1 The Wherefores and Therefores of the Competence-Performance Distinction

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INTRODUCTION

How has it happened that the competence–performance distinction has come to be seen as invalid, or if valid, irrelevant, or if relevant, actually harmful to psycholinguistic research? This paper suggests three reasons for the present obloquy of the competence–performance distinction. (a) The grammar of a language does not have an automatic performance interpretation. That is, a model of competence does not contain a specification of a model of performance and does not entail a particular model of performance. (b) Candidate grammars keep changing. (c) In response to these two difficulties, psycholinguists have attempted to specify performance independently of competence. To the extent that they have been successful, they have suggested that the distinction between competence and performance is unnecessary and that competence itself is not a useful notion.

   This paper begins with some definitions and then presents a sociohistorical review of the competence–performance distinction. Two conclusions can be drawn. First, since the competence–performance distinction is a logical one, all psycholinguistic theories observe it, although they differ in what they take the content of competence to be. Second, most arguments about whether there is a distinction between competence and performance have been over other issues.

   Chomsky (1965) has equated competence with knowledge and performance with use. Stated in that general form, the distinction is applicable to any area of psychology where it is assumed that the person has knowledge *of* something or *that* something. In the case of syntax, a person has knowledge of

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a language; the knowledge consists, by hypothesis, of rules which, in various combinations, predict the syntactic properties and relations of sentences and parts of sentences. In the case of logic, to take another example, knowledge may consist of rules of inference predicting certain logical relations that hold among statements. Even more generally, people know facts that they use in solving problems, making decisions, answering questions, and so on. I refer to the distinction between knowledge and use as the *general* distinction.

As Chomsky (1977a) points out, all psychologists observe the general distinction. It could not be otherwise, because the distinction is not an empirical matter, but a logical one. No data could be brought to bear on the *general* distinction, any more than data could be brought to bear on the distinction between knowledge by acquaintance (i.e., knowledge through direct experience) and knowledge by description (i.e., knowledge at second-hand, see Russell, 1912).

What is an empirical matter is the character of the knowledge (if any), and the character of the device that accesses and uses the knowledge. (Similarly, it is largely an empirical matter which knowledge is by acquaintance, which by description, and which is perhaps from another source.) With respect to syntax and logic, it is a question of empirical fact whether people's syntactic knowledge takes the form specified in the syntactic component of a transformational grammar, and a matter of empirical fact whether a predicate calculus characterizes a portion of people's logical knowledge.

A transformational grammar is a particular hypothesis about what form linguistic knowledge takes. Many particular hypotheses are possible. For example, transformational grammars in which the semantic component interprets the syntactic component claim that a syntactic level exists called *deep structure*; generative semantics theories claim that there is no such level. That dispute is a dispute over how competence should be characterized, not a dispute over whether there is such a thing as competence. A similar dispute is over whether a grammar should include information about sex of the speaker or other "social" generalizations that interact with sentence acceptability. The dispute in such cases is over *how* to draw the competence-performance distinction not over *whether* to draw it. The class of theories including the standard theory, the extended standard theory, the revised extended theory, and so on draws the competence–performance distinction in a *particular* way in that all the theories in the class label some generalizations extralinguistic, and, within linguistic generalizations, label some syntactic, some semantic, and some phonological. I refer to the distinction between knowledge and use as characterized by most transformational grammars as the *particular* distinction.

To summarize, the general distinction between competence and perform-ance is a conceptual one and impervious to data. The particular distinction is a question of theory and evidence. Disagreements about how to draw the distinction do not bear on the existence of the general distinction.

If competence refers to knowledge and a linguistic theory is one claim about the nature of that knowledge, what does performance refer to? Performance refers to how knowledge is used. In the psycholinguistic literature it is common to restrict performance to behavior on particular tasks, such as speed to detect a phoneme within different sentence structures (Foss & Lynch, 1969), or accuracy in detecting a nonspeech noise in sentences as a function of distance from constituent boundaries (Fodor & Bever, 1965; Garrett, 1965). If a particular linguistic rule or structure is functional in such an experiment, psycholinguists call it *psychologically real.* Put another way, if a linguistic entity must be referred to to explicate an aspect of behavior, it is called *psychologically real* and held to be relevant to performance.

Within Chomsky's definition (Chomsky, 1965, 1975, 1977a), however, it is incorrect to limit the term *psychologically real* to a narrow class of performance tasks; the notion of performance must include any and all forms of possible performance, including the act of having intuitions. A linguistic entity is psychologically real if it is part of knowledge. It would be strange if knowledge were never put to use, but it is logically possible, because the relevant situation might never arise. (For example, I know the meaning of the word *ruching,* but I may never be called upon to perform a task making use of that knowledge.) By Chomsky's definition, the correct linguistic theory characterizes knowledge. Because knowledge is mental or psychological, the correct linguistic theory is also a psychological theory.[1] Linguistic entities are psychologically real if they correctly characterize competence. I henceforth avoid the term *psychological reality* and use the term *performance,* or *use,* to include any and all performance data that require reference to linguistic entities for their explanation (so as to distinguish them from nonspeech performances like bicycle riding).

Thus far, competence and performance are each unproblematic, as is the distinction between them. I suggest that the difficulties involved in constructing a psycholinguistic theory have led some to the invalid conclusion that the distinction is problematic. As mentioned earlier, there have been two main difficulties in constructing a performance model. The first is that a theory of knowledge does not entail a particular theory of use. Essentially this point was made by Fodor and Garrett (1966). Transformational grammar entails the dismissal of some use theories; e.g., a theory stating that people have stored thousands of sentences and pick the one they need for each speaking occasion. Knowledge imposes constraints on use. Within the constraints imposed by knowledge, however, is an extremely large range of

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[1] A separate question is whether a grammar should be taken to be, as Chomsky proposes, a specification of competence, in addition to being a theory of a language. Katz (1978), for example, puts forward a Platonist view of grammar, in which a grammar is only a theory of a language. Another theory will characterize competence or knowledge of grammar. Even on a Platonist view, knowledge is distinct from use.

use theories. There is no automatic performance interpretation for a model of competence.

The second difficulty compounds the first. The candidate transformational grammar of English changed dramatically from 1957 to 1965, and since 1965 there have been many candidate transformational grammars. A use theory must be compatible with the correct knowledge theory; it must make room for the entities that a correct knowledge theory posits. For a psycholinguist the difficulty is extreme uncertainty about what linguistic structures and rules to accommodate within a use theory.

Both difficulties are inherent in any applied science. In response to the difficulties, psycholinguists have tried to constrain use and knowledge theories from the use end. By finding out more facts about performance, the reasoning goes, we can constrain what a performance model should look like. The projected performance model, the reasoning continues, will be also compatible with only some linguistic entities and will therefore constrain what a competence model should look like.

The reasoning is correct and is a good research strategy. Adopting it is not logically tantamount to dismissing the competence–performance distinction, but one unfortunate by-product of its application has been the dismissal of the distinction and the dismissal of transformational grammar as a model of competence. It also has a built-in danger. If performance is construed narrowly, to include only a subset of linguistic behaviors, the wrong projection will be made to a competence model as well as to a performance model.

DERIVATIONAL THEORY OF COMPLEXITY, PRO AND CON

Early psycholinguistic theory (Miller, 1962) proposed a close, direct connection between a model of competence and a model of sentence perception, one component of a model of performance. The theory came to be called the *derivational theory of complexity* (DTC). Every grammatical transformation between deep and surface structure represented a computation that listeners made in reverse in order to uncover the deep structure of a sentence. The computations were presumed to occur serially, so that total computation time would be some function of the individual computations.

The theoretical advantage of DTC lay both in its simplicity and in the fact that it provided a principle for a performance interpretation of the grammar: every grammatical operation corresponded to a real-time psychological operation. A further point is that the proposal was plausible in that it assumed that important listening operations, such as semantic analysis, were calculated over deep structure. Since the grammar relates deep and surface

structure by transformations, a reasonable first hypothesis was that the processing mechanism did also.

If DTC is incorrect, it is incorrect as a model of performance. From the falsity of DTC nothing would follow about whether a particular linguistic entity is or is not a feature of a model of competence. Nothing even follows about whether that entity is a feature of a correct model of performance. Transformations, for example, might properly play a role in a model of performance; the falsity of DTC would show it could not be *that* model.

The history of DTC has been reviewed in many places (e.g., Fodor, Bever, & Garrett, 1974), so only a brief summary is presented here. The initial experiments that tested DTC were confirmatory (Mehler, 1963; Miller, 1962; Miller & McKean, 1964; Savin & Perchonock, 1965; Slobin, 1963), although later work has failed to replicate some of the earlier studies (Matthews, 1968) or has suggested alternative nonsyntactic explanations for the effects (Epstein, 1969; Glucksberg & Danks, 1969). It should be noted, however, that no experiment "disproved" DTC, and as a first approximation DTC may be correct.

More important than the experimental results was the change in the type of transformational grammar proposed for English. DTC had been based on the 1957 grammar, in which negatives, questions, and passives were derived from active affirmative declarative strings. The theoretical changes resulting in the 1965 grammar meant that DTC was still a possible theory, but for a different set of sentences than those to which it was originally applied. What had previously appeared to be a homogeneous set of sentences (homogeneous in that they were all transformationally related to the same deep structure) was now a mixed collection. There was still a theory, and still a set of results, but the theory (updated to accommodate the 1965 grammar) was no longer applicable to the results.[2]

Fodor and Garrett (1966) make two important arguments about DTC and its implications for psycholinguistic theories. They suggest that a more abstract relation exists between a model of competence and a model of performance than DTC proposes and that structural descriptions are psychologically real, but transformations are not. They support their claim for the psychological reality of structural descriptions (by which they mean the deep and surface structure pair) by noting that listeners must have access to

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[2]Fodor, Bever, and Garrett (1974) point out that DTC, properly applied, would not even have predicted the obtained results with a 1957 grammar. For example, in converting a passive (P) to a passive question (PQ), only one transformation is required; but in converting a question (Q) to a PQ, three transformations are required. This is because the passive transformation must occur before the question transformation. Thus, a P can be directly converted to a PQ, but a Q must first be converted to a declarative, then to a P, then to a PQ. The obtained results do not show conversion of a Q to a PQ to be three times as hard as conversion of a P to a PQ nor was that asymmetry predicted, but it should have been.

structural descriptions in order to judge the ambiguity, grammaticality, and so on, of word strings. They support their claim for the nonreality of transformations by noting that not all examples of apparent greater transformational complexity are paralleled by greater perceptual complexity.[3]

Fodor and Garrett also note that there is no reason to expect isomorphism between the linguistic steps of generating a sentence and the steps of the listener's processing mechanism. The listener could use different rules to represent the same information that the grammar represents by using phrase structure rules and transformations.[4]

Fodor and Garrett's (1966) claim that transformations are not psychologically real only holds if psychological reality is equated with performance on a small set of tasks. Because that equation is not justified neither is the claim that transformations are not psychologically real. The justifiable claim is, at most, that people can perform many linguistic tasks without (implicitly) referring to transformations or intermediate phrase structures, i.e., without constructing a full derivation of the sentence being responded to.

Fodor and Garrett (1966) correctly infer that a performance model requiring that people always generate a full derivation of a sentence, no matter what the linguistic task, will be incorrect. (Since DTC was not spelled out thoroughly, it is not clear if it made such a claim about talker–listeners.) An "abstract" relation between competence and performance then means, simply, that a performance model is not a device that, in producing speech, always generates sentences from an initial S symbol nor one that, in understanding speech, always generates derivations from a surface string. It does not mean that a competence model is not "incorporated" in a performance model; it means that not all aspects of a competence model are used in every example of performance.

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[3] Some of the "negative" experiments Fodor and Garrett (1966) cite (e.g., the ease of short over full passives, of adjectives over relatives) are no longer relevant, because the putative derivations of the sentences involved have changed. (For example, short passives are not derived from full passives, so that they would not have more transformations, and, therefore, DTC would not predict them to be more difficult psychologically.) Modifications in transformational grammar have frequently had the effect of eliminating the psychologically negative cases, although that was not their aim.

[4] Both Fodor and Garrett (1966) and Bever (1970) split structural descriptions and grammatical "operations" as if structural descriptions were not themselves the output of grammatical operations or rules. In addition to proposing rules or strategies to replace transformations as a means of arriving at deep structure from a surface string, they must also propose rules to replace the phrase structure rules that determine constituent structure. In trace theory, the surface phrase marker provides all the information needed by the semantic and phonological components. It does so, however, because traces of all transformations that deform structure in ways that are relevant to semantic interprestations remain as clues to grammatical relations. It is not clear what consequences trace theory would have for Fodor and Garrett's attempted split between rules and structures.

Yet, Fodor and Garrett (1966) conclude that a performance model need not incorporate a competence model; what is left out of the performance model, in their view, is transformations. Their conclusion, however, would follow only if transformations were *never* exploited in a linguistic task. The existence of intuitions indicates that transformations are sometimes exploited, and in the concluding section I discuss other tasks that implicate transformations. Only under one interpretation of "abstract" is the relation between competence and performance, as Fodor and Garrett claimed, abstract: people do not construct a full derivation of a sentence in all linguistic tasks.

Fodor and Garrett (1966) can be interpreted as pointing out the first difficulty mentioned in the introduction, that there is no automatic performance interpretation of the grammar. Although Fodor and Garrett may have construed the concepts of psychological reality and performance too narrowly, such that some of their conclusions do not follow, their primary point holds. There is an abstract (i.e., indirect) relation between competence and performance.

WATT

Watt (1970), like Fodor and Garrett, also concludes that a performance model should not be thought to be isomorphic to a transformational grammar, but he goes on to propose a different relation between grammar and performance. Watt takes the lack of correspondence between transformational grammar and psychological operations to be an argument for the postulation of two grammars, one of which is an abstract linguistic grammar, taking a transformational form, the other a mental grammar, to which psychological operations would correspond.

Watt (1970) proposes that a linguistic grammar not be identified with a mental grammar because of the same sorts of evidence that Fodor and Garrett considered.[5] Whereas the evidence led Fodor and Garrett to argue for an abstract relation between grammar and performance, it leads Watt to propose a direct relation between a mental grammar and performance and nonidentity between a mental grammar and a linguistic grammar. Watt accepts what Fodor and Garrett specifically reject, that there should be a direct correspondence between grammar and performance. Since the evidence and need for a linguistic grammar are so strong, and therefore a linguistic grammar cannot be eliminated, the only way out is to conclude that there are two grammars, one mental and one abstract, such that a mental

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[5]Watt notes that many unexplored variations of DTC might be successful, although he does not discuss the variations. One of them, that basic operations may be relevant in performance, is explored by Foss and Fay (1975), Fay (1974), Mayer, Erreich, and Valian (1978); and Valian, Erreich, and Mayer (1978). Their research is discussed later.

grammar does not take the form of a transformational grammar. The mental grammar is that body of knowledge which a performance mechanism utilizes, where performance is narrowly construed. The mental grammar, like the abstract grammar, supplies derivations for sentences, but supplies different ones. According to Watt, for example, the linguistic grammar derives the truncated passive from the full passive,[6] but the mental grammar does not.

Watt (1970) rejects Fodor and Garrett's (1966) proposal for two reasons. First, he argues that "no plausible performance factor actually seemed capable of accounting for all discrepancies [p. 192]" between competence and performance. Second, Fodor and Garrett's assumption of an identity between a mental grammar and a linguistic grammar had no "a priori warrant." Both his arguments could be true without damaging Fodor and Garrett's primary point that competence and performance are indirectly related. First, the lack of sufficient performance principles to explain the apparent "discrepancies" between performance and competence could represent present lack of knowledge or could mean that the correct relation is even more indirect than Fodor and Garrett first supposed.

Second, although there may be no a priori reason to assume that a mental and linguistic grammar are identical,[7] there is also no a priori reason to suppose that a performance mechanism should directly interpret a mental grammar, even if a mental grammar is nonidentical to a linguistic grammar. Watt can be correct in supposing that the two grammars are distinct, but Fodor and Garrett's (1966) basic position that there is no reason to expect a competence model also to serve as a performance model can be correct, too.[8] Whether the grammar that is serving as a competence model is a mental grammar or a linguistic grammar is irrelevant. Watt does not challenge Fodor and Garrett's basic argument. He conflates two issues (Watt, 1970, 1974). Whether a linguistic grammar is a mental grammar is a different question from whether a mental grammar has a direct performance interpretation. Watt seems to assume that if a linguistic grammar is not a model of competence, then the actual competence model will have a direct performance interpretation. That does not follow.

Watt (1970) supposes, in addition, that the mental grammar's contents will be determined solely by investigations of on-line processing; a predictable problem results. Watt recognizes that people can make more generalizations

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[6]The example is no longer relevant since "short" passives are no longer considered to be truncated forms of full passives, but are independently generated (Culicover, 1976).

[7]There is at least a historical reason to assume that a mental and linguistic grammar are identical, because Chomsky's (1965) theory explicitly proposed that.

[8]If Watt's suggestion of a separation between a mental grammar and a linguistic grammar is correct, then, it is so independently of the arguments he gives for it. See Katz (1978) for discussion of this topic.

and grammatical judgments than would be possible on the basis of a mental grammar built to mediate on-line processing alone. To solve the problem, Watt suggests that the performance mechanism has access to the mental grammar plus an "archival faculty."[9] The two together comprise the "linguistic faculty." The archival faculty seems to include that portion of the linguistic grammar that was left out of the mental grammar because it did not directly relate to performance. Watt has reinvented the competence–performance distinction. Knowledge is distinct from use; use involves more than on-line processing; knowledge does not entail a particular theory of use.

One historical consequence of Fodor and Garrett's paper (1966) proposing an abstract relation between competence and performance was to make people think that there were no empirical constraints on a model of competence. Any deviation from what the grammar supposedly predicted could now be brushed away as the result of "performance factors," or so Watt (1974), Derwing (1973), and others have mistakenly concluded. The point, however, is that the grammar does not make predictions about performance; it makes predictions about grammatical properties and relations. Fodor and Garrett may have misstated a true claim, but the claim is true. The claim is that knowledge does not carry with it a blueprint for how it is going to be used. Fodor and Garrett's misstatement was that the performance model does not incorporate a competence model, as if incorporation required "direct" implementation of the grammar à la DTC.

From the true version of Fodor and Garrett's (1966) claim, nothing follows about whether empirical results constrain competence models. The view that Watt, Derwing, and others appear to take is that only some kinds of data count as empirical results. Watt (1974), for example, states that although intuitions "may warrant the psychological reality of the *language*, [they are] patently inadequate as guarantors of psychological reality for the rules of a *grammar* [p. 363, Watt's italics]." It is unclear what Watt might mean by this claim (which he attributes to Bever). All data, whether intuitional or "experimental," are subject to error and do not guarantee anything. If Watt's supposition that the mental and linguistic grammars are different is correct, one would, if anything, suppose that intuitions would reveal mental, rather than linguistic, structures.

Perhaps Watt (1974) means that intuitional data alone are insufficient to confirm a grammar. Just as it is a mistake to limit performance to a few aspects of on-line processing, it would be a mistake to limit it to the act of having intuitions. But saying intuitions should not be the only source of evidence is different from saying that they should not be a source of evidence at all. A theory that uses intuitions as data can be an empirical theory.

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[9] In a later discussion of the archival faculty, Watt (1974) abandoned it.

BEVER

In an extremely influential article, Bever (1970) suggested that the distinction between competence and performance was "artificial."[10] Bever's article has incorrectly been viewed as proof that competence and performance cannot be separated, that grammars are suspect because intuitions are subject to error, and that (as some psychologists and linguists had already believed) the competence–performance distinction removed the study of grammar from an empirical realm.

Two aspects of Bever's (1970) paper are particularly important. The first is the range of "nonlinguistic" principles, or strategies, that he proposes to mediate surface strings and meanings, replacing transformations. The second is his proposal that nonlinguistic structures and processes will determine which of the possible languages specified by universal grammar can be learned, and, of those that can be learned, which rules within them will be exploited. The second proposal has usually been referred to as proposing that performance affects competence. My analysis of Bever's proposals will claim that many of the "nonlinguistic" strategies may be linguistically derived, but that even if they are purely perceptual, their behavioral importance is irrelevant to the competence–performance distinction.

Bever's (1970) suggestion that perceptual and cognitive structures and processes constrain what languages can be learned is, at a general level, a suggestion that you can only learn what you can learn. That has to be true, because it is a tautology. On a Chomskian view (Chomsky, 1977b), linguistic theory specifies all the possible natural languages, of which the learnable ones are a subset. There is no more reason for linguistic theory to specify only the learnable languages than there is for a particular grammar to specify only the understandable sentences. (See Wexler, Culicover, and Hamburger [1976] for an opposing approach in which linguistic theory does specify only the learnable languages.) What picks out the learnable subset is other mental structures and processes that together compose a simplicity metric. (For example, some possible languages might be learnable if people lived longer, or communicated via pencil and paper.) Universal grammar specifies *potential* competence; other mental structures select what will become *actual* competence. Depending on the nature of the mental machinery in which universal grammar is embedded, different particular grammars will result.

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[10]In later papers, Bever (1971, 1972, 1974a, 1974b, 1975; Bever, Carroll, & Hurtig, 1976b; Bever & Langendoen, 1971) clarifies and develops his 1970 views. For example, the distinction is indeterminate rather than artificial, and his remarks are to be taken as concerning candidate, rather than true, grammars. Only the 1970 paper is discussed in the text because it had the maximal historical impact.

Similarly, the mental machinery of language users will determine how frequently various rules will be used and under what circumstances. Just as it is not the task of linguistic theory to determine why some rules are used more than others, it is not the job of linguistic theory to determine what languages are learned. Bever (1971) makes a similar point.

Within the context of a Chomskian linguistic theory, Bever's general form of the proposal is not a challenge;[11] it does not call the competence–performance distinction into question. Performance features affect the selection of a competence system that will be learned and the selection of forms within a competence system that will be used in performance. But what is learned is still distinct from how what is learned is used, and from the principles that determine what will be learned and how it will be used.

Bever's (1970) specific proposals, in which particular strategies are claimed to exert a particular effect on what is learned, are not tautologous and are, therefore, of more interest. Even if the strategies have the effect Bever claims, however, they still do not call the competence–performance distinction into question. They at most demonstrate properties of the performance system that select which of the potential languages will actually be learned, just as they may select which rules get the most use.

A separate question concerning the strategies Bever proposes is whether they work. They must work at two levels. First, they must be nonlinguistic, rather than being derived from linguistic rules or principles; second, they must select the to-be-learned grammar and the to-be-used forms within a grammar. Using two examples, I claim that the strategies have not been shown successful at either level.

As an example of a nonlinguistic strategy influencing the form of the (learned) grammar, Bever (1970) proposes that children interpret initial noun–verb (NV) sequences as starting the main (or only) clause of a sentence. He suggests that this is responsible for the fact that initial subordinate clauses are marked as such with a subordinating conjunction (such as *while, because*). Such marking insures the proper implementation of the nonlinguistic strategy. The child and the adult can use this strategy without fear of error, because a subordinate clause will always be marked as such by a conjunction if it is the first clause of a sentence. If there is no such marking, the listener can correctly interpret the first clause as the independent clause. Thus, Bever suggests, the existence of a main clause strategy is an explanation for the fact that initial subordinate clauses are clearly marked and that the (learned) grammar will not generate a sentence with an initial subordinate clause unless it is marked.

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[11]What would challenge a Chomskian linguistic theory is the demonstration that nonlinguistic mental structures specify what is a possible natural language, as opposed to a learnable one.

There are other kinds of strategies, however, that do not have the result of constraining the (learned) grammar. For example, children of a certain age and adults on certain occasions tend to interpret an NVN sequence as corresponding to an actor-action-acted upon (or object) relation. As a result, children at one age (3½ to 4 years) misinterpret passive sentences as actives, and adults are unable to find a meaning for, or unable to recognize as grammatical, sentences like (1), where there is a reduced relative clause.

(1) The horse raced past the barn fell.

It is interpreted as a sentence with an extra word, *fell,* tacked on at the end.

What is not clear in Bever's (1970) examples is why some strategies lead to the grammar's not generating certain strings (as in the main clause strategy), whereas others (as in the NVN strategy) do not affect the grammar. What are the properties of the main clause strategy such that the form of the grammar is influenced? What are the properties of the NVN strategy such that the grammar is not influenced? Bever does not address this question nor the possibility that the influence could be directed in the opposite direction, from the grammar to the strategies. The main clause strategy, for example, could be derived from the grammar, rather than the reverse.

The NVN strategy, however, could not be interpreted in this way. It operates strongly for adults with sentences like (1), but there are many other sentential constructions that violate this strategy and yet are easy to process, such as (2), (3), and (4).

(2) The horse was kicked by the cow.
(3) The army suffered defeat.
(4) The statue interested John.

Thus, not only does the (learned) grammar allow such sentences, but the NVN strategy seems to be bypassed in their interpretation. Children have trouble with at least some of these sentences, but adults do not. Sentence (2) is a typical "reversible" passive in which either noun phrase (NP) could logically be the actor. In (3) and (4) there is no correspondence between the nouns and the roles of actor and object.

The NVN strategy is not stated very precisely by Bever (1970), so that later formalization could eliminate some of the examples given above from consideration. With respect to the strategy's present form, examples (2), (3), and (4) suggest that the strategy is too broadly formulated or else that it admits of too many counter-examples. It seems clear that the initial development of the strategy comes through the statistical preponderance of certain sentence types (where NVN does correspond to actor-action-object) over others. This is only a partially successful explanation, however, because

it does not account for why the strategy should be abandoned for a large number of cases and reserved for a few such as (1). It could be the case that there are particular grammatical properties to those cases where it is in force.

From these two examples of strategies, it can be seen that when a strategy selects a grammar cannot be predicted and that in the case where the grammar is constrained in a particular way[12] it can be argued that the grammar's constraint selected the strategy, rather than the reverse.

Yet another argument that Bever (1970) uses to call the competence–performance distinction into question is that linguists' intuitions are fallible and are themselves the product, in part, of a performance system. His conclusion here is a non sequitur, because it can only apply to candidate grammars, not true grammars. In his 1974b paper Bever makes clear that he is talking about candidate grammars, not true grammars, whereas in his 1970 paper he does not specify that it is a candidate grammar that will be affected by the fallibility of intuitions. Although any actual candidate grammar may be subject to the properties of those who are constructing it, it does not follow that the actual candidate grammar is a correct grammar or that a correct grammar will be subject to such considerations. The validity of the competence–performance distinction is independent of what model of competence linguists propose. It assumes that there is a correct model but not that we can necessarily discover it. If Bever's reasoning is correct, it may be difficult to construct a "true" grammar, but this does not alter the theoretical status of the distinction between competence and performance.

The fallibility of intuitions is irrelevant to the status of the true grammar. It would also undermine current methods of grammar construction only if there were some other source of data which was infallible, or if there were a less fallible source of data. Intuitions are not, however, provably more fallible than any other source of data. Further, they have contributed to a systematic theory of language, which no other data source has. I am not arguing that other data sources should be ignored but that intuitions should not be ignored, either. Also see Bever (1971, 1974a) for discussion.

In a related discussion, Bever (1970) suggests that there may be no reflection of competence in performance, outside of the having of intuitions: "I have argued that a proper understanding of the behavioral and phenomenological nature of "basic linguistic intuitions" forces us to reject the claim that a linguistic grammar is in any sense internal to such linguistic performances as talking and listening [p. 344]." Then he proposes that the relation between grammar and performance may be not abstract, but nonexistent. Thus, Bever distinguishes two kinds of psycholinguistic behavior, both of which probably involve nonlinguistic determinants, one of

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[12]English's use of direct quotation may be a counter-example (see Fodor, Bever, and Garrett [1974] for discussion of this point).

which may have no linguistic determinants at all. One behavior is having intuitions, the other behavior is normal talking and listening. A grammar may be psychologically real in the sense of contributing to intuitions without being psychologically real in the sense of contributing to normal talking and listening.

A problem with this part of Bever's analysis is pointed out by Elizabeth Sharpless (personal communication, 1975). If there is a sharp division between intuitions and comprehension-production, such that the grammar partially accounts for intuitions but may account not at all for comprehension-production, there is no reason for properties of the comprehension-production system to affect intuitions and, thus, no reason for them to affect the form that the grammar will take. Further, the developing child's nonlinguistic strategies should not affect the form of the grammar, because, on Bever's view, the child is not acquiring a grammar for any purpose other than to systematize its intuitions; it is not acquiring a grammar as a base for talking and listening, but is acquiring some other system for that purpose. Thus, as in the case of adult performance principles, by Bever's own argument that intuitions and on-line processing are to be separated, there is no reason for the grammar, which deals with linguistic facts, to be constrained by performance. Sharpless's argument is compelling.

In summary, Bever's 1970 paper neither casts doubt on the competence-performance distinction nor on the validity of linguists' enterprise. Bever proposed the existence of strategies instead of transformations. Investigation of strategies was a way of finding out about a performance model and, in turn, seemed to be a way of specifying not only what was part of knowledge but what could be part of knowledge.

CLARK AND HAVILAND

Clark and Haviland (1974) make the most direct attack on the competence-performance distinction. As seen in the following discussion, their arguments are irrelevant to the *general* distinction and do not succeed in showing that the *particular* distinction (as drawn by Chomsky, 1965) is incorrectly drawn. Clark and Haviland are a paradigm case of incorrectly thinking that arguments about how to draw the distinction bear on whether the distinction should be drawn. Their paper represents a culmination of the attempt to have a theory of performance constrain and determine a theory of competence.

First, Clark and Haviland (1974) try to show, using four different examples, that many linguistic phenomena are better handled by "process" rather than "structure" theories. That is, appeals to the structures of the sentences in question do not suffice to determine people's interpretations of them. For example, the sentence sequence "See those two people over there?

He has rabies" will be considered unacceptable if both people are men (or women), but acceptable if one of the two people is a woman. Since there is just one sentence sequence, with just one structure, structure is insufficient to explain people's interpretation. That is true. Two possibilities are then open: (a) explain the facts outside of grammar, leaving grammar alone; (b) explain the facts within grammar, changing the grammar as necessary. In Clark and Haviland's example, as in the others they offer, they apparently assume that whatever contributes in an orderly, rule-determined way to acceptability judgements should be handled within grammar. But that assumption is illicit. The claim requires justification outside of the theory that assumes it to be true.

Presumably, some cases of unacceptability should clearly not be handled within grammar. For example, if someone utters obscenities in a religious ceremony, the utterance will be unacceptable, but to try to account for the unacceptability within grammar would be absurd.[13] Some argument must then be made for new cases, to see whether they are like swearing, where the *utterance* is unacceptable relative to the context in which it occurs, or like saying, "she been has running," where the *sentence* is ungrammatical, relative to the rule system of English. But Clark and Haviland (1974) provide no argument for supposing that their examples fall with cases of sentence nongrammaticality rather than with cases of utterance inappropriateness. Hence, their demonstration that appeal to structural factors alone is insufficient in explaining people's interpretation of some sentences is irrelevant. What is needed is not a demonstration that the disputed cases require process explanations, but a demonstration that *no* cases require only structure explanations. Linguists who claim that the disputed cases do not fall within grammar will be quite happy to hear that there is a performance explanation of them; that is what they would predict.

Clark and Haviland's (1974) examples, then, challenge neither the particular nor the general competence–performance distinction. To challenge the particular distinction, Clark and Haviland would have to show that no linguistic phenomena can be satisfactorily handled with just a structure explanation. They do not attempt to do that. To challenge the general distinction, Clark and Haviland would have to show, on logical and conceptual grounds, that the distinction was unmotivated. They do not attempt that, either.

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[13]If Clark and Haviland (1974) do not consider that absurd, everything is within grammar. We then have to divide the domain of grammar into subcomponents, because it would be unwieldy otherwise. The same dispute about how to handle certain phenomena, such as those discussed in the text, would still occur, but with a different terminology. See Chomsky (1972) for a similar reply to G Lakoff.

I turn now to the second relevant aspect of Clark and Haviland's (1974) paper in which they first doubt that the grammar will be a component of a performance model. They next argue that by specifying a performance model in complete detail, cognitive psychologists will be able to account for all grammaticality judgments and all judgments about grammatical properties and relations; grammatical phenomena will be a by-product of the performance model. They further explicitly assume that people have a variety of kinds of knowledge available to them, including linguistic, that will be divided into the usual subcomponents. There is an internal contradiction here. If a grammar is not a component of a performance model, then what are Clark and Haviland calling the linguistic knowledge that *is* part of a performance model? It is inconsistent to speak of grammatical phenomena as being a by-product of a performance model when a necessary ingredient for their explanation, a grammar, has been built into the performance model.

The remaining problem to discuss is Clark and Haviland's (1974) contention that even if grammar and performance are conceptually separable, they cannot be studied separately. In discussing their contention, I use arguments similar to those made by Dretske (1974) and Katz (1977), the latter of whose arguments are made on behalf of a Platonist conception of grammar. Their arguments are adapted to show that similar reasoning will work for a Chomskian conception of grammar.

One aspect of Clark and Haviland's (1974) contention is definitely true. Our explicit, scientific knowledge of grammar comes via intuitions, which are a form of performance. In Katz's (1977) terminology, the *source* of information about grammatical properties and relations is people's judgments, which are the output of performance. Similarly, physical properties and relations are investigated by using, among other things, people's judgments about meter-readings In the nonempirical realm, mathematical properties and relations are investigated by using people's mathematical judgments.

Yet, although the *source* of information is people's judgments, the *import* of the information is, as the case may be, a linguistic theory, a physical theory, a mathematical theory. The implication of Clark and Haviland's (1974) contention is that grammar should only be arrived at through a model of how people process sentences; grammar, on their view, will be a by-product of a performance model. Imagine telling a physicist that physical theory should only be arrived at through a model of how bridges, rocket ships, and so on, are built. Or imagine telling a mathematician that mathematical truths will be a by-product of a model of how people add and subtract, have mathematical intuitions, and so on.

The physicist or mathematician would properly reply that that would be a strange way to do physics or mathematics, although a reasonable way to study how people put physics or mathematics to use. The reason it would be a

strange way to do physics or mathematics is that you would have to put into the performance model the very facts you wanted to get out as a by-product.

My counter-argument to Clark and Haviland (1974) does not imply that study of, for example, what contributes to intuitions is useless for linguists (it is, of course, interesting in itself as a part of performance). It would allow linguists to check their intuitions in new ways. Linguists already perform mini-experiments to check their intuitions. For example, they try out sentences with different lexical items to see if the phenomenon is general across all words, whether it is an artifact of particular words, or whether it holds for one class of words but not for another. In other words, linguists already do experiments aimed at verifying and pinning down their intuitions. The psychological study of the act of having intuitions could help linguists do even better "experiments." In particular, the study of intuitions might help explain why unclear cases are unclear, why, for example, it is hard to decide whether "That slipped me by" (instead of "That slipped by me") is ungrammatical.

My counter-argument also does not imply that psychologists should forego trying to constrain performance models by investigating properties of performance that cut across the material used in performance. For example, facts about memory may be important to a performance model of language, even if the memory facts come from experiments on the memory of digits. Such experiments provide an important source of hypotheses for perform-ance theorists.

What my arguments do imply is that: (a) there is no problem with the general competence–performance distinction; (b) although there may be a problem with the particular distinction, Clark and Haviland (1974) have failed to demonstrate that; (c) performance models cannot supplant competence models because they must assume, as one component, the information they want to explain as a by-product; and (d) a truism: both competence and performance models are needed.

ARTIFICIAL INTELLIGENCE

The artificial intelligence (AI) literature provides a potentially serious kind of complaint about the particular form of the competence–performance dis-tinction, though not about the general distinction, because the AI research presupposes a distinction between knowledge and use. For example, Winograd (1972) suggests that a transformational grammar is inadequate as the linguistic knowledge component in a program built to respond appropriately (within a limited world) to English sentences. If he is correct, there is then empirical evidence that a transformational model of competence

is not a component of a model of performance, at least for computers operating within an extremely limited domain.

The demonstration of the inadequacy of transformational grammar would not bear on the general distinction between knowledge and use, but it would show that knowledge has been incorrectly characterized. Notice that in Winograd's programs, as with the performance (augmented transition network) model developed by Wanner and Maratsos (1974), linguistic (and other) knowledge is explicitly made a component of the model. In principle the different types of knowledge are separable, even though they are accessed in parallel (or semi-parallel) by the user. Although programs are procedural, the procedures operate on knowledge. The question is how to characterize the knowledge. Hence, my discussion here focuses on the particular competence-performance distinction.

Dresher and Hornstein (1976) argue that Winograd (1972) was too quick to reject transformational grammar as a model of the knowledge component. Halliday's Systemic Grammar, which Winograd incorporated in his program, accounts for the necessary syntactic facts, but so does transformational grammar. Systemic Grammar, however, was apparently more directly interpretable within a performance model than was transformational grammar, and, therefore, was easier to work with. The first difficulty mentioned in the introduction, that transformational grammar has no automatic performance interpretation, is relevant here. As a performance model for a computer in a restricted world, transformational grammar did not work as well as Systemic Grammar apparently did. In addition to the question of how many of the problems that arise with computers might be expected to arise with humans is the question of the importance of direct incorporability.

As Dresher and Hornstein (1976) point out, a performance model that accessed a transformational grammar in an indirect manner might also work, because the same information is also represented in a transformational grammar. It cannot be concluded that something is wrong with transformational grammar because one performance model incorporating it is incorrect. Their argument is similar to Fodor and Garrett's (1966) argument that an indirect relation exists between a competence model and a performance model. A transformational grammar is not a set of temporal instructions.

Tyler and Marslen-Wilson (1977) make a similar error to that made by many AI researchers. They assume that, because one performance model incorporating transformational grammar is incorrect, no performance model incorporating a transformational grammar could be correct. This is like assuming that if the Brooklyn Bridge falls down, physical theory should be scrapped.

Tyler and Marslen-Wilson (1977) demonstrate that listeners use semantic information within a clause to select one of two syntactic parsings; thus

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Fodor, Bever, and Garrett's (1974) claim that semantic processing proceeds clause-by-clause is false. So far so good. Tyler and Marslen-Wilson (1977) go on, however, to conclude that their results:

> cast doubt upon the viability of using a transformational generative grammar...as a basis for a psycholinguistic processing theory. According to the conventional interpretation of the implications of such a grammar for a performance system, the syntactic structure of an entire clause or sentence must be computed before a semantic representation can be assigned [p. 690].

The conclusion about the relevance of transformational grammar for a processing model is a non sequitur. The theory being tested in Tyler and Marslen-Wilson's (1977) experiment is not the theory of transformational grammar, it is a psycholinguistic theory about how people process sentences. Their evidence shows that the psycholinguistic theory offered by Fodor et al. (1974) is incorrect in one of its main claims. For their evidence to bear on transformational grammar's suitability for incorporation in a performance model, they would also have to show that there is no performance interpretation of a transformational grammar possible other than the "conventional" one they describe. That, however, they have not shown. Like the AI researchers, Tyler and Marslen-Wilson fail to support the claim that a transformational grammar cannot serve as a model of knowledge or competence.

For the purposes of assessing the compentence-performance distinction, two facts from the AI and related Augmented Transition Network (ATN) literature are important. Syntactic, semantic, and "phonological" information must all be incorporated; also, the various kinds of linguistic information are kept distinct, even though they are interwoven in performance. Although the programs focus on procedures, and on interactions among the components, the components exist. The general distinction is thus honored, and the particular distinction has also largely been observed.

EXPERIMENTS AND INTUITIONS

The main conclusion about psycholinguistic theory and research that comes out of this historical review is that it is important to look at the widest range of different kinds of performance phenomena in trying to specify the role of competence in performance. Performance tasks vary in what kinds of linguistic knowledge they exploit. Therefore, concern with too narrow a range leads to mistakes about the nature of the competence underlying performance. No given performance task, with its special knowledge requirements, can fully reflect the entire range of underlying competence. It is

like judging the contents of a library by looking at how frequently borrowers take out best sellers.

In the data I have reviewed so far, the intuitional data have seemed to stand alone as evidence of the psychological existence of transformations. There are at least two other tasks, however, that have important properties in common with intuitions, and, as would be predicted, they too yield performance which can be explained by assuming that transformations are a feature of competence. Syntactic intuitions are usually the outcome of asking whether a particular structure is permissible given the rules of the language. Having the intuition is preliminary to determining what rule has been violated or obeyed. A series of structures is submitted to intuitions to determine the domain of the rule in question. Hence, the having of intuitions is directly concerned with underlying rules (Bever, 1974). Two other tasks that also involve access to or creation of systematized knowledge are (a) successful behavior in a What? situation and (b) language learning.

Valian and Wales (1976) and Valian and Caplan (1978) have investigated adults' and children's behavior in a laboratory analogue of a What? situation. A What? situation is a common occurrence in noisy environments: The speaker says something the listener has failed to hear and understand. The listener queries the speaker by asking "What?" The speaker must then decide how, if at all, to modify the original utterance. For talkers to succeed in a What? situation (i.e., for talkers to make their utterance easier to hear and understand), they must have access to a wide variety of types of knowledge. Although the original utterance was grammatical, they must still evaluate it and determine whether it can be improved in any way. Unlike intuitions, the task does not require determining whether a rule has been violated, but like intuitions, it does require access to potential rule outputs. Otherwise, the talker will be making blind guesses about how to improve the utterance.

Two different formalizations of a What? situation were invented. In the procedure used for adults, sentences exemplifying different linguistic constructions were created and typed one to a card. Within each construction all the sentences were fully grammatical, but half of them had slightly more clearly displayed sentential relations than the other half. For example, complement sentences either had the complementizer *that* present (clear version) or absent (distorted version). The adult read each card and was usually queried with a What? by an experimenter in an adjoining room. Subjects were told to respond as they naturally would had they uttered the original sentence; they could repeat it verbatim or change it in any way they wished.

In the procedure for children, two experimenters were used. The first read the child a sentence and asked the child to repeat it verbatim (to insure that the child could encode the sentence properly). The second, sitting at the other end of the room, queried the child with a What?

Although adults and children often decide to limit themselves to pronunciation changes, even 6 year olds showed that they can coordinate their knowledge of syntax, semantics, and listeners' processing systems to produce a rapid, often elegant, response to a What? The children and adults showed that they are sensitive to subtle syntactic properties of the sentences they utter. For example, missing complementizers are reinserted more often than existing ones are omitted; verb–NP–particle constructions are reconstituted as verb–particle–NP constructions more often than verb –particle–NP constructions are changed to verb–NP–particle constructions. In both cases, transformational grammar postulates a transformation: in the case of complementizers the transformation is deletion; in the case of particle, the transformation is permutation.

Speakers do not always, however, "undo" putative optional transformations in a What? situation. For example, speakers never change "it surprised them that she was late" to "that she was late surprised them." (Such a change may not be transformational; see Chomsky, 1973.) One reason they may not is that such center-embedded sentences are, as we know independently, hard to process. Speakers show their knowledge of how listeners' processing systems work by changing center-embedded constructions to right-branching ones or coordinate-structure ones, and by changing two-clause sentences to one-clause sentences. In some cases, then, "undoing" an optional transformation may result in a sentence that is hard to understand for extra-linguistic reasons, and, therefore, it will not be an exploited option. More work needs to be done on setting up such conflicts to determine when transformations will be undone and whether there are equally plausible alternative explanations for those cases where speakers do show transformational sensitivity.

Speakers also make semantic changes. For example, they show their knowledge of converse verbs (Katz, 1972): adults and children often change a sentence like "the salesman sold a football to Jerry" to "Jerry bought a football (from the salesman)."

Although there are clearly many differences between responding to a What? and having intuitions, there is the similarity that producing or understanding the sentence is not enough. In the case of intuitions, one reflects on the sentence for the purpose of understanding something about it. In the case of responses to a What?, one reflects on the sentence for the purpose of determining how to improve it. Both behaviors lead to a focus on rules. Delis and Slater (1977) showed that subjects who speak about cellular energy to an uninformed audience use fewer reduction transformations than subjects who write about it to an informed audience. Here, too, the task demands paying attention to the form of the communication.

Another behavioral area where rules should be important is in language learning, as Chomsky (1965), Katz (1966), and others have long proposed. Syntactic knowledge is acquired in the form of rules (I leave aside for the

moment the question of what the content of the rules is), and rule learning requires systematication. Children cannot just talk and listen; they must systematize what they hear and say to form rules. In a series of papers, my colleagues and I (Mayer, Erreich, & Valian, 1978; Erreich, Valian, & Mayer, in press; Valian, Erreich, & Mayer, 1978; Valian, Mayer, & Erreich, in press) have proposed that transformations are among the rules that children learn and that learning takes place by projecting hypotheses about what basic operations compose transformations and about what the structural description and structural change of a transformation are.

To take a simple example, among the incorrect hypotheses a child could have about a movement transformation is the hypothesis that the transformation consists only of copying or only of deletion, because those are the two basic operations that compose movement (Chomsky, 1965). In the case of particle movement, an incorrect hypothesis that analyzed movement as copying alone would produce a sentence such as "the barber cut off his hair off" instead of "the barber cut his hair off" (Menyuk, 1969). We predict that child speech will contain errors due to the projection of incorrect hypotheses about the form of the rules the child is in the process of learning. Many of the predicted errors have been observed; some have not.

Fay (Fay, 1974, 1977; Foss & Fay, 1975) has suggested a similar transformational analysis of speech errors made by children and adults, but attributes the children's errors to performance limitations rather than, as we have, to competence limitations. Work on speech errors cannot be assimilated to the cases suggested so far in which production or comprehension of an utterance is not the processor's main concern. Yet, to account for syntactic speech errors, it seems necessary to posit that transformations are, in Foss and Fay's terms, "mental operations."

Thus, although tasks that require reflection show the relevance of linguistic rules to performance, so do some aspects of ordinary talking. Speech errors in adults presumably reflect stress points within the production system where there is too much overload for completely accurate functioning. When the system breaks down, it does so along the divisions that mark how it is put together. Hence, errors are a rich source of information; errors vary in kind and substance, requiring analyses that must meet a number of constraints.

What about the second difficulty mentioned in the introduction, that the candidate grammars keep changing and multiplying? I suggest that this is not as much of a difficulty as it seems. To set the stage, I first review some of the differences between intuitions and formal experiments/observations. (I refer from now on only to formal experiments, but I intend that to refer to formal observations as well.)

There has been much criticism of the method of intuitions, particularly, that they are subject to performance variables and are unreliable. Yet, intuitions are the data that have led to a systematic linguistic theory. (Chomsky [1977a] makes a similar point.) In contrast, although there has

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been much criticism of individual experiments, no one has criticized the method of formal experimentation and observation. Yet experimental data have not led to a systematic psycholinguistic theory or a systematic linguistic theory, and experimental results are also unreliable. The person who is skeptical about intuitions should, by parity of argument, be just as skeptical, if not more so, about formal experimentation. I conclude that since we should obviously keep the formal experimental method, we should also obviously keep intuitions.

Both sources of data are valuable, and intuitions have had the edge as far as building linguistic theory is concerned. It is not difficult to see why. Even though performance factors enter into the having of intuitions, limited time is not one of them nor is limited materials. Linguists can spend weeks, months, thinking over a set of sentences, adding new sentences, altering lexical items within sentences, seeing if they all passivize the same way, pseudocleft the same way, and so on. A final feature is that the point of the whole procedure is to come up with a grammar.

The intuition situation contrasts dramatically with formal experiments. Decisions here are usually speeded: they typically take less than 1 sec, and almost never take more than 10 sec. Significant time differences between experimental conditions are often between 25 and 65 msec (Cairns & Kamerman, 1975; Foss & Jenkins, 1973; Holmes & Forster, 1970; Tyler & Marslen-Wilson, 1977).

The materials in experiments are limited, because they have to meet many constraints. The sentences of one set cannot be longer than the sentences of a matching set, because subjects might respond in terms of length; the words of one set cannot be more frequent than the words of the matching set, because subjects might respond in terms of frequency, and so on. Because one must control for many irrelevant properties that might covary with the property being tested, one cannot use a wide range of materials.[14]

The conclusion of this discussion about intuitions vs. formal experiments is that formal experiments, by their nature, are more involved with performance features than are intuitions. Psycholinguistic experiments primarily test aspects of the performance model, and only indirectly test the grammar, even if the experiment was explicitly designed to test the grammar. Our knowledge about speech performance is relatively meager, and our experimental methods are relatively crude. For any experimental outcome it will be as easy to construct an alternate processing model with the same grammar as it will be to construct an alternate grammar with the same processing model. If all the data (intuitional and formal experimental) superficially converged on the

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[14]The requirement of using items as well as subjects as the random effect in statistical tests of significance only guarantees generalizability across all items within the same population as the test items. When items have been chosen to meet various constraints, that population *may* have funny characteristics.

same linguistic theory and the same psycholinguistic theory, there would be no problem, but the data do not superficially converge.

The conclusion that experiments only indirectly test the grammar suggests that it would be a mistake to try to test subtle aspects of the formalism. To take an example, adults and children in a What? situation consistently change "Lonely?" to "Are you lonely?" and almost never change "Are you lonely?" to "Lonely?" Current transformational grammar derives the two questions from two different deep structure representations: "Lonely?" has more empty nodes in its deep structure than "Are you lonely?" does. In other words, the two are not transformationally related. A different grammar might co-derive the two questions, designating the lexical items *be* and *you* as deletable. The What? results cannot choose between the two linguistic formulations; the results are compatible with both. The results show only that talkers in a What? situation prefer the full form over the short form and know how to construct the full form given the short form. Both grammars mark a difference between the two forms, and in both grammars the full form provides more specific information, namely, what the subject is.

It is not just the methodological properties of formal experimentation that limit it to indirect tests of a grammar. Exactly because there is no automatic performance interpretation of a grammar, it is impossible to use an experiment to decide on the correct form of a particular rule of condition. Since knowledge may be used in many ways, or not used except under rare conditions, all conclusions about the form of knowledge based on results on use must be highly tentative.

The research strategy suggested by this discussion is one which does not try to choose between two hypotheses about how a rule or condition should be formulated, since the precise statements of rules or conditions are what is most unstable in the grammar and what an experiment is least likely to be able to test directly. Thus, one should concentrate either on more stable aspects of the grammar or on aspects that an experimenter will be likely to test directly. The latter half of the research strategy is the same as that already discussed in the introduction; the only change is that here you do not throw competence away just because the focus is on performance.

ACKNOWLEDGMENTS

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