



What the Linguist is Talking About

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COMMENTS AND CRITICISM

WHAT THE LINGUIST IS TALKING ABOUT *

At the beginning of his article "Grammar, Psychology, and Indeterminacy,"[†] Stephen Stich announces that his goal is to tell the linguist "what he is talking about." The implication is that linguists are confused about important issues in the theory of grammar which Stich will proceed to clarify for them. In fact, much of Stich's essay recapitulates familiar descriptions of the theory of grammar. Where he departs from these, his approach seems to us fundamentally mistaken, though it does touch on important issues. The most important of such issues is Quine's indeterminacy thesis. Stich claims that, on his description of the theory of transformational grammar, this theory itself attributes indeterminacy to natural languages as an inherent feature. Thus, if Stich's interpretation of the present theory of grammar were right, indeterminacy would be implied by the theory, rather than merely a controversial philosophical doctrine urged on linguistics from the outside by Quine and his followers. Stich's argument is directed specifically against views that we have developed in various publications. We believe that his criticism is not well founded on this and other issues and that Stich's alternative descriptions of the theory of grammar are seriously in error.

Sections I and II of Stich's essay review the methodological backgrounds of current linguistic theory along conventional lines. A grammar is a theory of the language, an idealization similar to those in other branches of science such as the theory of gases. Grammars are constructed on the basis of various types of data that linguists have available to them: data on acceptability, ambiguity,

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relatedness of utterances and their parts, etc. The rules of a grammar assign a structural description to each of the infinitely many sentences of the language. The structural description of a sentence is in principle a complete account of its grammatical (i.e., semantic, syntactic, and phonological) properties. Grammars, moreover, form part of a more comprehensive theory of language use which takes into account such factors as the structure of memory and perceptual strategies in order to explain linguistic behavior and the judgments speakers make about sentences. In this way, the linguist seeks a full account of the entire range of linguistic data concerning a language.¹

At this point Stich departs from familiar formulations, and undertakes a critical discussion of them. Before proceeding to investigate the problems and solutions he presents, let us review briefly what is actually proposed in the account to which he specifically refers (*ibid.*, 24-27).

In addition to accounting for the linguistic data concerning particular languages, the linguist seeks to construct a general linguistic theory expressing the grammatical properties essential to all human languages. A grammar, regarded as a theory of an individual language, "is *descriptively adequate* to the extent that it correctly describes the intrinsic competence of the idealized speaker." In contrast, "*a linguistic theory is descriptively adequate* if it makes a descriptively adequate grammar [*dag*] available for each natural language," each system that is a possible language for this particular organism. A linguistic theory "meets the condition of *explanatory adequacy*" to the extent that it "succeeds in selecting a *dag* on the basis of primary linguistic data [*pld*]," namely, the kind of data that suffice for language acquisition. Such a theory would offer an explanation, not merely a description, of particular judgments that constitute part of the linguist's data, in that it offers a principled reason for the selection by the speaker-hearer of the grammar that entails these judgments. The theory is falsifiable in many ways: e.g., "by showing that it fails to provide a *dag* for *pld*" for some language. The linguistic theory may be regarded as an abstract theory of language acquisition: it postulates certain "innate predispositions" that enable the child to develop competence on the basis of *pld*. The linguistic theory "constitutes an explanatory hypothesis about the form of language as such," and, at the same time, "an account of the specific innate abilities that make [the achievement of language acquisition] possible."

¹ Cf. Chomsky, *Aspects of the Theory of Syntax* (Cambridge, Mass.: MIT Press, 1965), ch. I, and other sources.

For convenience of exposition, we will refer to the approach summarized so far as the "standard account" of "what the linguist is talking about."

Stich argues that the standard account is badly confused about the relation between grammars, linguistic theory, and acquisition models. There is, according to Stich, a "riddle" here:

Constructing a theory of grammar acquisition is surely a fascinating project and one which would naturally catch a grammarian's eye. But, at first blush at least, it would seem to be a new project, largely distinct from the job of constructing grammars for individual languages. Why, then, do Chomsky and others view the study of acquisition as intrinsic to the construction of grammars for individual languages? (805/6)

We find nothing puzzling in this. Linguists who concern themselves with acquisition models do so in the belief that grammar construction gains its primary intellectual significance from the role it plays in the study of language acquisition. Grammar construction is a worth-while enterprise in itself, but the study of language bears directly on philosophical and psychological issues, specifically, we have argued, on issues of rationalism and empiricism.² For this reason, we and others have been particularly concerned with developing a general theory that has something of the character of traditional "universal" or "philosophical" grammar. We have argued that the study of these more abstract questions leads to a rationalistic account of the acquisition of language. The speaker-hearer does not acquire competence by inductive generalization from regularities in a corpus of *pld*; rather, a rich framework of linguistic universals defines a set of what Stich calls "humanly possible grammars" (*hpgs*). Language learning is a process of determining which of these is the actual grammar of the language to which the child is exposed. The procedure, we suggest, is akin to theory construction. Learning of language is a matter of selecting among *hpgs*, filling in details in a fixed framework or schematism. Generative grammars of individual languages thus provide the empirical basis for construction of a theory that deals with the common structure of natural languages. This theory leads to specific hypotheses about the innate principles underlying language acquisition.³

² Cf. *ibid.*, ch. I, and Katz, *The Philosophy of Language* (New York: Harper & Row, 1966), pp. 240-282.

³ Transformational analysis inherently reflects this concern with more abstract questions. For one thing, in revealing underlying levels of sentence structure, transformational analysis overcomes structuralist objections to many of the claims of traditional universal grammar, namely, apparent counterexamples to cross-language generalizations based on features of surface structure. A trans-

Stich believes that the study of acquisition and the related concern with general linguistic theory (universal grammar) are motivated by the "puzzle" of how to choose among alternative *dags*. The puzzle he sees is that there are variant grammars "each of [which] makes identical claims about the grammarian's data—not only the data on hand, but *all* the data he might acquire . . . grammar is afflicted with an embarrassment of riches" (807).

We have already given the answer to Stich's "riddle": the study of acquisition and universal grammar does not arise as an ancillary problem in the course of constructing grammars, but is rather the focus of our interest—grammars interest us insofar as they contribute to these further investigations. Stich's "puzzle" is nothing more than his formulation of the defining characteristic of empirical science: nontrivial theories are underdetermined by data. If working linguists are unconcerned with the further problems that happen to intrigue us, they will simply exercise their acquired skills to formulate the best hypothesis they can with regard to the grammar of the language under study. Like other working scientists, they will not worry about the problem of induction nor about rationally reconstructing criteria for choosing hypotheses. Rather, they will exercise a skill that might as well be on all fours with chicken sexing: linguists and other scientists use these skills without knowing the details about their nature. Stich gives no argument to show that linguists and other scientists are wrong to rely on such criteria and their skill in applying them.

We would argue, moreover, that the linguist who happens to be so lacking in curiosity as to be unconcerned with universal grammar is excluding significant data that bear on the choice of a grammar for the language. For us, data from language L' can contribute to the choice of a grammar for language L . Thus, suppose that two *dags* G_1 and G_2 are under consideration for L , but that only G_1 is an *hpg* in accordance with a general linguistic theory T which serves as a hypothesis concerning the human "language faculty" as such and which provides a basis for explaining how speaker-hearers of L' acquired a *dag* for L' . Any scientist would tentatively select G_1 over G_2 on this more extensive data base. Linguists unconcerned with

formational-generative grammar reveals deeper levels and more abstract properties which appear to be more uniform across languages, thus permitting the formulation of nontrivial general hypotheses about language structure. Furthermore, it seems reasonable to expect that important aspects of the traditional distinction between "logical form" and "grammatical form" can be explicated on the basis of syntactic structure and its semantic interpretation; Katz, *Semantic Theory* (New York: Harper & Row, 1972),

linguistic theory would be forced to disregard this evidence, to the detriment of their work. For a linguist with broader interests, data from *L'* can be used to confirm or reject a hypothesis for *L*—on the reasonable assumption that speaker-hearers are not genetically predisposed to acquire *L* but not *L'*, that the capacity to acquire language is a fixed human capacity, not differing for members of different races, political groupings, etc.

Stich presents several versions of his puzzle. He asks:

... what justification can there be for [a] grammar as contrasted with a variant interchanging NP and VP throughout, or yet another variant in which NP and VP are systematically replaced with a pair of symbols that occur nowhere in any grammar of English? (806)

And he asks how one decides among different sets of grammatical rules all of which make the same predictions about all the data a linguist might acquire. As far as interchange of symbols is concerned, this is merely a notational change unless linguistic theory presents—as we think it must and can—a set of substantive conditions on the choice of such symbols as NP and VP. As for the more general question that Stich raises, the linguist, like any scientist, will attempt to choose among variants that are empirically indistinguishable in principle by simplicity considerations and other methodological principles. Where such variants exist, choice of a best hypothesis in empirical science is not only underdetermined locally, that is, by particular samples from the data available in principle, but also globally: that is, even given “all data,” there are variant hypotheses consistent with the data. But, assuming there could be nonequivalent hypotheses that are consistent with *all* relevant data (of any sort), there is no argument in Stich’s essay to prevent us from saying what surely any other scientist would say: simplicity and other methodological criteria, applied by skilled scientists, may determine a unique best choice among these hypotheses.

According to the standard account, linguistic theory specifies the schematism for grammar, thus generating the class of *hpgs*, and furthermore provides an evaluation measure—an algorithm for selecting among *hpgs* that are compatible with given *pld*. Much of the linguistic work of recent years has been an effort to determine the character of the schematism and the evaluation measure. Plainly, all hypotheses about these matters are empirical; hence, if non-trivial, underdetermined by evidence. Notice that the schematism and the evaluation measure are not to be confused with the criteria used by scientists (including linguists) in choosing among possible

theories, whatever these criteria may be. The latter are general methodological criteria employed to select a preferred hypothesis; the former are hypotheses about the form of linguistic rules, conditions on rules, relations among subcomponents in grammars, linguistically significant generalizations, etc. That is, the latter constitute a hypothesis that falls within theoretical human psychology. This hypothesis concerning a schematism and an evaluation measure is itself to be judged on the basis of general methodological criteria that guide ordinary scientific practice.

Consider now Stich's approach to the "puzzle" and "riddle" that he has posed. He outlines two strategies: strategy I, which he "disparages," and strategy II, which he "endorses." Strategy I incorporates a determinacy thesis; strategy II, an indeterminacy thesis. Strategy I is "thoroughly misguided and rests on a mistaken picture of what a grammar is" (807). Strategy II, he contends, "suggests an illuminating solution to the puzzle of alternative grammars" (807). Stich's strategy I is alleged to be a summary of the standard account, recapitulated above. However, his strategy I misstates this account in a crucial respect, implying an absurdity which leads him to reject strategy I.

According to Stich's strategy I, linguists postulate "all known *dags*" for the languages for which they have data. They seek to determine "universal features" of "all known *dags*," incorporating such features in their "acquisition model." This model defines the class of *hpgs*. If the theory is not so restrictive that there is only one *hpg* compatible with *pld*, then the theory must also provide an "evaluation measure or weighting of *hpgs*," which selects the highest ranked among them as the grammar which, it is postulated, is "internally represented" by the speaker-hearer who has learned the language, who has acquired competence.⁴ But, Stich remarks, this strategy is "wholly wrongheaded." The reason is that there is a

. . . superabundance of *dags* for any natural language. For every *dag* there are alternatives which are also descriptively adequate. But the linguistic universals were taken to be properties of all *dags*. Thus each *dag* for every natural language will be among the *hpgs*. So if any *dag* is compatible with the *pld*, all its alternatives will be as well. And we have made no progress at selecting a single *dag* as the right one (810).

⁴ He refers to the selected grammar as "this 'explanatorily adequate' grammar which the child actually internalizes and which the linguist seeks to uncover." Notice that in the account he is attempting to reproduce, "explanatory adequacy" is a property of linguistic theory, not of grammars.

Furthermore, the same problem holds in the case of an evaluation measure:

... once such a measure *has* been found there will be indefinitely many alternative measures which select different *dags* for the same body of *pld* . . . Thus if we can design a measure which ranks any one of these *dags* highest in the sub-class, there will be another measure which ranks a different *dag* highest (810).

Notice in the first place that Stich has misrepresented the account he is disparaging. The standard account defined a descriptively adequate theory as a theory that provides *some dag* for each set of *pld*. In Stich's version, we must first consider "all known *dags*" and then arrive at the theory inductively by considering features common to all of these. Naturally, this strategy will fail, given the superabundance of *dags*—that is, given the general underdetermination of theory by evidence. There will, to be sure, be mutually inconsistent grammars, all descriptively adequate so far as can be determined, but differing in their properties. We will get nowhere if we insist on first considering all known (imaginable, contrivable) grammars compatible with evidence, then proceeding to determine their common properties. But this was not the enterprise outlined. Rather, the account suggested that we construct a linguistic theory, which defines *hpgs* and offers a measure to select among them, as a hypothesis about human language and the capacity to acquire language in the normal way on the basis of *pld*. We then test this theory for descriptive adequacy by determining whether it provides *a dag* (not *all dags*) for each human language. We test it for explanatory adequacy by determining whether it selects, on the basis of *pld*, *a dag* for each language. Replacing an existential by a universal quantifier, Stich converts a sensible and properly limited inquiry into one that is "utterly wrong-headed," but has never been suggested in the literature.

In formulating his strategy I, Stich argues that "It is essential that the linguistic universals be taken as the properties common to each descriptively adequate grammar of every natural language," as against "An alternative notion that took the linguistic universals as the features common to each of the actually internalized grammar of every natural language" (810). The latter notion, which is in fact that of the standard account, is "useless," Stich claims: "until we *know* which grammars are internalized we cannot discover which features are universal to such grammars" (810). But the objection is senseless. We cannot "know" which grammars are internalized in

advance of constructing a linguistic theory. We can only *hypothesize* that certain grammars are internalized. The natural and appropriate strategy is to construct hypotheses with regard to particular grammars *and* with regard to linguistic theory, confronting the entire complex with data from various languages for confirmation. That is, we ask whether the grammars selected on the basis of *pld* by the postulated linguistic theory, with its definition of *hpg* and an evaluation measure, are, so far as can be determined, *dags*. We do not "know" that we are correct in these hypotheses. Nor will we ever be certain about this. All of this is commonplace, and it is perhaps surprising that it is necessary to reiterate it. Once it is recognized, however, it is plain that Stich has offered no objection at all to the account that has actually been proposed, though we agree with him in rejecting his strategy I, which misrepresents the standard account by a change of quantifiers. The latter seems to us quite adequate as far as it goes. The problems that arise are in principle no different from those which arise in any other empirical inquiry.

Of course, having constructed a linguistic theory meeting (to some extent) the conditions of descriptive and even explanatory adequacy, we will always be able to construct another theory, with a different characterization of *hpg* and a different evaluation measure, which will meet these conditions (to the same extent), selecting different *dags* that are just as compatible with evidence. Thus Stich is perfectly right in saying that "if we can design a measure which ranks any one of the *dags* highest in the subclass, there will be another measure which ranks a different *dag* highest" (810). This, he asserts, is the "crucial point." The point amounts to nothing more than the assertion that linguistics is an empirical science, not a branch of logic or mathematics. It thus has no force whatever with regard to the standard account, which of course also insists that linguistic theory, being a nontrivial empirical theory, is underdetermined by evidence.

Stich then expands the argument as follows:

But whatever justification there is for holding the *dag* selected by one measure to be the grammar actually internalized is equally justification for holding that the other is. And we are back where we started, with too many *dags* each with equal claim to be the 'right one' (811).

The statement is uninteresting, no matter how we understand the notion "all evidence" that figures in the definition of *dag* and "justi-

fied linguistic theory." If by 'all the grammarian's data'⁵ we mean "all available data," then the linguist faced with alternative theories that are equally justified by all the data would search for more data to choose among them. Furthermore, he would consider how general methodological criteria (not to be confused with the evaluation measure that forms part of empirical linguistic theory) apply to the choice between these theories. If by 'all the grammarian's data' we mean "all possible data" (granting some sense to this notion), then the linguist, by hypothesis, is restricted to methodological considerations. The linguist's "dilemma" would be exactly that of a physicist faced with the question: what would you do if you were presented with incompatible theories each "justified" in terms of all possible data?

Here we come to the real crux of the matter. Clearly, the physicist will reply that under such conditions the choice between these theories will be made on the basis of simplicity and other methodological criteria. What, then, would Stich say to this reply? He can only try to argue that the particular methodological criteria to which the physicist appeals are not the only criteria to which appeal could be made in choosing among these theories, and that—and here is the critical point—the criteria to which the physicist has appealed enjoy no special privilege among imaginable criteria. If philosophically informed and not caught napping, the physicist will surely point out that the criteria appealed to are justified by their success in other cases of scientific choice. For example, let the argument be about whether the physicist can motivate a choice of a specific curve to express the relation between the temperature and pressure of a gas on the basis of a set of observed values (supposing them to include every possible piece of data). Let us suppose further there are a number of curves C_1, \dots such that each passes through every observed point, and that the methodological criteria K to which our physicist appeals lead to selection of the curve with the least average curvature, say C_1 . Then, if Stich suggests that there are other criteria K', K'' , etc. that rank these curves differently, the physicist would, on our account, respond that K is preferable to the alternatives because in other cases in which K has been used to select a relationship between physical properties the subsequent evidence has confirmed the choice of K . Curves with the least average curvature, in general, turn out to state physical relationships

⁵ Stich defines a *dag* as a grammar that "correctly captures the intuitions of the speakers of the language (and the rest of the grammarian's data) within the limits of accuracy allowed by idealization" (807).

more correctly than curves with any of the curvature properties that are evaluated more highly under K' , K'' , etc. Now, Stich has to reject such "empirical justification" by arguing that it depends on a second-level inductive extrapolation from similar cases in which K has been used and that such an extrapolation itself might be judged by varying criteria. But now Stich is in the position of asking that the physicist justify inductive criteria over counterinductive criteria, that the working physicist solve the Humean problem of induction in order to make practical decisions among competing theories. Unless Stich makes the solution of the philosophical problem of justifying an inductive policy a condition of scientific practice, he allows empirical justifications of methodological criteria of the kind that physicists and linguists use to choose hypotheses and theories. This concession, of course, means that in principle we may always be able to show, for any two *dags*, that they do not have "equal claim" to being the "right one," since only one of them may be in accord with the empirically justified evaluation measures. Thus Stich equivocates on the term 'justification'. If in asking for a justification of a simplicity criterion or other methodological criteria, he means 'justification' in the sense in which this term is used in connection with the classical riddle of induction, then he is clearly asking too much of the scientist. On the other hand, if he is not asking too much, then the sense of 'justification' must allow empirical evidence in the form of higher-level inductions to count as justifying a simplicity criterion and other methodological criteria.

Suppose it were further argued that the totality of possible observations concerning dispositions to respond by verbal (or other) behavior is consistent with *dags* and linguistic theories that are incompatible with one another. This observation would not distinguish linguistics from physics, since it is also true of physical theory that it is underdetermined by all possible data regarding dispositions (again, granting that this is a coherent notion). Suppose it were further argued, as it has been by Quine,⁶ that there is nevertheless a difference between physics and linguistics because "theory in physics is an ultimate parameter," and we must "go on reasoning and affirming as best we can within our ever underdetermined and evolving theory of nature, the best one that we can

⁶ "Replies," in D. Davidson and J. Hintikka, eds., *Words and Objections: Essays on the Work of W. V. Quine* (Dordrecht: Reidel, 1969), pp. 303/4.

muster at any one time"; whereas in linguistics,⁷ "there is no real question of right choice; there is no fact of the matter even to *within* the acknowledged underdetermination of a theory of nature" because there is no "right English answer which is unique up to equivalence transformations of English sentences" to the question: "What did the native say?" This effort to distinguish linguistics from physics amounts to nothing more than the unargued claim that meanings do not exist.⁸ But if physicists were to argue, say, that vital forces or atoms do not exist, they could not merely appeal to the undeniable fact of underdetermination of theory by evidence. Rather, they would either show some incoherence in the postulation of vital forces or atoms or construct a better theory to account for the data without any such assumptions. In the case at issue, Quine does nothing of the sort. There is no sense in which theory is an "ultimate parameter" in physics, more so than in other domains of empirical inquiry (granting the vast differences in depth of theory and empirical confirmation). We see no force whatsoever to the claim that there is some fundamental problem here that distinguishes linguistics from physics, and until some alternative is proposed to the theories of semantic representation now being explored in various ways, we see no reason to accept the arbitrary and unargued assertion that these theories have nothing to be right or wrong about.

Moreover, there is something conceptually incoherent in Quine's position. Quine accepts atoms and other inferred entities in physics, but would refuse to accept meanings even if the best theory to explain linguistic phenomena were to posit them. But surely Quine would not reject in principle a reduction of linguistics together with meanings to physics, say, a reduction on which meanings are inter-

⁷ It is unclear to us whether Quine intends to restrict his remarks about indeterminacy in linguistics to "indeterminacy of translation," which he explicitly discusses, or to indeterminacy of all linguistic theory that goes beyond the Gedankenexperiment on "stimulus meaning" to which he arbitrarily restricts attention at certain points in his exposition. It is obvious and uninteresting that, given an arbitrary restriction on permissible evidence, there will be incompatible hypotheses compatible with this evidence. It is less obvious that there are incompatible hypotheses such that no imaginable evidence can bear on the choice between them. We will not pursue this matter, since, even if we were to grant the latter claim, it would in no way distinguish linguistics in principle from other empirical inquiries. Stich, incidentally, agrees with our conclusion that Quine's arguments do not distinguish linguistics from physics, a matter to which we return. For further discussion of Quine's efforts to distinguish linguistics from physics, see Chomsky, *Current Issues in Linguistic Theory* (The Hague: Mouton, 1964); and "Quine's Empirical Assumptions," in Davidson and Hintikka, *op. cit.*

⁸ As is shown in Katz, *Semantic Theory*, pp. 286-292.

preted as equivalence classes of brain states. On this possibility, linguistics is part of physical theory and semantic theory is part of our theory of the brain. If physics is able to deal with questions of truth, then surely it can deal with such questions in connection with states of the brain. How, then, can there be no truth in translation when there is something to be right or wrong about in physics generally?

Let us now consider the strategy that Stich endorses, his strategy II. In accordance with this strategy, the linguist seeks to construct an "acquisition model [which gives] as output *some* grammar that is true of the accomplished speaker," some grammar that is selected as "true of the speaker" given *pld*. Note that this is exactly the standard account, with one exception: that account took the linguist's hypothesized *dag* to be a description of the postulated "internal representation" of linguistic competence, whereas Stich speaks of this *dag* as "true of the speaker." Furthermore, Stich asserts that his strategy II does not "pretend to exhaust the class of grammars that humans can learn," whereas the standard account does propose that a theory meeting the condition of explanatory adequacy be taken as an empirical hypothesis concerning the intrinsic human ability to acquire language on the basis of *pld*. Thus we note two respects in which Stich's strategy II appears to differ from the account that is familiar in the linguistic literature.⁹

We have already noted that Stich offers no objection to the standard account. Let us now compare strategy II with the standard account. The latter proposes that linguistic theory is falsifiable by the demonstration that, on some *pld*, a speaker-hearer will acquire competence that is not as described by the grammar selected by linguistic theory—or, in Stich's terms, that the grammar selected by the acquisition model will not be "true of the speaker." Since Stich does not impose on his acquisition model the condition that its outputs "exhaust the class of grammars that humans can learn," it would seem that he would not regard the result just described as a refutation of his model. On the other hand, since he does impose on his acquisition model the condition that, for each choice of *pld*, "the

⁹ Stich suggests another respect in which his strategy II differs from the standard account, namely, he is concerned only with "quasi-universals" which are properties of all the outputs of the acquisition model, but "are in no sense universals—there is no claim that all *dags* must share them" (812). Again, this observation reflects his misunderstanding of the standard account, which did not hold that universals are properties of all *dags* but rather of those *dags* that are in fact descriptions of linguistic competence. In fact, Stich's "quasi-universals" are exactly the universals of the standard account, if we add the realist assumptions just noted.

acquisition model have as output *some* grammar that is true of the accomplished speaker," it would seem that he should regard the result just described as a refutation of his theory. Suppose that we interpret him as saying that this result does refute the theory. In this case, his strategy II does not differ at all, in this respect, from the standard account. Suppose that he would not regard this result as refuting the theory. Then we will disregard his alternative and less exacting enterprise until he suggests some interesting empirical conditions that it must in fact meet.

Let us turn then to the issue that Stich seems to regard as more important, namely, the distinction between grammars "true of" speakers and grammars that correctly describe what is "internally represented" by speakers. Note that this issue has no relation to anything discussed so far. That much is plain when his misunderstanding of the standard account is corrected. Thus that account could have been formulated, throughout, referring to the *dags* as "true of speakers" rather than as descriptions of the internally represented linguistic competence. The question now at issue is, in essence, the question of realism. Are we willing to postulate that our *dags*, and our linguistic theory, describe properties of the speaker-hearer? Is the mature speaker-hearer to be regarded as a "system" with the properties spelled out in detail in the postulated *dag*, and is the child to be regarded as a "system" with the properties spelled out in detail in the postulated linguistic theory?

The standard account follows normal scientific practice in answering "yes" to both of these questions. Thus, imagine that a psychologist, investigating properties of visual perception, were led to postulate a theory involving analyzing mechanisms of various sorts, say, devices that analyzed stimuli into line, angle, motion, and so on. Would the psychologist propose that the theory is "true of" the organism or that it describes mechanisms possessed by the organism? If the former, the psychologist would be unconcerned if neurophysiological investigation were to demonstrate that visual perception actually proceeds in some entirely different way, with entirely different and unrelated mechanisms. The theory would still be "true of" the organism in Stich's sense of this notion. But any actual scientist regards a theory as subject to disconfirming or confirming evidence from other domains and would abandon the theory if it were shown that it is incompatible with such evidence, say, from neurophysiology. Comparably, a linguist who postulates a *dag* will not merely propose that it is "true of" an organism in what appears to be Stich's sense of this notion. Thus suppose it were

discovered, say, by neurophysiological investigation or by psycholinguistic study, that all the linguist's data (and more) can be better explained by assuming that the organism has a system of perceptual strategies not involving the principles of generative grammar in any manner. The linguist who postulates a *dag* as "true of" the organism will be unperturbed. Linguists who take the realist position, claiming that a *dag* actually describes the speech mechanisms at work, might well abandon their formerly held comprehensive performance theory, with its idealized components and its specific principles and properties.

Now it might be argued that the perceptual psychologist, in the analogue, need not take a "realist" position; rather, if it is shown by neurophysiological investigation that some other theory is "true of" the organism, accommodating the data and much else, providing better explanations and deeper understanding, etc., then the perceptual psychologist can shift to this new theory, regarding it as "true of" the organism but not describing properties of the organism. Similarly, generative grammarians, faced with the discovery that a theory of perceptual strategies is superior in explanatory power, may shift to this theory taking it to be "true of" the organism but not as attributing to the organism specific properties and devices. If this tack is taken, then the alternative to the normal realist approach of the scientist is revealed to be merely a terminological quibble. Thus the physicist, following this approach, would no longer say that atomic theory postulates that matter consists of atoms, electrons, and so on; rather, the physicist says nothing about the structure of matter, but merely maintains that atomic theory is "true of" the world.

Stich's strategy II, understood narrowly, seems to amount to nothing more than an expression of lack of interest in certain problems; the problem of determining the nature of universal linguistic structure, the capacities that make language learning possible, the relation of logic to language, and whatever further insight the study of language might provide for psychology, philosophy, or neurophysiology. If grammars are not taken as theories about the internally represented systems of rules correlating sound and meaning, there is no problem for a theory of language acquisition to address itself to, since there is no longer a linguistic competence to account for as the product of language acquisition. Moreover, the notion of "linguistic universal" also is abandoned, since linguistic universals are principles common to the competence of native speak-

ers of every natural language.¹⁰ Abandoning this notion, we also relinquish any interest in the common structure of natural languages (should it exist). Moreover, we abandon the project of discovering general principles that determine the logical form of sentences in natural language (for example, determining how they are used in valid reasoning) on the basis of properties of postulated underlying structures; the search for such principles is hopeless, it would appear, if there are no universal properties of these underlying structures. All this follows, of course, only if we interpret Stich rather narrowly. If, on the other hand, he is merely proposing a terminological revision of the sort discussed in connection with perceptual psychology, we conclude that he is raising no substantive issue.

What, then, does Stich offer by way of counterbalancing advantages? He cites two. First, strategy II avoids the "puzzle" he brought up in connection with strategy I: now there is no need to make a unique choice among *dags* since there is no claim in strategy II that the selected grammar is actually internalized. Thus, the fact that there are competing evaluation measures for ranking *dags* is no problem. As Stich says:

Our project is the highly nontrivial project of producing a model that takes *pld* as input and yields an appropriate *dag* as output. Any evaluation measure that does the trick will be suitable (813).

This, however, is hardly an advantage if, as argued above, the alleged puzzle amounts to nothing more than the observation that linguistics is an empirical science. The second of the advantages that Stich claims for strategy II is that it accords better with actual linguistic practice than strategy I. Chomsky and others, Stich asserts, do not really study a broad range of languages in attempting to construct theories about universal grammatical structure and language acquisition, but merely speculate on the basis of "a single language, or at best a few closely related languages" (814). Stich's assertion is both false and irrelevant. Transformational grammarians have investigated languages drawn from a wide range of unrelated language families. But this is beside the point, since even if Stich were right in saying that all but a few closely related languages have been neglected by transformational grammarians, this would imply only that they ought to get busy studying less closely related languages, not that there is some problem in relating grammar construction to the study of linguistic universals.

¹⁰ Stich continues to use the term 'acquisition model' in connection with strategy II, but this usage seems to us incomprehensible on his grounds.

Stich confuses the matter further by introducing plainly irrelevant analogies. Thus he argues that "attributing knowledge of a grammar to a speaker is little more plausible than attributing knowledge of the laws of physics to a projectile whose behavior they predict" (816/7). Physicists postulate certain laws. Observing the behavior of a projectile, they attribute to it a structure that accounts for its behavior, given these laws. Thus, observing that the earth wobbles on its axis, physicists attribute to it the required bulge at the equator—and may go on to try to determine whether the earth is constructed as required by the theory. Following Stich's analogy, linguists postulate the "laws" of English grammar and attribute to the speaker a structure that accounts for its behavior, given these "laws." But what are the "laws," and what is the "structure"? Plainly, the "laws" are not like the laws of motion—they do not apply to rocks, or even to speakers of Japanese. In fact, under any intelligible formulation that has ever been offered, the "laws" apply only to an organism postulated to have an "internal representation" of the rules and principles of the grammar, along with other systems that interact with this internal representation to yield the judgments of grammaticality, acceptability, etc., that constitute the linguist's data, in accordance with the standard account. Since Stich does not address the problem why the "laws" do not apply to rocks or to speakers of Japanese, it is unnecessary for him to inquire into the structure of the organism that behaves in accordance with these "laws." Thus he is able to present such pointless and irrelevant analogies as the analogy to a projectile in motion that obeys the laws of physics.

Notice that a physicist might well conclude, observing a projectile, that it contains computing mechanisms that assess trajectory, change course, etc., employing an internal representation of the laws of physics. Thus, he might be led to this conclusion by observing how the projectile behaves under various modifications of the environment. If he were to obtain evidence suggesting that the projective has an internal computing structure, or even that it is not a servomechanism but seems to be aiming to reach a certain goal in some other fashion, or is acting in a still more complex way, he would not hesitate to postulate additional structure and to search for additional evidence of its existence and character. As a scientist, he would assume the reality of the structure he postulates in his theory of the projectile, and try to disconfirm or confirm this assumption. He would not be much interested if a philosopher were to tell him that he might rephrase his account of what he is doing, say-

ing rather than he is attributing no structure or properties to the projectile but is "merely" proposing a theory that is "true of" the projectile.

There remains the question whether the structure that we attribute to the speaking person can properly be called "knowledge of a language," "knowledge of grammar," "possession of a cognitive state," or something else. Stich does not raise the issue here, but he has elsewhere. His conclusions in this regard seem to us unacceptable.¹¹ At best, it is an open question whether more than an uninteresting issue of terminology is involved.¹²

Stich observes that in his strategy II (the nonrealist version of the standard account) "an element of indeterminacy still lurks." This is quite true. Again, it is a defining characteristic of empirical science that theories are underdetermined by raw data. As already noted, Stich rejects Quine's conclusion that there is some special kind of indeterminacy that plagues the study of language beyond that of physics. He then goes on to claim (815) "If we are disinclined to say that in all science, modulo the indeterminacy, there is nothing to be right about, it is because the theories we are willing to allow as correct are those whose arbitrary features have the sanction of tradition." Thus neither linguistics, nor biology, nor physics, has anything "to be right about," tradition aside. Stich promises elsewhere a defense of this position. It will be interesting to see whether his defense will amount to something more than (a) a *reductio ad absurdum* argument against the indeterminacy thesis as formulated, or (b) a terminological variant of familiar realist approaches. We remain skeptical. In any event, we see no reason for taking seriously Stich's proposal that physics and linguistics, as well as every other science, have nothing to be right about, tradition aside.

Moreover, Stich's claim that there is a "strong analogy" between his thesis about the selection of *dags* in strategy II and Quine's thesis about the indeterminacy of translation is, as far as we can see, both a misunderstanding of Quine's thesis and an unwarranted claim. Stich seems to think that the fact that a *dag* has no claim to uniqueness and that an analytical hypothesis on Quine's thesis has none either is enough to establish Stich's thesis as analogous to Quine's except for their implications (815). However, on Stich's

¹¹ See Graves, Katz, *et al.*, "Tacit Knowledge," this JOURNAL, LXX, 11 (June 7, 1973): 318-330.

¹² See Chomsky, "Knowledge of Language," *Times Literary Supplement*, 15 May 1969, excerpted from an essay to appear in K. Gunderson and G. Maxwell, eds., *Minnesota Studies in Philosophy of Science*, vol. VI. And also *Problems of Knowledge and Freedom* (New York: Pantheon, 1971), ch. I.

strategy II, a *dag* "correctly captures the intuitions of the speakers of the language (and the rest of the grammarian's data) within the limits of accuracy allowed by idealization" (807). Hence, the *dag* correctly predicts phonological, syntactic, and semantic properties of sentences (though without hypothesizing anything about an internalization). Therefore, since Stich's strategy II allows for the selection of a *dag*, it ought to choose between alternative analytical hypotheses, since Quine's analytical hypotheses state predictions about semantic properties of sentences. For any analytical hypothesis, say, *X* is synonymous with *Y*, the *dag* will correctly predict whether it is true or not. The point is this: Quine's thesis is a thesis about the tenability of meanings and the clarity and scientific status of semantic properties. Stich's is not, at least as far as we can determine.

Stich apparently interprets his strategy II in such a way that it leaves linguistics without the least interest to other scientific disciplines. He observes that "The modesty of a grammar, on my account, stands in stark contrast to more flamboyant portraits" (816).¹³ The "flamboyant portrait" turns out to be nothing more than the claim that the grammar in the form it would take in models of speech production and perception must structurally correspond to some features of brain mechanism. Stich wonders how a linguist could presume to know so much about "the structure of the brain, having left the skulls of his subjects intact" (816). His skepticism seems to derive from some variant of instrumentalism. Clearly, the linguist does not produce a priori pronouncements on the details of neural connections and pathways, but rather presents a modest inference from some very general features of effects to very general features of their causes, providing a sketch of what the causes would have to be like to be capable of producing the effect.¹⁴

But, given Stich's charge, it is perhaps worth while to sketch the rationale for such realistic inferences from effects. Such inferences are standard practice in science. Early physicists had no opportunity to look at the microstructure of matter, but, like present-day linguists, had to leave the "skulls of their subjects intact." Nonetheless, they were able to determine a great deal about the microstructure of matter from its behavior. For instance, they could infer that matter is discontinuous, composed of many tiny particles that are invisible,

¹³ Note that in Stich's view the best that one can say for doing grammar is to hope that it may lead to "deep insights into exciting phenomena of comprehension and communication" and "that it is something to do." Vague hopes and busy hands.

¹⁴ Katz, "Mentalism in Linguistics," *Language*, xl, 2 (April/June 1964): 133.

because on this account they were able to explain such phenomena as diffusion. But to explain observable effects like the change of the color of water when dye is put in it in terms of the migration of dye molecules into the spaces between the water molecules commits scientists to the reality of the unobserved causal conditions, since such explanations make sense only if we adopt the realistic assumption that the theoretical terms in which they are couched actually refer to things in nature. If one were to say that the term 'molecule' in the foregoing explanation is a mere *façon de parler*, that it denotes nothing real, the "explanation" would become nonsense.

These considerations are quite general, applying to linguistic explanations as much as to explanations in physics. We postulate certain properties of the brain, e.g., that it must contain some neural mechanism that stores the information in the grammar,¹⁵ on the grounds that properties of linguistic behavior can be explained by assuming them to be causal consequences of brain mechanisms with access to such information. To explain the grammatical judgments of speakers on the basis of such a hypothesized causal chain while denying the existence of essential links in the chain makes no more sense than to explain diffusion while denying the existence of molecules.¹⁶ Stich asserts:

Though Katz's claims about grammar are more expansive than those I have made, the evidence he uses to confirm a grammar is of a piece with the evidence indicated in my account. Thus, it remains something of a mystery how the grammarian has learned as much as Katz would have him know (816).

In reply, we can point out that some people may well be interested in making hypothetical inferences about underlying causes on the basis of certain evidence, while others with different interests and outlook may choose to restrict their attention more narrowly to the evidence. We can certainly imagine that some early physicists might have been quite happy to accept diffusion and similar phenomena at face value, chiding their Democritean colleagues for "flamboyant portraits" of atoms.

Finally, Stich argues that it is "perhaps misleading" for the grammarian to claim that he is "constructing a theory of the language of his subjects. Rather he is building a description of the facts of acceptability and linguistic intuitions" (817). Apparently this remains true even if the grammarian succeeds in the task that Stich correctly regards as "a monumentally impressive feat," namely, con-

¹⁵ This is the disputed question: cf. Stich, 816.

¹⁶ Katz, *Semantic Theory*, pp. 28/9.

structing an acquisition model in accordance with his strategy II. The reason why even this feat would produce only a description, not a theory, is that "A theory of a language seriously worthy of the name would provide some insight into what it is to *understand* a sentence, how sentences can be used to communicate and to deal more effectively with the world, . . . But a grammar does none of this" (817; see our note 13). Stich continues: "Indeed it is logically possible that there be a person whose linguistic intuitions matched up near enough with our own, but who could neither speak nor understand English" (817). Stich's point is that, since such a person would be equivalent to a native speaker for the purposes of grammar construction, grammar construction has no contribution to make to explain how speakers use and understand sentences. Since this is the only argument Stich offers to show that Chomsky's notion of grammatical competence is "explanatorily vacuous" and that grammars provide no "insight into what it is to *understand* a sentence,"¹⁷ it is worth considering it in some detail.

To begin with, we agree—in fact, we insist—that grammars are not full theories of sentence use. Indeed, this is the essence of the competence-performance distinction. But to conclude that theories of competence are "explanatorily vacuous" or mere descriptions is plainly a *non sequitur*. With equal force, one might argue that physics is explanatorily vacuous and provides only descriptions, not theories, because it gives us no insight into human behavior; who, after all, could be interested in mere inanimate objects and their properties? Plainly the distinction between "theory" and "description" and the notion of "explanatory force" are not dependent on the extent to which one or another person may be intrigued by the subject matter under discussion.

Pursuing Stich's argument, since we adopt the competence-performance distinction, we agree with him that it is logically possible that some creature might have full linguistic competence but lack the devices that are postulated in the more comprehensive theory of language use. Since, on Stich's account, the *dag* that is "true of" this speaker describes all his intuitions about grammatical properties and relations, then it describes in particular all semantic properties and relations. For each sentence, it determines whether it is meaningful, analytic, how many senses it has, to what sen-

¹⁷ Stich 816/17. We are, of course, by-passing the question of attributing knowledge of a grammar to a speaker, which does not really play an essential role in the present discussion. Cf. Stich, "What Every Speaker Knows," and Graves, Katz, *et al.*, *op. cit.*

tences it is synonymous (on some reading), etc.¹⁸ We fail to see, then, how this *dag* can describe all the speaker's intuitions about such properties and relations while offering no insight into how sentences are used and understood—though the creature postulated in Stich's logically possible case would, by hypothesis, not be able to proceed to use and understand sentences as do persons with the same linguistic competence embedded in a performance system. The fact that there could, in principle, be a creature whose linguistic intuitions "match up near enough with our own but who could neither speak nor understand English" shows no more than what the standard account assumes: a more comprehensive theory of performance must be constructed, incorporating grammars, to account for the use and understanding of speech.

In effect, Stich is arguing that a certain type of aphasia is logically possible: a case in which a speaker can give judgments about sentences and thus "serve almost as well as an English speaker as an informant for constructing a grammar of English," while being unable to speak and understand the language. From this observation he concludes that it is misleading to describe a grammar as a theory of the language of the linguist's subject. The conclusion, if anything, should be quite the opposite. The existence of this type of aphasia would be most congenial to the standard account, since it would indicate that it is possible for the competence system to function even when it is "disconnected" from one component of the performance system in which the standard account assumes it to be embedded. Whereas the standard account does not imply that such a case must exist, discovery of such a case would surely confirm its specific empirical assumptions about the speaker.

In summary, we see no reason to believe that Stich has offered any coherent challenge to the standard account of what the linguist is talking about.

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PARADOX REGAINED: A REPLY TO MEYERS AND STERN *

In "Knowledge without Paradox," Robert G. Meyers and Kenneth Stern argue that the problems raised by the Gettier counterexamples

¹⁸ Cf. Katz, *Semantic Theory*, ch. 1.

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