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METHODOLOGICAL REFLECTIONS ON
CURRENT LINGUISTIC THEORY

I want to make some broadly methodological remarks on a variety of issues. To begin with I'll talk of *rules*, and dwell a while on the distinction between *fitting* and *guiding*.

Imagine two systems of English grammar: one an old-fashioned system that draws heavily on the Latin grammarians, and the other a streamlined formulation due to Jespersen. Imagine that the two systems are *extensionally equivalent*, in this sense: they determine, recursively, the same infinite set of well-formed English sentences. In Denmark the boys in one school learn English by the one system, and those in another school learn it by the other. In the end the boys all sound alike. Both systems of rules *fit* the behavior of all the boys, but each system *guides* the behavior of only half the boys. Both systems *fit* the behavior also of all us native speakers of English; this is what makes both systems correct. But neither system guides us native speakers of English; no rules do, except for some intrusions of inessential schoolwork.

My distinction between fitting and guiding is, you see, the obvious and flat-footed one. Fitting is a matter of true description; guiding is a matter of cause and effect. Behavior *fits* a rule whenever it conforms to it; whenever the rule truly describes the behavior. But the behavior is not *guided* by the rule unless the behavior knows the rule and can state it. This behavior *observes* the rule.

But now it seems that Chomsky and his followers recognize an intermediate condition, between mere fitting and full guidance in my flat-footed sense of the word. They regard English speech as in some sense *rule-guided* not only in the case of the Danish schoolboys, but also in our own case, however unprepared we be to state the rules. According to this doctrine, two extensionally equivalent systems of grammatical rules need not be equally correct. The right rules are the rules that the native speakers themselves have somehow implicitly in mind. It is the grammarian's task to find the right rules, in this sense. This added task is set by demanding not just any old recursive demarcation of the right totality of well-formed

sentences, but rather a recursive demarcation of the right totality of trees. The trees used to be mere *ad hoc* scaffolding by the aid of which the grammarians, each in his own way, contrived to specify the objective totality of well-formed sentences. According to the new doctrine, the trees are themselves part of the objective linguistic reality to be specified.

We have all known that the native speaker must have acquired some recursive habit of mind, however unconscious, for building sentences in an essentially treelike way; this is evident from the infinitude of his repertoire. We can all go this far with Postal when, in his review of Dixon, he writes:

The claim that there are linguistic rules is simply the claim that individuals know their language and have not learned each of its sentences separately.¹

His word 'claim', even, seems ill suited to anything so uncontroversial. What is more than trivial, in the new doctrine that I speak of, is rather the following: it imputes to the natives an unconscious preference for one system of rules over another, equally unconscious, which is extensionally equivalent to it.

Are the unconscious rules the same, even, from one native speaker to the next? Let us grant that the generated infinitude of well-formed sentences is itself the same for two natives. There may then seem to be a presumption of sameness of generating rules – just because any appreciably different but extensionally equivalent system of rules is apt to be prohibitively complex and artificial. However, this suggestion gets us nowhere. Insofar as it is true, the grammarian can just follow his old plan, after all, of settling for *any* system of rules, naturally the simpler the better, that demarcates the right infinite set of well-formed sentences. If the new doctrine of the grammarian's added burden has any content, it owes it to there being appreciably unlike and still comparably manageable systems of rules for generating the same infinite totality of well-formed sentences. From experiences with axiom systems in mathematics, incidentally, we can easily believe in the existence of such alternatives. In my parable of the Danish schoolboys I have already assumed the existence of just such alternative systems for English; though it should of course be said, if we are to be fussy about the facts, that Jespersen's grammar and that of the old-fashioned textbooks really fall short of extensional equivalence at some points.

We see then that the new doctrine of the grammarian's added burden

raises the problem of evidence whereby to decide, or conjecture, which of two extensionally equivalent systems of rules has been implicitly guiding the native's verbal behavior. Implicit guidance is a moot enough idea to demand some explicit methodology. If it is to make any sense to say that a native was implicitly guided by one system of rules and not by another extensionally equivalent system, this sense must link up somehow with the native's dispositions to behave in observable ways in observable circumstances. These dispositions must go beyond the mere attesting to the well-formedness of strings, since extensionally equivalent rules are indistinguishable on that score. It could be a question of dispositions to make or accept certain transformations and not others; or certain inferences and not others.

Certainly I have no quarrel with dispositions. Nor do I question the notion of implicit and unconscious conformity to a rule, when this is merely a question of fitting. Bodies obey, in this sense, the law of falling bodies, and English speakers obey, in this sense, any and all of the extensionally equivalent systems of grammar that demarcate the right totality of well-formed English sentences. These are acceptably clear dispositions on the part of bodies and English speakers. The sticking point is this Chomskian midpoint between rules as merely fitting, on the one hand, and rules as real and overt guides on the other; Chomsky's intermediate notion of rules as heeded inarticulately. It is a point deserving of close methodological attention.

Ironically these same linguists have expressed doubt about the relatively clear and humdrum notion of a disposition to verbal behavior. Chomsky writes:

Presumably, a complex of dispositions is a structure that can be represented as a set of probabilities for utterances in certain definable "circumstances" ... But it must be recognized that the notion "probability of a sentence" is an entirely useless one ... On empirical grounds, the probability of my producing some given sentence of English ... is indistinguishable from the probability of my producing a given sentence of Japanese.³

I am puzzled by how quickly he turns his back on the crucial phrase "in certain definable 'circumstances'." Solubility in water would be a pretty idle disposition if defined in terms of the absolute probability of dissolving, without reference to the circumstance of being in water. Weight would be a pretty idle disposition if defined in terms of the absolute probability of falling, without reference to the circumstance of

removal of support. Verbal dispositions would be pretty idle if defined in terms of the absolute probability of utterance out of the blue. I, among others, have talked mainly of verbal dispositions in a very specific circumstance: a questionnaire circumstance, the circumstance of being offered a sentence for assent or dissent or indecision or bizarreness reaction.

Chomsky's nihilistic attitude toward dispositions is the more puzzling in that I find it again in the newspaper account of his recent lectures in England, despite an intervening answer of mine³ to the earlier statement. I seem to detect an echo of it also in a footnote in Postal's review of Dixon.⁴ This rejection of dispositions would be bewildering by itself. It is doubly so when contrasted with the rather uncritical doctrine just previously considered – the doctrine of unconscious preferences among extensionally equivalent grammars. I'd like to think that I am missing something.

Now some more remarks on the task of the grammarian. What I have said suggests, too simply, the following notion of the grammarians' classical task: that it is the task of demarcating, recursively and in formal terms, the infinite totality of the well-formed strings of phonemes of the chosen language. It would seem from my remarks up to now that this is the basic or classical task, which, then, is added to if one insists further on some distinction between right and wrong rules, right and wrong trees subtending this same superficial mass of foliage. The trouble with thus stating the basic or classical task is that it presupposes some prior behavioral standard of what, in general, to aspire to include under the head of well-formed strings for a given community. What are the behavioral data of well-formedness? Passive observation of chance utterances is a beginning. The grammarian can extrapolate this corpus by analogical construction, and he can test these conjectures on an informant to see if they elicit only a manifestation of bewilderment. But of course the grammarian settles for no such criterion. Traditionally, at any rate, the grammarian has accepted wide ranges of sentences as grammatical which an informant would reject as bizarre. I think of sentences such as Carnap's example, 'This stone is thinking about Vienna.'

A more realistic characterization of the grammarians' classical task is an open-ended one. He does not have a prior behavioral criterion of well-formedness; he just has some sufficient behavioral conditions. Strings heard from natives count as well-formed, at least provisionally. So do

sentences which, when tried on an informant, elicit casual and unbewildered responses. What I then picture the grammarian as doing is to devise as simple a formal recursion as he can which takes in all these confirmably well-formed strings and excludes all strings that would bring really excessive bizarreness reactions. He rounds out and rounds off his data. Sometimes of course he will even reject a heard string as ill-formed, thus rejecting a datum, if he can appreciably simplify his system in so doing; but it would be regrettable to do much of this.

In this somewhat melancholy version of the grammarian's task, I have held Chomsky's doctrine in abeyance. Chomsky believes that the linguistic community itself has a sense of grammaticality which the grammarian can and should uncover; that grammaticality is not just the grammarian's rounding off of performance data. Up to a point I agree; the native's disposition to bizarreness reactions is an implicit sense of grammaticality of a sort. But Chomsky would of course credit the native with a full and precise sense of grammaticality, this being of a piece with the native's purported fund of tacit rules – the native's purported bias even among extensionally equivalent grammars. Now this doctrine is interesting, certainly, if true; let me only mention again the crying need, at this point, for explicitness of criteria and awareness of method.

An attitude that is closely linked to this doctrine is a readiness to recognize linguistic universals. The problem of evidence for a linguistic universal is insufficiently appreciated. Someone says, let us suppose, that the subject-predicate construction occurs in all the languages he has examined. Now of course all those languages have been translated, however forcibly, into English and *vice versa*. Point, then, in those languages to the translations of the English subject-predicate construction, and you establish the thesis; the subject-predicate construction occurs in all those languages. Or is it imposed by translation? What is the difference? Does the thesis say more than that basic English is translatable into all those languages? And what does even this latter claim amount to, pending some standard of faithfulness and objectivity of translation?

To make proper sense of the hypothesis that the subject-predicate construction is a linguistic universal, we need an unequivocal behavioral criterion of subject and predicate. It is not enough to say that if we take these and these as subjects and those and those as predicates then there are ways of so handling the rest of the language as to get general English

translations. The trouble is that there are extensionally equivalent grammars. Timely reflection on method and evidence should tend to stifle much of the talk of linguistic universals.

Insofar, on the other hand, as one is prepared to impute to the native a specific and detailed though inarticulate grammatical system, one is apt to conceive of the notions of subject and predicate and similar notions as objective and as unequivocally apprehended by the native himself. To conceive of them thus is no more of a strain, surely, than to suppose that the native favors one of two extensionally equivalent grammars over another. In all this there is no folly, I feel sure, that conscientious reflection on method and evidence cannot cure; but the cure is apt to take time.

I think it is instructive, before leaving this topic, to fit an idea of Geach's into the picture. Besides singling out the well-formed strings, Geach argues, our grammar must distinguish between proper and spurious components of well-formed strings. One of his examples of a spurious component was 'Plato was bald' in the context 'The philosopher whose most eminent pupil was Plato was bald.'⁵ This demand is reminiscent of Chomsky's demand that the grammarian show how to generate not only the well-formed strings but the right trees. Yet Geach is not committed to finding a bias in the native community between extensionally equivalent grammars. I expect Geach's demand is reconcilable even with the humdrum view of the grammarian's task as the task merely of generating the well-formed strings; for the thing that Geach demands, the marking of the proper components of each well-formed string, would doubtless be a valuable auxiliary to the rules for generating further well-formed strings. The same case can be made, more generally, for Chomsky's insistence that the grammarian's proper product is the whole tree rather than just the well-formed strings that it issues in. The argument is simply that rules for generating further well-formed strings (and trees) can then be formulated in terms of past trees and not just past well-formed strings. This is a strong argument, and it does not depend on any obscure doctrine to the effect that the natives tacitly prefer one system of grammar to another that is extensionally equivalent to it. It would be well to sort out these motives and benefits and see whether the obscure points of doctrine might not be cheerfully dropped.

Such an inquiry could, I suppose, convince us that there is indeed an unarticulated system of grammatical rules which is somehow implicit in the

native mind in a way that an extensionally equivalent system is not. For me such a conviction would depend in part upon clarification of criteria.

To get down more nearly to cases, suppose again a language for which we have two extensionally equivalent systems of grammar; two extensionally equivalent recursive definitions of well-formed string. According to one of these systems, the immediate constituents of a certain sentence are '*AB*' and '*C*'; according to the other system they are '*A*' and '*BC*'. The enigmatic doctrine under consideration says that one of these analyses is right, and the other wrong, by tacit consensus of native speakers. How do we find out which is right?

An unimaginative suggestion might be: ask the natives. Ask them, in their language, whether the real constituents of '*ABC*' are '*AB*' and '*C*'. Does this pose an embarrassing question of translation? Well, then let the native language be English. The essential problem remains; we do not really understand our own English question. We are looking for a criterion of what to count as the real or proper grammar, as over against an extensionally equivalent counterfeit. We are looking, in the specific case, for a test of what to count as the real or proper constituents of '*ABC*', as against counterfeit constituents. And now the test suggested is that we ask the native the very question which we do not understand ourselves: the very question for which we ourselves are seeking a test. We are moving in an oddly warped circle.

Better and more imaginative suggestions may be forthcoming for determining, less directly, what to regard as the real constituents of '*ABC*' from the point of view of tacit native grammar. I suggested some time ago that it could be a question of dispositions to make or accept certain transformations or inferences. But I want now to make use of the unimaginative suggestion as a point at which to take off on a tangent, leaving at last this whole question of a native bias toward one of two extensionally equivalent grammars.

The unimaginative suggestion was: ask the natives. The same question, and the same warped circle or one very much like it, are encountered from time to time in semantics. People like me challenge the notion of synonymy and ask for a criterion. What is synonymy? How do you tell whether two expressions are synonymous? Ask the natives. This essentially was Arne Næss's answer some decades ago, as I analyze it.⁶ Moreover he suited the action to the word, disseminating questionnaires and

claiming significantly uniform results. This was also essentially the answer more recently of Fodor and Katz,⁷ as I analyze it; and I have sensed suggestions of it in Chomsky. Now a reason for pausing over this oddly warped circle is that an empirical investigation, however odd, that yields uniformities has a claim to attention. Grant for the sake of argument that Næss's questionnaire on synonymy yielded statistically significant uniformities; what do they mean? Do they show that Næss's laymen are pretty much alike on the score of their synonymy pairs, obscure though it be to us wherein synonymy consists? Do they show something also, or instead, about how Næss's laymen use the obscure word 'synonymy' or its paraphrases? Separation of these components presents an odd problem.

Essentially the same question is raised outside linguistics by work of Smith Stevens on subjective magnitudes.⁸ For years he gathered subjective testimony of the pitch and loudness of sounds: whether this was twice as high as that, or half again as loud as that. He plotted these findings against the physical frequencies and volumes, and came out with significant correlations – not linear, but logarithmic. Significant, but of what? Was it uniformity of error in his subjects' effort to estimate physical frequency and volume? Or was it uniformity of subjective experience, coupled with uniformity of meaning attached to enigmatically subjective expressions like 'twice as high' and 'half again as loud'? Or did the subjective experience vary from subject to subject, while the meaning attached to the subjective expressions varied in a compensatory way? The uniformities surprise me and I am prepared to find them instructive, but I am at a loss to sort them out. It is the same warped circle.

Turning back to synonymy, or to the semantical notion of analyticity which is interdefinable with synonymy, I might mention also a questionnaire experiment which avoided the warped circle. Apostel and others⁹ in Geneva compiled various lists of sentences. One list contained only sentences that the experimenters regarded as analytic. Other lists had varied and irrelevant motifs. Subjects were given these lists, untitled, and were asked to sort various further sentences into the appropriate lists. The experiment, much the same as one proposed more recently by Katz,¹⁰ sought evidence of a felt similarity among analytic sentences, without benefit of title. The outcome was reported as at best indecisive.

A controversy over semantical notions has simmered for twenty years. Some of us have criticized these notions as insufficiently empirical. Others

have defended the notions without improving them. Their defense has been visibly motivated by a sense of the indispensability of these notions in various applications. We would have been spared much of this rearguard action if the defenders of semantical notions had taken the criticism of these notions to heart, and sought seriously to get along without them. In one, certainly, of its most conspicuous applications the notion of synonymy is not needed; namely, in the definition of the phoneme. According to the familiar definition, what shows that two sounds belong to distinct phonemes is that the substitution of one for the other changes the meaning of some expression. Surely, however, meaning enough for this purpose is afforded by the innocent and uncontroversial notion of stimulus meaning.

The behavioral definition of stimulus meaning is as follows, nearly enough: the stimulus meaning of a sentence, for a given speaker, is the class of all stimulatory situations in the presence of which he will assent to the sentence if queried. Stimulus meaning is at its best among observation sentences. The behavioral definition of an observation sentence is as follows: an observation sentence is a sentence whose stimulus meaning is the same for just about all speakers of the language. Examples: 'It is raining', 'This is red', 'This is a rabbit'.

Sameness of stimulus meaning is no appreciable approximation to the general notion of synonymy to which semantics has aspired. Within observation sentences, however, sameness of stimulus meaning is synonymy enough. For distinguishing phonemes, consequently, it is enough; for surely, if two sounds belong to distinct phonemes, the meaning of some observation sentences will be changed by the substitution.

For that matter, phonemes can also no doubt be distinguished by appealing merely to well-formedness of expressions; by appealing, that is, to the capacity of a string of sounds to occur in the native stream of speech. Presumably, if two sounds belong to distinct phonemes, the substitution will render some coherent string of sounds incoherent. This way of defining the phoneme was proposed by Anders Wedberg,¹¹ and was already implicit, I think, in Zellig Harris. I wanted to bring in the definition in terms of stimulus meaning, however, as an example of how stimulus meaning can sometimes do the work that is desired of meaning or synonymy.

I turn, for the remainder of my remarks, to the notion of deep structure

and its relation to logical analysis. Take, first, logical analysis. What do we do when we paraphrase a sentence by introducing logical symbols for truth functions and quantifiers? In principle it is the same as when in highschool algebra we were given some data about rowing up and down a river; we paraphrased the data into algebraic equations, with a view to solving these for the speed of the river. In principle it is the same also as programming a computer.

I find the phrase 'logical analysis' misleading, in its suggestion that we are exposing a logical structure that lay hidden in the sentence all along. This conception I find both obscure and idle. When we move from verbal sentences to logical formulas we are merely retreating to a notation that has certain technical advantages, algorithmic and conceptual. I mentioned the analogy of the computer; but essentially the same thing is happening in a more moderate way when in natural history we switch to the Latin binominals for genera and species, or when in relativity physics we paraphrase our temporal references into a spatial idiom using four dimensions. No one wants to say that the binominals of Linnæus or the fourth dimension of Einstein or the binary code of the computer were somehow implicit in ordinary language; and I have seen no more reason to so regard the quantifiers and truth functions.

What now of deep structure? If we believe that native speakers have a detailed though inarticulate grammatical system, specific even as between extensionally equivalent systems, then certainly we believe that deep structure, whatever there may be of it, is there to be uncovered. How to tell whether we are getting it right, whether we are matching the inarticulate native analysis or just carving out an extensional equivalent, is a methodological question that I have mentioned already.

If on the contrary we hold every grammar to be as authentic as every extensionally equivalent grammar, and to be preferred only for its simplicity and convenience, then deep structure loses its objectivity but need not lose its place. Deep structure, and the transformations to and from it, might still qualify as auxiliaries to the simplest and most convenient system we know for demarcating the class of well-formed strings. They would stay on in this role just as the trees would stay on, and Geach's discrimination of proper and improper ingredients.

Thus conceived, the grammarian's deep structure is similar in a way to logical structure. Both are paraphrases of sentences of ordinary language;

both are paraphrases that we resort to for certain purposes of technical convenience. But the purposes are not the same. The grammarian's purpose is to put the sentence into a form that can be generated by a grammatical tree in the most efficient way. The logician's purpose is to put the sentence into a form that admits most efficiently of logical calculation, or shows its implications and conceptual affinities most perspicuously, obviating fallacy and paradox.

These different purposes, the grammarian's and the logician's, are not in general best served by the same paraphrases; and for this reason the grammarian's deep structure is not to be identified with logical structure, suggestive though the one may be for the other. I have two major examples in mind to bring out the divergence.

One example is the elimination of singular terms other than variables. Let '*a*' represent such a singular term – perhaps a proper name, perhaps a complex singular term – and let '*Fa*' represent a sentence containing it. We can paraphrase '*Fa*', to begin with, as ' $(\exists x) (Fx.a=x)$ '. In this way all singular terms, other than simple variables such as the '*x*' here, can be confined to one specific manner of occurrence: occurrence to the left of '='. Then, as a next step, we can reckon this identity sign to the singular term as an invariable suffix, thus re-parsing the singular term as a general term or predicate.

The advantages of this transformation are specific and limited. Laws of logic become simplified, through not having to provide for the instantiation of quantifications by terms other than variables. The simplification is the greater for the fact that the instantiations thus avoided were ones that depended awkwardly on existence assumptions. Certain gains in philosophical clarity ensue also. Variables, rather than names, come to be seen as the primary avenue of reference. Little puzzles about names that fail to name anything are swept aside.

This elimination of singular terms is not all good, however, even for logic and mathematics. Inference moves faster when we can instantiate quantifications directly by names and complex singular terms, rather than working through the variables and paraphrases. And complex singular terms are in practice vital for algebraic technique. An algebraist who was not free to substitute complex expressions directly for variables, or to substitute one side of a complex equality directly for the other, would soon give up.

The important point thus emerges that logical analysis itself – better, logical paraphrase – may go one way or another depending on one's specific logical purpose. The image of exposing an already present logical structure by analysis is a poor one. And when our interest turns to English grammar, again we are bound to find that the elimination of singular terms is to no purpose. Surely it yields no deep structure that would help to simplify an account of English grammar. Thus take the distinction between the referential and the non-referential use of singular terms. Work of Geach and Strawson suggests that this distinction is vital to an appreciation of English; but the logical paraphrase obliterates it utterly.

In my view the logical structure and the deep structure, or let me say the logician's paraphrases and the grammarian's paraphrase, differ not in kind but in detail and purpose. They differ in the same sort of way that the logician's two paraphrases differ from each other: one the austere and pellucid paraphrase containing no singular terms but variables, and the other the algorithmically efficient paraphrase bristling with complex singular terms.

The elimination of singular terms was one example of the difference between paraphrasing for logic and paraphrasing for grammar. Now the other example I have in mind is the treatment of time as a fourth dimension. A while ago I referred this to physics, but it is vital equally for logic and philosophy. A logic of tense is a towering triviality which we have no excuse to put up with if our concern is merely with the scientific use of language rather than with the scientific study of it. We program language into the simple neo-classical logic of truth functions and quantifiers, by eliminating tense and treating times on a par with places. The resulting simplification of formal logic may be sensed from this example, which I have used before: George V married Queen Mary; Queen Mary is a widow; therefore George V married a widow. We cease to have to provide against this kind of thing, among others.

Philosophical clarification ensues as well. Thus consider the following puzzles. How can things be related that do not coexist at any one time? How can a variable range now over things that no longer exist? or range ever over things that never coexist? How can a class have members that never coexist? How can a class, which is an abstract object, be said to change, as it must when its members change or cease to exist? We make a clean sweep of all such puzzles by dropping tense and treating all past,

present, and future bodies as four-dimensional substances tenselessly scattered about in spacetime.

This is a paraphrase which, we see, works wonders for logic, philosophy, and physics as well, but presumably is not wanted for English grammar. A deep structure without tense seems unpromising, at any rate, as a means of simplifying a grammatical account of an Indo-European language. Here again, evidently, is a wide divergence between the structure that the logician is after and what the grammarian wants under the head of deep structure. And yet, reading Postal's typescript 'Coreferentiality and physical objects,' I begin to wonder whether the four-dimensional view might be useful sometimes in grammar too.

My previous example, the elimination of singular terms, spoke for pluralism not just as between logical structure and grammatical deep structure, but within logical structure; one logical paraphrase served one logical purpose, another another. Perhaps now there is a case also for pluralism within grammatical deep structure: one paraphrase might serve one grammatical purpose, another. A paraphrase into the tenseless idiom of four dimensions might play an auxiliary role in connection with some grammatical twists, while a different deep structure, retaining tense, might still be exploited for other grammatical ends. So let me conclude with a plea against absolutism.

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