyday topics. Pirahã appears to have no propositional attitude verbs taking finite complement clauses, and no overt syntactic coordination with words like *and*, and modified constituents

[1] Langendoen and Postal (1984) took it to be a serious issue whether sentences of infinite length exist, and Katz (1996) concurred, in this journal; but I see no sensible issue here. The arguments given by Langendoen and Postal for the existence of infinite sentences—in fact non-denumerable sets of them—depend on unsupported assertions that natural languages, qua collections, must be closed under certain infinitary operations. No linguists have taken these arguments seriously. Under MTS the whole pseudo-question can be avoided. There could in principle be infinite expressions, if they met the right structural conditions, but the issue has no implications and grammars need not settle the matter.

do not occur as modifiers. But only the GES viewpoint forces upon us a choice between insisting that the set of all expressions in Pirahã is finite and trying to find some reason why it must be countably infinite. MTS avoids requiring us to answer meaningless questions of this sort.

It seems to me that the notion of 'a language' should not be regarded as scientifically reconstructable at all. We can say in very broad terms that a human language is a characteristic way of structuring expressions shared by a speech community; but that is extremely vague, and has to remain so. The vagueness is ineliminable, and unproblematic. Human languages are no more scientifically definable than human cultures, ethnic groups, or cities. The most we can say about what it means to say of a person that they speak Japanese is that the person knows, at least to some approximation, how to structure linguistic expressions in the Japanese way (with object before verb, and postpositions, and so on). But in scientific terms there is no such object as 'Japanese'.[2]

What we can be precise about is the structure of expressions. That is what grammarians study: expressions, not infinite sets of them. MTS encourages precision concerning the structure of linguistic expressions without suggesting that there is any particular scientific interest in the putative unique set that contains all of the expressions of the language and nothing else.

4.2 Gradience of Ungrammaticality

It is a substantive and potentially important consequence of MTS grammars is that they provide directly and immediately for a finely gradated classification of the structures that are *not* fully well-formed, as well as those that are.

It is accepted by essentially all syntacticians that judgments of well-formedness are not boolean: speakers judge badness of utterances in their language by degrees. Some (Schuetze, 1996 being a clear example) take this to be purely a consequence of performance: grammaticality is strictly all-or-none, and only acceptability, with its multiple-factor ætiology, exhibits gradience.[3]

But others explicitly maintain that it is grammaticality that is gradient, in the sense that there are degrees of failure to meet the standards that the grammar defines. For example, Lasnik and Saito (1984, pp. 266–269) confidently distinguish five degrees of ungrammaticality. They prefix sentences with '*', '?*', '??', '?', or no mark, and take these to be differences in ungrammaticality level, not mere unacceptability as

[2] If that is what Chomsky (1986) means by suggesting that 'E-language' (externalized language) is not a suitable object for scientific study, then one can only agree.

[3] Earlier work of Chomsky's such as Chomsky (1964) referred to 'degrees of grammaticalness', but the terminology is as ill-chosen: grammaticality is best thought of as perfect well-formedness; it is deviation from that standard that is a matter of degree. To paraphrase Tolstoy, all grammatical utterances are alike, but each ungrammatical utterance is ungrammatical in its own way. Chomsky's attempts at constructing a calculus of degrees of grammatical deviance were not successful; see Pullum and Scholz (2001).

a matter of judgment performance. For example, they give *What*$_1$ *do you believe the claim that John bought*___$_1$? (with *what* binding a variable that is the direct object of *bought*) the mark '?*', but give *Why*$_1$ *do you believe the claim that John left*___$_1$? (with *why* binding the reason for John's departure) the mark '*', noting that the former 'is a ''mere'' Subjacency violation and not an ECP violation' whereas the latter violates both Subjacency and the ECP. Lasnik (2004, pp. 219ff) makes similar discriminations and attributions of grammatical origin.

I believe Lasnik and others are correct in seeing ungrammaticality as a matter of degree. For those who accept this view, grammars should entail that ungrammaticality is a gradient matter. Defenders of pure GES do not appear to have noticed that GES grammars fail in this. They entail a sharp boundary between the perfect and the nonexistent, and do not even permit gradience in ungrammaticality to be represented. A pure GES grammar either generates it, thus claiming that it is perfect, or does not, thus claiming it is nonexistent. There is nothing in between; no degrees of deviance, partial waivers, or near approaches to grammaticality are defined by the grammar itself.

MTS grammars, on the other hand, automatically define a fine-grained classification of ungrammatical expressions, simply because it is perfectly coherent to say that a structure is almost well-formed but not quite, provided we keep the individual clauses of the grammar separate: some structures will satisfy most of them but not quite all of them at all nodes.

This does not stem merely from facts about the model class, of course. Thus the classification of ungrammaticality a grammar provides is not invariant under reaxiomatization. An MTS description will have overall truth conditions that are identical with the conjunction of all of its constraints, but if we take the grammar to be that conjunction, ill-formedness will be boolean. So MTS can define a boolean distinction between the perfectly well-formed and everything else if that is needed. But it can alternatively be stated as a set of separate constraints, which determines that ungrammaticality (not just unacceptability) will be massively gradient. This accords with the intuition that (for example) the series of examples in (8) exhibits progressively increasing ill-formedness (though of course the numbers of asterisks shown should not be taken as serious metric):

(8) a. *He is the chair of his department.*
b. * *He are the chair of his department.*
c. ** *Him are the chair of his department.*
d. *** *Him are the chair of he's department.*
e. **** *Him are chair the of he's department.*
f. ***** *Him are chair the he's department of.*
⋮ . . .

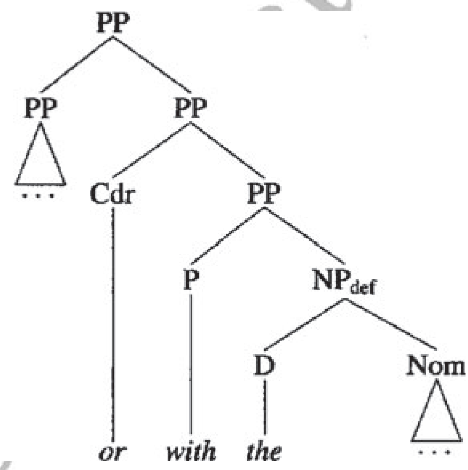
It is important that degrees of ungrammaticality are not just definable extraneously under MTS, they are defined by the grammar directly, in the form in which the linguist states it. An MTS grammar necessarily defines some structures as more grammatical than others, assuming only that the grammar has more than one constraint and/or the structure has more than one node. Ordinary finite model-checking suffices to locate any ill-formedness — assuming only that finite model-checking is decidable for the description language and class of models, which it is for all the description languages we have considered.[4] In the structure for (8b), for example, there would be a verb node marked with agreement for plural number or 2nd person, in violation of a constraint saying that it should bear the 3rd person agreement to match *he*. But the rest of the constraints in the grammar will be satisfied.

It is also worth noting that the theory outlined in Chomsky (1981), popular among GES syntacticians during the 1980s and beyond, was a hybrid system in which a GES subpart was filtered by an informally sketched MTS overlay: 'binding theory', 'Case theory', 'θ theory', etc., were constraints on the output of an underlying GES system. This did make it possible in principle to draw distinctions in grammaticality as opposed to acceptability, as Lasnik and others did. But the mechanisms involved were not present in transformational GES grammar before the mid-1970s, and have entirely disappeared again in the 'minimalist' GES framework of Chomsky (1995).

4.3 Fragments and Quasi-Expressions

Under MTS, fragments and quasi-expressions have syntactic properties. Under a GES account they do not. That is, while something like *or with the* is not generated by a GES grammar for English (and is not even a subconstituent of something that is generative), and thus gets no linguistic properties of any kind attributed to it by such a grammar, an MTS grammar can be used in a natural way to represent the undoubted fact that it has at least some syntactic properties. Consider this tree:

(9)

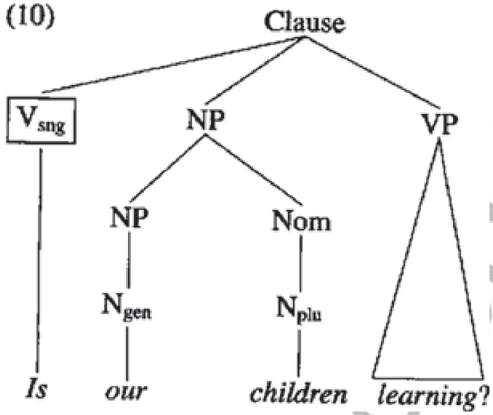


[4] Kepser (2004) describes an interesting case of a description language (designed for HPSG) which turned out to have an undecidable finite model checking problem.

This is an incomplete structure representing part of a coordination of prepositional phrases. The parts with '···' as their terminals represent material that might be present in a fuller structure, but it is the other parts that I am concerned with. Under quite obvious assumptions about the constraints of English syntax, it is fully compliant. The right-branch PP has *or* as left branch, the correct location for coordinators; the right-branch PP has the preposition head *with*, which has an NP complement, as is required; the left child of the NP is the definite article, and that is the right position for a determiner; the NP bears the definiteness feature, compatible with *the* ... Nothing about (9) violates any reasonable conditions on well-formedness in English. Thus an MTS account of syntax makes sense of the fact that we can to some extent process even as small a non-constituent fragment as *or with the*.[5]

A quasi-expression such as George W. Bush's * *Is our children learning*? can also be represented as having structure — perhaps as in (10).

(10)



This will not be generated by any correct GES grammar for English, so it will be assigned no status at all. But it is perfectly coherent to say that it satisfies nearly all of the constraints we would want to posit for a grammar of English, and that the one it fails to satisfy is the one saying that tensed verbs agree with the number feature of the subject NP's head noun, and that the node at which it violates that constraint is the leftmost, and so on.

4.4 Lexical Independence

An important consequence of MTS is that syntactic regularities are freed from dependence on the lexicon. That is, a statement of syntactic regularities that are

[5] This may also be the case with composition-oriented GES grammars like categorial and 'minimalist' grammars. If so, this is a rare case of a difference between composition-oriented and expansion-oriented GES grammars. Thanks to Ewan Klein for this observation.

not lexically conditioned can be given in a way that is entirely separate from the accidents of which words belong to the language at any given time.

For one example of how heuristically valuable this is, consider the case of a language like Pirahã (mentioned earlier), where there appear to be no complement clauses. An MTS account enables us to separate the issue of whether any sentences containing complement clauses exist from the question of whether there is a constraint excluding them. The syntactic constraints might allow for complement clauses exactly as in English, but if there happened to be no verbs currently in use that actually licence clausal complements (verbs like English *believe*, *conjecture*, *wonder*, *inquire*), sentences illustrating this syntactic possibility would be lacking. A verb with the relevant meaning might be introduced in a single act of lexical innovation, the result being that the language immediately had clausal complements, with no change whatever in its principles of syntactic organization. This strikes me as an interesting possibility, which GES seems to exclude.

But perhaps the most striking and straightforward observation about the independence of syntax from lexicon concerns the way we can understand expressions containing words we don't yet know. This point has been discussed by Postal (2004) in connection with the claim that the lexicon is open. Postal notes that the items that can appear in sentences may not even be denumerable: as Zellig Harris (1968, p. 11) pointed out, in utterances like *He went* ____ the complement of *go* can be any of an in-principle nondenumerable infinitude of utterable sounds. Postal adds that they might even include gestures or grimaces. But the more important issue seems to me to bear on the point made earlier by Johnson and Postal (1980, pp. 675–677) about how syntactic structure is to be described.

A number of different logicians and philosophers have remarked on our ability to grasp the structure of utterances in our native language that have lexical items unknown to us. For example:

(11) a. Lewis Carroll (*Alice in Wonderland*, 1865):
'Twas brillig, and the slithy toves
Did gyre and gimble in the wabe ...
b. Ogden and Richard (*The Meaning of Meaning*, 1923):
The gostak distims the doshes.
c. Carnap (*The Logical Syntax of Language*, 1934):
Pirots karulize elatically.

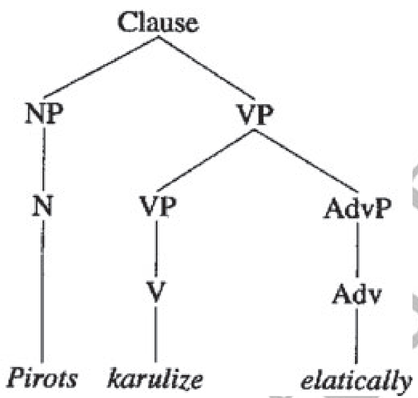
Such examples have often been quoted in the linguistics literature, but never to illustrate what seems to me to be the crucial point about grammatical description, which is that no GES grammar can provide any basis for an account of our ability to understand such sentences. They are not generated, because they are not even strings over the vocabulary of words used in the expressions that are generated (they are deliberately constructed to be). Yet we do understand them, not just as syntactically well-formed but as meaningful, with clear truth conditions.

We learn from (11b), for example, that doshes can be distimmed, and that at least one gostak does this. How could we grasp such things if our knowledge of our language took the concrete form of a mentally inscribed GES grammar?

Under a GES description it is not even clear why utterances like those in (11) are recognized as being linguistic material rather than just noise. An expansion-oriented GES grammar will not complete a derivation for any of them, and a composition-oriented grammar will not even get started — there can be no operation of combining *elatically* with *karulize* to produce a VP headed by the latter when neither is present in the lexicon so neither has any category.[6]

MTS offers at least a chance of explanation here. Consider a plausible structure for *Pirots karulize elatically*:

(12)



The point to notice is that there is nothing wrong with it according to any plausible set of syntactic constraints over the relevant set of categories. And that is true even for the terminal nodes. What constraint of English forbids *karulize* from being a verb? None — there are no constraints mentioning *karulize*. It is true that no dictionary includes *karulize*; but no dictionary comes with a firm guarantee that no other words exist.

We can account for such facts by giving a description in which syntactic constraints on NPs and VPs and adverbs are stated independently of anything about specific lexical items, and the conditions that define lexical items are stated as requirements placed on particular phonological shapes. So there might be a constraint saying that *the* is to be used only as a definite determinative, or that *giraffe* is to be used only as a count noun species name distinct from *lion zebra*, etc. Such a lexicon would simply not say anything at all about how *karulize* should be used.

[6] Chomsky (1957, pp. 104–105) discusses Carnap's example (misspelling 'elatically' as 'etalically'), and calls it a 'sentence', but only in the context of attacking the 'dubious' nature of appeals to structural meaning. He appears not to see that our ability to see anything at all in such a sentence goes entirely unexplained by a GES grammar.

That would leave *karulize* free in principle to be a verb — though of course it is not linked to any meaning. What (11c) tells us is clear in a sense (that elatic karulization is one of the things that pirots do), but in another sense it tells us little, because a pirot could be anything, for all we know, and karulization could be any sort of process, and elaticity could be any kind of quality of karulization. We simply don't know what pirots are or what karulization or elaticity might be. But these are not questions about English!

4.5 Language Acquisition

As mentioned earlier, GES thinking led directly to a conception of language acquisition as guessing a GES grammar for the relevant set, as formalized by Gold (1967). Gold proved that no interesting classes of formal languages have the property of algorithmic learnability from sequences of positive examples under his definitions. Some linguists, psycholinguists, and philosophers mistakenly concluded that this meant much of the structure of grammars had to be innate.

MTS fits naturally with a very different view of first language acquisition: incremental amassing of constraints in a way that facilitates increasingly improved matching with other speakers. Notice, the constraint system acquired need only be roughly comparable to those of other speakers. No recursive specification of a target set of expressions must be attained, and there is no necessity for the internal representation of the overall effect of the assumed constraints to be similar between individuals. Humans are extraordinarily tolerant of divergence and error, and approximate similarity of observed consequences will suffice to permit conversation.

Consider a rather trivial case: imagine that you and I disagree (perhaps only tacitly) on whether split infinitives are allowed. Attaining perfect agreement about the legitimate positions for adverbs is not a prerequisite for intelligibility between us. You could just relax the constraint banning adjuncts after infinitival *to* that I appear to ignore, or simply recognize that I relax it, taking my *to boldly go* as conveying what *to go* and *boldly* convey, though not quite put together the way you would have put them together. Exact generation is unnecessary as well as implausible; approximate similarity of effects of constraints on form will suffice.

Linguists sometimes suggest that negative constraints cannot be learned from positive experience. For example, Fodor and Crowther (2002, pp. 122ff) state baldly that 'without negative evidence it is impossible to acquire constraints.' It should be obvious that this cannot be right. Consider how you know that people do not have lawnmowers in their bedrooms. It's not impossible for there to be a lawnmower in a bedroom. And the fact that you yourself do not is hardly probative. We are (surely) not innately equipped with information about bedroom contents or lawnmower locations. And you've seen the inside of only a very few bedrooms. So how do you know? Yet you do. And we are undoubtedly correct in our assumptions about this negative constraint on bedrooms.

The fact is that we continually acquire new negative dispositional beliefs by generalizing and conjecturing from our experience. We develop new negative constraints every day. The notion that one cannot is just a mistake, and linguists should stop making that assumption in the context of the theory of language acquisition.

A precise account of how we might develop negative constraints from positive experience of the world comes from an 18th century observation about probability: Bayes' Theorem says that for a generalization G and a body of evidence E, the probability of G given E varies in proportion to not just the absolute probability of G but also to the probability that the evidence would look like E if G were true.[7] In saying this I do not intend an endorsement of Bayesian theories either in developmental psycholinguistics or in practical epistemology. We still know very little about how human infants (or people generally) acquire the knowledge that they ultimately attain, or about which relevant cognitive properties might be innate. I merely point out that Fodor and Crowther have no basis for dismissing as impossible the learning of constraints from positive experience.

4.6 Syntactic Quandaries

Assume that constraints are independent, and evolve separately as patterns of structure become established in a linguistic community. Nothing guarantees that a set of constraints will evolve in a way that ensures smooth cooperation. Constraints may clash in a way that makes something unsayable, or at least, makes it unclear how to say it without a major change of plan.

Take the case of choosing between *whoever* and *whomever*, in formal style, in cases like this:

(13) a. ?*You should marry whoever pleases you most.*
b. ?*You should marry whomever pleases you most.*

The analysis of such constructions motivated in Huddleston *et al.* (2002) says that an expression like the underlined part of *I guess whatever he thought about the matter died with him* is a noun phrase with *whatever* as head, but that noun phrase contains nothing but a relative clause in which *whatever* is an initial *wh* -phrase. The constituent *whatever* plays a dual role. But that means two obvious constraints conflict in the case of (13):

(14) a. A pronoun head of a direct object noun phrase must be in accusative case.
b. A pronoun that is subject of a tensed verb must be in nominative case.

[7] Note that where G = 'people don't have lawnmowers in their bedrooms' and E = 'no observed bedrooms have contained lawnmowers', the probability of G given E is proportional not just to the probability of lawnmowers in arbitrary locations, but also to *the probability that you would see no lawnmowers in bedrooms if G were true*. The latter probability, of course, is very high.

In (13), a single *wh*-pronoun contrives to be both the head of a direct object and the subject of a tensed verb. It is completely unclear how the conflict should be resolved. And sure enough, native speakers generally do not know. There is a syntactic quandary.

A grammar consisting of independent constraints predicts this possibility. If English has constraints with content essentially as in (14), it would be understandable why native speakers are undecided between (13a) and (13b).

Of course, the *whoever/whomever* contrast is moribund, and entirely limited to formal English, and the issue of (13) is one that prescriptivists cavil over. But quandaries can be independent of prescriptive pressure. Take agreement in finite copular clauses with disjunctive subjects:

(15) a. **Either Richard or am usually there.*
b. **Either Richard or I are usually there.*
c. **Either Richard or I is usually there.*

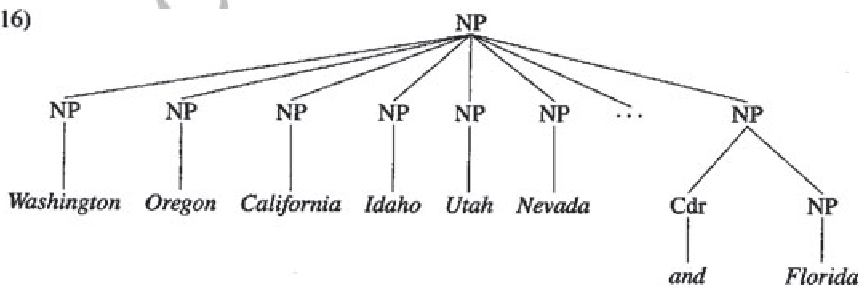
The form choices for the verb *be* in the present tense are 1sg *am*, 3sg *is*, and 2sg or plural *are*. But the subject here is a disjunction of 1sg and 3sg. Each of the three agreement possibilities strike a native speaker (or me, anyway) as wrong. This is a case that was raised by Fillmore (1972), and Fillmore correctly realized that it was a problem for the GES conception of grammars.

These two cases cited should suffice to convey the general idea: a grammar composed of independent constraints on structure can make a construction unusable under certain conditions because of a conflict between requirements. There is no GES analogue. Yet it seems that human languages are, to some modest degree, prone to quandaries.

4.7 Unbounded Branching Degree

Almost all the standard types of GES grammar I am aware of are unable to provide for trees of unbounded width, i.e., they set a bound on the number of children a node may have. This conflicts with the idea that the structure of an arbitrary multiple coordination like *Washington*, *Oregon*, *California*, *Idaho*, *Utah*, *Nevada*, . . . , *and Florida* has a single root node with an arbitrary number of coordinate child nodes:

(16)



In expansion-oriented grammars where rules are in effect saying that some string φ may be replaced by some other string ψ, this limit is set by the length of the longest ψ in any rule.[8] In composition-oriented grammars where pairs of strings or other objects are put together by a binary composition operation, the limit is in effect stipulated to be 2 (and this is common in many modern views of phrase structure as well). Various attempts have been made to reanalyze coordination with a limit to binary branching; see Borsley (1994 and 2003) for a convincing critique of such analyses.

It is an interesting fact about the MTS perspective that the problem of branching degree melts away. To explain very briefly, as long as we do not include constraints in the grammar that entail something like 'Every node has less than *k* children' (for some constant *k*), trees will satisfy (or violate) the constraints regardless of how many children any given node may have. A description of English coordination has to guarantee that either all non-initial coordinate children have the same coordinator (*celery and apples and walnuts and and grapes*), or just the last one is marked (*celery*, *apples*, *walnuts*, *and grapes*), or none of them are (*celery*, *apples*, *walnuts*, *grapes*). But constraints capturing those truths can be given in a way that is silent on how many children a node may have.

Rogers (1999) provides a beautiful technical development and a demonstration that MSO on trees of unbounded branching degree has the same mathematical properties as on bounded-branching trees; but the main point is clear enough when stated informally. The problem of unintended limits on branching degree disappears when instead of trying to generate all the required trees with rules or schemata you assume that trees already exist, and simply state the constraints that define well-formedness for them.

5. The Curious Issue of Transderivationality

The last twenty years have seen the re-emergence of an idea first mooted in the early 1970s under the name 'transderivational constraints'. The intuitive idea is that whether a certain sentence is well formed can depend on what other sentences are well formed. The first proposals were motivated by phenomena suggesting avoidance of ambiguity. Taking the idea seriously would have to mean, in GES terms, that the step sequence in one derivation could sometimes govern what was

[8] One framework that does not entail bounded branching is that of (Gazdar *et al.* 1985). There the existence of a longest rule is avoided by means of a notational device equivalent to a metagrammar generating an infinite rule set. For example, a schema like 'NP $\rightarrow$ NP$^+$ *and* NP' can be understood as an abbreviation for an infinite set of rules, one for each $n \geq 1$, each yielding $n + 1$ NP children with the rightmost prefixed with a coordinator. But a grammar is now stated not with a set of rules but with a set of schemata, and the relationship between schemata and trees is more complicated than the one holding between rules and trees.

permitted in another. Linguists have been oddly unwilling to appreciate that this is a very strange and problematic idea.

The re-emergence of transderivationality actually began with Chomsky's principle 'Avoid Pronoun' (1981, p. 65). Its intent is to define a derivation as illicit if there is another mostly identical with it except for the absence of a certain pronoun.[9] The further development in the 1990s involved increasing allusiveness and metaphoricity. As Pullum (1996) noted, the metaphors often seemed to emanate from transport economics: there was talk of one derivation not being allowed because another would get to the same result in fewer steps ('economy of derivation'), or with shorter distances traversed by 'movements' ('shortest move'), or with its journey delayed till a 'later' stage ('procrastination').

For three reasons, I think we should be extremely suspicious of such notions. The first is that serious examinations of the detailed consequences of proposed transderivational constraints have repeatedly shown them to be untenable. The devastating response by Langendoen (1975) to the transderivational proposals of Hankamer (1973) is just one example. The detailed discussion of an alleged Somali case in Zwicky and Pullum (1983) comes out similarly. A Russian case is discussed and refuted more briefly in Pullum and Scholz (2001).

The second reason is that even if transderivational constraints were not demonstrably inaccurate, it is extraordinarily hard to see how they could be taken seriously either as effective characterizations of languages or as proposed mental mechanisms. This is not a topic that can be treated properly in the space available here, but it should be clear that derivations would have to be individuated so that they could be quantified over, and that the domain of quantification would typically be infinite, and that structural relationships amounting to partial isomorphism between derivations would have to be available. The constraints would have to say things like (17), where f is some function defining a comparison between D and D'.

(17) 'A derivation D is illicit if some distinct derivation D' is identical with it in all respects down to a certain stage i but at stage $i+1$ they differ in that $f(D,D')$ ...'

What f will have to be able to do will differ from case to case, but clearly it will have to be capable of some kind of powerful node-by-node comparison between internal structures of derivations. No advocate of minimalism has ever specified a way of stating such things with clarity. Only critics like Johnson and Lappin (1999) and Potts (2001) have even attempted to wrestle with the issue of stating them.

The third reason for suspicion is that, uncontestably, transderivational constraints are inexpressible in the GES frameworks that posit them. Linguists who have taken an interest in trying to formalize GES theories (Stabler, 1997, for example) have

[9] This is not clear enough, of course, but it as clear as I am able to make it on the basis of what Chomsky says.

always found it necessary to drop transderivational notions from the formalization. Whether it is possible to state them or not, it is certainly not possible within the mechanisms actually assumed within a GES framework. The only reason they can continue to be posited and discussed is that discussions of them always remain extraordinarily sketchy and informal.

Given that there has been no answer to the powerful critique of minimalist 'economy conditions' by Johnson and Lappin (1999), or to Jacobson's (1998) probing response to the more recent transderivational analyses of scope facts found in Reinhart (1995) and Fox (1995), I am inclined to regard it as a virtue of MTS that it very clearly cannot accommodate the analog of such constraints.

Just to make sense of the question in MTS terms, of course, we have to reconstrue it: the relevant question is whether structural properties of one expression can have an influence on the grammaticality of another. The answer is no. Constraints apply within the structure of an individual expression. The domain of interpretation for determining whether a tree T satisfies some formula φ is the set of nodes in T. The nodes of other trees are literally not in the universe of discourse. So the analogs of economy conditions, ambiguity avoidance conditions, and the like are simply impossible to state.[10]

6. Conclusions

The question I have addressed is whether grammars for natural languages should be framed in MTS rather than GES terms. I believe there is a case for this: MTS is favoured by its dissolution of pseudo-issues about expression finitude and language infinitude, and by its satisfyingly intuitive predictions concerning the gradient character of ungrammaticality, the independence of syntax from the current content of the lexicon, the syntactic properties of fragments and ill-formed quasi-expressions, the description of unbounded branching degree, and the existence of syntactic quandaries. I think it also receives passive support from its exclusion of transderivationality.

We cannot always classify actual syntactic frameworks, because they are hardly ever rendered fully explicit in all of their components. I am not suggesting that

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[10] Johnson and Postal (1980), despite their MTS approach, endorse trans-structural constraints under the name 'corpus laws' (pp. 20–21, 677–687). In an attempt to state them, they allow themselves brute-force quantification over infinite sets of structures, ignoring the finite 'pair network' structures used throughout most of their book. They tacitly reinterpret their description language over an infinite universe with whole structures as atoms. Moreover, if their description language were to be capable of stating the ambiguity-avoidance constraints that they adumbrate (pp. 684ff), it would have to be capable of expressing arbitrary inter-structure isomorphism checks, hence much more powerful than MSO. The first-order description language used throughout most of their book could not suffice. In short, Johnson and Postal's theoretically misguided endorsement of trans-structural constraints only underlines the anomalous character of such devices.

they should be: my emphasis on the heuristic benefits of formalization does not imply an adoption of the absurd view that all empirical science should be fully mathematicized at every point. But as I have noted, APG and HPSG look like clear examples of the MTS perspective; Chomsky's 'minimalist program' appears to assume purely GES devices; and the GB and GPSG frameworks are hybrid. From hybrid systems we get a mix of properties: some of the consequences of MTS that I have argued to be good ones, like the possibility of representing degrees of ungrammaticality, but also some of the qualities of GES that I think are undesirable, like lexical dependence.

Nothing I have said here should be taken to imply that I think there is some easy route to good theorizing, in linguistics or any science, of course. Launching and navigating a theoretical ship involves far more than simply choosing a framework-type and nailing its flag to the mast. I have not been attempting to present here any fleshed out theory of the syntactic properties of human languages: the discussion has been at a more abstract level than that. Nonetheless, I hope I have been able to suggest some reasons for thinking that perhaps syntactic theory should not have adopted such unthinking allegiance to GES conceptual foundations, and that the alternative MTS mode of description deserves more attention than it has hitherto received.

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