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MEANS AND ENDS IN PSYCHOLINGUISTICS

At its birth, a new scientific discipline is baptized with the names of the two parent disciplines, Science X and Science Y. Subsequently, usage tends to shorten this compound name by giving preference to one or the other of the constituting terms so that the new term is less disconcerting and novel. Take biophysics, for example. Is it more closely related to physics than to biology, or the other way round? Today this question seems naive but it did not appear so a scarce thirty years ago, that is, at the beginning of the creative activity of the most eminent scientists in the field of biophysics: it was then the subject of intense discussion.

The author of the present article belongs to a generation that was constantly faced with a question of the same order regarding mathematical linguistics. It is interesting that the answer given to this question twenty years ago is different from the one given today. Twenty years ago, mathematical linguistics took in any linguistic study that employed mathematics in their broadest sense. Since then, the limits of mathematical linguistics have been redefined: one part—for example, the theory of formal language—has been going toward mathematics; another part—for instance,

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computational linguistics, has acquired more precise outlines. Nowadays a linguistic research which only contains quantitative data and/or rudimentary statistics is not considered as pertaining to true linguistic mathematics, whose field is much more defined.

With this in mind, we shall not try to define strictly the field of study of present-day psycholinguistics. It is obvious that psycholinguistics is closely related to both linguistics and psychology. If, therefore, we desire to make clear the status of psycholinguistics as a scientific discipline, it is expedient to see what is applicable in this regard to both linguistics and psychology, and we will show why this course seems essential to us.

There are usually at least two conditions to be met if a scientific discipline is to be considered a separate area of knowledge. One is a clear picture of its subject matter, that is, a definite idea of which problems are within its competence and which are outside it. A second condition is that every autonomous scientific discipline must have specific techniques and procedures at its disposal that are adapted to the intrinsic properties of the subject matter. For linguistics and for psychology, there is no doubt that these conditions are met. However, it is important to reconsider certain points as far as linguistics is concerned.

Linguistics is defined as a science whose subject matter is natural language. This seems to be a perfectly clear and unambiguous description. However, let us go a little further and try to see what follows from this definition. For instance, can linguistics answer the following questions: How do we learn our mother tongue? Does this process have anything in common with the learning of a foreign language? Can linguistics explain the passage from thought to its verbalization? What "device" accounts for reading? Can speech perception be conceived of as a process symmetrical in structure to speech production? If it is possible that patients suffering from specific local brain damage will produce certain patterns of distorted speech, why should it be precisely those patterns?

Essentially, the significance of these questions would seem obvious; their solution is decisive for not only the study of verbal behavior but also for the study of human intellectual activity. This is clear to both behavioral and natural scientists. To try to separate what is purely linguistic from what is purely

psychological would lead to failure. To be able to face these problems a specialist should be thoroughly at home not only in linguistics and psychology but also acquainted with physiology, psychoacoustics, neurophysiology of speech and related subjects —at least with their basics. "Pure linguistics," whose first and foremost objective is the study of speech as a system of signs, a "language," is not concerned with these questions; yet a psychologist without a solid linguistic background would be at a loss. It is obvious that all these problems should be considered as the subject matter of "psycholinguistics." However, what is important is that up until now no valid answers have appeared for the questions. Even more important, those questions should be sharpened and more precisely formulated. We cannot help asking how a science can function when it is unable to answer its basic questions. What becomes of its self-image and self-respect?

At some stage in the development of a science there comes a critical moment that forces the scientist to stop routine work and reconsider his activity, that is, the means he is using and the goal he is pursuing. The investigator begins to doubt the well-foundedness of the questions he puts to Mother Nature and to re-examine the validity of the procedures at his disposal. Usually, this methodological reflection follows, in a given science, two interdependent directions: one, what should be studied to discover the essentials of the subject, which puts the emphasis on the ends; the other, what methods should be adopted to reach those ends, which puts the emphasis on means. In the following discussion, we shall try to examine the relationship between the means and the ends in the study of a natural language conceived of as an open (unbound) system, a system arising from the reality and communications of speakers. The reader will choose his own label for this area of research, whether it be the psychology of language, linguistics, psycholinguistics, or whatever else he deems appropriate.

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At the highest scientific level, that of the final "super" goals, the problems usually seem to be well defined and concisely stated. Both linguists and psychologists appreciate the elegant formu-

lations of the goals as set forth in works such as "What Are We Really Doing When Speaking?" (Keyser and Halle, 1963)[1]; "Why the Mind Is in the Head" (a well-known classic by McCulloch)[2]; or Osgood's "Where Do Sentences Come From?"[3] Now, the more precisely the problems are stated, the more the approach to their solution becomes vague. Indeed, the controversies between scientific schools may be presented as the consequence of the zeal shown in trying to find the best way to workable hypotheses. In the study of natural language and language behavior the rapport between the problem (that is, the "ends") and a testable hypothesis is very subtle, since the real subject of research—behavior mechanisms—is unavailable for direct observation. Each time we want to test something we must first work out a long series of related elements to discover, as a corollary, what is really subject to proof. At the end of this procedure, exact problems appear, such as "Situation of Phoneme Boundaries Between the Vowels I and E" (Chistovitch)[4] or "Perceptual Confusions Among Some English Consonants" (Miller and Nicely)[5]. It is at this point that a scientist feels alienated from The Problem: what are we really doing when we speak? Any scholar will understand what I mean. Unfortunately, the subject is too academic to appear in scientific journals.

Actually, at the beginning of the work, when we proceeded from the attempt to find a solution to the major problem to the study of more limited questions, we were certain we had established a hierarchical order of the questions to be clarified. Now, having accumulated a certain amount of relevant data we feel lost and find, instead of a clearly-marked path, a labyrinth of paths leading nowhere. We could say that the very excess of data is

[1] *Recognizing Patterns*, ed. by P. Kolers and M. Den, Cambridge, Massachusetts, M.I.T. Press, 1963.

[2] W.S. McCulloch, "Why the Mind Is In the Head," *Cerebral Mechanisms in Behavior*, New York, Wiley, 1951.

[3] Charles E. Osgood, "Where Do Sentences Come From," *Semantics. An Interdisciplinary Reader*, ed. by D. Steinberg and L. Jacobovitz, Cambridge, Cambridge Univ. Press, 1971, pp. 497-529.

[4] "Analiz retchevykh signalov tchelovekom," *Nauka,* (Problemy fiziologitscheskoj akoustiki) Vol. VII, Leningrad, 1971.

[5] G.A. Miller and P. Nicely, "Analysis of Perceptual Confusions Among Some English Consonants," *JASA,* 1959, Vol. 27, No. 2.

an obstacle to an appropriate synthesis. Let us take as an example a scholar who wants to contribute his particular stone to the edifice of a science. He has done all his research and made his calculations only to discover at the end of his meticulous labor that there is no place for his stone. There are certainly many gaps in the wall, but none of them fits his stone. What can we conclude, if not that we struggle to solve a problem but use the wrong methods? However, and this is widely accepted, this state of things is not new; dogged efforts toward solving such or such a problem even with inadequate tools have often given stimulation and impetus to the advancement of scientific knowledge. Thus the conflict between means and ends does not apply only to linguistics or psychology. However, since the matter in both these disciplines is intrinsically connected with this conflict, it is useful to examine its sources in some detail.

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Basically, any scientific activity has for a premise an absolute faith in the goals of science. This faith bears the distinct imprint of what I should like to call "naive realism," and I should like to show that our forerunners in science have always drawn from this vital source to accomplish their work. This naive realism should not, of course, be reduced to knowledge arising from simple common sense. It is rather a set of basic scientific axioms together with a large mass of data, more or less accepted facts, a scientific activity that is admitted on the individual level as well as on the collective level by virtue of a consensus of opinion as to what should be accepted. To the fervor of naive realism we owe the achievements of machine translation in the mid-fifties, as well as research in pattern recognition. Naive realism was essential for initiating the revision of classic linguistics and for the assumption that pattern recognition by machine was a real problem to be solved and not science fiction. These achievements could be thought of as leading to the idea of analytical approach as all-powerful, or nearly so. Such a belief in the miraculous virtues of analysis, it also tinged with naive realism, infallibly generates a simplified point of view: whatever the complexity of the subject under study, an astute researcher can

find a suitable method to reduce it to analyzable components, and this method would be exactly the one to permit getting to the bottom of the problem. The deep-rooted belief in a so-called "finality" of knowledge may also be considered as an expression of naive realism, because every scientist has the instinctive desire to bring to science the bit of knowledge he himself has. It is truly a tragic and primordial desire, to believe that we have done something future generations will profit from; no one can proceed with his work if he thinks its results will soon be nullified.

Certain conservative tendencies in science go well with the picture we have just drawn; to stay within a paradigm, that is, a pattern, and exist as a part of it presupposes that it is accepted. Each period in the life of a science has its basic rules and conventions that make up the conditions of "science for science's sake," as T. Kuhn remarked. As long as a scholar does not think of his work as a *Glasperlenspiel*, to quote Hermann Hesse, he has no choice: he must be a naive realist to believe that he can grasp the essential and not just scan the surface. He has faith in the omnipotent wisdom of an investigator who will conquer the complexity of the subject under study.

The naivety of this realism only appears in a later stage of development of this science. If we apply it to linguistics and psychology, the phenomenon reveals the contradiction in the goal of our research and the means we use to achieve it. We can only study what is observable, that is, speech and verbal behavior. However, we strive to understand the non-observable: cerebral activity, the mechanisms of verbal behavior. It is not verbal behavior *per se* that interests us, but we have nothing else to experiment with. We can only study language through the accumulation of a mass of data that can only be obtained by sampling speech. Any proposal concerning our knowledge of language is only connected with observable data by the "black box" approach, so that any pattern of verbal behavior can only be functional and must be regarded as such. A theory (or pattern) of language must not be considered as true or false but only as more or less plausible. However, this type of reasoning is not well thought of in linguistics or in psychology, since it is by definition assumed to give several interpretations to the results of a single experiment. This makes looking for one interpretation

useless: the only sensible thing to do is to rank the possible interpretations according to their plausibility.

At a certain stage in the development of a scientific discipline, however, this situation becomes somewhat disquieting for the scholar. He feels lost before a large accumulation of data which does not fit into any framework for satisfactory study. Most functional models only give an account of observable facts and so do not permit a unified concept. As a result, we see the emergence of a large number of subsidiary mini-theories. Now, there is no more bitter disappointment for a scientist than to find, instead of a well-ordered system, a group of empirical, disparate data and a jumble of theories that can be compared to the fragments of some gigantic and incoherent mosaic. For specialists in questions of human behavior this situation appears absurd and brings about the need for a radical change that will restructure the mosaic into a solid scientific theory.

All that we have just said is true for both linguistics and psychology, but with some reservations.

We have proceeded from the hypothesis that speech is a manifestation of the mental activity of a speaker. However, speech is multiform and must be studied in different contexts, such as cultural anthropology and history, leaving aside the questions of verbal behavior. It may also be studied from the semiological point of view, that is, as a system of signs, as was done by Ferdinand de Saussure, for example.

In this regard, a distinct difference between linguistics and psychology in epistemology must be stressed. For psychologists, physics offers the pattern of a mature science; for linguistics, it is mathematics that plays this role. We could expect, therefore, that this difference would produce a conflict in psycholinguistics or at least that origins of the two tendencies would be brought out and their possible interaction carefully evaluated. This is not the case. How may it be explained?

It seems to me that one of the underlying reasons is the striking dissimilarity in the roles played by experiment in linguistics and in psychology. Psychology became an experimental science with Wundt, its founding father. It could be said that psychology became a science when experimentation became its principal means of problem solving. Now, it is quite otherwise

with linguistics. Even in that part of linguistics called "experimental phonetics" the status of the question led Malmberg in 1956[6] to give detailed explanations of the difference between experimental measurement of certain speech parameters and experimentation as a scientific method.

At the end of the fifties, linguistics was no less scientific than psychology, but it was formalization and not experimentation that permitted it to progress. As we know, it was Saussure who opened new horizons for the formal analysis of speech through his conception of it as a system of signs. Since Saussure the attainments of linguistics have proceeded from this conception of goals and content of a valid description of speech, leaving aside the problem of verbal behavior and the process of communication by speech. Neither is it surprising that in the fifties the impact of cybernetics on behavioral sciences was extremely fruitful for linguistics, since its phenomenology was particularly suited to reveal certain mathematical structures that were essential for the understanding of language as a sign system.[7]

Fruitful as it was, however, it was only one of the many possible approaches to the analysis of a natural language and only one branch of linguistics. Another tendency appeared: not only was language considered as a sign system, but special attention was granted to the problems of verbal activity and behavior. Still, the crucial question of what we really do when we speak remained unanswered, a fact that disappointed linguists and psychologists alike.

At the time, however, the contradiction of having recourse to the "black box" as the only way to speculate on what is not observable at the same time as we try to explain what we do when we speak did not appear too flagrant. It has taken a lot of time and effort to understand that the study of a phenomenon arising, as speech does, from a mental activity presupposed access to a completely new field about which nothing was known.[8] Afterward, the methodological controversies over the objectives

[6] B. Malmberg, "Questions de méthode en phonétique synchronique," *Studia linguistica*, 1956, Vol. X, No. 1.

[7] O.S. Koulaguina, "Ob odnom sposobe opredelenija grammatitcheskikh nonjatij na base teorii množestv," *Problemy Kibernetiki*, 1958, No. 1, pp. 203-214.

[8] M.A. Schreider, "Složnye sistemy: Kosmologitcheskie printsipy," *Sistemnzeiss-ledovanija* (annual pubblication) 1975, pp. 149-170.

of the study of spech and verbal behavior became sophisticated. They were brought about first and foremost by the conflict between goals of the research and the technical methods available. Inevitably, as we pointed out above, more circumscribed questions had to be answered before a decision could be made about what should be studied in order to know what we do when we speak and communicate, as well as what methods to be used to that end.

The development of two very important lines of speech research—machine translation and machine speech recognition—may serve as an example to show the inevitable conflict between means and ends in scientific activity. Although the reader may not necessarily be interested in either automatic translation or automatic recognition of speech, for the sake of clarity with respect to what follows we must first bring up those questions that are continually being reformulated; then we must isolate some of their aspects for closer scrutiny, which may seem at first glance to be going into great detail.

Let us begin with the problem of machine translation. It consists of furnishing an input of a text written in a natural (source) language. This text must undergo transformations that will result in the output of a target language, semantically equivalent to the input and grammatically correct. If this procedure is thoroughly formalized, we can construct an algorithm (a set of symbols) for a computer. It is important for a linguist to find out which differences between the source and the target languages are relevant in order to develop an algorithm. He must also know what grammatical and lexical information about both languages may be stored in the computer memory and in what form. The algorithm could be considered a computer-analogue model of specific information reproducing the activity inherent in "human translation." A computer-analogue obviously has nothing in common with real thought process, but because it is operationally directed, it gives the same result.

As for machine recognition of speech, it could be formulated as follows (and here we over-simplify Chistovitch[9]): An acoustic

[9] L.A. Chistovitch, A.V. Ventsov, *et. al.*, "Fisiologia retchi Vospriatie retchi tchelovekom." *Nauka*, Leningrad, 1976.

stream of speech sounds is used as an input. These sounds must be transformed if the output is to be a sequence of symbols appropriate for phonetic interpretation, that is, specific indications as to how the perceived sequence should be pronounced by the human receptor. A system that can produce such decoding may be considered as a functional model of speech recognition. It is obvious that if both types of system were really constructed we would be much closer to a very tempting achievement, a machine functioning as a "man-machine," communicating in a natural language.

The first move made by those who were brave enough to attack this major problem was, naturally, to look for relevant data in the literature, whereupon they discovered that they radically differed in point of view. Certain linguists working on machine translation problems were hampered by a strictly theoretical linguistic knowledge, from the classic comparative grammar to different structural approaches, at that time still unclear. The most noteworthy achievements of theoretical linguistics contained, as we said above, a description of speech as a sign system. On the whole, however, linguistic reasoning was not precise enough to be formalized, so that it could not immediately serve as a basis for the formal representation of the structure of a language that would imply an algorithmic construction.

The first generation of linguistics to attack machine translation had first of all to devise a new conceptual framework in order to discover appropriate formal equivalents for linguistic terms and relationships. It was of utmost urgency to set aside Meillet's well-known statement that there are as many linguistics as there are linguists. The classic authors were revisited—Jesperson, Saussure, Sapir, Fortunatov—and scrutinized: fundamental linguistic postulates were reviewed with a critical eye. These stimulating activities unleashed a scientific revolution in linguistics that gave rise to a new paradigm.

Leaving aside for the moment further comments on the development of machine translation, I should like to emphasize here the salutary effect that this conflict between means and ends, that I have just described, had on linguistics. To translate by machine there must be an algorithm of the structure of

a language that can only be obtained by the use of a strict and sound linguistic theory. Obviously, a collective effort aimed at a critical revision of linguistic theories could only become an end in itself. For my part, I should see a valuable contribution to linguistics achieved in research in machine translation. This contribution to the understanding of human ways of processing linguistic information requires further discussion, and I shall return to the subject.

At this point, some comments should be made on research in speech recognition by machine. The point of departure for research dealing with the problems of automatic speech recognition has nothing to do with that of machine translation. The data applicable to the former are really impressive. First of all, there is an enormous mass of reliable experimental findings in communication engineering, speech physiology, psychoacoustics and linguistics (phonetics). At least part of the experimental data is perfectly integrated by more or less formalized theories.

Let us stress here that linguists dealing with the series of speech sounds, whatever their theoretical orientation in linguistics may be, could not succeed without taking into account the verbal activity of the speaker. In any case, there is no choice: the person speaking and hearing is the one who encodes and decodes the signal. Any theory in this domain, however formal it may be, must be connected to the empirical reality of meaning and communication. The best example is furnished by the classic *Grundzüge der Phonologie* by Trubetzkoy.[10] His phonology based on structural premises is one of the most important achievements of structuralist aspirations. He himself did not think of his system as belonging to the natural sciences, but his constructions, precise and closely linked to the empirical universe of speech, provide the alert scholar with many possibilities for experiment.

We know that Trubetzkoy did not use the phoneme as a basic constructive element; he chose instead the distinctive feature, thus defining every phoneme via a set of distinctive features. There are fewer distinctive features than phonemes; this is true for every language. This phonological level was represented as a system whose elements had specific relationships with one

[10] N.S. Trubetskoy, *Grundzüge der Phonologie,* Travaux du Cercle linguistique de Prague, VII, 1939.

another. The set of relationships could account for both phonological universals and single phonological features. The most widely-accepted and general definition of a signal (including spoken signals) describes it as a process transmitting information on the state of the system that generated it, which means that we could begin the search for an empirical analogue of a distinctive feature by the study of processes involved in either speech perception or speech production.

A hypothesis has been put forward in which a listener receiving a signal induces his motor commands to transmit to his articulatory muscles, so as to produce such a signal. This model of speech perception has been called "analysis by synthesis." It has led to a great deal of experimental research in speech perception and production that has greatly enriched our knowledge about speech. This "analysis by synthesis" explains a large number of discoveries, many of which had been explained by other conceptual models. However, it is a basic premise that experimental research has not been able to corroborate: motor commands cannot be taken as a standard. The same phoneme may be the result of different articulations, from which it follows that an isolated articulation cannot serve as a possible empirical analogue for a distinctive feature.

At the same time, many efforts have been directed to the study of the acoustical parameters of the perceived speech signal. A new and promising method, dynamic spectrography, was elaborated, its goal being a visual transformation of the spoken symbol. "Visual speech" was submitted to linguistic analysis; the results may be found in the Jacobson-Halle system of twelve distinctive features,[11] which admits the description of spectral segments corresponding to phonemes. This model has shed light on both the merits and the deficiencies of the method. Visual inspection of a spectrogram admits different interpretations of how a certain extension of speech might be divided into segments corresponding to phonemes. The "visible" acoustical parameters do not correspond to either distinctive features (a)—in Trubetskoy's meaning—or to subjectively useful "psychological"

[11] R. Jacobson, G. Fant, M. Halle, *Preliminaries to Speech Analysis. The Distinctive Features and Their Correlates*, M.I.T. Acoust. Lab. Techn. Rep., No. 13, 1955.

features (b), that is, those used by human subjects for decoding speech (c). Some distinctive features may be read from the visual transformation of speech, which means that there is at least a certain amount of correlation between (a) and (b); this is also true for (b) and (c), but we are far from a term to term correspondence between these different elements.

I should like to stress that if a human being can divide the verbal stream into phonemes there must be a special "device" in the brain permitting the reliable phonemic interpretation of the acoustical input. This device works as a black box, which it is up to us to change into a "white" one, if we may use that term.

The reader may infer from the above that Trubetskoy's concepts have been assimilated by modern science and that his ideas reveal their stimulating force even in areas which were not defined when *Grundzüge* was written. As a matter of fact, the distinctive features described by Trubetskoy within the framework of a formal theory are in strict correlation with those human speakers use to recognize the features they perceive (or produce) as psychologically different. In this regard, certain distinctive features are first and foremost like certain markers pertaining to the mental activity of speakers, so that attempting to understand the complex relationship between the phoneme as a group of distinctive features and the perceptual (or generative) activity of a human subject, we quite naturally come to experimentation.

The works of the Soviet scholar L.A. Chistovitch provide the best example.[12] Her major achievement aside from the interesting experimental results obtained in her laboratory at the Pavlov Institute of Physiology in Leningrad is her methodology itself. It is interesting to note that the role of experimental research in linguistics was emphasized as long ago as the thirties by the leader of the Leningrad school of linguistics, L.V. Scherba, so that linguistics has greatly benefited from research in machine recognition of speech.[13] If I have insisted on research in automatic

[12] See Notes 4 and 9.

[13] L.V. Scherba, "O trojakom aspecte jazykovikh javienij i ob eksperimente v jazykoznanij," (1931) in Scherba, "Jazykovaja sistema i retchevaya dejatel'nost'," Leningrad, *Nauka*, 1974, p. 24-39.

speech recognition, it is because in this area the relationship between goals and methods, ends and means, seems to be very evenly balanced and promising. As we saw above, working hypotheses were continually questioned, as were methods then in use. Needless to say, no one waited for new techniques to develop by themselves; the problems were attacked with the tools at hand. If it sometimes happened that a method turned out to be of limited use (as was the case for dynamic spectrography) it was still considered a step forward and not a move into a blind alley.

The main line of research in machine recognition of speech may be likened to a bionomic approach, that is, a living system is studied as a possible prototype of an automaton.[14] When Chistovitch (USSR), Fant (Sweden), Lieberman (USA) and their collaborators speak of a system, they mainly have in mind a human subject perceiving and producing speech. For example, it is to a human subject that a series of synthetic speech sounds are presented for imitation for the input, and it is the human subject's reactions—the sounds produced—that yield an output. To give an account of the behavior observed, a functional model is set up that not only would encompass the results of experiments of the kind just described but would give an account of an entire category of experimental outcomes. Now, if these outcomes predicted by the model are not in contradiction with the observable behavior of a living system, we may try to construct an automaton that would function according to the operational principles serving as basis for the model.

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Let us now return to the problem of machine translation. It is clear from what we know about it that it may be called cybernetic. The first successes in machine translation impressed scientists as a genuine discovery, not because they believed humans use the same process to go from one language to another but for quite different reasons. To tell the truth, nothing is more vague or more unsubstantial than a "tacit knowledge" of a language.

[14] There is also a purely technical approach pertaining to the field of communication engineering.

As was seen, however, tacit knowledge could become overt through formalization and could be presented as an algorithm. As a direct consequence of this formalization of our tacit knowledge, the first experiments in machine translation were made (from Russian to English in Georgetown, 1954, and from French to Russian in Moscow, 1956). The translations were not outstanding for their style, but semantically they corresponded exactly to the source texts.

It is important to call attention to the fact that most of the scholars working with machine translation understood perfectly that human translation and processing of information from one language to another had nothing in common with the processing of the text by the computer, at whatever stage in the procedure. Since that time, we have come to understand better why, in the mass of articles devoted to machine translation, we find only one title dealing with the analytical ways of a human translator, namely, a well-known article by V. Ingve on "depth hypothesis."

Is the question well put, however? To tell the truth, what we really do when analyzing a sentence happens in the "black box" and is thus not observable. We deal with only the input and the output. Nevertheless, some hypotheses on the "human" ways of, for example, syntactic analysis could be tested. A phrase such as "my brother's mother-in-law's nephew" could be treated in a linear sequence or first restructured into an oriented graph, and then analyzed from the other direction, from "top to bottom," or vice versa. At least, corollaries could be tested from this hypothesis; it could be accepted or rejected according to observable verbal behavior. Theoretically, we could use this kind of data to work out the algorithm, but it would be taking the longest way round, for sure.

Progress achieved in automatic translation is due, if we may say so, to the invention of the wheel and not to that of the animated robot. If this is the case, the question arises of what model we are constructing in working out an algorithm for machine translation. Is there a significant relationship between our tacit knowledge (competence) of a language, such as revealed by our ability to translate semantic information, and an algorithm doing the same thing automatically? The answer is affirmative: such a relationship exists, but it is not simple and

requires some particular comments. Any algorithm of machine translation is of course based on the linguistic competence of its author, but the indispensable intermediary link in the chain is of utmost importance.

We said above that the algorithm for machine translation requires a special formal representation of language structure. Such a representation involves the transformation of tacit knowledge into overt knowledge, a very complicated process. This is exactly what we intend by "formalization." If there is not the conversion of tacit knowledge to highly formalized knowledge, there can be no algorithm, but this conversion has nothing to do with an algorithm of machine translation, which only presents the results of this process. It must be clearly stressed, therefore, that an algorithm of machine translation cannot be held as a model of the process of human translation nor of the process of formalizing tacit knowledge.

An example will make our point more clear. A person knowing English and Russian (or English and German) translates the expression "confectioners' sugar" into Russian or German. Obviously, if this person has a real command of the languages in question the phrase need not be broken down into its formal constituents, that is, the two words "confectioners" and "sugar." Someone who knows Russian well would find a direct semantic equivalent of the expression: *sakarnaja poudra* (or in German, *Zuckerpuder*). To convey this competence to the computer, we must encode into the computer's lexicon special indications of how the word "confectioners'" and the word "sugar" must be analyzed when they are paired. Whether this information be referred to one or the other of the words is purely technical. What is really important is that a precise semantic equivalent be found to render the meaning of the English phrase, something like "an extrafine variety of powdered sugar." The corresponding words in the target language must also be provided with this semantic labeling. If an algorithm is perfectly worked out, the translation from the input in the source language toward the semantic representation of a word or text, and, through its intermediation, to the equivalent in the target language, will have few or no errors. This means that we will have a perfectly functioning model.

Now, what does it mean to verify the functioning of algorithmic models? Usually, this verification is made during experiments in which complicated texts are presented to the computer, and a scientist studies the results of a synthesis (analysis) made by the computer. There are numerous experiments of this kind.[15, 16] It is clear that all we can really verify with this procedure is the quality of the algorithm used. However, should even a minor detail escape the attention of the investigator, mistakes would appear in the output. Still, since a language is an open system, it is hardly possible to obtain high-quality results in machine translation by merely augmenting the amount of information stored in a computer.[17] We can also disregard these questions; they arise from the more pragmatic aspects of the problem and are not pertinent to the present study. On the other hand, the theoretical aspect of the problem of machine translation, which we have stressed, is much more positive. In Russia, research in machine translation has always been theoretically, not technically, oriented. Here we are close to the intrinsic ties between the development of linguistic theory and the evolution of research in machine translation.

We remind the reader that *a priori* nothing could have predicted that translating—delicate and creative human activity as it is—could be done well by machine. We must give all due praise to those pioneers who had the courage to respond to Weaver's Memorandum (1949),[18] now famous, but at the time just a personal letter from a renowned scientist to his colleagues. We must pay tribute to the many research workers in many countries who since then have never spared their efforts to find the way to say "Yes" to the problem we have been discussing. As we saw earlier, research in machine translation has not only accelerated the development of linguistics but has created a new branch of mathematics that has permitted research in informa-

[15] B. Vauquios, *La Traduction automatique à Grenoble*, Grenoble, Dumond, 1975.

[16] O.S. Koulaguina, "Ob istorii i sovremennom sostojanii machinogo perevoda," *Kibernetika*, Kiev, 1976, No. 6, pp. 124-131.

[17] P.L. Garvin, "Machine Translation in the Seventies," *Papers in Computational Linguistics*, Budapest, Akadémiai kiadó, 1976, pp. 445-459.

[18] W. Weaver, "Translation," *Machine Translation of Languages*, ed. by W. Locke and A. Booth, New York, 1955.

tional retrieval and, in general, in artificial intelligence in the broadest sense.

Impartially, let us say that it is rare that a scientist finds a problem so stimulating in such varied areas. If the story of research in machine translation is sometimes presented as a series of trials and errors, it is due to many misunderstandings. If only one enterprise had turned out well, for example the work Terry Winograd devoted to the understanding of natural languages,[19] it alone would represent a formidable progress in the practical development of the "man-machine" system—in fact, a crowning achievement in machine translation research.

We hope we have clearly presented one point of view on the contribution of this research to linguistic theory. Therefore, we will return to our main interest: what kind of research can really contribute to the solution of the basic problem, that is, what we really do when we speak. We have seen that computer experiments cannot solve the problem of the underlying human process. However, we are on the right track with experiments in speech perception though we apparently do not have adequate means for going further ahead with testing hypotheses concerning only the phonetic level of language analysis. I believe that in order to get ourselves out of difficulty it would be well to have recourse not to modern linguistics but to modern psychology. Today, in the seventies, linguistics is a natural science. On the contrary, psychology has only a few general theories touching on the principal aspects of human behavior. Neither is there a general theory of verbal behavior. This makes the contrast between psychology and linguistics even more striking. Under the impact of cybernetics and the theory of information, linguistics had constructed a new paradigm, but since then another, remarkably fruitful paradigm has emerged: ideas relative to generative grammar and generative semantics. Now, we see no contribution from the psychologists positive enough to open new horizons in verbal activity.

However, let them speak for themselves: "Throughout the past decade, psycho-linguistic research has consisted of borrowing the latest linguistic formulation in the experimental field. The

[19] T. Winograd, "Understanding Natural Language," *Cognitive Psychology,* 1973, Vol. III, No. 1.

direction and movement of psycholinguistics were tributaries to linguistics, at its most highly-developed level; in fact, linguistics was a parent discipline" (Weimer).[20] This quotation is an excerpt from the general discussion at a conference held by linguists (for the most part followers of Chomsky), psycholinguists and psychologists, who borrowed from the latest linguistic theories (according to Weimer) seeking a common language. I agree with the meaning Professor Weimer gave to the term "to experiment" in order to apply it to this group of scientists.

Periodicals, especially in the United States, are flooded with articles describing psycholinguistic experiments of doubtful value. Many university members attempt to find ties between Chomsky's postulates and observable verbal behavior. These experiments, although meticulously conducted, are often devoid of any sound theoretical basis. It is not my purpose here to discuss the bearing Chomsky's views have on linguistic theory, but I should like to make some remarks on the inopportune way his fruitful ideas are sometimes applied. We know quite well that modern science has shifted from the description of states to the description of procedures. This implies that the dynamic description is often considered more valid than a static description. Chomsky chose the dynamic description of language structures and conceived of it as an evaluation, that is, an abstract theory. Such a theory has no direct ties with the empirical universe of behavior and, of course, makes no assumptions about the nature of the processes underlying speech perception and production. The attribute "generative" was used by Chomsky only figuratively to emphasize the dynamic aspect of his theory, so that any attempt to find empirical analogues for such terms as generation, transformation or deep structure seems senseless. The generative grammar of Chomsky does not suppose this degree of empiricsm and, being a formal calculation, why should it? A formal theory must be tested by its own rules and not through experimentation. Let us note that the psycholinguistic approach Weimer alludes to has nothing in common with Chomsky's own position. As he stated in 1967, "We have now discussed a model of competence. It would be

[20] *Cognition and the Symbolic Processes*, ed. by W. Weimer and D. Palermo, Hillsdale, Wiley, 1974.

tempting, but quite absurd, to regard it as a model of performance as well... A theory of performance (production or perception) will have to incorporate the theory of competence—the generative grammar of a language—as an essential part."[21] As far as we can judge, there is as yet no such theory, and we cannot blame Chomsky for the erroneous way his theoretical premises have been treated. However, his disciples sometimes blame him for his position that "generation starts from nothing."

It is true that the "nucleus" phrase has no significant interpretation, so there is no meaning that can be converted into more complex structures. Chomsky, however, has never pretended to suggest a theory dealing with the semantic interpretation of the symbols used, so that there is a striking contradiction between his objectives and current experimental research on the development of generative grammar. When a psycholinguist finds that there is no psychological theory valid for verbal behavior, he is tempted to use the best (or most recent) linguistic theory available. Moreover, since good theories do not abound, we are delighted to have a completely new one, even though it may turn out to be unsuitable. Then it suffices to go one step further to accept the "engendered" as a true mental process, "deep structure" as something like an engram written in our minds, and so on. Starting from this point, many "experiments" could be set up with the view of testing theoretical assertions that have no basis. Here we have a phenomenon that presents a contrast with the picture we gave of developments in research in speech perception, where it is the problem that takes precedence: methods and techniques follow. In the work in psycholinguistics we have mentioned, the means to use are somewhat illusory, so that methods take precedence and the problems emerge later. Experimentation resulting from such a distorted relationship between means and ends can only be futile. Then what explanation can be given for the fact that psycholinguists have tried to test by experiment completely deductive theories, having thus no rapport with the experimental approach? Another question arises, connected with Weimer's statement: why in the field of verbal

[21] N. Chomsky, "The Formal Nature of Language," *Biological Foundations of Language*, Appendix A, ed. by E. Lenneberg, New York, Wiley, 1967.

behavior has "linguistics led the way and psychology followed meekly behind"? (Weimer). This is a question to which we cannot give a brief answer: it requires a long and detailed discussion.

Let us be satisfied with some remarks that come within the scope of this article. In my opinion, the situation may be associated with certain characteristics of modern psychology, which seems to be a less mature science than linguistics. Mini-theories and mini-paradigms are legion and provoke a strange sentiment that could be called professional insecurity. This usually creates, as with Kuhn, the transformation of the existing paradigm into another, totally new one. We can see this in many attempts to revise the essential premises of psychological research. Curiously enough, and long before the word "cybernetics" came into use, psychologists had been using the black box system without realizing they had been "speaking in prose." It was the mathematicians and physicists, not the psychologists, who drew attention to the shortcomings of a purely functional approach.

A decade ago the Soviet physicist M.M. Bongard[22] pointed out, in discussing the human ways of recognizing patterns, that the solution to ill-defined problems by a human is based on principles that are radically different from those used by an automaton. That is, the human subject does not arrive at information by resorting to exhaustive descriptions of perceived stimuli but by using descriptions that are in some way "degenerate," that retain only the distinctive features of the stimuli. This means, in essence, that our faith in the omnipotence of the analytical approach should be counterbalanced by teleological considerations: behavior is always deliberated and efficacious distinctive features should be used with a definite end in mind.

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Rather than drawing a conclusion, I should like to draw the reader's attention to a new and promising approach to the study of speech as a phenomenon of the mental activity of the speaker: the study of communication as a game or contest of thinking individuals (a reflexive game). When a scholar studies the verbal

[22] M.M. Bongard, *Problema ouznavanija*, Moscow, Fizmatgiz, 1967.

behavior of a person, he is necessarily forced into some sort of association with that person.[23] The relationship between an investigator and a human subject is very special, because it is not a subject-object relationship but a subject-subject one. Now the subject under investigation may very well be an individual as highly organized cerebrally as the investigator himself. He may, for example, be able to work out a theory concerning his own behavior, act accordingly and impose on the investigator the corresponding representation, that is, a theory. Within the limits of a reflexive approach, we see that it is not rare that the sagacity of the investigator cannot overcome the complexity of the object. If we keep this in mind, we will be able to attack more easily other spheres that are as yet unknown. This could be the case for understanding the phenomenon of human speech.

[23] See A.A. Toom, "Nesimmetritcheskaja kommunikatsija, focalisatsija i ynpavlenije v igrakh," in *Semiotika i informatika*, No. 7, 1976, pp. 112-127.

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