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FORMAL ANALYSIS AND FUNCTIONAL ANALYSIS
OF VERBAL BEHAVIOR: NOTES ON THE
DEBATE BETWEEN CHOMSKY AND SKINNER

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“In fact, the decline of behaviorism appears to be linked to the birth of modern psycholinguistics.” — J. Mehler

Mehler's affirmation (Mehler, 1969, p. 3) is doubtless based essentially on the famous critique made ten years earlier by N. Chomsky (Chomsky, 1959) of B.F. Skinner's work, *Verbal Behavior* (Skinner, 1957). It also appears in a collection of articles (Mehler, 1969) among which is found the French translation of this manifesto-like text by the master of transformational linguistics. If one considers the actual vitality of the behaviorist movement, especially that of Skinnerian inspiration — a vitality marked by the fecundity of its methods, the extent of the behavioral domains submitted to its variety of experimental analysis, the multitude of its applications, not to mention the repercussions of its philosophical extensions (see especially Skinner, 1971), one can only be astonished at such a declaration, which we have elsewhere attributed to “wishful thinking” (Richelle, 1971, p. 48), but which its author unhesitatingly presents, not as an opinion, but as a statement of fact. This gives an indication of the influence of Chomsky's review on certain trends in modern psychology and psycholinguistics. While giving his critique an unaccustomed breadth and intensity, Chomsky undertook to explicate the various concepts of the analysis of behavior adopted by Skinner. If not informed firsthand, the reader might believe himself duly edified after reading Chomsky's presentation, and would rally without hesitation to the critiques made therein. Consequently, he

*Translated from Richelle, M. Analyse formelle et analyse fonctionnelle du comportement verbal: Notes sur le débat entre Chomsky et Skinner. *Bulletin de Psychologies*, 1973, 26, 252-259. The French translation of Chomsky's review of *Verbal Behavior* appeared in Mehler (1969). Professor Richelle's article was prompted by that publication, and is here translated to provide English readers with a skeptical Continental reaction to Chomsky's review. All quotations and page citations from Skinner (1957) and Chomsky (1959) refer to the original English texts and not to translations thereof. We are especially grateful to Kenneth MacCorquodale for his careful reading and highly useful suggestions on an earlier version of this translation.

would probably dispense with reading *Verbal Behavior*, convinced that it concerns a totally anachronistic scientific enterprise, and that Chomsky has provided him with an accurate representation of the theses of modern behaviorism in general, and of Skinner's ideas on language in particular. This opinion, reflected in Mehler's citation, is quite widespread among specialists in contemporary psycholinguistics, and not exclusively among orthodox Chomskians (see, for example, H. Sinclair-De Zwart, 1967, p. 150: "As for Skinner's (1957) interpretations, based on such notions as *response strength* and *schedule of reinforcement*, Chomsky (1959) has definitively demonstrated the meaninglessness of these notions when applied to verbal behavior.").

Since the translation of Chomsky's critique is quite recent,¹ it could be useful to furnish to French readers several elements of a counter-critique.** In this article, we can undertake neither a novel account which will illuminate Skinner's work nor a point-by-point discussion of Chomsky's arguments. We will be limited to offering several points in reply to the following two questions: First, does Chomsky's text furnish an accurate representation of Skinner's work which the unforewarned reader can trust? Second, does the linguist's refutation have the substance that its incisive style and confident tone make it appear to have?

In response to this critique, Skinner has remarked, "it missed the point" (Skinner, 1972, p. 345). This expression summarizes well what an unprejudiced reader would conclude were he to attempt to answer the first of the above two questions while examining *Verbal Behavior* for himself. Chomsky's critique testifies, on the one hand, to a misunderstanding of Skinner's project (unless this is a case of a deliberate attempt to remain ignorant of it) and, on the other hand, to a misunderstanding of the fundamental conceptual tools of a functional analysis (unless this is a case of a deliberate attempt to caricature them in order to justify the "attack" leveled against them).

Approaching *Verbal Behavior* as a linguist inclined to formal analysis, Chomsky fails to clearly present Skinner's intention and the manner in which Skinner relates his thesis to the traditional study of language and linguistics and to his own contribution to the experimental study of behavior.

In defining his project as a functional analysis, Skinner notes those characteristics which differentiate it from formal analyses of language.*** He in no way denies the value or legitimacy of these formal analyses. His analysis of verbal behavior in no way excludes the linguistic approach; even less does his approach pretend to replace it. This appears clearly in the choice of the title *Verbal Behavior*, abundantly justified in the

¹For the convenience of the [French] reader, it is generally to this translation [Mehler, 1969] that we will refer, although it is not above reproach; we will take the liberty of retaining the English terms coined by Skinner, *mand* and *tact*, which were quite improperly rendered in French as *requête* and *dit*.

**Translators' note: Wiest (1967), MacCorquodale (1970), Salzinger (1970), Catania (1972), Verhave (1972), Catania (1973), Salzinger (1973), Bricker and Bricker (1974), and Segal (1975) have also made highly pertinent contributions to an emerging behaviorist counter-critique.

***Translators' note: Skinner (1972, p. 346) and Catania (1972, 1973) have commented further on the distinction between structural and functional analyses (i.e., analysis of the *form* of a behavior pattern versus analysis of the *circumstances* under which a behavior pattern occurs) and on the fact that much of the controversy between Skinnerians and Chomskians appears to be due to their mutual failure to recognize this distinction.

introductory chapter: the aim of the work is to study the *individual speaker*, to see by what mechanisms his verbal behaviors are shaped and maintained. The aim of linguistics is principally to study the system of language, and it is clear that numerous properties of the system can be isolated by an analysis of linguistic facts completely independent of psychological facts for which the speaker is the locus. (The question of knowing whether this independence is absolute, or if, on the contrary, it has limits, and if, as a consequence, a linguistic theory must not ultimately become a psycholinguistic theory, would be beyond our purpose. Further on, we will touch upon one aspect of this problem.) If Skinner gives his preference to *verbal behavior* rather than to *language* or to *linguistic behavior*, it is because these latter terms refer to “practices of a linguistic community rather than the behavior of any one member” (Skinner, 1957, p. 2). On several occasions, he sets forth in his analysis certain categories of facts that are relevant to the study of “characteristic practices of a given verbal community, and hence to the commoner preoccupations of linguistics” (Skinner, 1957, p. 28). But, as we have already stressed in another context (Richelle, 1971, pp. 29-35), a formal analysis cannot in itself authorize any inference as to the functional mechanisms at work to develop and maintain the facts on which it rests.²

The rules isolated by linguists describe the linguistic code, not the functioning of the organism that uses them. They cannot, of course, be ignored in a functional analysis: they define, in fact, an important part of these “contingencies” — or, the set of conditions, or, independent variables — which regulate verbal behavior. One can easily provide multiple examples which show that Skinner does not neglect them, although he has taken the tack of not dwelling upon them.

It is not the linguists that Skinner reproaches for being limited to a formal analysis (since, from their point of view, it is perfectly justified), but rather the psychologists who, in this domain as in others, have assembled and sometimes ordered facts, but have not succeeded in demonstrating the significant functional relationships central to any scientific analysis. This failure is the result of their attachment to explanatory systems which do not lend themselves to fruitful investigation because they appeal to fictional internal entities to which a causal status is attributed. Curiously enough, Chomsky remains completely insensitive, as we will see later on, to this preoccupation of Skinner's, this central theme of behaviorist methodology, and he ostentatiously employs such mentalistic entities as *attention*, *set*, *caprice*, etc. Instead of refuting Skinner on the basis of sound linguistic arguments that would possibly convince his reader of the fecundity of the formal approach in any tentative functional analysis, Chomsky thus presents himself as a poorly-informed defender of a psychology drawn largely from common sense. We will return later to this point.

Throughout his critique, Chomsky more or less explicitly reproaches Skinner for abusing the halo of scientific prestige of experimentation on very elementary aspects (according to Chomsky) of animal behavior in order to “palm off” his analysis of verbal behavior: “He uses the experimental results as evidence for the scientific character of his

²Skinner himself evokes the analogy with the problems posed by the analysis of intellectual behavior which logic approaches only in a formal manner.

system of behavior, and analogic guesses . . . as evidence for its scope" (Chomsky, 1959, p. 30). For Chomsky, the extrapolation offered by Skinner is deceptive in several respects: it introduces an illustory appearance of rigor, but is baseless (being purely analogical); it reduces the most complex human behavior to simplistic schemas valid solely (and with numerous restrictions) for animals; and especially, it unduly assimilates the behaviors of "real life" under summary laws derived from the study of behaviors in the laboratory.

These arguments are banal and traditional: they have been brandished throughout the history of scientific psychology to attack the efforts of experimental analyses of human behavior and to avert the threat that these efforts pose to the self-conception of Occidental man. It is to these arguments that Chomsky's critique probably owes most of its success, in that they unite deeply rooted resistances to the scientific approach to behavior.

It must first be stated that Skinner never promised to furnish in *Verbal Behavior* a rigorous experimental analysis: he presents his essay as "an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results" (Skinner, 1957, p. 11). It is clear that such an enterprise would necessarily fail to illuminate numerous points not accessible to an experimental analysis, in the strict sense. But why are they inaccessible? For Skinner, it is because of their complexity and the inadequacy of our methods, and there is no reason to think that we cannot one day attain them. For Chomsky, they are inaccessible by their nature: they belong to a category of phenomena to which do not apply the methods and the concepts which were effective in the study of more simple phenomena.

But to accuse Skinner of reducing the complex to the simple, even to the simplistic, is once again to betray his intention. Does he not state, in the very first pages of his book, that verbal behavior "has so many distinguishing and topographic [one could say *structural*] properties that a special treatment is justified and, indeed, demanded" (Skinner, 1957, p. 2). The misunderstanding is based, in fact, on an essential difference between Skinner and Chomsky in their respective scientific endeavors. Rightly or wrongly, Skinner believes that the laws which govern operant behavior play a role in the selection and organization of the organism's behavior which is equivalent to the role of natural selection in the evolution of species. In both cases, the same fundamental mechanisms operate on materials which vary considerably in complexity. The differences between the virus and the human brain are of a magnitude comparable to those between the rat's lever press and verbal behavior; but even though each level of complexity calls for its own description, one is not compelled to renounce the fundamental explanatory unity introduced by the key concepts of selection of behaviors by the intervention of the environment, on the phylogenetic scale in one case, and on that of the individual in the other case. The fact that their application to the most complex levels appears to defy a rigorous analysis is not a sufficient pretext for affirming that the fundamental explanatory mechanisms there become invalid: we will probably never succeed in providing a detailed reconstruction of the interplay between mutation and natural selection which led to the jay's plumage or to the human hand, but we need not thereby conclude that the "will of a Creator" or a "vital force" constitute better explanations for such astonishing phenomena. This is, however, what Chomsky does concerning the analysis of verbal behavior; faced with the insufficiencies (inevitable,

given the current state of our knowledge) of Skinner's thesis, he proclaims the explanatory superiority of certain kinds of concepts which the behaviorist analysis attempts to show cannot contribute to the progress of a scientific study of behavior. A single citation will suffice to illustrate this sort of scientific primitivism: "In the present state of our knowledge, we must attribute an overwhelming influence on actual behavior to ill-defined factors of attention, set, volition, and caprice" (Chomsky, 1959, p. 30).³

Chomsky's apparent inability to discuss the data within the guidelines of several very simple (at least from the moment they were formulated: what is more simple than the Darwinian theory?) fundamental principles without sacrificing any of the complexity of the phenomena, leads him to remarks as absurd as this: "If it were true in any deep sense that the basic processes in language are well understood and free of species restrictions, it would be extremely odd that language is limited to man" (Chomsky, 1950, p. 30).

One could assert equally well and equally absurdly that "if it were actually true that the fundamental processes which direct the development and maintenance of swimming were well understood and free of species restrictions, it would be very curious that only certain species can swim." It is needless to repeat that general principles in no way exclude species differences — like individual differences — but that, on the contrary, they permit one to understand them.

In a final aspect of his argument, which he seems to consider the most convincing, Chomsky contrasts experimental laboratory findings with real life. One finds here the most classical objection of the man on the street to every scientific endeavor concerning his psychology. Scientists have always believed that the eventual explanation of reality, in all its complexities, is better served by trusting in the most rigorous and best verified experimental data, as remote as they appear to be from the complicated phenomena that one wishes to comprehend, rather than by trusting in speculation, starting with the usual concepts or with those forged by pure reflection. The physicists who studied falling bodies or elementary mechanics had no answer to the innumerable questions posed by the physical universe, but they took the tack of beginning with relatively simple things. If they had accepted objections similar to those that Chomsky makes to experimental psychologists, modern physics would, of course, not exist. In a passage which follows the one cited above — where Chomsky accords an *overwhelming* influence to factors such as caprice — the linguist goes so far as to state: "Hence the psychologist either must admit that behavior is not lawful . . . or he must restrict his attention to those highly limited areas in which it is lawful" (Chomsky, 1959, p. 30). Speciously, he adds a third possibility to the first, between parentheses, as if it were an equivalent: "or that he cannot at present show that it is — not at all a damaging admission for a developing science," a

³It is interesting to observe Chomsky's propensity for arguments referring to the authority of tradition — arguments of a reactionary nature, in some sense: he has devoted numerous pages, and some of the most brilliant, to show the relationship between his conceptions and those of classical authors eclipsed by the errors which have followed (see his analyses of Descartes, of the Port-Royal grammar, etc.). There is no doubt that this rehabilitation appears to his readers to be an argument for the validity of his own theses. It is clear, however, that rediscovery of an historical figure whose thought resembles one's own, no matter how prestigious he may have been, confers in itself no particular validity to one's ideas.****

****Translators' note: Aarsleff (1970), Stam (1972), and Verhave (1972) have critically examined Chomsky's interpretation of the history of linguistics and philosophy.

possibility which should be associated with the last, rather than with the first of the branches of the alternative. The scientific postulate assumes that the most complex phenomena obey laws. The fact that the known laws are insufficient to account for everything is not a serious argument for asserting that the path followed is not the right one.

It must readily be granted that, quite apart from his polemics, Chomsky does not reproach Skinner for believing in the capacity of experimental research to explain "real life" (since this reproach would put into question all of physics, biology, medicine, etc.), but rather for prematurely and groundlessly generalizing simple laws to complex phenomena and for attributing a totally unwarranted importance to concepts derived from the laboratory.

Now, after having considered the question of Chomsky's representation of Skinner's position, we will pass to the question of the substance of Chomsky's argument. How valuable are Chomsky's comments on the fundamental concepts of the experimental analysis of behavior and on the functional analysis that Skinner uses? Although it is impossible here to enter into all the details of an answer to this question, it is possible to show by several examples that Chomsky has not at all understood the meaning of Skinnerian concepts.

In the first place, on the basis of his writings, Skinner is regarded as a *stimulus-response* psychologist. But it is perhaps Skinner's most original contribution to have surpassed, from the very beginnings of his scientific work, the psychological conception summarized by the S-R formula, without falling into traditional appeals to the *intervening variables* dear to the majority of neobehaviorists (see especially Chapter I of Skinner, 1969).

The discussion of the notion of *stimulus control* testifies to a complete misunderstanding of Skinnerian theory, and a misunderstanding of even the most general concepts of neurophysiology. Using the most peculiar casuistries, Chomsky attacks the definition of the stimulus. What appears to bother him the most is that we "identify the stimulus when we hear the response." "But the word 'stimulus' has lost all objectivity in this usage. Stimuli are no longer part of the outside physical world; they are driven back into the organism" (Chomsky, 1959, p. 32). These remarks, made in conjunction with verbal responses, are of equal concern with any motor or glandular response studied in animals, for which the problem occurs in exactly the same fashion: the fact that "we identify the stimulus when we observe the response" is true, in fact, of all stimuli, even of the most elementary stimuli that one presents to elicit a reflex or to determine a sensory threshold. The stimuli of the physiologist and of the psychologist — let us say, to be brief, the biologist — constitute a class of events of the physical world that share the property of influencing the reactions of an organism (either by eliciting them, as in the case of a reflex, or by altering their probability of occurrence, as in the case of an operant response).⁴ Thus defined, stimuli are not removed from the physical world, and, other

⁴If the reader harbors any doubt concerning the definition of *stimulus*, he should consult a glossary or dictionary of psychology or physiology. Thus, English and English (1958, pp. 524-525) distinguish between three meanings of the term *stimulus*. In each of them, one finds this essential characteristic of action upon an organism. A stimulus is defined as a "physical event, or change in physical energy that causes physiological activity in a sense organ." Alternatively, it is defined as "a particular part of the environment that initiates a response in an organism." More generally, it is defined as "any phenomenon, object, aspect of an object, or event, however conceived or described, which modifies behavior by eliciting activity in a sense organ."

things being equal, one can vary their characteristics to see at what moment they cease to exert an influence on the organism. It is difficult to see how the stimulus "loses all objectivity" from the fact that it is identifiable only by the response. If Chomsky were right on this point, all biology would be denied objectivity, in that it studies the relations between living organisms and their environment. If the definition of the stimulus were not inseparably tied to the occurrence of the response, there naturally would be no need to define a particular class of physical events: all physical phenomena would be equivalent and the "environment" aspect of biology would be confounded with general physics, which is clearly not the case. Based on such a gross misunderstanding, Chomsky's conclusion: "the talk of 'stimulus control' simply disguises a complete retreat to mentalist psychology" (Chomsky, 1959, p. 32) is beyond comment.

Despite this misunderstanding, which nonetheless would prompt most examination graders to read no further, let us take the trouble to examine Chomsky's comments on the examples advanced by Skinner to illustrate the notion of stimulus control. When, confronted by a painting hung on the wall of a salon, we say "Dutch," Chomsky disputes whether we react to the subtle properties of the object, to which is applied the term *Dutch*, notably because of the complex contingencies of linguistic conventions. He bases his objection on the following argument: we might as well have exclaimed *clashes with the wallpaper, I thought you liked abstract work, never saw it before, tilted, hanging too low, beautiful, hideous*, etc. (Chomsky, 1959, p. 31). But in what way does the fact that other responses are theoretically possible modify the causes of the response which was actually observed? Chomsky's objection is typical of the formalist, more preoccupied with a combination of *possibilities* than with an explanation of *actualities*. Note that the same remark could be made, with equally little foundation, about any motor act: given the act of reaching for a cup of coffee and carrying it to one's mouth, one might offer an explanation of that act in terms of the properties of the stimulus (a cup full of coffee or milk, rather than empty, present at a time when one usually drinks coffee and this act has a *heightened probability* of occurrence, etc.), properties always tied to a behavioral *history*, as we shall see in a moment. Chomsky could retort: "Your explanation is senseless, because you could very well have done something else, for example, thrown the cup on the ground, tossed the coffee in your hostess' face, poured it on the houseplants, emptied your pipe in it, etc." A functional analysis seeks to explain what does happen, not what could happen.⁵ That which could occur may, in fact, sometimes occur or never occur, in the case of a given individual: it is a matter of the history of contingencies to which he has been exposed. For example, among the possibilities proposed by Chomsky, the probability of the response *never saw it before* is practically nil, given a subject who, in fact, has already seen the painting in question (forgetting, or lying, would not be causes, but variables to be explained in their turn). The properties of the stimulus that control the response reflect clearly the behavioral history of the subject. The explicit response *hideous* is less probable in the case of a polite individual, careful not to vex his hosts: audience variables here determine the verbal response. One could

⁵By their basic nature, formal disciplines tend to operate on *possibilities*, a tactic which is safe, and, in fact, quite fertile, when one cannot operate on any particular, real datum. One can imagine an infinity of mathematical models, all of which are legitimate in a purely formal perspective. But only a limited number of these models can be applied to physical reality. Neither linguistics nor logic can have the freedom of pure mathematics.

imagine other responses in addition to those that Chomsky proposes. *Primitive Italian* would be a response attesting to control by poorly discriminated stimulus properties — the criterion for *poorly discriminated* being the adequacy of the contingencies of linguistic usage with respect to a certain portion of the physical environment. It is not caprice that determines whether one response or another comes, as the good mentalist Chomsky says, to the mind of the subject: it is a particular history of reinforcement contingencies to which he has been exposed.

It is wrong to place inside the subject the properties of the object which exert control over the verbal response: just as one may vary, other things being equal, a sound frequency for determining the range of frequencies which evoke a response from the subject — the *stimulus* frequencies —, one can imagine varying the properties of an object and observing the modifications in the probability of occurrence of such and such a verbal response. Contrary to Chomsky's assertion ("the stimulus controlling the response is determined by the response itself," Chomsky, 1959, p. 50), there is no epistemological or experimental impasse. For example, what deviation from the vertical is it necessary to impose upon the above mentioned painting so that the first response which "comes to mind" will be *tilted* rather than *Dutch*? Or, given a color vocabulary acquired in a certain linguistic context, what modification is necessary so that the response *red* loses all chance of occurrence and the responses *orange* or *yellow* become more probable? These questions do not find their answers in the examination of the mind of the subject or in the analysis of the language system, taken in itself. They are, in principle, susceptible to an experimental analysis, even if it must be admitted that no one would have the patience to conduct this sort of inquiry for the set of all lexemes and morphemes, an enterprise which would be as useless as experimentally controlling all the situations of the physical world in order to build a serious physics on solidly established principles.

Not having understood the meaning of *stimulus control*, Chomsky becomes thoroughly mired in his discussion of reference and meaning as related to the functional class designated by the term *tact* (very inappropriately translated [Mehler, 1969] by *dit*). If one rereads the paragraph devoted to the discussion of the reaction to the word *fox* (Chomsky, 1959, p. 48), one can easily apply the immediately preceding remarks. The explanation proposed by Skinner does not, of course, exclude the possibility that the subject, listener or speaker, might never have seen a real fox: the word could have entered his verbal repertoire by another route. Contrary to what Chomsky would have us believe at several points in his critique, the direct physical experience of a stimulus is in no way a necessary condition for the establishment of a corresponding verbal response, according to Skinner's analysis. When he quibbles about the usage of terms such as *Moscow* and *Eisenhower* (Chomsky, 1959, p. 32), which for him do not refer to any direct physical experience, he reveals that either he has not read Skinner's book in its entirety (a long discussion of these cases may be found in *Verbal Behavior*, page 128, concerning the example *Caesar crossed the Rubicon*, which is even more convincing than *Moscow* and *Eisenhower*), or that he is engaging in deliberate misrepresentation, or that he has not understood the text that he has taken the risk of criticizing.

Chomsky can only oppose Skinner with traditional formulations, with which, one suspects, he is himself not entirely satisfied. We cannot dwell on the discussion of

denotation and *connotation*. Chomsky rejects Skinner's effort to use the controlling properties of the exterior or interior environment to account for the meaning of elements in the language as they appear in the usage of the speaker, and he pretends that one is no more advanced than appealing to the notion of *concept* (Chomsky, 1959, p. 50). This is to accept mental entities as a final explanation of behavior, a tactic possibly convenient for the purposes of the logician or linguist, but with which the psychologist may not rest, since he must, in turn, account for these entities. Here again, that which most eludes Chomsky is the importance of the behavioral history in the selection of specific responses, verbal or otherwise. Chomsky reproaches Skinner for resorting to notions of obscure, internal stimuli, although, in reality, Skinner does not lend them much importance (except in the verbal expression of internal states, a problem admirably approached by Skinner, but which in itself demands a long discussion). What matters is the set of contingencies which have shaped the verbal behavior of the subject: they are never understood by an analysis of the current situation only (which the formalist will neglect, so as to consider only the linguistic structures which manifest themselves, inasmuch as they are a sample of an infinite set of structures governed by the system of the language), but by an examination of the antecedent conditions to which the observed behavior is related.

The analysis of the notion of *mand* (unhappily translated [Mehler, 1969] by *requête*, which introduces a restriction that Skinner precisely wishes to avoid) also testifies to a profound incomprehension of the Skinnerian point of view. It highlights the fact that Chomsky does not grasp psychology's essential distinction between classes of responses defined *functionally* and *formally*, between *psychological* description and *linguistic* description of the facts of language. When read in Chomsky's critique, the expression *pass me the salt* is a textual response, not a *mand*. It becomes a *mand* if one uses it at the table in order to salt one's soup. Formally the utterance is the same, susceptible to the same analysis. Certainly, one does not deny that formal analysis reveals certain things, in fact, many things, about the functional value of verbal behavior: the contrary would be surprising, if one admits that human language itself could not have been differentiated except through contingencies of reinforcement, that is to say, in a functional evolution. But the contingencies are not fixed in a univocal fashion in the conventions of the linguistic community, and scores of extralinguistic variables intervene in all sorts of ways to modify a raw material more flexible than the current insistence on *rules* permits us to believe. Thus, there cannot be a strict parallelism between formal description and functional interpretation.⁶ Even if the former always illuminates the latter, it does not exhaust it, and cannot, therefore, substitute for it, as is well expressed by Skinner: "the traditional classifications suffer from a mixture of levels of analysis. In particular, they show the influence of formal descriptive systems in which sentences are classified with little or no reference to the behavior of the speaker. It is here that the shortcomings of grammar and of syntax in a causal analysis are most obvious. . . . The usage of the *mand* as a unit of analysis does not mean that the work of linguistic analysis can be avoided, but

⁶Concerning the partial overlap of formal classes and functional classes in language, one should reread *Verbal Behavior*, pp. 43f.

it simplifies our task by isolating the behavior of the individual speaker as an object of study" (Skinner, 1957, p. 44).

No formal analysis permits us to decide if the expression *when will you finish your chores* or *will you be quiet* are, functionally, *questions* (one category of *mands*) or *orders* (another category of *mands*). The best guide for deciding, in a given context, is to observe the reaction of the listener, and to see if it reinforces the verbal behavior of the speaker. If the child to whom the second utterance is addressed regularly reacts by declaiming upon his willingness or his refusal to be silent, it is almost certain that his utterance would lose its status as an *order*, and would appear thereafter in the speaker's repertoire only with the status of a *question*. To pretend that the functional status of the utterance is revealed by the *intention* of the speaker is obviously to elude the crucial problem, from Skinner's viewpoint: *intention* explains nothing. The notion of intention refers to certain aspects of behavior resulting from a particular history of contingencies of reinforcement. Chomsky states: "replacing 'X wants Y' by 'X is deprived of Y' adds no new objectivity to the description of behavior" (Chomsky, 1959, p. 47). For the scientist, who deems it essential to control variables if their influence on behavior is to be ascertained, this substitution of terms is of prime importance: one has the capacity to modify the duration of deprivation of Y, and to observe the variations which that deprivation causes in the behavior on which the commonsense inference is based that X *wants Y*, whereas one can in no manner act upon the *intention* itself.

Of course, it is the fundamental thesis of the Skinnerian methodology that is here in question, and Chomsky demonstrates once again that he has not fathomed it, and that, consequently, he cannot oppose to it one single, pertinent criticism. Chomsky's thesis is that formal analysis is primary, if, in fact, it does not totally supplant functional analysis. However, one may ask if formal analysis can indeed successfully ignore functional analysis when confronted on certain points. Let us take a simple example. In English, the selection of the article is determined by a certain number of constraints which grammar describes (and which are doubtless susceptible to a convincing transformational analysis). But the factors indigenous to linguistic systems do not always suffice: imagine an individual in front of a known object, for example, a carburetor, placed in a very complex structure foreign to his experience; he will utter *a carburetor* more probably than *the carburetor*; the latter utterance would be more probable in the presence of the same object placed in a familiar structure such as an automobile engine. The criterion of familiarity which intervenes in the selection of the response (article) obviously has no formal status; it refers, by definition, to the history of the speaker.

Another inadequacy of Chomsky's presentation of Skinnerian behaviorism is the discussion of drive theory and the notion of reinforcement. Chomsky writes: "The most common characterization of reinforcement . . . is in terms of drive reduction" (Chomsky, 1959, p. 39). Accurately enough, he notes in a parenthesis, that Skinner explicitly rejects this definition, notably for reasons (and these Chomsky does not mention again) identical to those that Chomsky uses. That is, the concept can have no meaning unless one defines drives independently of that which is, in fact, learned. And although he is dealing with a work by Skinner, and not with the notion of drive in the diverse forms of neobehaviorism, Chomsky treats us to numerous pages on the debates pertinent to latent learning, to the drive for exploration and stimulation, and to imprint-

ing, none of which have any relation to Skinner's theory or to his treatment of verbal behavior. That which precisely distinguishes Skinner from other American behaviorists on this point is to have put aside the notion of drive as an *ad hoc* variable postulated account for learning. This was done by Skinner in order to consider only those variables over which one can actually exert control, such as the reinforcing event, the duration of an alimentary or social deprivation, the reduction of afferent stimulation, etc. In lengthily introducing the facts of latent learning and exploration, Chomsky appears to categorize Skinner as a drive-reduction theorist. This is yet another mistake. The reinforcing character of sensory stimulation (under certain conditions) and of the manipulation of objects is perfectly compatible with Skinner's conceptions. Note that a large number of the experiments which Chomsky cites as if they contradict the Skinnerian viewpoint were in fact executed with the aid of operant conditioning methods. In another domain of extrapolation to human subjects, that of instruction, Skinner has sufficiently insisted on the "intrinsically reinforcing" character of activity so that it is unreasonable to cite the experiments of Harlow or Montgomery (Chomsky, 1959, p. 40) in opposition to Skinner's position (see Skinner, 1968, several articles of which appeared before 1959).

For Skinner, the phenomenon of imprinting raises no particular problem. To the extent that it constitutes a type of learning outside the domain of the laws of reinforcement control, it furnishes an argument neither against the existence of these laws nor against their relative generality, due to the highly restricted domain of imprinting (which the zoologists were the first to recognize). The "innate disposition" (Chomsky, 1959, p. 41) that Chomsky invokes is not an abstract faculty. It is a precise condition of the organism during the critical period, in which the organism reacts in a certain manner to stimuli which possess certain characteristics. In Skinnerian terms, imprinting is a particular case, in the course of ontogenesis, of the acquisition of the reinforcing value of a stimulus: there is nothing about it that puts reinforcement theory into question. In an analogical fashion, food ingested by mouth is not reinforcing until after birth.

These are only a few of the aspects of Chomsky's critique which testify to a distortion of Skinner's position.***** One could enlarge the list, but that would involve a closer examination of *Verbal Behavior* and of several notions that lend themselves poorly to a concise discussion for readers with little exposure to Skinner's works. However, one may cite a few of these notions which a more thorough discussion should consider: the *unit* of behavior, the verbal *repertoire*, the distinction between the particular *instance* and the *class* of responses, *abstraction*, *private stimuli*, *audience effects*, the *language-thought* relation, the composition of *novel utterances*, grammar and syntax, etc. These are notions which Chomsky leaves out of his discussion, for the most part.

The several preceding commentaries suffice to show that Chomsky's long critique, if it is representative of the thinking of this American linguist, whose radically mentalistic,

*****Translators' note: Fodor's (1975, pp. 100-102) comments on Chomsky's critique reveal that, even today, a similar state of affairs persists in psycholinguistics. His remarks on the environment-independent, "causal" status of such entities as *memory*, *attention*, *motivation*, *belief*, and *utility*, on the "taxonomy-defying" interaction of causal variables determining verbal behavior, on the "difficulties" posed by utterances which refer to absent things, on the learning of *S-R connections*, and on the "nonequivalence" of *verbalizations* and *responses* all indicate a remarkably durable misunderstanding or misrepresentation of Skinner's position.

nativistic, and formalistic position it perfectly expresses, does not give an objective image of the work and the author that it criticizes, but, on the contrary, a singularly distorted and incomplete image. Skinner's theses are, assuredly, not above attack, but is not upon the points treated by Chomsky that they present failings. In this text which has made its name among certain circles of psychologists, the master of transformational grammar has not refuted Skinner. At most, he has reproduced the traditional formulations that Skinner's efforts sought to surpass. The question is not to know whether one theory is more worthy of calling itself scientific than the other, nor whether one theoretician is more or less dogmatic than the other, nor whether "real life" is near or far from a scientific analysis, but rather to know whether certain methods and certain concepts are more useful than others to the progress of our knowledge of verbal behavior. Two schools so irreducibly opposed have no other choice than to follow their respective lines of research and to let the future pass judgment upon their success. Perhaps Skinner's book was too advanced for its time to challenge the new linguistic vogue. Since then, this formalistic fever has run into numerous impasses, and it appears that the functional approach, which is in fact inherent in any biological discipline, is reassuming its rightful place in the study of language and is defining the methods which, surpassing the interpretative efforts offered by Skinner, will provide a contribution rigorously founded in facts.

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