C E N T E R F O R R E S E A R C H I N L A N G U A G E

September 1987 **Vol. 1, No. 7**

The monthly newsletter of the Center for Research in Language, University of California, San Diego, La Jolla CA 92093. *(619) 534-2536*; electronic mail: *crl @ amos.ling.ucsd.edu*

• • •

CONTENTS

EDITOR'S NOTE

This is the 7th newsletter of the **CENTER FOR RESEARCH IN LANGUAGE**, a research center at the University of California, San Diego, which unites the efforts of researchers in such disciplines as Linguistics, Psychology, Computer Science, Artificial Intelligence, Communication, Sociology, and Philosophy, all of whom share an interest in language. We regularly feature papers relevant to language and cognition (1 - 10 pages, sent via email) and welcome response from friends and colleagues at UCSD as well as other institutions. Please forward correspondence to

Teenie Matlock, Editor
Center for Research in Language, C-008
University of California, San Diego 92093
Telephone: (619) 534-2536
Email: *crl @ amos.ling.ucsd.edu*

SUBSCRIPTION INFORMATION

If you are currently receiving a hardcopy of the newsletter only and have an email address to which the newsletter can be sent, please forward that information to CRL.

Can we send you the email version only to save printing and mailing costs? If you require a hardcopy in addition, please request it and we will be happy to send you one.

If you require the unformatted nroff version of the newsletter, please request it from CRL after you have received the current regular formatted version.

If you know of others who would be interested in receiving the newsletter, please forward the email or postal mailing address to CRL. Thank you.

BACK ISSUES

Back issues of this newsletter are available from **CRL** in hard copy as well as soft copy form. Papers featured in previous issues include the following:

The Cognitive Perspective
Ronald W. Langacker
Department of Linguistics, UCSD
no. 3, vol. 1, March 1987

Toward Connectionist Semantics
Garrison W. Cottrell
Institute for Cognitive Science, UCSD
no. 4, vol. 1, May 1987

Dimensions of Ambiguity
Peter Norvig
Computer Science, UC Berkeley
no. 6, vol. 1, July 1987

Where is Chomsky's Bottleneck?
S. Y. Kuroda
Department of Linguistics, UCSD
no. 7, vol. 1, September 1987
(2nd printing of paper in no. 5, vol. 1)

****

Recent Dissertations

Michael Smith
Department of Linguistics, UCSD
Title: *The Semantics of Dative and Accusative in German: An Investigation in Cognitive Grammar*

JOB ANNOUNCEMENT

The Department of Linguistics at the University of California, San Diego seeks to fill a tenure-track Assistant Professor position, beginning September 1988. The candidate should be someone who approaches the study of meaning and grammar from a cognitive and/or functional perspective. Extensive experience with one or more non-Indo-European languages is desirable. Annual salary is $29,800-$37,200. The Ph.D. in linguistics is required. Send letter of application, curriculum vitae, names of 3 referees, and 1 representative publication, to:

Cognitive Search Committee
Department of Linguistics, C-008-C
University of California, San Diego
La Jolla, CA 92093

Application materials must be received no later than December 1, 1987. The University of California is an equal opportunity, affirmative action employer.

The Department of Linguistics at the University of California, San Diego seeks to fill a tenure-track Assistant Professor position in the area of syntactic theory, beginning September 1988. Annual salary is $29,800-$37,200. The Ph.D. in linguistics is required. Send letter of application, curriculum vitae, names of 3 referees, and 1 representative publication, to:

Syntax Search Committee
Department of Linguistics, C-008-C
University of California, San Diego
La Jolla, CA 92093

Application materials must be received no later than December 1, 1987. The University of California is an equal opportunity, affirmative action employer.

COGNITIVE SCIENCE

University of California, San Diego

The University of California, San Diego is considering the establishment of a Department of Cognitive Science and is seeking candidates for tenured or tenure-track positions at the Assistant Professor, Associate Professor, and Professor levels.

The Department will take a broadly-based approach to the study of cognition. It will be concerned with the neurological basis of cognition, individual cognition, cognition in social groups, and machine intelligence. It will incorporate methods and theories from a wide variety of disciplines including Anthropology, Computer Science, Linguistics, Neuroscience, Philosophy, Psychology, and Sociology.

We intend to develop a new curriculum for both undergraduate and graduate students and applicants should be interested in participating in the construction of new approaches to the study and teaching of cognition. We seek people whose interests cut across conventional disciplines. Interests in theory, computational modeling (especially PDP), application, and education are encouraged.

Candidates should send a vita, reprints, a short letter describing their background and interests, and names of at least three references to:

Search Committee
Cognitive Science, C-015-u
University of California, San Diego
La Jolla, CA 92093

Applications received prior to January 15 will be given the fullest consideration, however applications will be accepted until all positions are filled. Rank and salary will be commensurate with experience and qualifications, and will be based upon UC pay schedules.

Women and minorities are especially encouraged to apply. The University of California, San Diego is an Affirmative Action/Equal Opportunity Employer.

Editor's note: Due to errors which arose when retyping and formatting *Where is Chomsky's Bottleneck?*, by S.-Y. Kuroda (no. 5, vol. 1; June, 1987), we have decided to reprint the paper. Printed below in its corrected form, the paper was written in response to *The Cognitive Perspective*, by Ronald W. Langacker (no. 3, vol. 1; March, 1987). Copies of that paper are available upon request.

****

Where is Chomsky's Bottleneck?

S.-Y. Kuroda
Department of Linguistics
University of California at San Diego

In a recent issue of the **CRL Newsletter** Ronald Langacker informed us of the emergence of an alternative perspective to what he calls *the generative view*. Langacker begins by noting that "for the past quarter century, theoretically oriented linguistic research has been dominated by transformational grammar and its descendents." He then continues to state that "it is not grossly unfair to speak of a *generative grammar world view* despite," as he notes "the diversity of theory and opinion within this tradition." The emerging alternative is "the cognitive perspective." This perspective "[implies] a far more immediate and intimate connection between linguistic investigation and specific developments in other branches of cognitive science than is suggested by the generative world view." After referring to a by now large body of findings within the cognitive perspective, he concludes, somewhat modestly, that "[his] point is simply to indicate that the most basic issues concerning the nature of grammatical structure and its proper analysis are far from being resolved, and that a conception of grammar differing radically and fundamentally from generative orthodoxy may nevertheless prove capable of offering a coherent view of linguistic organization and achieving significant theoretical and descriptive results."

Langacker then goes on to state that "supporting this assessment are developments in related disciplines." And "rather than listing .... trends congenial to the cognitive perspective in linguistics, [he] will focus on one particular development that merits the label *revolutionary* by virtue of its fundamental character and sweeping implications." That is **connectionism**.

Connectionism is, of course, now a familiar term, referring to a new approach in computer technology and computer modeling of cognitive processes, expanding its influence over cognitive sciences. Until the advent of connectionism, computer technology had been solely built on the **von Neumann architecture**, which depends on a rapidly executed series of computations. In contrast, connectionist models involve massive arrays of weighted *connections* which allow parallel, rather than serial, processing of information. As Langacker notes, connectionist models are "neurally inspired." Not only has the neurological description of the brain supplied "guiding metaphor" to connectionism, discoveries in neurology point more and more to the neurological reality of connectionist models.

It is reassuring to be informed that "**cognitive grammar** (at least [Langacker's] own formulation of it) is basically compatible with the connectionist philosophy." Thus, "rules and their instantiations make no distinction"; "analyses are based on the overt form of expressions"; "derivation from abstract 'underlying' representations is precluded, as is any sort of algorithmic computation"; " a linguistic system is viewed simply as an inventory of *cognitive routines*, ...recurrent patterns of activation that are easily elicited by virtue of connection weights"; "the construction of complex expressions reduces to the co-activation of appropriate routines and *relaxation* into a pattern of activation that simultaneously satisfies all constraints"; etc.

What particularly strikes us as remarkable, however, is Langacker's finding that "linguistic theory in the generative tradition presupposes the von Neumann architecture, accepting without question the need for discrete and explicit rules couched in some 'propositional' format, and which constitute an algorithm specifying the sequential manipulation of abstract strings of symbols." What has appeared to us as an epochal confrontation of the two alternatives, the *generative world view* and the *cognitive perspective* is, in Langacker's mind, nothing less than a manifestation of the contrast between the von Neumann architecture and connectionism.

Recall that the "generative tradition" in Langacker's characterization is "transformational grammar and its descendents." It would not seem grossly unfair to assume, then, that in Langacker's view, transformational grammar presupposes the von Neumann architecture, which is then inherited by its descendents.

Now, it is said that the von Neumann architecture is characterized by a *bottleneck,* von Neumann's bottleneck. This is of course an impressionistic but nonetheless valid allusion to the fact that all information fed to and processed by a computer of the von Neumann architecture has to go through its unique central processing unit serially, thus constraining the capacity of the computer. But where is there a bottleneck in transformational grammar? I cannot find one. Where is Chomsky's bottleneck?

Chomsky's bottleneck does not exist in transformational grammar, though, not, of course, because it is a connectionist model. Chomsky's bottleneck does not exist because transformational grammar (or, put it in a more general form, the concept of grammar presupposed by transformational grammar) is not an information processing device, either of the von Neumann architecture or of the connectionist architecture. Chomsky's bottleneck cannot exist, simply because the notion of such a bottleneck implies a category mistake.

Language behavior implies information processing, but transformational grammar is not a model of an information processing mechanism. Instead, it is a theory (of knowledge) that characterizes (or, at least, partially, determines) the type of information that is to be processed in language behavior, the very type of information the processing of which makes language behavior language behavior. Grammar itself does not say anything about how the processing of information is done.

At this point one might say that I am misrepresenting Langacker. What he meant, one might say, is not that transformational grammar has the von Neumann architecture; rather, he meant that what transformational grammar characterizes as forms of information presupposes the von Neumann architecture of a processing mechanism, whatever that might be. His expression, "linguistic theory in the generative tradition presupposes the von Neumann architecture" taken by itself indeed might leave us with some uncertainty as to how it is to be interpreted. I doubt, though, that that is what Langacker meant. The above quote is directly followed by the participial adjunct "accepting without question the need for discrete and explicit rules couched in some 'propositional' format, and which constitute an algorithm specifying the sequential manipulation of abstract strings of symbols." He does not mention any processing device (e.g. parsing mechanism) that might be developed "in the generative tradition"; and rules as conceived by transformational grammar (if not of all its descendents) might indeed be said to be "discrete and explicit, couched in 'propositional format'." It is hard, given the context, not to interpret Langacker otherwise than as attributing the relative clause that modifies "rules" in the above quote, ...i.e. "which constitute an algorithm specifying the sequential manipulation of abstract strings," ... to rules in transformational grammar. But rules in transformational grammar are rules of grammar, not of processing operations. Langacker does not distinguish transformational grammar from a processing mechanism for it, whatever that might be, and attributes the von Neumann architecture to grammar. He straightforwardly attributes the von Neumann architecture to transformational grammar on the basis of the form of rules which it assumes, thus committing a category mistake.

To repeat, I doubt that Langacker's intended point is that a processing mechanism must necessarily have the von Neumann architecture if information it is built to process is given in the form of "abstract strings of symbols" whose well-formedness is determined by "sequential manipulations" "[specified by] an algorithm" with "discrete and explicit rules couched in some propositional format." This may be true, but if so it must be shown to be true.

In fact, if we consider Langacker's other words, it is doubtful that this thesis is true in the form Langacker presents. For elsewhere in the paper Langacker mentions the learning of English past-tense verb forms as an example of a connectionist model applied "with considerable success." Since no information is otherwise given, it is hard to imagine that English past-tense forms were given to this model in any way other than in the form of "abstract strings of symbols" whose forms can be characterized by some discrete and explicit "propositional format". (Even if all the irregular verbs are simply listed in the model, it fits this specification.) Indeed, if not, it is intriguing how connectionists confirmed that they achieved considerable success. Perhaps they listened to a speech synthesizer. But they could have easily tested their model with printout forms. But how connectionists achieved their task shouldn't really concern us here. The point is, characterizing (or, learning) English past-tense forms is of course a simple matter. A system of knowledge that can be described in abstract strings of symbols in discrete and explicit propositional format, if it is of a simple type, can be handled by connectionist models, demonstrating their considerable success, but if the required 'propositional format' is as complex as transformational grammar implies, the von Neumann architecture must be presupposed and hence is beyond the reach of connectionist models. If this is the way Langacker wants to say it is today in the world of connectionism, I wonder what message he wants to convey about the bright prospects of the connectionist perspective by informing us of it. Be that as it may, we then have here an empirical question to be answered.

But, I think this is besides the point. For, as I indicated, Langacker assumes that derivations in terms of the application of grammatical rules are involved in actual real-time derivation of sentences in the generative perspective. So interpreted, derivations constitute serial computation, which presupposes the von Neumann architecture. But he assumes what adherents of tranformational grammar do not. It is not unfair to say, then that attributing the von Neumann architecture to transformational grammar is not even an unproven empirical issue, nor is it even false; it commits a category mistake.

Not everyone in the generative tradition, of course, shares the assumption of transformational grammar on this point. In fact, as is well known, there has been growing concern about and dissatisfaction with the stand transformational grammar takes on this issue, and a major contention of some influential descendents of transformational grammar against transformational grammar is precisely about this issue. They subscribe to the concept of grammar according to which grammatical rules are (more) directly involved in processing, under the name of *psychologically real grammar*. Thus, if the statements of Langacker's that we examined in the preceding couple of paragraphs are meant to apply only to this trend in the generative tradition, then he could not be charged with a category mistake. He is making a claim on an empirical issue without substantiation, insofar as the claim can be considered well-formed.

It might be that many, or even most, influential schools within the generative tradition, "descendents of transformational grammar" by Langacker's definition, subscribe to psychologically real grammar. To attribute a common feature of major descendents to their ancestor may be a logic permissible in historical linguistics, but not in the history of linguistics. Besides, transformational grammar is not just a dead "proto-school" of the generative tradition, but a leading contender among descendents. What Langacker wants to depict is a false picture of transformational grammar, and hence of the generative tradition in general. It is nonetheless another matter if one wishes to claim that connectionism obliterates the basic idea of what grammar is held by generative tradition, or at least of the generative tradition that remains basically faithful to the original Chomskyan idea of the study of grammar, if not to the development of the particular execution of it by the Chomskyan school in the narrow sense. According to Chomsky's conception of linguistics, linguistic theory is a study that endeavors to determine the form of knowledge that is put to use in linguistic behavior. The study of grammar, in this view, in some methodological sense precedes that of linguistic performance; the latter is crucially dependent on the former. We would be able to succeed in understanding linguistic behavior only to the extent that we understand grammar,the form of knowledge that is involved in linguistic behavior that characterizes it as linguistic behavior. Holding this view is different and more fundamental than maintaining that transformational grammar, or a particular version of it, is the right theory of grammar. The emergence of connectionism might put an end to this view. Associating transformational grammar with the von Neumann architecture may easily and rightly be dismissed as a category mistake, but what is at stake may be the very presupposition that legitimizes this charge of a category mistake against Langacker, that legitimizes the transcendence of grammar over the modelling of mental processing. The von Neumann architecture may well not be attributed to transformational grammar, but this countercharge may well be itself senseless if the conception of grammar as a form of knowledge does not make sense. To quote Langacker again, "crucially ... a connectionist model makes no use whatever of explicit rules, nor is the information responsible for 'rule-governed' behavior separately or discretely represented. All the system's 'knowledge' lies distributed in connection weights, which collectively determine what coalitions of units are likely to participate in stable patterns of activation. There is no difference in this regard between general and specific knowledge, i.e., no qualitative distinction between rules and their instantiations." The apparent confusion by Langacker between rules as elements of grammar as conceived by transformational grammar and rules (instructions) in computational processes with the von Neumann architecture makes the exact interpretation of this statement difficult, but it may well be taken as a (implied) refutation of any position that acknowledges any role of 'knowledge' in linguistic theory other than, if anything, that which "lies distributed in connection weights." The connectionist research program and its achievements have obliterated the foundation of the generative tradition, or at least of the view transformational grammar subscribes to. This may be taken as an ultimate message of Langacker even if he may have been confused in his exposition due to transformationalists' perhaps not altogether particularly happy choice of terminology.

In this regard an illustrative example that Paul Churchland exhibits in his exposition of advances in cognitive neurobiology ("Some Reductive Strategies in Cognitive Neurobiology," *Mind* 1986, pp. 279-309) strikes me as particularly instructive. He does not refer to connectionism as such, but the "powerful conception of representation and computation -- drawn from recent work in the neurosciences -- " that he outlines in the article provides a neurological basis of connectionist models. An examination of Churchland's example would help us determine the significance of the connectionist perspective. Churchland constructs "a crablike schematic creature" (let us call it *chchl*). The chchl has two eyes and one arm. Churchland simulates its behavior of grasping with its arm an edible object caught in its eyesight. The angles with which the two eyes are focused on the target give the information to the chchl as to where the object is located. The arm has two joints, at the shoulder and at the elbow. The angles with which the two sections of the arm are bent determine the location of the hand, with which the chchl catches the target. The great cognitive question for the chchl is how to coordinate its arm's movement with that of its eyes. The solution turns out to be quite simple. The chchl's brain is provided with two topographic maps, one for the eyes and the other for the arm. Each may be considered as a two-dimensional manifold, a network spanned by two coordinates. A point on each of the maps corresponds to a definite point in the outer space in front of the chchl that is to be located relative to its present position. The point is determined by two coordinates, i.e. for the eye map, the two eyes' angles, (**a**, **b**), and for the arm map the angles with which the arm is bent at the two joints, (**s**, **t**). Now, a network of neurons is spanned between these two maps, so that points corresponding to the same physical locations on the two maps are connected. When a point (**a**, **b**) on the eye map is stimulated by the movement of the eyes, this information is immediately sent to the corresponding point (**s**, **t**) on the arm map through a neuron directly connecting these two points, and the arm is extended with the two joints bent exactly by degrees, **s** and **t**, determined by the point stimulated on the arm map. This action of the arm puts the hand exactly at the point where the eye is focused on the target object and the hand catches the target.

This neurological device might be described very simply in mathematical terms. The outer space in front of the chchl is represented in two different coordinate systems, one by two parameters conforming to the eye movement, and the other by two parameters determined by the arm anatomy. They each identify locations in the vicinity of the chchl as points in a two-dimensional manifold. The network of connections between the two topographical maps corresponds to the transformation of one coordinate system to another.

Assume this is a finding of neurosciences. What message are we to get from such an exciting new development? Finding a massive network of connection in the chchl's brain, we understand the significance of it because we understand it as an actualization of a coordinate transformation of a two-dimensional manifold. It might be rather surprising to find that the chchl 'knows' that the environment it deals with in his predatory behavior is a two-dimensional manifold that can be represented by different coordinate systems conforming to its anatomy of the eyes and the arm and that a coordinate transformation from one coordinate system to another can be established in order to identify the same points represented in the two systems. But that is what the chchl achieves to do. (And in fact observing the chchl's predatory behavior and its anatomy what else could we have expected to find after all?) It is neurologically confirmed that a particular type of knowledge chchl deals with in its "cognitive process" can be described as coordinate representations of a two-dimensional manifold and a coordinate transformation.

If we do not want to, let us refrain from attributing any "knowledge" to the chchl at this moment. But certainly we have to attribute the necessary knowledge to us, who claim to understand what we have found. If we do not understand what the chchl does we do not understand what it does; we would only be able to marvel at it. We, educated as we are, take our mathematical knowledge of space so for granted that we may not realize that we understand it because we do (while it would be obvious that we don't understand transformational grammar if we don't.) It might be difficult to imagine how it is not to know what space is. But imagine that for some miraculous reason or another a cave man in the stone age discovered a massive network of connections in chchl's brain and understood how it functioned to the extent he could, that is, he understood that when the chchl saw a target and grasped it one part of the network "lighted" and then another part linked by a connection did. What sense would this finding have made to the cave man? He did not have any idea of dimension that our mathematically objectivized knowledge of space involves.

He might have shrugged his shoulders, pushed the chchl aside and resumed his unfinished work of wall painting. He might have been of somewhat more curious character, might have succeeded in determining that there was a causal relation between the chchl's eyes' and arm's movements and might have speculated that the shifts of "lighting" in the network was somehow responsible for their relation. But a mind of a scientific genius and a lifetime of solitary devotion to inquiry might not be sufficient for a cave man to grasp what we immediately understand about the network of connections inside the chchl's brain. Two pieces of information transmitted from the eyes, **a** and **b** are first put together to form a point, (**a**, **b**), on the eye map, and then after this information is transformed into a point on the arm map, (**s**, **t**), by a neuron connection, it is again broken into two pieces of information, **s** and **t**, each being transmitted to appropriate motor nerves. After all, we have advantage of having behind us the history of geometry from the unrecorded activities of Mesopotamian surveyors through Euclid to Descartes which we take for granted without remembering. The reason why the cave man can at best only marvel at while we understand the "cognitive process" of the chchl involved in its predatory activity should now be obvious. We know ourselves what we assume the chchl to "know" for it to engage itself in its "cognitive process." We have here of course an "equivocation" of "know". The chchl does not have to know what it knows in the sense we know it to understand it.

Whether we attribute "knowledge" of space to the chchl is a terminological question that should not concern us here. The point is that we do not understand what the massive system of connections is about unless we understand the structural characteristic of the information, i.e., the cognitive objects, the chchl's cognitive process deals with.

Langacker mentions *tensor network theory*, and he assures us that a relation between it and linguistic theory is not at all fanciful, referring to Patricia Churchland: "Churchland thus envisages the integration of neurosciences with theories of mind and cognitive processing. As examples, she cites not only connectionism, but also 'tensor network theory', which offers a way of making sense--in functional terms--of some important discoveries by neuroscientists... Tensor network theory hypothesizes that topographic maps and relation-preserving interconnections solve a fundamental functional problem for the brain: to coordinate processes involving different representational systems ... the interconnections effect a kind of matrix multiplication, whereby representations in one array of neurons are related systematically to representations in another." The key word in the above quote is *relation-preserving*. A tensor, to quote Langacker, "is a mathematical function transforming one vector into another." But it is not an arbitrary function. It is relation-preserving. We would not understand the significance of a tensor network unless we understand ourselves what relations are preserved, what the "geometry of spaces" is which tensor networks relate, that is, unless we have brought to our knowledge, in one sense of the term, what the organism knows in another sense of the term.

We should, of course, not be tied too much to the ideas of "tensor" and transformations of space. A tensor network theory would simply represent one stage in the development of neuroscience. Neuron networks of different characters (for which the term "tensor" may not apply literally or would not be appropriate metaphor) will certainly be found in the future. But the general point is valid: we do not understand the significance of a network of connections that executes an organism's "cognitive process" unless we understand the nature of "knowledge" that cognitive process deals with. If "all the systems' 'knowledge'" that we are to find "lies distributed in connection weights," if all we understand is massive connections and connections weights, understanding a cognitive process would be like trying to disentangle connections of branches and roots of a banyan tree. If there is nothing that we understand but a mass of weighted connections we are like a cave man looking at the chchl's brain, not understanding what the chchl knows and how it does what it does. Paul Churchland's chchl example is artificially constructed for an expository purpose, but (or, rather, and) serves its purpose rather well. Its only pitfall is that we take our knowledge of space, in both senses of the term, so for granted that we may end up with seeing nothing but neuron connections, like a cave man, and forget the chchl's "knowledge" of space, which underlies the network of connections and determines its significance. We may thus be led to misinterpret the real significance of the advance being made by neuroscience.

To sum up, the point is that if we do not understand a cognitive structure (the structure of a relevant type of knowledge), we do not understand the cognitive process that deals with it. This is a methodological point. It is another matter, in principle, how we attain our understanding. Here the recent rapid advance in neuroscience becomes relevant. Until recently one could only speculate on neurological implementations of cognitive processing on the basis of understanding or hypotheses on cognitive structures. But now biologists directly uncover neurological architectures for cognitive processes. It is in principle possible that neurological studies can lead our inquiry into cognitive processes; we might be able to determine the nature of cognitive structures from the observation of networks of weighted synoptic connections. But that does not mean that the study of cognitive faculties is reduced to the study of networks of weighted connections. One has to understand what is reduced to the working of networks of weighted connections. In any event I suspect that advances in neurology, impressive though they are, would for the moment help us speculate more, rather than provide any hard factual information on the neurological architecture of higher cognitive faculties of the human being such as language and reasoning.

The objective of linguistic theory is to construct a theory of grammar, a cognitive structure that underlies our linguistic acts, to make our tacit knowledge of language an object of our rational understanding. So far we have not gotten and probably still for some time we would not get much substantive direct help from neurosciences, possibly except for certain areas related to phonetics. Theoretical linguistics will continue, as it has always, to be a speculative science ultimately based on data provided by native speaker's intuitions.

Transformational grammar is an attempt at such a theory. It goes without saying that nothing I have said above argues for its validity as a theory, or its superiority over other theories. What concerns us is a proper understanding of its general objective, which is perhaps shared by many of its descendents, i.e., the generative tradition, according to Langacker. This objective is independent of the controversy over the von Neumann architecture and connectionism in computer technology and artificial intelligence. The processing mechanism for linguistic performance as it is actualized in, no doubt, massive neurological networks in the human brain, if and when we can determine it, may well look like a connectionist model; it would be most unlikely to look like a digital computer with the von Neumann architecture. The real issue is whether or not the information it is built to deal with is so structured as to lend itself to human rational comprehension, at least to a substantial degree. It is a priori possible, of course, that our faculty of language is beyond our rational comprehension. If one is convinced of the impossibility of a rational comprehension of language, there would be no point of pursuing it.

Even though we are not concerned with the question of which particular theory is right, a remark on the concept of the right formal representation may be in order here. Recall our earlier example of the chchl drawn from Paul Churchland's article. As Churchland points out, what is involved in this illustrative example is a coordinate transformation of a two-dimensional space, using the notation introduced above, a transformation from a coordinate representation (**a**, **b**) to another (**s**, **t**). But what are these coordinates? One can, as Churchland does for expository purposes, describe the coordinate transformation in question in terms of real numbers. But one would be misguided if one takes such a description seriously and attributes the knowledge of real numbers to the chchl. The relevant point about the chchl's cognitive structure is that the coordinates are drawn from linearly ordered structures, two independently determined parameters are paired to represent a point (a coordinate representation of a point), this coordinate representation is mapped to another point, which also lends itself to a coordinate representation and hence is then split into two separate parameters, thus resulting in effect in a coordinate transformation. One would have a better description if one simply says that coordinates are drawn from a linearly ordered set rather than real numbers. Such "simpler" mathematical concepts as linearly ordered set, however, are more abstract than the concept of real number, and have made their way into our basic scientific vocabulary only relatively recently. They were not available, for example, to the contemporaries of Descartes. They could only have described the chchl's knowledge of space in terms of real numbers. Nonetheless, they can be said to have the right understanding of the chchl in comparison with the cave man who would have only observed a massive network of connections. (In fact, the concept of set as an entity consisting of elements might not be abstract enough to achieve the most felicitous formalization required.) Similarly, even if transformational grammar is a correct theory in a certain substantial sense, it is likely that we do not yet have the right mathematics for the most felicitous presentation of the theory. It might take some generations before more adequate but perhaps more abstract mathematical concepts than now available are formed and enter into the basic scientific vocabulary.

Grammar determines the form of linguistic knowledge. It leaves aside the problem of how information conforming to this particular form of knowledge is processed in real-time mental behavior involved in language behavior. It might be conceded, however, that there are good reasons why rules in transformational grammar are misunderstood to be those involved in real-time psychological processes, in particular in Langacker's perspective. Let us recall that the so-called standard theory of transformational grammar in the mid sixties assumed that the deep structure of a sentence determines its semantic representation and that the phonetic representation of a sentence is determined by its surface structure which was derived from the deep structure by successive application of grammatical transformations. This idea of the standard theory gave rise to generative semantics, which claimed that the initial structures to which transformations apply in order to derive phonetic representations of sentences are nothing but semantic representations. This conception of the organization of grammar fits quite well with the age-old and plausible idea that language is a symbolic process of expressing thought in sound. It might be quite natural that transformational rules in the standard theory, and in particular in generative semantics, are taken as more precise formulations of computational processes involved in real-time mental activity of expressing thought in sound. From what little I know of Langacker's cognitive grammar it would seem to me not grossly unfair to say that it is an extension of this line of development, a distinguishing characteristic being that the parallel processing of a connectionist model is envisioned as the psychological process converting meaning to sound involved in language behavior. Language is the actual activity of symbolization in phonetic content (or whatever medium) of semantic content. The biographical record of Langacker in fact traces this track of development from the standard theory through generative semantics to what he now calls cognitive grammar.

While the standard theory of transformational grammar developed into generative semantics and then to cognitive grammar on the one hand, transformational grammar itself, on the other hand, has undergone substantive innovations, and it may now be difficult to imagine how cognitive grammar and government and binding theory have both descended from the standard theory of transformational grammar in actual history, though, apparently, the former has not, according to Langacker's idealized definition of "its descendants" (*it* = transformational grammar). It is interesting to note at this juncture that Chomsky expressed the opinion that the ideal history of transformational grammar should have started from generative semantics. (See Frederick Newmeyer, *Linguistic Theory in America*.) Chomsky's real intent of making such a remark is not totally clear. Nonetheless if in the development of grammatical theory as Chomsky conceives it, the emphasis was to shift from the conception of derivation to the form of sentences (syntactic representations) determined by constraints, a path from the standard theory to generative semantics in the actual history could look like tracing backward what should have been. We might now wish to trace this ideal history of Chomsky further back to its ultimate conceptual origin. Or is it in real history that the clock is moving backwards? If so, that would be a counter-revolution.