Grammar as an Underground Process

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GRAMMAR AND PSYCHOLOGICAL REALITY

There has been a nagging problem of incompatibility in psycholinguistics between linguistic and psychological theories and results. Linguists should be bothered by this as much as psycholinguists. That they are not I take to be due to differences of development, scope and emphasis within the two disciplines. Linguists tend to concentrate on linguistic problems of description within a given framework (or "paradigm") and pay little need to outside evidence or other considerations. Psycholinguists, on the contrary, tend to take information about grammar as one of their starting points, and see how this fits in with what they can find out about cognitive functions.

Quite a few authors have noted the conflict between transformational linguists' dictations and the results of psychological research. I shall mention a few, but many more could also be cited. Levelt (1974, III, p. 141) concludes that "on the one hand the role of linguistic grammars in models of the language user is diminishing in a way, although it is decidedly not to disappear, and on the other hand, the theory of formal languages and grammars appears to be of increasing importance for the nonlinguistic aspects of such models." Clearly, there is a problem here as long as we adhere to a realistic conception of scientific theories and to the minimal requirement of consistency of science. At the official inauguration of the Max-Planck Projektgruppe für Psycholinguistik (May 4, 1977), Levelt remarked in his address that the capacity of human short term memory is far too limited to store the complex procedure involved in transformational processing from semantic input to phonetic output.

A similar point was made by Ingram (1971). She observes (p. 337) that the transformational machinery, if taken as a model of performance, would far exceed any reasonable time limit given that "no single cognitive process takes less than one tenth of a second." Her view is "that the phenomena to which linguistic theory is applicable are essentially different from the phenomena to which psychological theory is applicable, and that therefore linguistic models and psychological models relating to language must be different; and further that the differences can be found by considering the proper place of the rules of algorithms and of heuristic strategies in each model" (p. 335). Rejecting the principle of the consistency of science, only the internal consistency of each particular science remains. The place assigned to grammar is only specified negatively: algorithms (i.e., grammars) "cannot function as models for language behavior, because they cannot function in real time" (p. 337; cf. p. 345). No answer is provided, however, to the question of where and how grammar functions if it is banned from real time.

Fodor, Bever and Garrett (1974) (henceforth FBG) put forward the interesting view (p. 241) that "experiments which undertake to demonstrate the psychological reality of the structural descriptions [of sentences] characteristically have better luck than those which undertake to demonstrate the psychological reality of the operations involved in grammatical derivations." They go on to say: "Why this should be so is a question of considerable theoretical interest." Which, of course, must be true if one considers that, barring magic, the language user must somehow convert his psychologically real deep structures into psychologically real surface structures. If transformational grammar (TG) does not provide the correct description of that process, then there must be another description of it, requiring fewer operations and complexities. But then the latter would, by all standards, be empirically superior to TG. No such alternative has been developed, however. We are stuck with TG, even though it looks as if TG is psychologically indigestible.

Marslen-Wilson (1976) concludes (pp. 225-26) that "the mental representation of linguistic knowledge cannot be adequately characterized by a transformational linguistic theory. In other words, it seems appropriate for psycholinguists to look for ways of characterizing the structure of human linguistic knowledge that are more in tune with active perceptual processes, and less constrained by the requirements of descriptivist formal linguistics."

His main argument is the fact that in current transformational theory sentences are generated from deep to surface structure, whereas many experimental results suggest that, at least in the comprehension process vigorous forces are at work which operate on a left-to-right basis. A defender of TG could answer straightaway that the left-to-right axis is known to be essential for certain phenomena in TG (pronominal anaphora, quantifier scope) (see for example FBG, 1974, pp. 214-17, and Seuren, 1969, pp. 104-19) but however that this particular dimension has not been systematically explored in TG theory. In principle, no incompatibility between psychology and TG has been demonstrated, on this score at least. TG allows for extensions and developments (possibly in the area of surface structure constraints) that could well prove to resolve the left-to-right dilemma. But, needless to say, the linguistic world has not so far shown much interest in this particular objection, just as it hasn't to most objections of this nature. And no empirically justified solution to this, or similar problems, has so far been offered.

In some cases, such as Marslen-Wilson's position quoted above, it looks as though an expansion and elaboration of linguistic theory might provide a solution. But in other cases, such as those quoted earlier (Levelt, Ingram, FBG) the opposite strategy can be put to some advantage. There a solution might be found in doing something about psychological theory. This is what I shall attempt to do in this paper. In order to help solve the problem of the incompatibility between psychology and linguistics I shall try to draw a distinction between two kinds of psychological mechanisms. The view will be put forward that there are routine procedures which escape any form of introspection and one "central control," which is (largely) open to introspection and whose operations can be brought to awareness.

THE PROBLEM REPRESENTED IN LINGUISTICS

It should be noted that the problem at hand is as unresolved in linguistics as it is in psychology. In linguistics it is known as (or is very similar to) the distinction between competence and performance, a distinction which has proved to be both problematic and useful.

Since Katz's famous article or mentalism (1964), linguistics has been avowedly mentalistic (i.e., realistic), as opposed to instrumentalistic. The object of grammatical description, *competence*, is meant to be psychologically real. Competence must be acquired by the young child (or other language learner); a special psychological mechanism, LAD (the Language Acquisition Device) was postulated by Chomsky to account for the (still largely mysterious) processes of language acquisition. In a formal linguistic grammar items from the lexicon are selected and inserted into linguistic structures according to special rules. Linguists differ considerably as to the precise details. No one, however, in either psychology or linguistics, denies the psychological reality of the lexicon. The question of the psychological reality of the rules involving lexical items is therefore moot.

However, in linguistics no clear account has so far been given of the relation between competence and performance. Chomsky (1965, pp. 139-40) claims that it is absurd to regard "the system of generative rules as a point-by-point model for the actual construction of a sentence by a speaker." This would be "totally misconceiving its nature." No argument is given to support this view other than that "it seems absurd to suppose that the speaker first forms a generalized Phrase-marker by base rules and then tests it for well-formedness by applying transformational rules to see if it gives, finally, a well-formed sentence." *In Language and mind* (1972b), Chomsky again (pp. 116-17) asserts the absurdity of such a view, though a little more circumspectly. He proposes that the grammar should be seen as a part of the theory of performance, not as identical to it. No further specification as to how precisely a theory of competence is to be incorporated into a model of performance is given.[1] Statements concerning this question are usually negative ("we are not entitled to take this [i.e., the grammar] as a description of the successive acts of a performance model"), in spite of the explicit statement (p. 115) "that a person with command of a language has in some way internalized the system of rules that determine both the phonetic shape of the sentence and its intrinsic semantic content--that he has developed what we will refer to as a specific *linguistic awareness*."

One wonders why deep structures, surface structures, semantic representations, and the lexicon are awarded psychological reality and are declared open to psychological experiments, whereas the rules are refused the status of psychological operations. Trivializing the problem by speaking of "simple distinctions" which must not be overlooked if "great confusion" is to be avoided (Chomsky, 1972b, p. 117) is of no avail. Concealing the problem by adopting window-dressing terminology ("generative rules...may be interpreted as (purely static) conditions on the well-formedness of derivations"--Huddleston, 1977, p. 249) is no help either. The problem remains real and of considerable theoretical interest.

It is fair to say that the problem has been repressed in linguistics. This repression may well have serious consequences. Neglecting the problem is already leading to an increased isolation of linguistics as a discipline. Its closest neighbors, the psychologists, can dismiss linguistic theory as irrelevant with increasing ease. Linguistic theory, for many years a central discipline in the human sciences, may lose much of its interdisciplinary relevance.

Internally, the question is equally important for linguistic practice and theory. Although in many cases the linguist's daily work will not be affected by the problem of the psychological reality of transformational rules, the days are approaching when conflicting general theories of language or grammar will be subjected to comparative tests of psychological, or even neurological, plausibility. On the day of judgment, who will attend to the linguist's cry that his grammar is meant to be nothing more than "a system or rules that expresses the correspondence between sound and meaning" (Chomsky, 1972a, p. 62), or "a characterization of the intrinsic tacit knowledge or competence that underlies actual performance" (Chomsky, 1965, p. 140)? It simply is a fact of life that confirmation by neighboring disciplines strongly reinforces a theory, whereas incompatibility creates embarrassment.

Expressions such as those used by Chomsky to gloss over the incompatibility problem are fundamentally unclear within a mentalistic (or realistic) conception of science. The notion of *formal characterization* is, or can be made to be, perfectly clear as a description of certain properties of a hypostatized object in terms of an axiomatized system. It is then a mathematical notion. But transformational grammar is not a mathematical notion, though a formal characterization of it can be given in mathematical terms (see Peters & Ritchie, 1973). It is, on the contrary, an empirical notion. As such it is in search of an ontology.

The most natural ontology for a transformational grammar is provided by the processes of actual production and understanding of utterances. If a particular theory of grammar is psychologically absurd, or contains elements which are psychologically absurd (as the *Aspects*-model, with its base-generated phrase-markers and its awkward rules of lexical selection as presented on pp. 79-106), then this is a strong indication that that theory of grammar is seriously inadequate.

AVAILABLE EVIDENCE AND AWARENESS

In most recent psycholinguistic literature (cf. FBG pp. 241, 273-74, 368) it is stressed that there is no clear indication of a transformational procedure being used by the speaker-hearer. However, one interesting possibility has not generally been considered. In no way does the available evidence rule out the possibility that the language user's brain runs through a transformational procedure as an automatic algorithmic program which is completely screened from introspective access or control, i.e., from awareness.

It is decidedly odd that, in linguistics as well as in psychology, the concept *psychological reality* has almost invariably implied the criterion of accessibility to awareness, in spite of the declaration, often repeated, that competence consists of *tacit* knowledge. The term *knowledge*, albeit *tacit*, may have contributed to this. Intuitively, one would like to be able to say that the point of having knowledge is precisely that it (that is, its very propositional contents) can be put to use in the complex functions of thinking and acting, both of which are to some extent open to introspection. We tend to speak of *skill*, rather than *knowledge*, in the case of functions whose underlying principles are neither accessible nor useful to awareness. Skills in this sense are called upon in relation to some purpose to be achieved. We know of the purpose, and we know we have, or do not have, the skill to achieve the purpose. But only in rare cases, if ever, can we provide an exact analysis of the skill.

The use of a language is a case in point. We know whether or not we have command of or competence in a language. When we do, we can use it for certain purposes we have knowledge of: we know what we want to say, or what the utterance is we want to understand and interpret. But the principles, or rules, by which we guide the processes of production and comprehension escape our introspection. They can only be approximated by way of scientific hypotheses and theories. All this has been said so often that one hesitates to repeat it once more. Yet it looks as though there has been a certain reluctance to accept the consequences. In the psychological literature on the reality of transformational grammars this distinction is not made in any systematic way. Ingram (quoted above) discredits TG on account of the time factor involved: no cognitive event, it is claimed, takes less than a tenth of a second. But the examples given--recognizing speech sounds or printed characters, scanning words or sentences for meaning--involve operations which require full awareness, followed by a report. It is difficult to see how this could have a bearing on processes taking place "out of sight," with no possibility of reporting on them. Such processes require time, as well as the accessible operations, but the time intervals involved are of a completely different order. It is a priori to be expected that the cognitive events mentioned by Ingram should take a lot longer than mere routine processes.[2]

An interesting case is provided by the series of experiments carried out by Miller, McKean and Slobin (MMS), as reported and discussed by FBG (pp. 227-34). MMS measured the time intervals needed by subjects to either identify or produce a target sentence which differed from a stimulus sentence in that one of the two was negative (N) and the other was not, or one was passive (P) and the other not, or one was a *yes/no*-question (Q) but the other not, or any combination of these. Sentences without either N, P or Q were called "kernel" (K). They found:

(a) a linearly additive function: "for example, sentences which involve both the negative and the passive transformations appear to require a time approximately equal to the sum of the average time required for negative or passive applied separately" (p. 229);

(b) uniform time differences according to the parameter involved: e.g., negative takes less time than passive; pairs of sentences one of which is K take less time than corresponding pairs without a K;

(c) no time difference corresponding with the direction (from stimulus to target sentences or vice verse): e.g., from K to N takes as much time as from N to K.

FBG are impressed with these results. They feel that on the basis of these results many would be inclined to accept the psychological reality of transformational rules. Yet, they then proceed to deny the relevance of these results for this question, arguing as follows (p. 231):

> "We must bear in mind that transformations are ordered with respect to one another in grammatical derivations. Thus, if Miller and McKean's subjects were to apply the grammatical transformation *T* to a given stimulus sentence in the experimental task, they would first have to recover a representation of the sentence structure as it appears in the derivation *prior* to the application of any rule ordered later than *T*. When we take this requirement into account, however, we predict relations among the sentence types that are quite different from those obtained experimentally. The clear inference appears to be that Miller and McKean's subjects could not have been using the grammatical transformations to perform the experimental task."

FBG proceed to illustrate this point by calculating the predictions which follow from this procedure on the basis of certain grammatical assumptions about the relative ordering of transformational rules. These predictions then turn out to be widely at variance with MMS's results. Some linguists would take issue with FBG on the grammatical assumptions underlying their calculations (an all too familiar pattern when psycholinguists test linguistic theories), but that is of no relevance here. Presumably, on any ordering of the rules the results would conflict with (a-c) above. Much more relevant is FBG's tacit assumption that speakers should have control over decisions to go to one particular intermediate stage in the transformational derivation, stop there and carry on adding or subtracting one or more transformations. As FBG correctly observe, there is no evidence at all that they do so. Nor is there any reason to suppose that they would. If a TG forms an algorithmic automatism, one would expect that (for the speaker) input structures are under the speaker's control, and that the auditory output is or can be checked (monitored), but that the processing takes place beyond his control. There is thus no argument against the psychological reality of TG, only, it seems, the absence of experimental evidence for it.

In fact, this picture of controlled semantic input, monitored phonetic output (and, analogously, perceived phonetic input and recorded semantic output), the intervening processing being kept entirely "underground," fits MMS's results rather well, provided we assume the following procedure. Let us suppose their subjects analyzed the input sentence till the level of semantic representation (SR), then carried out changes in the semantic contents, and subsequently processed the new SR's into a new surface structure. The following diagrams illustrate this for K to N, and N to P, respectively.[3]

<u>Diagram 1</u>

$$\begin{tikzcd} SR \arrow[r] & N \arrow[r] & SR' \arrow[d] \\ K \arrow[u] & & N \end{tikzcd}$$

<u>Diagram 2</u>

$$\begin{tikzcd} SR \arrow[r] & \begin{Bmatrix} -N \\ +P \end{Bmatrix} \arrow[r] & SR' \arrow[d] \\ N \arrow[u] & & P \end{tikzcd}$$

In this interpretation the linearly additive function mentioned under (a) above is the result, not of any traffic up and down the vertical (transformational) arrows, but of modifications in SR's. This is compatible with (c), provided there is no time difference between either adding or subtracting a semantic element (N, P, or Q) in SR's.

Is there no evidence, then, confirming the status of transformational rules as "underground" processes? It would be disquieting if this were so. The data referred to under (b) above might well be seen as resulting from transformational processes. As long as the stimulus sentences are kept at a simple enough level, one would expect that both SR-modification and the grammatical processing are simpler for negation than for the passive. This is what the data show. It is equally predictable (though as far as I know untested) that considerably longer times will be needed for less straightforward negatives, as in the following pairs:

(1) a. Steve still lives in Paris.
    b. Steve no longer lives in Paris.

(2) a. I had already left.
    b. I had not yet left.

(3) a. Nigel is as big as Frank.
    b. Nigel is not so big as Frank.

(4) a. Every morning he reads two poems.
    b. He doesn't read two poems every morning.

(5) a. Most strikers have started work again.
    b. It isn't true that most strikers have started work again.

Since the SR-operation of adding a negative should not be significantly different from the "normal" cases,[4] the expected differences in processing time would be attributable to the grammatical processing.

We have, furthermore, the evidence obtained by Weigl and Bierwisch (1970). They found a particular patient suffering from aphasia, alexia and agraphia who could not write down sentences, either spontaneously or on dictation. But she could copy written sentences. She could also, having copied a sentence, then write it down spontaneously or on dictation: the sentence was then "deblocked." Weigl and Bierwisch found that, having thus deblocked a given sentence S, the patient could, in addition, write down dictated sentences which were considered transformational variations of S, i.e., S with a preposed object, the passive of S, WH-questions where the WH-element replaced the subject or the object of S, etc. In these cases, the patient showed some hesitation. They write (p. 15): "Even these hesitations and trials, however, show in a very impressive way how the process is governed by the transformational network relating these sentences to the common underlying structure." Although it is not immediately clear how the details of these findings should be interpreted, it does look as though they are in good agreement with the notion of grammar as an underground process.

But apart from all clinical or experimental data, there is the whole of linguistic theory, insofar as it has been argued for on "linguistic" grounds, providing evidence for the reality of transformational processes. The chief difference between a linguistic and a psychological argument consists in the fact that, in linguistics, data collecting is less systematic and notionally less clear, whereas theory seems more developed than in psychology. But if linguists were more conscientious in registering native speakers' linguistic attitudes (following Labov's, 1975, admonitions), and if the categories employed (*grammatically well-formed, acceptable, semantically well-formed, substandard, dialectal,* etc.) were given a more solid basis, few psychologists would maintain that "experimental investigations of the psychological reality of grammatical rules, derivations, and operations...have generally proved equivocal " (FBG, p. 368).

Given that transformational processes are screened from introspective access, there is little else one can do but approximate them by hypothesis, i.e., theory. Suppose, for example, that it has been established experimentally, under controlled conditions, that speakers of English (of either all or only some social classes or geographic areas) accept and use sentences such as (6) and (7)

(6) a. Anxious as Edith always was to please her son, he grew up a spoiled child.

b. Reluctant as I was to leave, I kept holding up my glass.

(7) a. Difficult to erase as it was, the slogan remained visible for a long time.

b. Easy to fool as Henry was, he soon lost all his money.

c. Hard to persuade as Jimmy is, he'll have to suffer.

much more naturally than sentences with the systematic difference between (6) and (7) inverted ("Anxious to please her son as Edith always was,....; difficult as it was to erase, the slogan...."). Then it is difficult to imagine anyone objecting on scientific grounds to the inference that there is, in some way, a stronger cohesion in constructions of the type "easy to fool" than of the type "anxious to leave." Given a sufficient amount of specialized theory, a linguist might then propose different constituent structures for both classes.

Suppose further that it has been established experimentally that subjects provide close-enough synonyms for particular combinations involving adjectives of the (7)-group[5] with much greater ease and frequency than for those of the (6)-group. In the light of bits of existing theory of lexical formation, one might then feel justified in postulating that (7)-type collocations are dominated by a single categorial node (in this case "Adjective"), as in Diagram 3, whereas (6)-type constructions have a structure as in Diagram 4.

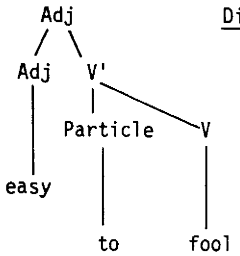

Diagram 3

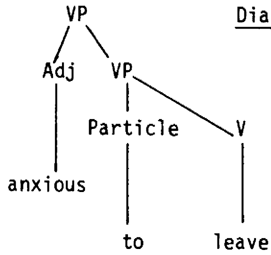

Diagram 4

The theory might involve the assignment of special, definable, properties to subtrees dominated by a categorial node (as in Diagram 3), such as the immediate substitutability by a single lexical item, or the relatively strong resistance to interruption (=cohesion).

Whether theoretical statements such as these are, or are not, correct, useful or fruitful, is a question to be answered primarily by the specialist in linguistic structures, namely the linguist or grammarian. He, however, has no reason to be distrustful, or even wary, of experimentally obtained data (as has become the prevalent attitude in linguistics[6]). Likewise, the psychologist has no reason, as far as I can see, not to accept the theory of grammar as an integral part of psychological theory. This applies to the linguistic structures as much as to the rules. One cannot accept evidence about the reality of deep and surface structures without postulating transformational processes occurring between them. If they are beyond introspective reach, as we have to admit they are, the simplest theory, i.e., the one capturing the widest generalizations with the least apparatus, will count as the strongest.

TWO MECHANISMS

The claim that grammar is an underground process makes little sense if one is not prepared to postulate two kinds of cognitive machinery, one being of a more routine-like nature, the other having an integrated central control function. For lack of better terms I shall speak of *routine procedure* (RP) and *central control* (CC), respectively. Since the distinction is not intuitively repellent and also seems to have some explanatory potential, I shall try to formulate what seem to me to be necessary minimal conditions for the two kinds of mechanism.

Let us suppose that RP's are characterized by the fact that they consist of self-contained algorithmic programs of a non-recursive nature, not sensitive to independent variables other than the inputs received. The input is processed into an output without any outside interference. Once the program has grown or has been acquired, and given the right physical condition of the organism, the only condition for a proper functioning is the feeding in of the right kind of input.

The other kind of machinery, CC, receives inputs from various RP's and provides outputs for and instructions to other RP's. It operates throughout in terms of stylized representations. These cannot be a "language" in the linguistic sense of the word, i.e., tree structures subject to certain strictly defined grammatical constraints. Fodor (1975, pp. 55 ff.) is very clear on this (although I regret his use of the term *language* for thought-structures: in this context the term is an unhappy metaphor). They will no doubt be as severely constrained as linguistic structures, but in entirely different ways. The important thing here is that they are non-linguistic. We shall speak of them as *cognitive representations* (CR), as opposed to semantic representations (SR) which are linguistic structures. No attempt will be made here to specify more precisely the structural or functional principles of CR's. It must be mentioned, however, that the notion is as old as, and largely identical with, that of proposition (see Nuchelmans, 1973). Inasmuch as the traditional notion of proposition is meant to refer to non-linguistic cognitive representations (thought-structures) underlying sentences, it coincides with what we call CR here.

Routine procedures have been described as having an input and an output. Since their main function and *raison d'être* is to do the groundwork for central control, it is reasonable to suppose that CR's, which are the typical representations of CC, are always involved one way or another. So we stipulate (provisionally) that RP's operate from given inputs (possibly sensory data) to cognitive representations, or from these to outputs of a different nature (such as linguistic outputs), or from CR's to CR's.

We want, of course, central control to be the part of our mental machinery that we "have access to." We are, or can make ourselves, aware of what goes on there, and we can give verbal reports (*protocols*) of that. In fact, that is what is meant by *having access*. However, this impressionistic parlance sounds as though somebody inside us, a "homunculus," is keeping an eye on what happens in CC. This, of course, cannot be literally so, since that little man would again have his mind, with a CC and an even smaller homunculus in it, and we would be led into an infinite regress. On the other hand, there does seem to be some recursion. I can make myself aware of the fact that I am aware that I am slowly getting drunk. Ideally, though not in practice, this could be carried out *ad infinitum*. Only limitations of storage and processing capacity seem to keep us from going up too high in the awareness spiral. In other words, there must be some recursive mechanism in CC.

An obvious way to account for this form of recursion is to remember that we have a memory. Our memory, needless to say, is very unlike the kind that computers are equipped with. What exactly it is like, is still very much an unresolved question, but enough seems to be known to venture the inference that it must be structured according to categories, propositions, theory-like sets of propositions, unifying generalizations. The basic element of human memory seems to be the proposition, or as we have dubbed it, CR.[7] There also seems to be a gradual downward movement in our memory system, from isolated *ad hoc* occurrences which are kept for very short periods only, to integrated propositional complexes which have the structure of deductive theories and are kept for much longer stretches of time (according to some, forever).[8] Only some data seem to pass into longer term strata of the memory. So one way or another, there must be a filtering procedure. As the memory gets deeper, less is known about its organization. In particular, we must confess to almost total ignorance of the ways and means by which items sink deeper and deeper into memory while they undergo gradual transformation so that they are, in the end, encapsulated into integrated cognitive complexes. The deeper strata are usually called *knowledge*.[9]

But apart from the other mysteries of memory, there is one assumption that has to be made a little more explicit here. Let all events occurring in CC be recorded in memory, and only these. Let CC have the possibility of putting a special kind of RP into action, which will search memory for certain "information" and bring it back to CC. Then, the instruction given to the search procedure is itself kept in the memory, and CC can therefore switch on the search procedure again to get the previous instruction, and so on *ad infinitum*.

If the information to be retrieved from memory is at its freshest, i.e., if a CC-event is being, or has just been, recorded without interruption, the search procedure will stay at its minimum: it will immediately "report" (and the report can be verbalized) that recording is going on. But this report is again recorded, and that process can be reported on again, and so on. I believe that this immediate reporting of the recording of current CC-events is a fairly close approximation of what we call *bringing to awareness*.

If it is true that all CC-events, and only these, are recorded in memory, they must be extremely costly in terms of brain capacity (and probably also in terms of time). It certainly makes a great deal of sense to postulate routine mechanisms that are less costly and do the jobs which have to be done but whose step-by-step procedure is irrelevant for the decisions to be taken by the organism as a whole. There is no point in storing these procedures in memory. Quite clearly, it is a matter of good functional economy that RP's, which are nothing but ancillary routines, are screened from access so that they cannot be interfered with by some CC-command, retrieved or reported upon.

It is quite thinkable (and in the case of grammar probably true) that a given complex RP has the following property. For each input there are a number of successive stages, s_0, $s_1, \ldots,$ s_n, it has to go through, where s_0 is the input, and s_n the output of RP. However, there are s_i's ($0 < i < n$) such that the transition (by rule) to s_{i+1} is contingent not only, or not at all, upon the structural properties of a s_i, but (also) on one or more previous stages. RP will then have some means for keeping "tabs" for the duration of the processing of s_0 into s_n. A psychologist who likes computers would now say that this RP is a system with a memory attached to it, since it has this tab-keeping device. Yet it should be clear that the term *memory* is not applicable here. CC has no access to these tabs. No reporting is possible on them. And there is no reason at all to suppose that they have a propositional structure. When I speak of memory, I wish to refer specifically to that faculty in our minds which enables us to remember things, and not to just any cognitive functions causing storage of any kind to take place.

It often occurs that a particular activity, which was learned explicitly and in full consciousness (the learning taking place, so to speak, under direct supervision of CC), becomes an automatism after a while and with practice. This phenomenon, which is extremely well-known both in the literature and in each individual's personal experience, implies that it must be possible for particular classes of occurrences in CC (most probably not for all kinds of CC-events) to be degraded to the status of RP. Something of this nature seems to happen when adults learn a foreign language by an explicit method. If, and to what extent, some such process also occurs during first language learning in early childhood, is an interesting question.[10] Other examples are car-driving, skating, bicycle-riding, but not, apparently, the making of sums.

Unlike RP's, we must assume that CC is under heavy pressure from all kinds of independent variables. This is more or less implied by our saying that CC receives inputs from various RP's feeding into CC, and that some of these RP's receive their inputs from outside (sensory data, sensations, emotions). To the extent that CC is influenced by the inputs it receives, it is subject to independent variables. I think that it is the interference, occurring with greater or lesser intensity, brought about by various RP's feeding into CC, which determines whether or not CC will switch from one chain of operations to another (shift attention, that is). This, of course, takes us right in the middle of a very old and extremely complex problem: what, if anything, is *free will*? I do not claim to know the answer. But I do claim that it is possible to construct a model where the decision regarding the kind of activity that CC will be occupied with next is a function of a complex interference pattern of RP's feeding into CC. What we are going to think about next, in other words, and also, therefore, what we are going to do next, is determined by the intensity with which certain RP's deliver their product to CC, in relation with the strength of other programs being planned or carried out.

Constructing an adequate model of CC will require a great deal of theory, so far non-existent. Among other things, such a model will have to specify degrees of intensity and strength, as indicated above. It will have to specify the form of CR's, the kind of operation that can be performed on CR's, the way or ways in which a definable set of operations can become an automatism, and many other things. But in a way, and from a philosophically safe distance, these are technical problems. There still remain the philosophical problems raised in certain quarters, especially the philosophy of personal identity, that of mind, and the philosophy of action. Does our distinction between CC and RP's provide the possibility in principle to capture the elusive *I* so much discussed in the philosophical literature?[11] For example, do *I* have control over the relative intensity with which *my* RP's deliver their output to *my* CC? If so, what does this mean for the mechanism that I am, or have?

One question which has bothered philosophers engaged in the body-and-mind problem (or the dualist-materialist issue) is presented by the fact that the word *I* can be replaced by *my body* in some cases but not in others (and, analogously, *you* can sometimes be replaced by *your body*, *he* by *his body*, etc.). Thus, in expressions such as *I am heavy*, *I am still alive*, *I was hit by a bullet*, *I am turning grey*, the word *I* can be replaced by *my body* (or, as the case may be, by an expression denoting a part of the body, such as *my hair*). But when I say *I am sad*, *I am reading*, *I like ships*, the same substitution is out of place.[12] Does one not detect some underlying primordial dualism here? How is this to be reconciled with the monist-materialist view of man which has been current in psychology at least since the advent of behaviorism? There is, it seems to me, some perhaps considerable mileage in the position that if we oppose body and mind, taking *body* in a narrow sense, then the term *body* stands for all RP's plus all the other non-cognitive routine processes that keep us going. *Mind* then stands for what we have called CC here. *Body* in the wider sense embraces both *body* in the narrow sense and *mind*. When substitution of *my body* for *I* is inappropriate, this is no more than one of those cases where an expression denoting a part of the body must be used, in this case *mind*. The conclusion will then be that there was, after all, just a confusion between the two senses of *body*, and the dispute was, after all, based on a terminological equivocation (see footnote 12). The real answer, in the end, is monist-materialism. I do not know to what extent this line of reasoning will be tenable eventually. But the answer will depend largely on what CC can do to help us understand the ways in which we talk about ourselves.

Questions such as these may seem remote to a psychologist. Yet they are real and will have to be answered. The distinction between CC and RP's implies an acceptance of questions such as "Can a machine think?",[13] and it implies an attempt at forging an answer. But it is not primarily such philosophically tainted questions that motivate the distinction. There are much more direct, empirical reasons for it. One reason we have given: it helps to solve the incompatibility problem of transformational grammar and psychological data. But, of course, such heavy machinery needs stronger, and more general, support. And this it finds in the following general consideration.

Behaviorism has had the great merit of setting the stage for an exact formulation of the problem of the causation of human behavior. It also claimed to provide an adequate answer which involved absolutely minimal assumptions about the organism. The behaviorist effort consisted mainly in correlating stimuli and responses (behavior). The variables determining behavior were placed in the outside world, not, or as little as possible, in the organism. This position has, by and large, now been abandoned. Occam's razor was too sharp; we have had to enrich our assumptions about the organism. Chomsky (1959) neatly demonstrated the weakness of behaviorism with respect to linguistic behavior, but his arguments can be generalized to most or all forms of behavior. Only some stimuli correlate with a well-defined class of responses. But in many, if not most, cases we have an inescapable intuition that there is a correlation between this stimulus-token and that response-event even though we can give no clear behavioral definition of the class of responses associated with that stimulus.

To give an example, let us imagine a town where open civil war is raging. After a period of relative calm there is a rapid succession of very loud and very sudden diffuse noises originating from the Northwest area of the town and heard all over. There is a variety of responses. Some taxi-drivers suddenly turn left, others turn right, others stop. Some people, in houses relatively remote from the source of the noises, suddenly open their windows. But others, closer to the bangs, run out of their houses into makeshift bunkers. Armed men on rooftops start aiming and preparing their mortars, and so forth and so on. Clearly, there is no behaviorally definable class of responses to what we interpret as the series of explosions. Whatever predictions we can make about people's responses will depend on what theory we hold of the situation generally, of each person's position in the general situation, and of each person's ideas of life and death, safety and danger. Our predictions may well be rational, but any attempt at giving an explicit account of their rationality will necessarily have to make strong claims about the various responding organisms. The argument extends to long-range effects of a given stimulus. It is normal for us to detect the effects of a stimulus on a person's behavior over a period of time. Apparently, we are able to select those elements of behavior which correlate with the stimulus, even though there is not a shadow of a behavioral criterion. We do so on the basis of certain implicit assumptions about the other person's "organism." Any attempt at predicting and selecting such effects scientifically will have to make those assumptions explicit.

The absence of behaviorally definable classes of response-events (provided we accept intuitions about relatedness of stimuli and responses), implies the existence of some central clearinghouse where inputs are brought together, processed and integrated into structures that determine elements of behavior spread over large numbers of behavioral categories and often over extended periods of time. In other words, a preliminary problem analysis regarding the causation of human behavior quickly shows that there must be a powerful integrating mechanism relating stimuli with vast varieties of responses. An explicit account of this mechanism will be an account of what we intuitively call "rationality." A rejection of this argument will lead, and has led, to an intolerable impoverishment of psychological theory.

The introduction of a CC-mechanism is precisely a recognition of the reality of certain stimulus-response correlations which can only be understood on the assumption of some rationality in the organism involved. Since behavior caused by CC-intervention is not limited to any precisely definable behavioral category or class of categories, the laboratory is not the ideal place to test organisms on this score. Under laboratory conditions, behavior has to stay within clearly defined boundaries. Yet no one could seriously impose the condition on scientific work that it should *per se* be restricted to the intrinsic conditions and the material possibilities of a laboratory. A great deal of scientific work consists in constructing empirical theories (i.e., theories which can be evaluated on account of their explanatory power) on the basis of data that cannot be gathered in a laboratory.

A distinction between processes that are and that are not accessible to introspection has been proposed here as a way of resolving the incompatibility problem in psycholinguistics. Innocent as it may have looked at the outset, in fact it turns out to involve bold theoretical claims about the organization of the human mind. It has been the purpose of this paper to show (a) that under the CC/RP-distinction there is, so far, no risk of incompatibility between the theories and findings of linguistics and psychology, and (b) that the distinction is needed anyway on general and independent grounds.

Some qualification is necessary, however, with regard to the statement that under the CC/RP-distinction linguistic and psychological theory are not incompatible. The statement is much more obviously true for that development in linguistics known as generative semantics or semantic syntax, and much less so for Chomskyan autonomous syntax. In semantic syntax, the deep structure is identical with the SR, which can be considered the result of a mapping from a given CR (where the CR is a non-linguistic and the SR a linguistic structure). The transformational rules turn SR into a surface structure. This process is quick, uncontrollable by any non-grammatical factors, and is not recorded in memory (although some auxiliary tab-keeping may be involved). The grammar is merely a processing mechanism operating on an unbounded set of SR's, whose origin is postulated in CC. This model of grammatical description is not generative, but only transformational.[14]

In autonomous syntax, on the contrary, SR's result from special semantic interpretation rules operating on surface structures. The deep structures of the syntax are "generated" by the rules of the base component. As we have seen, Chomsky maintains that it is absurd to suppose that a speaker first builds up a deep structure through base rules, and then runs the structure through the T-rules to see if a well-formed surface structure is yielded (1965, pp. 139-40). Chomsky is probably right here: such a procedure sounds highly implausible. His base rules are not "generative" in this sense, but are mathematical specifications of the well-formedness conditions of his deep structures. Similar conditions can be formulated for SR's, as for any class of tree-structures. But if deep structures are psychologically real, they must have a psychological origin. Thus the question of the psychologically real generation of the Chomskyan deep structures is still wide open. And it is hard to see how this question could be answered without the deep structures being mappings of thought structures, i.e., SR's. If, on the other hand, deep structures are not psychologically real, one wonders what sense it makes to say that a child must acquire the grammar of his language, or, more generally, that a grammar describes "a mental reality underlying actual behavior" (Chomsky, 1965, p. 4). In short, the whole fabric of Chomskyan autonomous syntax suffers from psychological implausibility. And, not surprisingly, this particular branch of grammatical studies also suffers from fundamental unclarities regarding such basic methodological issues as the correspondence between the postulated theoretical entities and the object of investigation. When I say, therefore, that under CC/RP-distinction there is, for the moment, no fear of incompatibility between psychology and linguistics, this applies to semantic syntax, rather than to autonomous syntax.

FOOTNOTES

1. FBG, pp. 370-71, distinguish five ways in which grammar can be thought to interact with a model of performance, and there is no indication that their list is exhaustive. These five ways, moreover, are so general in their own ways that many further subdivisions could be made.

2. See Clark and Clark (1977, pp. 56 - 81) for some experiments that are relevant to processing time of grammatical automatisms.

3. In accordance with dominant opinion in TG I assume that there are semantic differences between corresponding active and passive sentences, even though current semantic theory does not provide us yet with the apparatus to describe these differences systematically.

4. In the pairs (1) and (2) the SR-operation of negativizing might be argued to be more complex than in the "normal" cases, since not only does a negation have to added, but a presupposition has to be taken into account, as in (1), or modified, as in (2).

5. In fact, there are numerous examples of this type of correspondence in English, as in many other languages: *amiable* (easy to like), *handsome* (good to look at), *touchy* (easy to offend), *fragile* (easy to break), *readable* (nice to read), *gullible/credulous* (easy to fool), *light* (easy to digest), *stubborn/obstinate* (hard to persuade), etc., etc.

6. McCawley is a clear exception to this rule. In a recent interview (Aarts, 1977) he declares his interest in experiments conducted by Fodor at MIT, of which he says (p. 244) that they "may very well be very productive with regard to justifying particular analyses or to showing that certain supposed similarities aren't quite as much of similarities as they have been taken to be." McCawley also holds the opinion, still rare among linguists, that no strict separation can be made between competence and performance: "that to make sense out of what a person's knowledge of language consists in, you will have to talk about language processing: what goes on in the production and comprehension of language" (p. 240).

7. See Anderson and Bower (1974, pp. 86-87), Clark and Clark (1977, p. 136) on the propositional nature of (long-term) memory.

8. The distinction usually made between short-term memory and long-term memory (e.g., Clark & Clark 1977, pp. 135-41) does not seem to capture the whole truth. Instead of this dichotomy, it looks much more as if there is a structure causing either gradient or step-by-step transitions from short-

term to long-term memory representations.

9. In fact, memory is some kind of induction mechanism. It certainly shares many of its mysteries with induction. If the conditions for induction (i.e., for theory formation) are not fulfilled, the integration process does not work and the items disappear from memory. In such a case, the items are unrelated and fail to fall into a meaningful pattern. In order to remember such items we have to resort to memorization, and memory does not seem to like that very much. See Clark and Clark, 1977, pp. 134-35, 137, 143.

10. See, e.g., Kaper (1959), Read (1973), Slobin (this volume).

11. Cf. Wittgenstein, *Tractatus*, 5.631 - 5.641; Strawson (1959, pp. 87-116).

12. According to some philosophers (Carnap, Quine), such a question is uninteresting, since it is language dependent. Others (Ryle, Cohen) reject the language dependency of philosophical questions. See Gyekye (1977) for an interesting comparison with the Akan language, where a dualistic philosophy seems to manifest itself in expressions of the type discussed here.

13. It is remarkable that in the branch of psychology where model construction is taken most seriously, in artificial intelligence, this kind of problem is avoided or rejected. E.g., Hunt (1975, p. 444) does not accept questions of the type "Can a machine think?", and falls back on a theoretically minimal position which involves an agnosticism with respect to the materialist-dualist issue, and a rejection of the criterion of AI-programs that they simulate psychological performance. The cost of this stance is a considerable reduction of the scientific relevance of AI work.

14. For a systematic exposê of the difference between autonomous and semantic syntax, see the introduction edited by Seuren (1974).

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