An Interview with George Lakoff

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BIOGRAPHICAL NOTE

George Lakoff teaches linguistics and cognitive science at the University of California, Berkeley. Together with such figures as James McCawley, Paul Postal, and Haj Ross, he was one of the founders of the Generative Semantics movement in the 1960's. This represented the earliest major departure from the standard Chomskyan Generative Transformational Grammar by demonstrating the fundamental role played by semantics and pragmatics in grammar. Since the mid-1970's Lakoff has become a leading figure in the development of Cognitive Linguistics, which grounds linguistic theory in empirical findings from the field of Cognitive Psychology. He has studied conceptual systems in depth, although the bulk of his most recent research has increasingly focused on metaphor and its consequences for abstract reasoning.

He is the author of ***Women, Fire, and Dangerous Things,*** and co-author of ***Metaphors We Live By*** (with Mark Johnson), and ***More Than Cool Reason*** (with Mark Turner). His most recent book, ***Moral Politics: What Conservatives Know That Liberals Don't***, analyses the language of political discourse and demonstrates that conservative and liberal ideas are based on opposite metaphorical models of the family and morality. At present, he is writing ***Philosophy in the Flesh,*** in collaboration with Mark Johnson, which interprets philosophy from the point of view of cognitive science. He is also working, with Rafael Núñez, on the metaphorical structure of mathematics.

INTERVIEW'

Good afternoon, Professor Lakoff. First of all, I'd like to go back to rhe late 1960's and early 1970's. This is the penod of Generative Sernantics for you and a number of other people, like Paul Postal, Haj Ross, and James McCawley, who had been studying wirh

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Chomsky. What are your recollections from that period? What was going on?

A lot was going on. Generative Semantics started forme as early as 1963. At the time I was just a first year graduate student and I had previously done work on the structure of discourse. I had done an undergraduate thesis which was the first story grammar. I had tried to put a narrative structure into a transformational framework, and in doing so, it occurred to me that one could possibly generate whole stories, starting with plot structure and working down to sentences.

But in order to do that I had to have a semantic base for the sentences, since the plot structure I proposed was given in semantic terms. At that time semantics meant logic, mainiy the traditional formal logic. And so I asked if it was possible to have logical forms be underlying structures in a transformational grammar. I figured out how to restructure logical frorns so that they would fit transfromational rules. Technically, it might be possible. The question was whether there was any independent empirical evidence from syntax that this was the right thing and to do. I set out to get such evidence.

I conceived the following idea. Look at the properties of logical forms. There are seven basic properties:

(1) predicate-argument structure;
(2) scope differences;
(3) co-reference, that is, identical variables;
(4) binding of variables by quantifiers;
(5) propositional function structure (where one or more variable is unfilled);
(6) inferences, and
(7) sublexical structure.

The question I asked back in 1963 was: Do these properties of logical forms, rnost of them structural (except for inferences), show up in grammar? Then I went looking for evidence.

The first evidence I noticed concerned prelexical structure. I looked at causative sentences like, *He warmed the milk,* where you have something like "he caused the milk to become warm". I argued that, in order to state the proper generalizations about (1) the distribution of the word "warm" and (2) the relationship between the causative, the inchoative, and the stative constructions, one would have to posit underlying structures, that is, logical forms, in which there was an abstract causative predicate and an abstract inchoative predicate. Such analyses have now become commonplace.

I also looked at some sentences from baseball -1 was an avid baseball fan at the time. I still am. The sentence I chose was about a Boston Red Sox player named Carl Yastremsky (who was a young player back in 1963, but now is in the Hall Of Fame). The example was: *Yastremsky doubled off the wall,* where what went "off the wall" was the ball. The generalization about where directional phrases like "off the wall" occur depends on looking at the meaning of the verb "doubled." You have to know that "double" means to hit a ball so that you run for two bases. "Off the wall" expresses where the ball goes; it requires a therne that is a moving entity. Its syntactic occurrence, therefore, depends in part on the sernantics of the verb in the sentence it occurs in.

One could not say, for example, **Yastremsky slept off the wall,* because there would be

nothing moving off the wall. Similarly, in *Yastremsky **doubled to lefrfield,*** the ball went *to **lefrfield.*** You can't say ***Yastremski stood to left field*** because there is nothing going to left field. I argued that, to state the generalizations governing the distribution of such adverbs, one had to postulate abstract underlying logical forms that represented meanings.

That was in 1963. 1 wrote "Toward Generative Semantics" for a surnrner research project where I was working with Gil Harman and Jim McCawley, who were then graduate students. That summer, Harman wrote the first paper showing that phrase structure grammars could do everything transforrnational grammars of a reasonable sort could do (almost 15 years before Gazdar showed the same thing). McCawley was working on phonology. We became friends and I gave him one of the 20 copies I made of the paper. I had met Paul Postal and I also gave him a copy of the paper. And I gave Haj Ross a copy of the paper. Chomsky hated it. He thought the idea of logical form in grammar was ridiculous.

But he later adopted the idea, or didn't he?

He later adopted it, but at first he was dismissive. He thought it was stupid.

Why?

I don't really know. Paul Postal suggested that this was because I suggested this before Chomsky's ***Aspects*** theory -this was '63. Chomsky's ***Aspects*** theory was formulated in '64 and published in '65. Postal knew that the ***Aspects*** theory was being developed and he suggested that, if I waited, then there might be a Chomskyan framework in which to frame my ideas. So I didn't push Generative Semantics right away.

I waited, but meanwhile I looked for more evidence that logical form occurred in grarnmar. McCawley, Ross, Postal, and I found a great deal of evidence. Consider, for example, evidence for propositional functions. We looked at sentences like ***The children are ready to eat and so are the chickens.*** On Chomsky's account at that time, the sentence should be four ways ambiguous; that is, ***the children are ready to eat*** or ***be eaten,*** and ***the chickens are ready to eat*** orbe ***eaten.*** But the sentence is actually only two ways ambiguous. That was explained by Jirn McCawley, who suggested that the sentence was formed not simply by verb phrase deletion; rather, what occurred was the deletion of a verb phrase paired with its propositional function; if you included the semantics of the verb phrase as well as the syntactic form in the rule of verb phrase deletion, then identical propositional functions would be involved. McCawley's proposal was later decisively demostrated by Ivan Sag in his dissertation, in a very different framework, about ten years after that.

Then there were other kinds of examples. For example, in Logical Form, a negative is outside of the proposition it negates. The question I asked was: Are there any sentences that indicate that the underlying syntactic structure of negative sentences has a propositional-external negative in it underlying syntactic stmcture? We found some sentences that indicated that. For example, ***John didn't** marry Mary **although the** fortune teller **hadpredicted ir,*** where the "it" indicates that he ***did*** marry Mary, and has to refer to a proposition inside the scope of the negative. That is, the negative has to be seen as outside the scope of the sentence as you would have it in Logical Form.

That was another bit of evidence that Logical Form could show up in grammar. I looked at tense logics, where the tense is usually outside of the proposition. In tense logic,

a sentence like *John left* contains the proposition "John leaves", which is true in the past. The past tense is an operator outside of "John leaves". We found sentences like the *Republicans won in 1968 but it won't happen in 1972*, where the "it" refers to the "Republicans win", without a tense. That means you have to be able to isolate the constituent "Republicans win" from the tense, which means that the "it" must have as its antecedent a proposition completely inside the scope of the past tense operator, as in tense logic.

Would Chomsky have been able to handle that kind of example?

He never looked at them so far as I know, and he refused to discuss them with us, and to this day I don't know if he has looked at them. He refused to talk to us when we started discussing these examples so I simply don't know if he has thought about them at all.

Following the line of discussion we are having right now, would you like to outline briefly your own evolution from being a Generative Semanticist up to your present work on metaphor?

Well, what happened was this. In 1974 I discovered a class of sentences that showed that there could not be single underlying syntactic structures, whether they were logical forms or not. They were examples that could not be handled by any transformational theory, or any theory with syntactic derivations. Not oniy was Chomskyan transformational grammar inadequate, but Generative Semantics was as well: Generative Semantics could not work because both the logic and transformational grammar weren't adequate.

The sentences were called syntactic amalgams, and let me give you an example of one of two or three types. Take the sentence *John invited you'll never guess how many people to the party*. These sentences were brought to my attention by Haj Ross, who heard them from Avery Andrews when he was a graduate student at MIT. To handle this sentence. Andrews had postulated a very strange transformation. He said that *John invited you'll never guess how many people to the party*, was to be derived transformationally from *You'll never guess how many people John invited to the party*. He observed that the only way you could do it would be to move "John invited to the party" to the front of the sentence and then stick "you'll never guess how many" into the middle in exactly the right spot.

That seemed to me like a very unlikely transformation. But in order to prove that it couldn't work that way you had to show that you could have more than one in a given sentence, because on his analysis you would only be able to have one such higher sentence to put into the middle of the sentence. So I tried constructing sentences with multiple cases. It wasn't hard. I wound up constructing sentences with as many as six in the middle. Here's one with four: *John invited* you'll never guess how many people *to* you can imagine what kind of a party, *for* God knows what reason *on* wasn't it last Saturday?. That sentence has no single Logical Form or underlying structure, and it has no single transformational derivation. The reason is that, if it were to be derived transformationally, the topmost clause in the surface form would have to be embedded in logical form in *all* the other clauses at once. It just cannot be done technically. It occurred to me that you needed a very different notion of grammar, something like contemporary Construction Grammar or Cognitive Grammar, in order to account for sentences like this.

That was the beginning of my interest in construction-based grammars, back in 1974.

Then in 1975 I got a small grant from the National Science Foundation to have a Summer Seminar of eight people on linguistics, logic, and artificial Intelligence. I wanted to see how logic and artificial intelligence entered into the study of grarnrnar and I invited some remarkable people like David Lewis, Lauri Karttunen, Terry Winograd, and so on. The word went out through the linguistics underground that we were having this seminar. Many people asked if they could just come and join, and I said yes, and before I knew it 188 people had decided to come to Berkeley. I hired a graduate student at MIT —Ivan Sag, now a distinguished professor at Stanford— as my research assistant. He set up communes in fraternity houses for anyone who came. He managed to house over 180 linguists here for 6 weeks for what became an underground summer institute. We had no offical courses and no official teachers. What we did was set up a schedule of lectures from 10 in the morning to 10 at night in various rooms around the university, for 6 days a week, with the rule that anyone could lecture with 3 days' notice. Anyone who came and wanted to talk could simply talk. You just had to give us three days' notice to put you on the schedule. There were an enormous number of very interesting lectures that summer and among these lectures where lectures that convinced me that formal logic could not work for semantics at all.

Let me give you some examples of the people who lectured that summer. One of the first was Paul Kay. He presented results that he, Brent Berlin, and Chad McDaniel had gotten on the universals of color terms, and he explained these results on the basis of studies of the neurophysiology of color vision that was being done in the Psychology Department here by Russ DeValois. What Kay and his co-workers had observed in their color research was that the central members of colors were the same all over the world, even though the boundaries of the categories were different. Moreover that there is a hierarchy of colors constraining how new basic color terms get introduced into a language over time. What they showed was that the hierarchy and the centrality of the colors was predictable from the neurophysiology.

When I heard this lecture, I was still a believer in formal logic and model theoretic semantics. But I knew enough about the technical foundations of those fields to understand that this was a counterexample to my enterprise of what I had called "natural logic," the attempt to extend formal logic to deal with natural language semantics. The reason is this:

Model-theoretic semantics characterizes meaning in terms of reference and truth conditions. To get the meaning of a sentence, you have to relate symbols in the Logical Form to things in the world, or some set-theoretical model of the world. The mind and the brain are not in any formal model. Formal models are supposedly set-theoretical models of the external world. The assumption is that meaning is public, that the world has an objective structure, and that all meanings can be given in terms of the objective structure of the external world, with no reference to the mind or brain.

For example, to get the meaning of a sentence like ***The chair is red*** in formal semantics, you have to assume that there is a set of red things objectively out there in the world; the meaning of "red" is given by designating that set. The sentence ***The chair is red*** is true if and only is the object designated by "the chair" is in the set of red things in the objective external world.

What Kay and McDaniel together with DeValois and his co-workers had shown was that there is no such thing as a set of red things in the world. Red is created by the color cones in your retina and by the neural circuitry of your brain given certain conditions in the world. But the category red isn't in the world. What you see as red is not strictly a matter

of wave length. It depends on color conditions and all kinds of other local physical phenomena; given that, the category is created by your eye and your brain by the fact that you have three types of color cones and there are certain kinds of neural circuitry. That means that model-theoretical semantics for natural language cannot work for the meaning of color terms like "red." Since Generative Semantics used model-theoretic semantics, there was something fundamentally wrong with Generative Semantics as well. If logic din't work, Generative Semantics couldn't.

The next week I heard a lecture by Eleanor Rosch. It was one of her first lectures on basic-level categories. She was reporting on her new research. What that research had shown was that certain categories were psychologically basic, like "chair" as opposed to "furniture", or "rocking chair". What she meant by 'psychologically basic' was that "chair" was defined partly by mental images —you can get a mental image of a chair but not of a piece of furniture— and partly by motor programs and body movements. You have motor programs for sitting down in a chair but you have no motor prograrns for interacting generally with a piece of furniture.

What she showed was that this very basic level, which is the level learned first by children, seems to be the optimal level at which people interact with the world with their bodies. That level of categorization, with its special properties, is determined not by anything out in the world but in part by your body and your perceptual system. In short, the conceptual category chair has properties not in the objective external world, but rather determined by the body and brain. Basic-level categories, like color categories, cannot be handled by model theory, because there is no body in model theory. There is no brain or perceptual system in model theory. Again, formal logic didn't work for natural language semantics.

The next important talk I heard in the summer of 1975 was Chuck Fillmore's first talk on Frame Semantics, in which he discussed semantic fields like "buy", "sell", "cost", "price". He showed that their semantic relationships were determined by holistic stmctures, like Schankian scripts or scenarios. A word like "buy" or "sell" presupposes a script in which you have elements like a buyer, a seller, goods and money. At first the seller has the goods and the buyer has the money, the seller wants the money and the buyer wants the goods. In the second part of the scenano, they exchange goods and money, and in the third part the buyer has the goods and the seller has the money.

Once you see that you can see the meaning of the words is defined relative to this kind of stmcture —and Fillmore gave many, many examples in which the meaning of words requires stmctures of this sort. But the structures are not objectively out there in the world. In fact, sometimes there are altemative frame stmctures which are inconsistent with one another.

For instance, the words "thrifty" and "stingy". The word "thrifty" is defined with respect to a frame in which you are concerned about the efficient use of resources, and in that frame the opposite of "thrifty" is "wasteful". A sentence like *He is not thrifty, he is wasteful* makes sense relative to such a frame, where the issue is the use of resources.

Now take a word like "stingy", whose opposite is "generous". The frame with how badly someone wants to keep his money for himself, versus and how much he is willing to give it to somebody else. In a sentence like *He is not stingy, he is generous*, you are using that frarne.

Now consider the sentence *He is not thrifty, he is stingy*. What I'm doing in this sentence

is negating one frame and proposing another. I'm saying 'you should not think of his actions in terms of efficient use of resources; you should think of his actions in terms of greed'.

Now there is nothing in formal semantics that allows you to provide alternative framings of the world. In formal semantics you just have simply a state of the world that exists or doesn't. Fillmore was demonstrating that in a single sentence you could be talking abour the way in which you conceptually frame the world. To characterize the meaning of sentence about alternate conceptualizations, you need not a logical theory but a cognitive theory in which one can talk about alternate conceptual framings. In formal logic, there is no cognition, and no capacity for alternative frame in a single sentence. Such examples indicated that formal logic was inherently inadequate for dealing with natural language semantics. Instead, one has to bring the mind into semantics.

The next great talk I heard in the surnrner of 1975 was by Leonard Talmy. It was one of his first talks on spatial relations. What he showed was that there are certain universals of spatial relations that have to do with bodily orientation and with what he called cognitive *topology*, that is, with the topological structures we impose on space. For example, the word "in", in its most basic spatial sense, has to do with a bounded region of space — a container with an interior, a boundary, and an exterior. Such a cognitively imposed container can stretch — it can be any size, any shape. Containers preserve a certain topology. Talmy observed that the concept of a path is also topological in this sense. You can stretch or twist a path and it remains a path.

In addition to such cognitive topological concepts for spatial relations, Talmy pointed out that other spatial terms have bodily orientations, like "front" and "back" and "up" and "down" and so on. The fronts and back of objects are not part of a mind-free, humar-free world. They are imposed by human beings in certain regular ways. To characterize the meanings of spatial relations concepts, you have to bring in the peculiarities of the human body and the kind of cognitively structured topology that occurs with containers, paths, and other concepts. Again, the body was entering into natural language semantics.

So, in the face of all this evidence, in the summer of 1975, I realized that both transformational gramrnar and formal logic were hopelessly inadequate and I stopped doing Generative Semantics. I didn't know what to believe, and threw myself for a year and a half into rethinking what all this meant. I wrote nothing for almost two years. Finally I wrote "Linguistic Gestalts" (CLS, 1977) in which I argued that constructions were really the basis of grammar, the the meaning of the whole could not be the meaning of the parts. Fillmore and I were developing the same basic idea about constructions around the same time, talking regularly.

Then, in 1978, I was teaching an undergraduate seminar at Berkeley and I came upon evidence of conceptual metaphor. What happened was this: I was teaching Searle's paper on metaphor in the class. I thought it was a terrible paper but I didn't exactly know why at the time. There were five undergraduates in this seminar. On the day we were to discuss Searle's paper, one of the women in the class came in late, very upset, sat down. After a minute she said "I'm sorry, but I'm not going to be able to function in the class today. I've had a metaphor problem with my boyfriend. Maybe you can help".

We all said "yes". After all, it was Berkeley in the 1970s. And it was a true call for help. She said: "On the way over here, my boyfriend said that our relationship had hit a dead-end street". She said: "I don't know what this means". So someone said: "Look, if it's hit a dead-end street, you can't keep going the way you've been going". Someone else said:

"Yeah. You might have to turn back". And then we realized that there was a whole set of expressions in English for conceptualizing love as a journey. Expressions like *The marriage is on the rocks,* or *The relationship is off the track,* or *Look how far we've come,* or *We may have to turn back, We are at a crossroads in the relationship, We are going in different directions, We may have to bail out.*

There are many different linguistic expressions for conceptualizing love as a journey. But there was a generalization over all of these separate expressions. The generalization could be stated as a mapping from the conceptual domain of travel to the conceptual domain of love: the lovers are travellers: the love relationship is a vehicle; the lovers' common life goals are destinations; and difficulties in the relationship are impediments to travel. What we have here is a conceptual metaphor, a way of conceptualizing love as a journey, not just a bunch of metaphorical expressions. This conceptual metaphor is a way of *reasoning* about love using the inferential structure of the concept of a journey.

Again we have a counteraxample to formal semantics. The conceptual metaphor is not in the external objective world; it is in our minds. It characterizes how we conceptualize and reason about love in terms of a journey. Formal semantics cannot handle such cases. since there is no mind in formal semantics.

That is where the Theory of Metaphor came from. I then discovered that Michael Reddy, a year earlier, had written a paper called "The Conduit Metaphor" showing the same things. At that point I started to ask how many other conceptual metaphors there are in our conceptual systems. Over the following year, I discovered 30 or 40 of them.

In 1979, Mark Johnson visited Berkeley to teach in the Philosophy Department. I met him on the day after he arrived in Berkeley. He was going to edit a collection of papers on metaphor. We thought we might collaborate on a paper. The first thing we thought we would do would be to use the kinds of examples I'd found to argue against the recent papers by Searle and Davidson.

We thought about it for a week and then we decided that it was irnpossible to write such a paper. The reason that it was impossible was that, in order to argue against someone, you have to share their presuppositions. But the evidence contradicted what Searle and Davidson were presupposing. They denied that meaning was in the relationship between words and the world.

Instead, there are metaphorical aspects of meaning that are cognitive in nature, that are part of metaphorical thought. But Searle's and Davidson's theones excluded the very possibility of the existence of metaphoncal thought. If meanings were taken as relationships between symbols and the world, then there could be no room for metaphorical thought.

What we decided to do instead was to wnte first an article and then a book telling why our work contradicted the presuppositions not only of Searle and Davidson, but the whole theory of meaning within Anglo-American philosophy. We have been working on this project ever since, trying to show in detail how philosophy has to change in response to work on metaphor, categorization and other aspects of Cognitive Science.

How does all this compare with the general development of linguistics in the Anglo-saxon world. I mean, Chomsky and offshoots, and then functional theories, and the increasingly large number of grammars that are being devised?

Well let's start first with generative grammars and with Chomsky. Our work

cornpletely goes against it. And in an interesting way. When I was doing Generative Sernantics, I assurned that it would be possible to fit my work together with Chornsky's. I assumed that for a very long time. And I assumed that the argument between me and Chomsky was an argument within Generative Grammar. It turned out that I was wrong about that for a very interesting reason.

I had certain commitments that I undertook when I started to do linguistics. The first comrnitrnent is what I'll call the 'cognitive commitment'. It's the commitment to see language as part of the study of mind. It implied, therefore, that evidence about the nature of the mind could change a linguistic theory. And in fact that's just what happened in the work I've just described. That is, when Rosch found there were basic level categories and when Kay and McDaniel found that color categories could not be in the external world, I was obliged to change my theory of language. The cognitive commitment was my first commitment.

My second commitment was that I was concerned with stating generalizations over every aspect of language and across them if necessary.

The third thing I had originally assumed in Generative Semantics was the classical theory of semantics. I assumed originally that semantics was the study of logic and of the relationship between symbols and things in the world.

And fourth, I had accepted in doing Generative Sernantics certain aspects of generative linguistics: trees, precisely stated mles of gramrnar, accounting for all and only the well-formed sentences, but I included rneaning as part of sentences.

What I failed to recognize at the time was that Chomsky had a very different understanding of generative grammar. Chomsky had a guiding metaphor for gramrnar, a rnetaphor so deeply entrenched in his intellectual worldview that he could not possibly give it up. Chomsky's basic metaphor was this: A sentence is a string of symbols. A language is a set of such strings. And a grammar is a device for generating such sets of strings. That is how *Syntactic Structures* starts out.

Now, a sentence is not just a string of symbols. There is meaning and intonation, and the symbol strings are in writing, not in speech or sign. But nonetheless Chornsky adopted that metaphor, for reasons important to his Cartesian philosophical views. The next part of the metaphor is that a language is a set of such strings. This doesn't have to be tme; and in fact, one could argue that a language isn't a set in the technical mathematical sense of set at all. The third part of the metaphor is that a grarnrnar is a generative device for generating this "set", that is, a set of mles framed within that peculiar branch of mathematics, which I now see as having nothing whatever to do with real languages.

Chomsky was, of course, committed to his own metaphor, which he saw not as a rnetaphor but as a necessary truth. For him, recursive function theory (or equivalently the theory of formal grammars) is to be taken for granted as being the right mathematics for linguistics. This has important consequences. One of the main ones is that meaning cannot enter into the grammar. Why? Because the theory of formal grammars is *defined* so that the meaning of symbols cannot enter into the mles for manipulating the symbols. If you have a commitment to the theory of formal grammars as your required mathematics for doing linguistics, then you rule out the possibility that meaning *could* be used in mles of syntax. It is a straightforward entailment of Chomsky's metaphor. This consequence is called the "autonomy of syntax". It's built into the basic Chomskyan metaphor and it is his highest priority, something he would never give up. Given any putative evidence against it, he would assume that the "evidence" could not be real evidence, and so would come up with an

auxiliary hypothesis to explain that evidence away —even if it meant missing generalizations about language.

Chomsky's metaphor was something he could not give up, no matter what evidence was provided or generalizations were given. On the other hand, my highest commitment was the commitment to look at evidence from both linguistics and other cognitive sciences, and to take generalizations across different aspects of language very seriously. Because we had different commitrnents, and we were not able to articulate those differences of commitments clearly, conflict was inevitable. If generative semanticists found evidence that meaning entered into syntax, Chomsky could not accept it as real evidence, since it would conflict with his central metaphor for syntax.

If the face of what we saw as evidence, he just narrowed the scope of the phenomena he called "syntax". Any time semantics or pragmatics was shown to enter into grammatical constructions or the distribution of grammatical elements (e.g., words and morphemes that are clearly part of grammar), Chomsky further limited the scope of syntax. It is now so narrow and uninteresting that it leaves out 95 percent of what traditional as well as early transformational grammarians called syntax. Anyone interested in describing all of a language will find that most of it falls outside of his "core" grammar. That is not a problem for Chomsky. He just isn't interested in rnost of the phenomena of language.

Functional grammar, on the other hand, are very much in accord with Cognitive Grammar. It just happens to study different things. It looks at discourse function, especially notions like given and new information, topic, focus, and certain aspects of narrative structure. These are very real phenomena that are part of language.

As it happens most cognitive linguists happen to be interested in other things. But Cognitive Linguistics is certainly open to all of functional considerations. We think that they are real and we greatly respect functional grammarians for their empirical approach and their careful study of a much wider range of phenomena in the world's languages than generative linguists study.

How do you feel about Chomsky's emphasis on universalism, on his idea of a Universal Grammar?

I need to talk about what that emphasis is and where it comes from before I can answer that. Chomsky is a Cartesian, a follower of Descartes, in a very deep sense. Chomsky's politics also comes from Cartesianism.

Descartes was an essentialist. He believed that there was a universal human nature and that there were essential properties that all people share that distinguish people from animals. Chomsky agrees with that assumption. He is also an essentialist. Again following Descartes' philosophy, Chomsky assumes that the mind is separate from, and characterizable independently of, the body. Accordingly, he believes that the study of neuroscience can shed no light on the nature of reason or language.

Chomsky also seems to accept other tenets of Cartesian philosophy:

(1) Mathematics is the best example of human reason.
(2) Mathematical reasoning is a matter of form, not content.
(3) Hurnan reason is general is a matter of form not content.
(4) Reason and language comprise the essence of what it is to be human.

(5) Since the mind is separate from and characterizable independently of the body, so reason and language cannot be acquired through the body.
(6) Therefore they must be imate.

All of this is a priori Cartesian philosophy. There is no reason to accept this philosophical worldview, and many reasons to reject it. But let us continue.

What do these aspects of Cartesian philosophy have to do with Chomskyan linguistics? The link is in certain modern a priori philosophical assumptions that were especially prevalent in certain philosophical circles in the1950's and 1960's. Let us call this "Formalist Philosophy":

(1) Precise scientific descriptions can only be given by axioms systems. or by algorithms.
(2) Proof theory in mathematical logic, Turing machines, and formal gramrnars (or Post systems, formulated by Emil Post) are equivalent [Church's Thesis].
(3) All conceptual algorithms are characterizable by Turing machines (that is, the theory of recursive functions).
(4) The study of meaning in natural language can be characterized by "semantics" — the relationship of the abstract meaningless symbols of a formal language in mathematical logic to the world, or at least to objectivist set-theoretical models of the world. In mathematical logic, there is a "formal language" made up of meaningless symbols. Proofs operate independently of the meaning of the symbols.
(5) Logical proofs (especially in "natural deduction" systems) characterize human reason. Human reason is therefore abstract, a matter of formal symbols. Since proof theory and the theory of formal grammars are the same, the theory of formal grammars can characterize human reason.

Chomsky's metaphor fits the a priori philosophical assumptions of formalist philosophy perfectly. Language is seen as a matter of abstract formal symbols. The mathematics of language is the theory of formal gramrnars, which is the same as proof theory. which is what characterizes human reason.

Chomsky forged a link between 20th century formalist philosophy and 17th century Cartesian philosophy. Formalist philosophy takes it for granted that:

(a) Reason is characterizable independently of the body
(b) Reasoning is abstract, and a matter of pure form.
(c) Reason is universal.

Chomsky's metaphor fits both formalist philosophy and Cartesian philosophy perfectly in the way it adds language to the picture. Language for Chomsky, like Reason for Descartes, is what makes us human, what constitutes our essence, what separates human beings from the apes. (Incidentally, Chomsky for this reason must deny that animals have any language at all.)

Language must be universal and must be imate for Chomsky, for that same reason that Reason had to be universal and innate for Descartes: It characterizes the essence of what makes *all* of us human (hence universality) and it cannot be acquired through the body (hence

innateness).

Language innateness and universality theses are not empirical hypotheses for him. They are, for him, necessary truths that follow from his a priori philosophical assumptions and his central metaphor.

Moreover, following formalist philosophy, the oniy way to be scientific is to use axiom systems and formal logic or, equivalently, algorithms, or, equivalently, the theory of formal grammars. Therefore, the oniy way to do scientific linguistics is to follow Chomsky's metaphor and use formal grammars.

What is the role of empirical investigation in Chomsky's view of "scientific linguistics"?

For Chomsky, all scientific linguistics must be consistent with Chomsky's metaphor, formalist philosophy, and the above elements of Cartesian philosophy. There can be no empirical evidence against these. All evidence must be made to fit these a priori philosophical assumptions.

These philosophical assumptions fly in the face of a huge body of evidence found by generative semanticists, cognitive linguists, functional linguists —as well as a huge body of evidence in cognitive science and neuroscience.

Is reason purely a matter of form rather than content?

The answer is "No". All of the work on the embodiment of reason denies it, from research on color concepts to basic-level concepts to experimental studies of conceptual metaphor to Damasio's evidence of the link of reason to emotion to Terry Regier's modelling of the acquisition of spatial relations concepts and terms on the basis of elements of brain structures (such as topographic maps of the visual field). Reason and our conceptual systems arise from the body. Descartes was wrong.

Correspondingly, cognitive studies of grammar indicate that syntax is grounded in our conceptual system which is in turn grounded in the body. Chomsky is as wrong as Descartes was.

What would you say about the old Chomskyan idea of an innate Language Acquisition Device and the more modern idea of modularity?

What Chomsky has suggested cannot be true. There is no single place in the brain where language is located. There are different centers that do different things and they are linked in various ways. There cannot be such a thing in the brain as a language module.

This is not just a matter of localization. A Chomskyan theory might say the language module is really distributed across various centers in the brain. But that would not work either. The reason is this: Chomsky's syntactic component has to be characterized in terms of the theory of formal grammars. That is, it must consist of symbols that are manipulated (or related to one another) independently of their meaning or any other psychological or perceptual or motor or emotional input. Because Chomsky's syntax is autonomous, his syntactic module must also be autonomous, which means it cannot have any input, irnmediately or ultimately, from anything non-syntactic, for example, anything bodily. If you look at Chomsky's drawings of syntax boxes in the mind, they never have any input. They cannot if they are to be autonomous. But such autonomy is impossible in a human brain. Brains are just not structured like that. There are no brain modules, localized or distributed, without input.

***Yes,** still there are some psycholinguists thar seem ro support rhe idea of modularity.*

There are still Skinnerians too. As Tom Kuhn pointed out, old paradigms continue to exist long after they are viable.

In general the evidence doesn't bear out Chomskyan modularity. Part of the evidence was supposed to be that there are brain damaged people who can form gramatical sentences that are meaningless, and people who are the opposite, who have no sense of grammar, who have certain kinds of lesions where they cannot put grammatical sentences together.

It doesn't quite work out that way. There are people who have agrammatism who can make grammaticality judgments. They just can't say the sentences. Their agrammatism doesn't mean that there is a localized language box that is damaged. It only means that certain neural connections required by sentence production that have been severed.

Liz Bates reports on her research with a well-known Italian architect who had a stroke. He had the symptoms of agrammatism. He could only say isolated words, not sentences. She gave him a particular kind of sentence to see if he could repeat it. He could not repeat it, but he could say one word in response. That word was the name for the classical Greek "trope" describing what was odd about the grammar of the sentence. This agrammatic patient, who supposedly had his "grammar box" damaged, not only could make gramatical judgments but could describe them with the proper ancient Greek terminology!

The point is simple. Agrammatism is not evidence for a grammar module in the brain.

What S your opinion on parallel distn'buted processing?

PDP connectionism is useful for many things, like distinguishing sonar signals of mines from those of rocks. It may also be a reasonable model for how human beings map sound input into sequences of phonetic features. But it is too unstructured to characterize most of human reason and language. Parallel distributed processing ignores the details of brain structure. It asks: how much you can do with the simplest kinds of structures. The answer appears to be: a little bit but not that much.

What people like Jerry Feldman and Terry Regier have done is to take connectionist models and model aspects of innate brain structure, for example, topographic maps of the visual field and orientation sensitive cells. They then ask what concepts and aspects of language can be leamed given those innate NONLINGUISTIC structures. The question they ask is: how much of conceptual structure and language is embodied, in the sense that it arises out of brain structures that exist to serve other bodily functions: perception, motor functions, etc.

***Let's go back to the end of the 1970s.** That was a rime when there were some workers in artificial intelligence that used some versions of Frame Semantics. Did that influence any of your thinking at rhat time? I mean, proposals like Minsky's frames, or Schank & Abelson's scripts, or others?*

Yes. They came into being around the same time as Fillmore's Frame Semantics. Schank's work was based on Fillmore's earlier Case Grammar.

I was very much influenced by Schank's work. I leamed about his work on frames

about the same time as Fillmore was beginning to develop his frame semantics. I spent a month in 1977 in Schank's lab learning what he and his students were doing. I became a part of that cornmunity. It was an important experience.

You have written at length about prototypes and related phenornena in your book Wornen, Fire and Dangerous Things, and you discuss Schema Theory and basic-level categorization. arnong other things. Have you ever tried to put all this together in a unified framework?

I have thought it through a lot. I can see how many things might fit together. but I haven't really tried seriously to do it. It would be a huge task, much more than one person can do.

I've been doing other kinds of work. I've been trying to extend metaphor theory, to work out the details and apply it to many areas. That is a big job in itself.

At some time before very long, I would say within the next five years, some of us are going to have to sit down and try to unify all this work. But before then, I want to finish another book with Mark Johnson that we've been working on for some years. It's a big book on philosophy called "Philosophy in the Flesh". It is about the consequences for philosophy of the embodiment of mind, the cognitive unconscious, and metaphorical thought.

Those developments, if taken seriously, would radically change western philosophy. We want to show exactly why it changes it and how it changes it. Before I even think of entering into a project of unifying all of Cognitive Linguistics, I want to finish that book. I also want to do a book on the metaphorical sructure of mathematics. Anyway, I don't believe one person can do that unification. It is a job for the field as a whole.

It is a difficult job. One reason why it hasn't been tackled earlier was that the separate parts were not very well worked out. Metaphor Theory had to be worked out, the Theory of Categorization had to be worked out, Langacker's Cognitive Grammar had to be worked out. We had to get some understanding of what image-schemas were. We had to understand what constructions were. Now we have come to the point where it is almost possible to begin to think of the unified theory, although the recent Fauconnier-Turner work on blending shows that there is a lot in those phenomena that needs to be understood better.

Could you give me an idea of what the place of semantics is within grammatical theoty in general? What is its relationship to syntax and other components?

I believe, just as Langacker does, and as I believed when I started doing Generative Semantics, that grammatical structure consists of what Langacker called "symbolization" — conventionalized pairings of elements of semantic structure with aspects of phonological structure (including ordering constraints).

I believe that predicate-argument relations of semantics are the predicate-argument relations of syntax, once symbolization —that is, the conventionalization of semantic-phonological pairings— has operated.

I believe that the propositional structure of semantics is syntactic clause structure under symbolization. I believe that semantic event structure is syntactic aspectual structure under symbolization. And so on.

I believe that the kinds of structures that you find in syntax are conventionalized

reflections of semantic-phonological pairings, and that there is NO autonomous syntactic structure at all.

That is something that Langacker has been trying to show in very great detail with considerable success, I think. And it is something that we began to show in the mid 60s while looking at evidence in syntax for logical form.

However. to really demonstrate it would take a unification of all the subparts of Cognitive Linguistics. I think all the evidence points to it, but the unification has to be done and no one has done it.

You are very concerned with reasoning and you have explanations about whar reasoning is from the point of view of metaphor and metonymy. What I would like to know is what yourpoint of view is on work in pragrnatics dealing with the same field of reasoning? Like rhe theory of implicature or presupposition.

The earlier results in pragmatics that generative semanticists accomplished fits right in with Cognitive Semantics. Much of it can be accommodated in a relatively straightforward way. I think it is possible to take the mechanisms of Cognitive Semantics and Cognitive Grammar and use them to describe what goes on in pragmatics.

For example, take Grice's Principle of Relevance. That is just the use of frames and ICMs. Relevance is finding the appropriate ICMs for framing an utterance. Once you find them, there are inferences in context that follow naturally.

Grice's Principle of Quantity can be stated in terms of the metonymic principles involving scales. Indirect speech acts can be characterized metonymically as it was done in a paper at the ICLA meeting last month by Linda Thornburg and Uwe Panther.

Frame Semantics has a presuppositional background built into it. Composition of presuppositions follows the principles given in Fauconnier's mental spaces. Speech acts too can be characterized using frames.

In short, I think every aspect of pragmatics can be expressed in terms of Cognitive Linguistics. The reason it hasn't been an issue in Cognitive Linguistics is that many of the practitioners, such as myself, Fauconnier, Fillmore. and Sweetser worked on pragmatics in old days back in the 60's and 70's and know how it fits in.

So. it is assumed.

It is assumed.

However, for people outside Cognitive Linguistics who don't know the Cognitive Linguistic mechanisms, it is far from obvious. A good Cognitive Linguistics textbook should discuss the maner.

I don't mean to suggest that all the problems of pragmatics have been solved already and that Cognitive Linguistics has no new insights. Work now being done by Michael Israel on polarity items within Cognitive Linguistics solves certain classic problems in that area.

What do you think about structuralist concerns with synonymy, antonymy and all lexical relationships?

I think they are superficial consequences of Frame Semantics. If you do your Frame

Semantics right, then synonymy and antonymy fall out. They are relative to frames. That is one of Fillmore's major points.

Fillmore has shown that many apparent synonyms are not synonyms at all because they are defined by different frames. For example. he distinguishes between "on the ground" and "on land" and he points out that they seem to have the same truth conditions, but they don't really mean the same thing at all. "On the ground" has to do with the frame of airplanes and air travel and it is contrasted with "in the air"; whereas "on land" has to do with sea travel and it is contrasted with "at sea."

Two expressions may have the same truth conditions, but if they are defined with respect to different frames, they are not synonymous. Linguists who just talk about synonymy and antonymy without frames are missing most of the relevant semantics.

What about the idea of semuntic fields?

Semantic fields are very real and they are defined also with respect to frames. Indeed, the phenomenon of semantic fields is what led Fillmore to develop Frame Semantics.

What Fillmore showed in his very first paper on frames was that words in the same semantic field, like buy, sell, price, goods, cost, and so on, are defined relative to a single frame, and that you can only make sense of semantic fields with respect to Frame Semantics.

People who just say: these words are linked together or they form a set, miss the inferential relationships among them. They also cannot explain their possibility for metaphorical use, because metaphorical use depends on the frame semantic structure.

Is rhere any way in which Frame Semantics can be put in the form of a dictionary?

Absolutely. It can be and Fillmore is doing it.

Right now?

Right now. Fillmore is the main linguistic consultant to the Oxford English Dictionary. He has a major research project now in which he is trying to get computer representations of lexical entries using Frame Semantics. Some of his students are trying to incorporate metaphor and metonymy into that as well.

Can you give me an idea of how he is doing thut?

I don't know the computational particulars. The analyses are very much those he has done in the past and in many ways like those in Cognitive Linguistics in general.

Yes, how would you dejine your idea of an ICM?. Because I believe thut you have never attempted to work out a dejinition in your writings.

The idea of an ICM is actually very simple. It's basically a frame which can have metaphor appended to it. Its relational elements can be characterized in terms of image-schemas. An ICM can be of various scopes. It can be extensive enough to be a folk theory. It can function as a narrative structure, or it can be some relatively small thing,

characterizing the structure of some small conceptual domain.

Do you think machine translation will be possible?

There are many things that are meant by machine translation....

Computer-aided automatic translation. Programmes that do rhar on-line, the same way as a jlesh-and-bones rranslaror would do. And if ir is possible, whar would be rhe starting poinr?

I think there are real problems with it. And the reason I think there are problems with it has to do with the fact that conceptual structure comes out of the body and human experience.

Moreover. different languages may have different concepts. Many concepts are universal, but many are not. Members of a different cultures may have very different experiences and different cultural images that lead to different idioms. Sometimes, the conceptual metaphors can be different.

I'm very skeptical about machine translation. though I think that machines can aid in cranslation in many ways.

As they do righr now.

As they do right now.

I think that the best use is in particular domains where there is a limited vocabulary, particularly a largely intemational vocabulary, as in chemistry papers. There compucers may help a lot. But in general I don't expect to see particularly good machine translations.

Would you consider yourself still a linguist or are you a cognitive scienrisr, or are you midway between rhe mo, or are you borh?

I'm both.

I'm very much a linguist. Let me tell you what makes me a linguist. I study language including the conceptual smcture underlying language. I study linguistic regularities —regular patterns within the language, again including the conceptual system. The methodology I use in studying conceptual systems is an extension of traditional linguistic methodologies. Other fields don't use that methodology.

The methodological ideas that make linguistics what it is are, I think, extremely powerful, and can be extended in general to broad issues in cognitive science. That is what I've been doing, in part. I think like a linguist and even when I'm doing conceptual analysis of culture.

Now, I'm not the first linguist to do this. In fact, my first linguistics teacher was Roman Jakobson, who taught structuralism to Levi-Strauss. He knew that one could use techniques of linguistic analysis to study culture. Jakobson was a great inspiration to me in this respect.

I once heard Michael Halliday say rhar rhe problem with linguistics ar presenr is that

we have not been able to crack the code, but that is somerhing rhar they have managed to do in the realm of physics and orher natural sciences a long rime ago. Would you agree wirh rhar?

No, I don't agree with that at all.

The notion of "cracking the code" is a very strange notion. A language is incredibly rich. It depends on conceptual systems. Conceptual systems are incredibly rich; they are not a simple-minded code.

To understand conceptual systems you are going to have to understand their relationship to the body and neurobiology. Linguistics is inherently a cross-discliplininary enterprise. You cannot really understand linguistics without understanding conceptual structures. You cannot understand conceptual structure without looking at evidence from psychology and anthropology. And you can't understand why we have the conceptual structure we have without looking at neuroscience.

Often you can't even describe the conceptual structure we have without some insights from neuroscience. As Terry Regier has shown in The Human Semantic Potential (MIT Press), certain aspects of spatial structure can be described adequately only via neural computation.

That's a very profound result. That indicates that you cannot separate off linguistics as an autonomous discipline. Language and conceptual structure is enormously rich and it relates to perceptual systems, to motor systems, to all sorts of things in the body. It is not just a linguistic code that you are cracking. To understand spatial relations systems, you have to understand how relevant aspects of the visual system works. If you are going to do phonetics in an explanatory fashion, you have to learn how the auditory and articulatory systems work physically. That is not cracking a code. It is doing cross-disciplinary science.

Your approach is very empirical.

Entirely.

Now, whar do you think is the future for linguistics? Where is linguistics heading for righr now?

First, I should say that linguistics in America is in a very difficult position that Chomsky has placed it in.

There are very few linguistics departrnents in America. There are about 50. That is a very small number, and there are very few jobs for linguists in Linguistics departments.

When Chomsky's works first came out, a lot of people in other disciplines were very interested in the possibility that they could apply it to their disciplines —to the study of culture or politics or architecture or literature. But because Chomsky's theory predominated, and because it required that linguistics be autonomous and very narrowly defined, it became uninteresting to people in most other disciplines.

These days hardly anyone in literature or architecture or political science cares about generative linguistics because it says nothing to them of use for their disciplines. Governrnent and Binding theory doesn't even tell you very much about language or thought.

The work in Cognitive Linguistics is very different. I find many people in many other

disciplines that are interested in the work that I'm doing, and that Fauconnier, Langacker, Sweetser, Tumer, and many other people are doing. We find we can talk to people in other disciplines very easily. I get invited to contribute articles to journals in disciplines as diverse as art, political science, mathematics, education, psychotherapy, anthropology, philosophy, computer science, and public policy. As a cognitive linguist, I give lectures in many departments about ways in which Cognitive Linguistics matters to those disciplines.

I think that is wonderful. And I think it is absolutely necessary if linguistics is to thrive as a field that linguistics be of major relevance to other disciplines. I think otherwise it will die.

Cognitive Linguistics is right now in the position of making very serious contributions to many fields. Let me give you an exarnple. I have a colleague in the Political Science department who is doing a study of forms of democracy. In the democracy literature he has found over 600 types of democracy. He has been able to make sense of them using Cognitive Linguistics. He has found that they fit a radial category structure. Using the notion of radial categories, he can make sense of what was previously seen as being chaotic to political scientists. Feature semantics and the classical theory of categories could not make sense of this phenomenon.

The book I'm doing now on morality and politics and the notion of the family in American politics —it's called "Moral Politics"— depends on conceptual metaphor and radial categories. I believe it will allow us to make better sense of American politics and culture.

My sense of things is that Cognitive Linguistics is at the point of being able to be applied usefully and insightfully to virtually every discipline. And it is only if it is applied to a broad range of disciplines that linguistics will thrive and survive. Otherwise, linguistics is such a small field that it is really endangered.

Let me give you an example of the kind of danger that it is in. When the financia1 crisis at this university hit about two years ago and the university budget was going to be cut a great deal, there was a faculty committee put together to ask which departments should be funded and which part should be cut. The Social Science comrnittee did not contain any linguist. It contained people from political science, history and economics who did not have any use for linguistics as they knew it — generative linguistics. Their report said that linguistics is not a very interesting discipline because it only studies language. They understood language as having not to do with concepts at all but just to do with forms.

Now, if they were talking about Generative Linguistics, they would be correct. Ironically, they happened to be talking about Berkeley where we study conceptual structure, which can be applied to all of their disciplines. Our reply pointed that out.

Do you think Chomsky's Generative Linguistics will perish?

Yes.

Soon?

No.

It will thrive for some rime?

I don't think it will thrive. I think it will gradually decline, but I think Kuhn was basically right. A field doesn't die until its practitioners die.

The Linguistic Institute this past summer in New Mexico was very interesting in this respect. It was the first completely generative and functionalist Institute. And in fact the first time that any of these ideas was able to be taught at Linguistics Institutes, which have been largely dominated by Generativists. It was the largest Linguistic Institute ever. It had a wonderful faculty. Researchers in Functional and Cognitive Linguistics have become ven good at what they do. They have been working at it for 20 years now, and many of them are extraordinary scholars.

The students were suitably impressed -students from around the country and around the world. Many of the students who carne there came because they were tired of Generative Linguistics. They couldn't care less about it. They wanted to study real language. They didn't want to study GB. And they were very pleased with what they found. It was a very exciting and interesting Institute. And I think that tells us about how the field is changing.

One last question. Since you've been to Spain at least twice, what is the idea that you have about the development of Linguistics in my country?

I'm extremely impressed by what I see in Spain. I first went to Spain in 1976 at the time of the first election after Franco's death. That was a wonderful time. I was in Madrid during the election and it was a very beautiful thing to see. I spent that first election day walking through the streets of Madrid.

But in talking to linguists and people in artificial intelligence and people in other disciplines, I found that at that time intellectual life was very much closed off. Intellectual life was so awful under Franco that Spain was a very backward place.

I went back last year and I was astonished at the development that had taken place. In under 20 years, Spain has become a major intellectual center. In linguistics I found linguists who knew about the latest work in Cognitive Linguistics, in Functional Linguistics, and all sorts of related ideas that they understood very well.

They were writing excellent papers and doing interesting work, asking the deepest possible questions. I couldn't have been more impressed. I think that the potential of Spain as a center for linguistics is unlimited.

Thank you, Professor Lakoff. I appreciate your time.

Thank you. It's really been a pleasure.

NOTES

1. The text that follows is a revised version of a transcnpt from a tape-recorded interview that I held with Professor Lakoff at Berkeley University (California) on August 31, 1995. The revision was made by Professor Lakoff himself in December 1996. I would like to express my appreciation to Lorena Pérez Hernández for her assistance in making the transcripts.