# IS LINGUISTICS EMPIRICAL?

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This paper continues the discussion of issues raised by Braine's theory of "contextual generalization." The arguments for analyzing the English declarative as transformationally generated are discussed at length. Broader issues about the nature of confirmation of claims made by grammars are also considered. It is argued that while the direct experimental verification of such claims is often not feasible, considerations of simplicity and generality can provide adequate grounds for their empirical confirmation or disconfirmation.

Braine's (1965) reply to our (1965) paper successfully clarifies the two major issues between us. The first of these concerns a detail of the grammar of English: Braine claims (and all generative grammarians have denied) that the simple declarative sentence has a special linguistic character in that it has no transformations in its derivational history. The second issue concerns the possible bases for confirming or disconfirming particular claims about the character of the grammatical analysis of a class of sentences. In our critique we argued that the special status Braine seeks for simple declaratives can be obtained only at the price of the ad hoc decision to treat them in isolation from the rest of the types of sentences in the language. This is equivalent to saying that a grammar which treats declaratives as nontransformational is inherently more complex than a grammar which does not. The methodological issue between us is that Braine does not accept such arguments as supplying even prima facie confirmation of the transformational characterization of declaratives. Braine apparently holds that each decision in the construction of a grammar must be susceptible to direct experimental confirmation or else be considered a mere artifact of the linguistic description, adopted "at the convenience of the linguist." It seems to us that Braine is certainly wrong on both points. On one hand the grammar that Braine proposes for simple declaratives is thoroughly unacceptable; on the other hand to accept Braine's view of confirmation would be to

make linguistic science (or, for that matter, any other kind of science) impossible. We shall discuss these points in reverse order.

Progress in linguistic analysis of natural language has depended on the careful separation of the theory of the language ("*langue*" or "competence") from the theory of the use of langauge ("*parole*" or "performance"). In this way the linguist has insulated himself from the fact that the variables determining the character of speech behavior reflect features other than the formal structure of the spoken language. For instance, the fact that some sentences are difficult to say, to remember, or to understand is obvious. Evidently, such facts are the consequence of interactions between linguistic variables and variables of memory, perception, motor integration, etc. To fail to so represent these facts would render impossible the representation of *either* the systematic character of language *or* the systematic character of speech behavior.

To avoid this consequence, linguistic analysis does not accept as pertinent to the characterization of *language* all facts that are pertinent to the characterization of verbal behavior. Only certain types of information about language are considered, for example, whether a sequence is intuitively grammatical, its relations to other sentences, the units of which it is constructed, etc. Such information is quite real and as palpable as any other psychological data; that *all* facts about speech are not considered in linguistic analysis does not reduce the scientific ob-

jectivity of those that *are* considered. A theory that can explain the latter phenomena is in fact a sound, *empirically supported* part of the total theory of language behavior. The linguist and psychologist seek to discover how the theory of the language, which explains one restricted set of data, is embedded within the theory of the speaker of a language, which explains speech behavior in general.

It is not at all surprising that the analysis of speech behavior should proceed from two empirical and theoretical sources. Indeed, distinguishing among the different kinds of data that constitute superficially homogeneous phenomena is absolutely universal in scientific explanations; it occurs wherever considerations of simplicity and explanatory power require that the observations be represented as interaction effects. Consider, for example, the analysis of a block sliding down an inclined plane. There are two kinds of variables that interact to determine the block's behavior—first, the forces acting downward on the body and determining the acceleration for an ideal system; second, the reactive forces (e.g., friction) due to the character of the particular body and plane under study. The observed behavior is susceptible of systematic explanation only on the view that it is the product of interactions between these distinct systems.

Since the theory of verbal performance is directly concerned with the behavior of speakers, it may often be subject to fairly direct experimental examination. The theory of competence, on the other hand, is concerned with the formulation of the linguistic information underlying verbal behavior. The speaker's competence is only reflected in his behavior via the kinds of performance variables mentioned above. The direct experimental verification of the theory of competence is correspondingly difficult.

This is not to say that the theory of competence is in any sense conventional or arbitrary. For, its support rests not only on occasional experimental confirmation but also on considerations of theoretical simplicity and power, fruitfulness, availability for integration with theories of performance variables, and so on. It

must be reemphasized that this relative inaccessibility to experimental manipulation is not specific to theories of linguistic competence. On the contrary, it is obvious that the more a scientific theory concerns itself with the fundamental mechanisms underlying the observables, the less susceptible it is to direct experimental test.

## SOME EMPIRICALLY BASED FORMAL DECISIONS

We now consider some of the points where Braine feels that the form of the grammar is at the grammarian's (and his) arbitrary disposal. In each case we try to show that the particular claim about grammar we made in our original critique is responsive to some compelling facts about natural language. In any given instance it is, of course, possible to argue that the facts we invoke are wrong or that they might be accounted for in some more economical fashion. But it is *not* possible to claim that the form of the grammar is arbitrary. We will take the points in the order they appear in Braine's reply and omit discussion of Braine's new data or proposals and of the accuracy of our reading of his original paper.[1]

## THE NONPRIVILEGED STATUS OF SIMPLE DECLARATIVE SENTENCES

We argued that declarative sentences have neither linguistic nor psychological preeminence and that the underlying constituent structure of simple declarative sentences is itself an abstract object, just as in the case of the passive and all other constructions. Braine counters with several arguments indicating that we did not suggest evidence for this and that the only evidence he can think of relies on

[1] If we misunderstand Braine (1963) on some points in our critique, we apologize to him and to the reader and suggest that they both satisfy themselves that our misunderstandings were based on misreading. In this paper we quote Braine directly whenever possible. We do not utilize any linguistic discoveries made after 1957; recent theoretical modifications do not affect Braine's theory nor its incorrectness.

arbitrary decisions about the form of grammar:

[1.] . . . their long discussion of the passive transformation . . . is relevant only to the question of what the *passive* transformation is a transformation of. . . . This argument says nothing about the relation between the kernel grammar and simple declarative sentences [p. 484; emphasis ours].

2. The only argument he can recall in favor of an abstract underlying constituent structure (UCS) for simple declaratives is the treatment of discontinuous elements; he claims this is an artifact of an arbitrary decision about the form of grammar.

"Whether or not a phrase-structure (kernel) grammar can permit discontinuities is a *technical* question about the form of such grammars [p. 484; emphasis ours]." In particular, our claim that such constructions as *be chasing* are evidence for the distinction between manifest and underlying order is spurious: "These are of the $\lambda \ldots \mu$ form [p. 485]." Braine suggests earlier that such discontinuities can be treated by a phrase-structure rule, $X \rightarrow \lambda \ldots \mu$, where it is a formal convention that $\mu$ permutes with the next rightmost element. For example, this sequence of phrase-structure rules would give the phrase *be chasing* as an output of the underlying phrase structure:

VP $\rightarrow$ aux V; aux $\rightarrow$ be . . . ing; V $\rightarrow$ chase.

A grammar which forbids representing the constituents in the form *be . . . ing . . .* must necessarily generate [the order] *be + ing + chase* [and we presume John + sg + pres + run] and . . . having generated the items in the wrong order, it is of course necessary to permute them into the actual order. *The entire argument associated with [these structures] rests on a particular choice of phrase-structure model* [p. 485; emphasis ours].

3. Braine then laments the alleged lack of basis for any decisions between "competing" phrase structure and transformational solutions of descriptive problems in syntax.

If the terminal strings generated by the phrase structure are permitted to be arbitrarily different from any actual sentence structures, *there are no independent data against which the phrase structure and the transformational rules can be separately tested* . . . the grammarian can write the phrase-sentence kernel *partly on the basis of his convenience,* free to correct any poor fit with the manifest structure of the language by using transforms to reshuffle elements. This methodological looseness makes it impossible to accept empirical claims about the properties of phrase-structure grammars of natural languages [p. 485; emphases ours].

4. Braine claims, in summation, that we did not show that the simple declarative sentence has a distinct abstract form underlying it, and he asserts that to show it we . . .

. . . would have to show that there are no feasible forms of phrase-structure model which would permit transformational grammars to be written so that only the identity transform intervened between the phrase structure and the morphophonemic rules [p. 485].

Our replies can in some instances be quite brief.

1. Braine's argument on page 484 is simply wrong. On grounds of simplicity, *any* argument which shows that the underlying constituent structure for one sentence type is generated by a particular kind of phrase-structure rules is prima facie an argument that all sentence types have such rules underlying them. A grammar does not treat certain sentence types to the exclusion of others, and there is no reason to believe that a child does either.

2. It is difficult to understand what Braine can mean by the claim that the explanation of discontinuities is a "technical question." He correctly assesses the literature as showing that linguistic formalisms could provide either phrase-structure or transformational solutions. When nonequivalent explanations of the data compete, we must surely decide between them by asking which formalism accounts for the facts under consideration with the greatest economy and generality. If this makes the question of how discontinuous constituents are to be explained "technical," then *all* questions of scientific theory are technical, since considerations

of simplicity and generality underlie the solution of all such questions.

It is true that the use of discontinuous phrase-structure expansions could adequately generate number agreement and appropriate placement of the affix *ing* in the simple declarative sentence. But that is *all* it could do. The arguments in our critique showed that for other constructions (e.g., passive, negative passive, question passive, cleft-sentence passive, negative-question passive, etc.) the affix-movement rule must *follow* the passive rule, which itself follows the development of the base structure. Thus, the affix-movement rule needed for these constructions *cannot* be in the phrase structure and is consequently a transformation. Indeed, since the same transformation is formally capable of accounting for number agreement and affix permutation in the simple declarative as well as other constructions, we have a clear choice between two types of grammar. One grammar uses a discontinuous phrase-structure rule for number attachment in the declarative and a transformation for number attachment in all other constructions. The other grammar employs only the transformation. Clearly the latter grammar must be preferred since it explains the facts with less duplication and with a more restricted form of grammar.

Of course the facts *might* have shown that the discontinuous phrase-structure rules *are* the appropriate linguistic analysis. To show this, the facts would have to be (at least) that *no* sentence constructions require a transformational rule for affix movement which can also be utilized by the simple declarative. That is, the transformational solution *could* have been invalidated by the character of English, and thus the decision to reject the discontinuous phrase-structure solution is not based on the "grammarian's convenience," but on the facts of the language.

3. It is hard to imagine what the "independent data" might be that could "separately" confirm any linguistic analysis since the theoretical decisions themselves are based on all presently available empirical considerations extractable from the language.

Perhaps we can additionally support the decision to treat number agreement (and affix-movement phenomena) with one transformation by an appeal to the reader's intuition about some previously unconsidered facts. Is it not the case that the subject-verb number agreement (and formation of *be + ing*) is the same sort of relation in these two sentences?

*They were running.*

*They were being served.*

The analysis proposed by Braine generates these two subject-verb agreements by two distinct *kinds* of processes,[2] and we might expect that if this analysis were true, different intuitions about the nature of the agreements could be informally or experimentally extracted from native speakers. In fact, we have no reason to believe that such differences exist. Number agreement seems to be a psychologically unitary process in English. This is reflected in the formal analysis to which the linguistic facts led us.

4. In short, although there *is* probably a "feasible" form of grammar in which the underlying and derived constituent structures of simple declaratives would be identical, this analysis is blocked for English on empirical grounds. We have shown briefly that at very least such a solution would involve an unnecessarily complex system since there is a simpler analysis which accounts for the relevant facts. In this way we show that Braine's claim that there is a "base" or "kernel" grammar producing all and only simple declaratives to be *logically* possible but empirically unacceptable.

THE MORAL

We have argued that there is strong empirical support for the claim that declaratives have abstract underlying structures and that this support derives primarily from considerations of the simplicity and generality of grammatical rules. In conclusion it may be pointed out that, though it is true that a suitable complication of the grammar would permit Braine to treat simple declaratives

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[2] Discontinuous phrase structure for the first, transformational for the second.

as the only untransformed structures, it must be added that similar complications would permit that treatment *for any other sentence type.* There is no more reason for complicating the grammar in order to render declarative sentences uniquely nontransformational than there is for complicating the grammar in order to render passives, questions, negatives, imperatives, etc. uniquely nontransformational. The difficulty with playing fast and loose with simplicity constraints is that, once having started, it is hard to find a way to stop.

### THE EXCLUSION OF TRANSFORMATIONS

Braine states that the "...main purpose [of my discussion of transformations] was to show that [their] existence did not contradict my proposals concerning simple declaratives [p. 484]." And later (p. 486), "my [proposal] specifically excluded the learning of transforms ... my proposals could capture the structure of passives only insofar as they are similar to predicate phrases with adjective heads (cf. *The kazoo was blown by the child; The coffee was hot on the stove.*)" Furthermore he states that the fact that many transforms *appear* similar to simple declarative sentences explains why it is reasonable to assume that simple declaratives are learned separately by the child, even thought there is no evidence that they preponderate in the child's verbal environment:

I did not claim that children were mainly exposed to simple declaratives. . . . Information about positional and contingency relations in simple sentences is abundantly exemplified *in many transforms* . . . consider the similarity in verb-phrase structure between passives and sentences with adjective predicators (e.g., *George was served—George was sensible*). . . . Experience with the verb structure of one sentence type could hardly fail to transfer to the other type [p. 483; emphases ours].

This line of argument appears to involve Braine in a hopeless dilemma. To wit, if the child cannot exclude transformations, then our objections in the critique and in this reply obtain; if the child *can* isolate and reject transforms

from consideration, then he must already know the grammar *including its transformational component:* Transformational information is required to distinguish between declaratives and other types of sentences.

This dilemma runs deep. If the child cannot yet tell whether an *apparent* declarative sentence actually is one, how does he know when to transfer experience from one putative declarative to another? It is undeniable that many sentences other than declaratives appear in the declarative-like form "NP VP." How does the child know which such sentences are *sufficiently* "like" true declarative to permit relevant transfer of training unless he already has transformational information? How can "experience with one type . . . transfer to another" sentence of the same type unless the child can see that they *are* of the same type? That is, he must have the transformational information required to distinguish between real and apparent declaratives. Furthermore, *how does the child know what kind of experience to generalize?* Surely if everything the child knows about

*George was served.*

generalized to

*George was sensible.*

the child should expect the sentence

*\*Somebody sensibled George.*

to parallel

*Somebody served George.*

### THE DISCRIMINATION OF HOMOLOGOUS SEQUENCES AND "COVERT CATEGORIES"

Several different problems for Braine's earlier and present proposals are manifestations of Braine's inability to answer the question: How are sentence types differentiated when they have similar word-class orders? (See our critique for examples.)

Braine says, ". . . the kind of phrase-structure system for which my proposals are adequate is one which permits only one unmarked word list at any given location and which restricts the circumstances in which an unmarked list can occur at two nonhomologous locations [p. 487]." He proposes that this requirement be met

by an "indexing restriction" partially carried by "function words" which "index classes and distinguish their locations [p. 487]."[3] But, he notes, this is not an adequate solution: ". . . it is a fact that languages contain structures in which the same position can be occupied by two or more classes which are not differentially marked. Bever et al. are correct in pointing out that this sort of structure poses a problem for my proposals [pp. 487–488]."

As a solution, Braine invokes the use of "covert categories" which he presumes to be "semantic." Words that are otherwise undifferentiated in a particular sentence type thus must have an "internal" analysis which differentiates them.

We, of course, agree with this proposal since it reduces to the tautology that whatever *really* is different ought to be so described. But the tautology is useless unless a theory for providing correct internal analyses is also forthcoming, and Braine's own discussion suggests how unlikely it is that such analyses can be formulated in semantic terms. In fact, generative grammars do provide differing types of words in homologous position with distinct analyses; they do so largely by the use of transformations which can distinguish between items that have similar privileges of occurrence but which differ in the base structures from which they derive. Consider first a nontransformational case— proper names (an example proposed by Braine): Presumably there are different internal analyses for the lexical items "George" and "butter" which allow the sentences

*George is nice.*
*Butter is nice.*
and the sentence
*The butter is nice.*
but block the otherwise contextually generalizable
*The George is nice.*

[3] Since Braine's first paper, rules for the introduction of lexical items into underlying structures have been shown to be substitution transformations (Chomsky, 1965; Matthews, 1963) and not phrase-structure rules. His argument at the end of his section, "The Scope of the Proposals," is therefore unsound.

But what internal markers differentiate the participle in these sentences?
*Making mistakes can be annoying.*
*Recurring mistakes can be annoying.*
What "markers" allow
*Many mistakes were made.*
but block
*\*Many mistakes were recurred.*
The analysis which differentiates the participles successfully must refer to the underlying constituent structure, namely to the fact that "make" occurs with an object permissible in the UCS (and that "mistakes" is that object) and that "recur" does not (so that "mistakes" is the subject in the UCS).[4]

In short, we agree with Braine's proposal that words and homologous sentence types are differentiated by characteristics *not directly observable in the sequences themselves.* In addition we propose that differences in transformational derivation are necessary to provide the explanation for many of these "covert" distinctions. So far as we can see, Braine has provided no reason whatever for rejecting this suggestion.

### THE "UNCLARITY OF TRANSFORMS"

Braine states: "What might be learned in learning transformations is difficult to discuss in part because the transformation concept itself seems unclear [p. 488]." The "unclarity" appears in at least two general ways: (a) ". . . it seems impossible to deduce from Chomsky's (1961) definition . . . what features should be built into a verbal learning experiment in

[4] Notice that this exactly characterizes the distinction between "transitive" and "intransitive" verbs, although this distinction is not apparent in the surface structure of the sample sentences. We could decide that such notions as *transitive, intransitive,* and so on should be represented as "covert features" of lexical items without reference to the UCS. This possibility is empirically rejected because it would not eradicate an abstract level from the grammar, and it would result in duplication in the surface phrase marker of the distinctions already made in the UCS. It is never difficult to construct a theory *more* complicated than the simplest one currently available.

order to ensure that a subject ... [learns] a transformation [p. 488]"; (*b*) grammars are too strong and their character will change as they become refined. Rules for derived constituent structure are not well understood.

Regarding (*a*), it seems to us fantastic to require that experiments should be *deducible* from theories. Regarding (*b*), the questions in grammatical theory which await theoretical and empirical elucidation do not place in jeopardy the answers which we do have.

## ON LABORATORY EXPERIMENTATION WITH PRIMITIVE ARTIFICIAL LANGUAGES

In our critique we suggested that experiments on artificial languages are equivocal with respect to natural language. It is inconclusive to show that the subjects learn the intended structure when exposed to a relatively small number of instances either in artificial or natural language since the structure that must be assumed for natural languages is determined by the need to supply a simple, uniform treatment for a large variety of sentences of very specific formal character. Finally there is no reason to believe that simple phrase-structure artificial languages are on a psychological continuum with natural language.

Braine counters in his reply that he has other evidence indicating that the subjects did learn the assumed structure. He also points out that it is silly to assume that a child loses his natural language-learning capacity when he enters a laboratory.

Of course we were only questioning the *experiment's* capacity to bring out the child's ability. There is no support for the assumption that the experiment with this restricted "language" elicits the mechanisms employed in learning natural language. Evidence that these languages do not elicit first-language learning mechanisms can be found in the recent work of Wales and Grant.[5] They repeated Braine's Experiment III with adults and obtained exactly the same results. This means either that adults learn languages in the same way as children or that the experi-

[5] Wales, personal communication, 1965.

ment is not tapping the language ability indigenous to the child.[6]

## THE CONFLICT BETWEEN LINGUISTIC ANALYSIS AND CURRENT LEARNING THEORY

In our critique, we suggested that current models of learning are in fact incompatible with the empirically based results of linguistic theory. Braine is sensitive to this, and he implicitly uses it as an argument *against* certain features of linguistic analysis; in particular, he is bothered by the concept of an "abstract" underlying constituent structure for which no explicit representation appears.

The distinction [between manifest and underlying structure in kernel sentences] is certainly not convenient psychologically: The assumption that the learner learns structures to which he is never exposed would indeed pose "serious problems for *any* theory of syntax learning [p. 486]."

. . . the fundamental issue at stake in this interchange [is] whether the order of elements in simple sentences reflects the order in the underlying string. . . . Bever et al. are arguing that

. . . the learner is often never exposed to the pattern properties which must be assumed to be learned. Before accepting this argument, I would urge that psychologists examine the linguistic evidence for a distinction between manifest and underlying structure in kernel sentences very closely indeed [p. 490].

We of course agree, and we have tried to expand and clarify the empirical basis for the decision (*a*) that *all* sentences

[6] It seems to us unlikely *on any account* that adults learn languages in the same manner as children do. This does not depend on an assumption of "innateness" for language learning in children, nor must it reject the possibility that "proactive inhibition" explains the adult's difficulty with language learning. Whatever view you hold, it is obvious that an adult with 20-years experience in Language X will learn Language Y differently from a child that has had only 2- or 3-years active experience in X. The fact that children and adults yield identical experimental results thus indicates that the experiment is not stimulating first-language learning mechanisms in children.

have an abstract structure underlying them and (*b*) that this structure is not isomorphic with the simple declarative sentence. The essential themes of our discussion have been: (*a*) Related parts of a language cannot be considered in isolation; (*b*) decisions concerning the form of grammar are based on determining which formalism explains the facts of the language with the least duplication and the most generality.

The psychological role of the underlying constituent structure has not been carefully studied except insofar as it is an integral part of the account of grammatical sentences themselves. Though linguistic evidence of this latter sort is not in any sense arbitrary or equivocal, we do feel that the exact form of the empirical extensions of the underlying constituent structure into other aspects of language behavior requires thorough psychological investigation. Part of that study will no doubt include experimental effects of UCS upon the recall, perception, or learning of sentences. A crucially important part of that study must center on *how* the child goes about learning structures for which there are no explicit models.

There are those who agree with Braine that this is impossible either in fact or in principle. As the empirical basis for assuming an abstract underlying structure in language becomes broader and the explanatory power of that assumption becomes deeper, *we recommend to all psychologists that they seriously question the adequacy of any theory of learning that cannot account for the fact that such structures are acquired.*

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