Chomskyan Grammar Seen from Within and Without

Ray Jackendoff

Interview with Michael Schiffmann

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MS: Alright, I'm talking to Ray Jackendoff at the end of a conference at the University of Konstanz, and the intention is to basically have a three-part interview. The first part is supposed to be dealing with your personal background and career. The second part would be about your experiences at MIT and the history and development of generative grammar and Chomsky's contributions to it from what you personally were able to observe. The last part could be headed by the heading "in retrospect". How do you evaluate things? How have things developed also in terms of also being in interaction with previous colleagues? One of the things that I understand is that people like Tom Roeper are also old acquaintances of yours, so there is some connection. Things like that. So my first question would be, from your biography I take it that you came to MIT in 1965, and you were twenty years old. What made you want to do this in the first place, and what was your background as a twenty-year-old guy? [Laughs.]

RJ: I did my undergraduate work in mathematics. I always thought I wanted to do mathematics. But somewhere around my third year of college I realized that I was not going to be a very good mathematician. And I thought: What else can I do? I had done a term paper in high school about the history of English, and there was one linguistics course at my college (Swarthmore). So I said, Well, I'll take linguistics and see what that's like. It was mostly very traditional stuff, Bloomfield and the structuralists, but somewhere in the course of that semester we read *Syntactic Structures*,[1] and I thought this was very cool. I wasn't alone in this feeling. A few years ago, when I was giving the talks that were leading up to *Foundations of Language*,[2] I was talking about why I became a linguist. And I said, You know, I read *Syntactic Structures,* and when I figured out what Chomsky was doing in his analysis of the auxiliary, I just thought this was so cool, and I wanted to do more of this. And then I asked how many other people had had this experience and dozens of hands went up: it was just so neat, the way the affixes just clicked around and boom, out came the English auxiliary. So I decided to apply to graduate schools in linguistics. The only places doing transformational grammar at that time (1964) were MIT and Penn.[3] So I applied to them and got into MIT and went. And suddenly I discovered I had things to say, which I had never experienced before.

MS: Right, which wasn't the original plan.

RJ: Well, I didn't know that, but, you know, it came very easily to me, and it was a lot of fun. It was a period that I like to compare to the beginnings of nuclear physics, or the beginnings of jazz, or the beginnings of impressionist painting.

MS: Yes, something really new is happening right now.

RJ: And we're making it up as we go along, and we know all the characters involved.

MS: And you did!

RJ: And we did. My friend Adrian Akmajian[4] said that every time you opened your mouth

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[1] Chomsky, Noam, *Syntactic Structures*, Mouton, The Hague 1957.
[2] Jackendoff, Ray, *Foundations of Language: Brain, Meaning, Grammar, Evolution*, Oxford University Press, Oxford 2003.
[3] This refers to the University of Pennsylvania in Philadelphia.
[4] Adrian Akmajian was Professor of Linguistics at the University of Arizona and c-author of two very important

you made history – because almost nothing had been worked on. There were three or four books you had to know and a few articles, and aside from that we were making it up.[5] Everything was new. For instance Ross was developing his work on constraints at the time.[6] I don't remember what other work was going on, but it was really fundamental stuff. *Sound Pattern of English* was new.[7] It wasn't finished yet when I started graduate school.

MS: Well, I think they had worked on this for ten years.

RJ: This was the final period of their work on that. And *Aspects* had just come out.[8]

MS: When you read this older stuff like Bloomfield or Harris – one of the anecdotes [5.00] that Noam Chomsky likes to tell is that in structuralist linguistics the feeling was, well, basically the work is done. We have all the procedures, and depending on what exactly you want you kind of like put it in there and out comes the grammar which is neatly ordered, and so basically the future work would be done by computers. Did you also feel that from traditional stuff?

RJ: No, I don't think so, but I didn't have the kind of grounding in the traditional stuff that Noam did. There was one course at MIT on pre-generative linguistics, which everyone called "The Bad Guys," and we didn't take it very seriously. So I don't know how widespread that feeling of completion was in the field before that. All I know is that the ethos at MIT was that we were on to this hot new thing. The first graduates of the program had just gone out into the world to teach: McCawley[9] and Barbara Partee[10] and Arnold Zwicky[11], Terry

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linguistics textbooks: Akmajian, Adrian & Heny, Frank, *An Introduction to the Principles of Transformational Syntax*, MIT Press, Cambridge, MA, 1975, and Akmajian, Adrian, Demers, Richard A., Farmer Ann K., and Harnish, Robert M., *Linguistics. An Introduction to Language and Communication* (6th edition MIT Press, Cambridge, MA, 2010).

[5] RJ comments: "From today's perspective this attitude looks rather provincial. Recently I had occasion to read Lila Gleitman's 1965 article in *Language* on coordination, a remarkable work very much in the mode of early 1960s generative grammar. We never knew about it." See Gleitman, Lila, "Coordinating Conjunctions in English" *Language*, 41(2) (1965), pp. 260-293.

[6] John Robert Ross, also affectionately called "Haj," born in 1938, studied and got his Ph.D. (1967) at MIT, which was titled *Constraints on Variables in Syntax* at the time. Despite its great significance for linguistic theorizing, it was published only in 1986 as *Infinite Syntax!* by Ablex Publishing, New York. Ross has worked, and continues to work, on a huge variety of syntactic and linguistic phenomena. He was part of the short-lived linguistic current called "Generative Semantics," and using a metaphorically very gifted language, he has coined a number of technical terms such as "scrambling" (the shuffling around of syntactic constituents from their original position to other positions not canonically designed as end points of such shuffling), "islands" (syntactic structures that constituents are (more or less) unable to move out of, "pied piping" (constructions in which a question word such as whose "carries along" the phrase which it is a part – and in fact, the head – of, as in "Whose books by Chomsky did you never read?"), etc.
For some additional remarks, see Chomsky, Noam, "The Place of Displacement and Other Questions Demanding an Answer," Interview with Michael Schiffmann, Cambridge, August 4, 2010, p. 15, note 60, and Chomsky, Noam, "Poor God. Must Be Bored to Death!," Interview with Michael Schiffmann, Cologne, June 7, 2011, p. 17, note 31.

[7] Chomsky, Noam & Halle, Morris, *The Sound Pattern of English*, Harper and Row, New York 1968.

[8] Chomsky, Noam, *Aspects of the Theory of Syntax*, MIT Press, Cambridge, MA, 1965.

[9] James ("Jim") McCawley (1938 – 1999) studied at MIT where his Ph.D. thesis *The Accentual System of Modern Standard Japanese* was accepted in 1965. He later taught at the University of Chicago and did research in the areas of syntax, semantics, and phonology. McCawley was one of the more prominent proponents of generative semantics.

[10] Barbara Hall Partee (* 1940) is Professor Emerita at the University of Massachusetts, Amherst, and has worked in Montague grammar, other parts of semantics, and mathematical linguistics. She is co-author of the standard work Partee, Barbara H., ter Meulen, Alice & Wall, Robert E., *Mathematical Methods in Linguistics*, Kluwer, Dordrecht 1990.

[11] Arnold Zwicky studied at the MIT with Morris Halle and has worked in phonology, morphology, and syntax. He is both Consulting Professor of Linguistics at Stanford University and Professor Emeritus of Linguistics at

Langendoen,[12] Yuki Kuroda,[13] Tom Bever,[14] Paul Kiparsky,[15] and so on. And so it was the first time that generative grammar was really beginning to be widespread in the academic world. It was also a time – over the next seven to ten years – when, for whatever reason, American universities were just exploding. New campuses were being opened all the time all over the place. Probably in response to the scientific challenge from Russia, the satellites going up.

MS: Right. That's one of the things that you note on many papers, including Chomsky's, that's military funding.[16] Directly, not through MIT, but directly.

RJ: Yes, the Air Force was funding linguistics research. Yeah, and my graduate education was funded by the National Defense Education Act. So there was a lot of money around in the universities and they were starting new campuses –

MS: The other interesting thing that I understand is they never told people what to do. They just funded it.

RJ: That's true. But new campuses would open and they would say, What can we do that makes us distinctive? Ah, there's this hot stuff, transformational grammar! Let's hire some linguists! So all of these new universities, like UC San Diego, UC Irvine, the University of Connecticut, and the University of Massachusetts Amherst, started linguistics departments that turned into top departments. For example, Irvine hired Culicover[17] and Wexler[18], while Harvard and Yale and Columbia were just languishing, holding on to the old ways.

MS: Yes, no Sputnik shock in this realm, in the realm of linguistics! I mean one motive for the military would be to prevent that.

RJ: Yeah, I think the military was interested in linguistics because they wanted to know how

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Ohio State University.

[12] Today, D. Terence Langendoen is Professor Emeritus at the Department of Linguistics, University of Arizona, and Expert, Division of Information and Intelligent Systems, National Science Foundation.

[13] Sige-Yuki Kuroda (1934 – 2009) was Professor of Linguistics at the University of California, San Diego, and a pioneer in the application of generative grammar to Japanese.

[14] Thomas G. Bever (* 1939) is Professor of Psychology, Linguistics, Cognitive Science, and Neuroscience at the University of Arizona. One of his more recent works is, together with David J. Townsend, *Sentence Comprehension. The Integration of Habits and Rules*, MIT Press, Cambridge, MA, 2001.

[15] Paul Kiparsky (* 1941 in Finland) is a phonologist and Professor of Linguistics at Stanford University. He is the author of *Panini as a Variationist*, MIT Press, Cambridge, MA, 1979.

[16] This point is often discussed by Chomsky in interviews; for more extensive discussion, see Newmeyer, Frederick J., *The Politics of Linguistics*, University of Chicago Press, Chicago 1986, pp. 51-57 and particularly pp. 84-88. One example is Chomsky's work "Three Models for the Description of Language," which on the first page carries the footnote: "This work was supported in part by the Army (Signal Corps), the Air Force (Office of Scientific Research, Air Research and Development Command), and the Navy (Office of Naval Research), and in part by a grant from Eastman Kodak Company." The work was published in the Institute of Radio Engineers' (IRE) *Transactions on Information Theory*, Vol. IT-2, No. 3, pp. 113-124. Chomsky's first book *Syntactic Structures* carried the identical remark right at the end of the preface (see *ibid.*, p. 7). Additional works by Chomsky supported by Army research grants were *Current Issues in Linguistic Theory*, Mouton, The Hague 1964, and *Aspects of the Theory of Syntax* (see note 6).

[17] Peter W. Culicover is now Professor of Linguistics at Ohio State University; he is author of two major textbooks, *Principles and Parameters: An Introduction to Syntactic Theory*, Oxford University Press, Oxford 1997, and *Natural Language Syntax*, Oxford University Press, Oxford 2009. Together with Kenneth Wexler, he co-authored the book *Formal Principles of Language Acquisition*, MIT Press, Cambridge, MA, 1980; moreover he is the co-author, with Ray Jackendoff, of the volume *Simpler Syntax*, Oxford University Press, Oxford 2005.

[18] Today, Kenneth Wexler is Professor of Brain & Cognitive Science at MIT; he works in the realms syntax, semantics, and language acquisition.

to tap phones. And now they are interested in being able to have verbally instructed robots on the battlefield. They are still funding that pretty heavily.

MS: And there is also the Second World War story, which Mark Baker is talking about, the Navaho Code.[19] So coding and decoding, would that also –

RJ: That was I think less of an interesting avenue for linguistics, though during the war, I believe a lot of linguists were employed in writing manuals for language teaching. But there was also defense department funding for work on psychoacoustics, like Ken Stevens at MIT[20] and Al Liberman's[21] work at Haskins Labs.[22] They wanted natural language processing so they could have a computer automatically tapping your phone instead of having to actually listen to it. Then shortly after that, starting in the late seventies, there was a lot of funding coming from the Sloan Foundation to get cognitive science started,[23] and again that led to expansions in linguistics, building up all these departments and all these connections. That sort of ended sometime in the 1980s. So this sort of expansion really couldn't happen again. The point is that Chomsky arrived at just the right time in terms of the institutional situation for generative linguistics to really spread the way it did.

MS: And I would also guess, and I think he would tend to agree, [10:00] it wasn't just when generative grammar came on to the scene. It wasn't just one individual genius. It was ideas that kind of lingered in the air, like relativity was lingering in the air when Einstein came along.

RJ: I don't want to speculate on that. I don't know what was in the air before Chomsky came along, but, you know, Morris Halle certainly played a major role in making this happen. In addition to being a great linguist, he had all the administrative and political savvy to get this program going. I don't think Chomsky could have done that part on his own, building this institution that has cranked out PhDs, for fifty years now.

MS: Yes, so for a long time Halle would have been the organizational mastermind.

RJ: Yes, and then Keyser took over, and he has the same kind of political mind in terms of

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[19] The first chapter of Mark Baker's book *The Atoms of Language. The Mind's Hidden Rules of Grammar*, Basic Books, New York 2001 is devoted to "The Code Talker Paradox" and opens with a description of a U.S. military program of the early 1940s in which secret military messages were "encrypted" by translating them into Navajo. After reaching their destination, they were then retranslated by bilingual Navajo Indians who spoke both their native tongue and English. The program was used in the Pacific theater of World War II against the Japanese, and indeed the Japanese military was never able to crack the code: it didn't know how. Baker uses the episode to illustrate the situation of the new-born child which finds itself, just as the Japanese were confronted with a totally new language – or "code" – which all by itself doesn't contain any clue as to how it should be analyzed.

[20] Kenneth N. Stevens (1924 – 2013), an internationally leading specialist in acoustic phonetics, was Professor at the MIT's Research Laboratory of Electronics and author of the book Acoustic Phonetics, MIT Press, Cambridge, MA, 2000.

[21] Alvin Meyer Liberman (1917 – 2000) was a psychologist and a very important researcher in the area of speech perception. He served as a professor of both psychology and of linguistics and was president of Haskins Laboratories from 1975 to 1986. One important concept developed by Liberman was the "motor theory of speech," one of whose basic tenets, according to Morris Halle, "is the proposition that language is grounded in articulation." See Halle, Morris, "Introduction," in *From Memory to Speech and Back. Papers on Phonetics and Phonology 1954 – 2002*, Mouton de Gruyter, New York 2002, p. 8. Halle here refers to the sound side of language and opposes a grounding in articulation to, e.g., a grounding in acoustic or auditory phonetics.

[22] A multidisciplinary scientific laboratory founded in 1935 and located in New Haven.

[23] The Sloan Foundation is a non-profit organization for the advancement of scientific research in technology, science, and culture with a budget of more than$ 1.5 billion (2011). It was founded in 1934 by Alfred P. Sloan, then President and CEO of General Motors.

getting money from the institution and making sure everything works smoothly, and that's something that I suspect Noam had no interest in. So that was a very important part of the enterprise.

MS: What were some things that you were doing? I understand that you spent four years at MIT. Is that correct?

RJ: Yes, yes. In those days that you could finish your degree in four years (since there was virtually no previous literature to master). I came across an old letter the other day that suggested I had actually been considering trying to finish in three years, but it was easier not to. Well, that was the period when Lakoff's[24] dissertation on "abstract syntax"[25] and many of Ross's papers were trying to push the limits of Deep Structure. They were pushing it back farther and farther towards meaning, culminating in generative semantics probably in 1967-1968. I'm trying to remember, but I think by my second year of graduate school I was sort of questioning that approach. The way people did pronouns in those days was to say that a pronoun was a full noun phrase – a copy of its antecedent, and what you did was delete that and replace it with a pronoun. For some reason I got interested in trying to do it another way, saying the pronouns are really there and you're trying to find their antecedent, sort of an early version of binding theory.

MS: Yes, the reference for that.

RJ: And that led to the more basic issue of how you derive the meaning from the syntax. There was this notion that you take the syntax and you project it into the semantics. So there came to be a time when what was sort of in the air – or maybe it was just in my mind – was to ask: How much of the structure of the sentence can you derive from the meaning? – And without various conditions *in the syntax*? So, for instance, to what degree can you predict that a verb has certain kinds of complements depending on what it means? I remember I was working on control also, for some reason, and I had this idea that was probably an early version of PRO.[26] The way control was done in Rosenbaum's dissertation,[27] which was brand new when I came to MIT, was that "The man wants to go," is derived from a deep structure something like "The man wants it that the man goes."

MS: Right. Once again you put in the full item and then it is deleted.

RJ: Right, and the deletion rule was called Equi-Noun Phrase Deletion.[28] But people noticed

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[24] George P. Lakoff (* 1941) is one of the leading practitioners of cognitive linguistics and co-author of the well-known book Johnson, Mark & Lakoff, George P., *Metaphors We Live By*, University of Chicago Press, Chicago 1980. In the late 1960 and early 1970s, he was one of the leading proponents of generative semantics. On his own perspective of this aspect of his career, see "George Lakoff in Conversation with John Goldsmith," in Huck, Geoffrey J. & Goldsmith, John A., *Ideology and Linguistic Theory. Noam Chomsky and the Deep Structure Debates*, Routledge, New York 1995, pp. 107-119.

[25] George Lakoff's 1965 University of Indiana dissertation *Irregularity in Syntax* was published by Holt, Rinehart & Winston, New York 1970.

[26] Control structures are structures like [Michael promised Iwo [PRO_{Michael} to leave]] and [Michael persuaded Iwo [PRO_{Iwo} to leave]], where, depending on the verb in the main clause, either the subject or the object of the main clause "controls" the reference of the unpronounced subject of the embedded clause. This latter subject is designated as "PRO."

[27] Peter Rosenbaum's dissertation *Grammar of English Predicate Complement Constructions* was accepted at MIT in June 1965, and published by MIT Press in 1967.

[28] The earlier treatment of the phenomenon sketched in the previous note. "Equi" stands for "equivalent," the equivalence of course being with the subject or object NP of the main clause.

(it might have been Haj Ross) that this didn't work with quantification. If you say "Every man wants to go," it doesn't mean "every man wants that every man goes". So I started playing with the idea of having an invisible pronoun there, and at some point Haj said to me, You know, if you do that – I can't remember his exact examples – but if you do that you'll have to get interpretation from the *surface*, and that can't be the case. [I suspect the examples were things like "John would hate to be arrested," where the invisible pronoun can only be licensed after the passive transformation has moved it to the subject of its clause] Remember, at that point the reigning theory was the Standard Theory,[29] where Deep Structure determines the meaning. So I was worried. Well, Chomsky was on sabbatical then.

MS: When exactly would that have been?

RJ: Fall of 1966 maybe. Anyway, he came back from sabbatical and I said, I've been working on this and it looks kind of interesting, but it requires surface structure interpretation. And Chomsky said, *of course* it is surface structure interpretation. Wow, OK! [15:00] Well, that encouraged me to look for other cases that might need surface structure interpretation, which got me working on quantification and negation. And showing that negation requires surface structure interpretation was the breakthrough that really brought me some attention in the field.[30]

MS: Wouldn't one of the differences between what was to become the interpretivist[31] approach and the generative semantics approach be that the latter stuck to the idea that we have a Deep Structure and we just have to get deeper and deeper and more abstract, and actually surface structure doesn't contribute anything to interpretation?

RJ: Well, see, in 1965-66 the theory was that Deep Structure does all the work, it contributes everything needed for interpretation. If you look back in *Aspects*, Chomsky is very cagey about what he thinks Deep Structure does. Sometimes he says that you *project* the interpretation from the Deep Structure, sometimes he says the Deep Structure *determines* the interpretation. He uses different verbs with different implications. So generative semantics grew out of so-called abstract syntax. One important argument of abstract syntax was that if something has a pronoun referring to it, it must be a noun phrase. So take "Bill sees that Harry is doing something, but Fred didn't see it." For the abstract syntax people, it showed that "that Harry is doing something" has a noun phrase over it.[32] And then they started introducing abstract causative verbs to get "John broke the window" from "John CAUSE the window break."

MS: Yes. In sentences like the one you just mentioned you would also have to have a category changing transformation. For example "it" with the actual meaning "that so and so," you would have to transform that into a noun phrase.

RJ: No, not exactly. This complement clause had a noun phrase over it with "it" as its head.

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[29] As elaborated in Chomsky's *Aspects of the Theory of Syntax*.

[30] Jackendoff, Ray, "An Interpretive Theory of Negation," *Foundations of Language* 5.2 (1969), pp. 218-41.

[31] The "interpretivist" approach was the one pursued by Chomsky at the time, and its mainstay can be said to have been that syntax generates autonomous structures which are then semantically interpreted, whereas generative semantics starts with abstract structures of the meaning of a sentence, which are then transformed by a number of operations to yield the surface structure of the physically realized sentence.

[32] The pronoun in question is of course *it*, and the idea is that in a structure like that of the example sentences, the NP *it* dominates the propositional expression aka sentence *(that) Harry is doing something*.

That's the way Rosenbaum did it, so that he could get "It bothers me that the moon is full" from "[it [that the moon is full]] bothers me," by extraposition, moving the clause to the end. So all complement clauses were noun phrases.

MS: Which also meant that you had to have an enormous amount of transformations.

RJ: Yes. But then you also need an enormous number of conditions on transformations. You know, the complexity has to be captured *someplace* in the grammar. But the key question that I think I have continued asking since then, is: If you have semantic structure, which you *need* because the sentence has to *mean* something, and your inferences have to be based on it – if you have semantic structure, how much of the structure of language can you determine from the semantics alone, and it's only the balance for which you need syntax? That was the idea behind interpretive semantics, I think, whereas generative semantics was saying no, no, no, it's all syntax all the way up, or all the way down, however you care to think about it.

MS: Yeah, one of the ideas that occurred to me when I was reading your book,[33] and I was also looking in the interview you gave for the Goldsmith volume about the debate between interpretivist and generative semantics,[34] was your current position as I understand it is, well, we could put it that way, a radicalized interpretivist position.

RJ: Yeah.

MS: You say, I started out with that, but it should be even simpler, it should be even leaner than I supposed at that time.

RJ: That's right. You keep building on the assumptions you have, but if they're not working, you also want to let go of them incrementally.

MS: So they would pull in one direction and if one looks across your career, you would pull in the other direction.

RJ: The interesting thing is, I think Chomsky in his heart wanted to say Deep Structure *is* meaning. I think that's what he's now saying in effect, where he says the narrow syntax is very close to the "conceptual-intentional interface." I think he's really trying to say that you couldn't think combinatorially unless you had language. And that was the generative semanticist position in effect. What that forces you to say is that every time you find a [20:00] semantic distinction, there has to be a syntactic distinction that it's derived from. And the result in both cases is these giant trees.

MS: Yes, it has to be represented.

RJ: Yeah, that assumption drove the generative semanticists completely into these baroque fits. And that's in fact what we have today since late Principles and Parameters,[35] since the

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[33] Jackendoff, Ray, *A User's Guide to Thought and Meaning*, Oxford University Press, Oxford 2012, a book that had come out just a few weeks before the interview.
[34] "Ray Jackendoff in Conversation with John Goldsmith," in Huck & Goldsmith, *Ideology and Linguistic Theory. Noam Chomsky and the Deep Structure Debates* (see note 22), pp. 98-106.
[35] Examples for the growing complexity of syntactic trees are Larson's 1988 split VP, Pollock's 1989 split IP (upon which Chomsky's 1993 AgrS, T, AgrO model is built), Rizzi's 1997 split CP, Cinque's 1999 ff. cartographic project, and many more.
The respective sources are: Larson, Richard, "On the Double Object Construction," *Linguistic Inquiry* 19 (1988), pp. 335-391; Pollock, Jean-Yves "Verb Movement, Universal Grammar, and the Structure of IP," *Linguistic Inquiry* 20 (1989), pp 365-424; Chomsky, Noam, "A Minimalist Program for Linguistic Theory," in Hale,

early or middle 1990s.

MS: Yes, I mean one difference would be, of course, that technically there isn't such a thing as Deep Structure any more, but of course you have to have an enormously rich syntax, giant trees.

RJ: That's right, you need very rich syntax. The way you generate it is a little different. But, you know, generative semantics inserted words after some raising operations; well, that's exactly what happens in Distributed Morphology.[36] So we have that today, too. All of these elements have reappeared in slightly different technical form and maybe some leaner machinery, but it's still the same conception of language. And what happened to me was this: At some point very early on after I'd started working on this interpretive theory, somebody [I think it was Dick Carter] said, well, you're talking about how syntax relates to meaning, but you haven't really said anything about meaning! Interpretive semantics was trying to figure out the syntax-semantics interface, but it was only looking at what was on one side of the interface, the syntax side.

MS: Yes, that would be when something like linkage rules would come in, right?

RJ: Right, so really you have to ask, What are the meanings like? And I started working seriously on meanings – well, there was some material in my dissertation – but I really got serious in a paper I published in 1976.[37]

MS: What was your dissertation about?

RJ: It was called *Some Rules on Semantic Interpretation in English*,[38] and it was a proto-version of *Semantic Interpretation in Generative Grammar*, my first book.[39] The conclusion I came to in that book was – and it didn't make any sense at the time – to say that there are four different aspects to semantic interpretation that are sort of coordinated with each other. Everybody thought of a meaning as this sort of unified object, as do the formal semanticists now: you just cram everything into one massive lambda expression.[40] Well, a few years later, the phonologists started thinking about *tiers*. And I said, Oh, that's exactly the right idea, that is, semantics comes in *tiers*. So there is a tier that has to do with arguments and functions and modifiers, and there is information structure (topic and focus), which is completely orthogonal to that. Things in information structure have to be kept in registration with function-argument structure, but you don't notate them in the same structure. They have different primitives, in the same way that syllable structure and intonation are correlated, but

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Kenneth & Keyser, Samuel J. (eds.), *The View From Building 20. Essays in Honor of Sylvain Bromberger*, MIT Press, Cambridge, MA, 1993, pp. 1-52; Rizzi, Luigi, "The fine structure of the left periphery," in Haegeman, Liliane (ed.), *Elements of Grammar*, Kluwer, Dordrecht 1997, pp. 281-337; Cinque, Guglielmo, *Adverbs and Functional Heads. A Cross-Linguistic Perspective*, Oxford University Press, Oxford 1999.

[36] On Distributed Morphology, see Halle, Morris, and Marantz, Alec, "Distributed Morphology and the Pieces of Inflection," in *The View from Building 20, Essays in Linguistics in Honor of Sylvain Bromberger*, MIT Press, Cambridge, MA, 1993, pp. 111-176, and more recently, Embick, David, and Noyer, Rolf, "Distributed Morphology and the Syntax/Morphology Interface," in Ramchand, Gillian, and Reiss, Charles (eds.), *The Oxford Handbook of Linguistic Interfaces*, Oxford University Press, Oxford 2006, pp. 289-324.

[37] Jackendoff, Ray, "Toward an Explanatory Semantic Representation," *Linguistic Inquiry 7.1* (1976), pp. 89-150.

[38] This dissertation was accepted at MIT in September 1969.

[39] Jackendoff, Ray, *Semantic Interpretation in Generative Grammar*, MIT Press, Cambridge, MA, 1972.

[40] Mathematical calculus developed in the 1930s by the mathematician Alonzo Church and others; it is extensively used in formal semantics.

they have different primitives. And then when Lerdahl and I started working on music –[41]

MS: When did you start that?

RJ: Oh, we started around 1973 or '74, after Leonard Bernstein came to give the Norton Lectures at Harvard in 1973. He had just read Chomsky, and so he was really wild about working out a grammar of music, and showing that there is a universal grammar of music. He had really the right meta-theory but his theory was terrible. But, you know, his heart was in the right place, he wanted to do the right thing. And Lerdahl and I started working on music trying to carry out his program and do it right. The way we started out was with the standard generative grammar view: you start with Piece goes to Phrase, Phrase goes to something else, and so on – and we found we couldn't do it. We couldn't make it work.

MS: What was the obstacle, looking back?

RJ: Well, we realized at the time that for any piece we could generate, we could generate it not only with the right structure, but also with vast numbers of wrong structures. So you needed a component that would say, Out of these possible structures, which one is the best. And then we found that, as in semantics, we had to partial the structure into different components with different properties that interacted with each other. One component was rhythm, which in turn was a composite of grouping and meter, which were independent representations. The metrical theory we more or less borrowed from…

MS: It would also be multi-tiered.

RJ: Yes, but we borrowed the metrical theory from phonology, actually. I had a student at the time, Alice Singer, who already had started doing that, who had borrowed the Halle-Keyser metrical theory to do the metrics of Balkan dances,[42] which are really very, very complicated. So we ended up again [25:00] with this multi-component grammar for music that was not generative, it was constraint-based. There was no such thing at the time. It looked completely weird and everybody said, Ugh, what is this? Nowadays, you know, LFG[43] has multiple components, and so does my Parallel Architecture, and of course phonology has multiple components. So a lot of this would seem more natural now than it did then. Going back to semantics, I started developing this theory of meaning, starting out with Gruber's[44] insights about the relations of different semantic fields to spatial semantics, and this was really at the beginning of the cognitive science movement.

MS: Yeah, so for one thing you did a lot of work and you can also see it in the titles of your books like *Semantics and Cognition*,[45] *Semantic Structures*,[46] which had the obvious reference

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[41] For one of the results of this work, see Lerdahl, Fred & Jackendoff, Ray, *A Generative Theory of Tonal Music*, MIT Press, Cambridge, MA, 1982.

[42] Alice Singer, "The metrical structure of Macedonian dance," *Ethnomusicology* 18.3 (1974), pp. 379-404. Transcriber Iwo Iwanov (p.c.) remarks: "These dances usually display odd-numbered metrical organization. West: 4/4, East 7/8, 11/8, 13/8. Very non-intuitive if you haven't been socialized with it – your ear is just not tuned to it. Worst is Indian stuff. African stuff goes straighter, but sometimes not less complex. The radio killed it all."

[43] Lexical Functional Grammar, the syntactic model developed by Joan Bresnan and Ronald Kaplan in and since the end of the 1970s; for two presentations within roughly two decades, see Bresnan, Joan (ed.), *The Mental Representation of Grammatical Relation*, MIT Press, Cambridge, MA, 1982, and *Lexical-Functional Syntax*, Blackwell, Oxford 2001.

[44] See Gruber, Jeffrey, *Studies in Lexical Relations*, Ph.D. dissertation at MIT accepted in September 1965, published under the same title by North-Holland, Amsterdam 1976.

[45] Jackendoff, Ray, *Semantics and Cognition*, MIT Press, Cambridge, MA, 1983.

to the 1957 book[47] and your dissertation, so you were doing a lot of work in semantics.

RJ: Yeah, and the thing that really struck me was if all this stuff is based on spatial language, then we should look at spatial language, because it's the richest of all the domains. You can draw pictures of it, so you can tell what it means, you can do psychophysics on it, it can access the visual system. And the understanding of space should be evolutionarily old. So it has all these interesting –

MS: Yes, because it also has the necessary component for animals to finding their way around the world.

RJ: Exactly. It gives you a way of grounding semantics in something other than language, of finding boundary conditions other than language. So you could start thinking about an evolutionarily based theory of meaning in the spatial domain. And I worked for many years on spatial language.

MS: Did that also lead you to write this book which kind of reminds me of your latest book, namely *Consciousness and The Computational Mind*,[48] because that was, as far as I understand it, very much about embedding the language faculty in the rest of cognition?

RJ: That's right. My old friend John Macnamara,[49] who was a psychologist, a child language person at McGill, wrote a paper, which he never published, called "How Do We Talk About What We See?" What a great question, right? His point was that the information has to get from the visual system across to language. How does it do that? Through what representations? And also around that time, David Marr's work started coming out on a representational theory of vision[50] and I said, Oh, now if we have an interface between syntax and semantics, and semantics is this algebraic structure that I've been using, then we can make another interface from this algebraic structure to Marr's 3D model, and now we can get all the way from the eye to the vocal tract. We can talk about visual processing culminating in the 3D model, which can then be expanded into some notion of spatial representations in general. And then that teams up with the algebraic structure to say how we understand an action in terms of its actors and reference objects and so on, and that's what maps into language. So now we have language embedded in the structure of the mind in general. That struck me as really the right way to go. And since then, a lot of people have been working on spatial language, such as the whole Levinson group and so on.[51] Leonard Talmy[52] was also deeply into this stuff at that time.

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[46] Jackendoff, Ray, *Semantic Structures*, MIT Press, Cambridge, MA, 1990.

[47] Chomsky's *Syntactic Structures*.

[48] Jackendoff, Ray, *Consciousness and the Computational Mind*, MIT Press, Cambridge, MA, 1987.

[49] John Macnamara (1929 – 1996) was Professor of Psychology at McGill University, and the author of Names for Things, MIT Press, Cambridge, MA, 1984.

[50] Useful descriptions of Marr's work can be found in Part II ("Vision") of Johnson-Laird, Philip, *The Computer and the Mind. An Introduction to Cognitive Science*, 2nd edition, Fontana, London 1993, and chapter 10 of Gardner, Howard, *The Mind's New Science. A History of the Cognitive Revolution*, Basic Books, New York 1985. The most important primary source is Marr, David, *Vision*, Freeman, San Francisco 1982.

[51] Representative for some of this, see Levinson, Stephen C., *Space in Language and Cognition: Explorations in Cognitive Diversity*, Cambridge University Press, Cambridge 2003.

[52] Leonard Talmy (* 1942) is Professor Emeritus of Linguistics at the University at Buffalo, State University New York and the author of the two volume work *Toward a Cognitive Semantics* (Vol. 1: *Concept Structuring Systems*; Vol. 2: *Typology and Process in Concept Structuring*), MIT Press, Cambridge, MA, 2000.

MS: Yeah, and of course you also had the books, the first, the popular one, *Patterns in The Mind*,[53] which to my understanding was also very much about that. To say, listen folks, on the one hand we have language, and basically of course it is about language, and it has this structure, but there is much more in the mind going on with which this thing language interacts somehow.

RJ: That's right. So I was doing all this work on spatial language and integrating language into the mind, and somewhere in there, as the semantics got richer and richer, [30:00] it started to look more complicated than the syntax. And I remember sending some paper to Chomsky [laughs] and at the end it said, This is interesting, because it looks like semantics is getting more complex than syntax. And he really didn't care for that conclusion.

MS: Yes, what did he say?

RJ: I don't remember, but it was something like: Well, we really don't know how complex syntax will turn out to be.

MS: When about would that have been?

RJ: Oh, I don't know, probably the late 1970s, something like that. My feeling was, OK, even if that's what Noam thinks, I've got to keep working on this anyhow, because it's really interesting. I can't trace all my lines of thinking, but they were pushing me towards this parallel model that said, Well, in addition to syntax, there is the semantics, and in fact you need the semantics just to be a chimpanzee, right!

MS: When would you say it took shape? Or is it even possible to say that?

RJ: Well, it's hard to tell. You know, a couple of years ago I published this collection of my old papers,[54] and now I read them and find myself saying, Wow, I already knew that, but I didn't *really* know it. [Laughs.] It was just beginning to unfold. Another important influence came along when I was spending a year (1983-84) at the Center for Advanced Study in Stanford, and there were a whole lot of primatologists there. Robert Seyfarth and Dorothy Cheney were writing their book *How Monkeys See the World*[55] and Richard Wrangham[56] was there and Tom Struhsaker[57] and Barbara Smuts.[58] They were talking about all this primate social behavior, and I said, Hmm, this looks familiar, a lot like humans. Seyfarth particularly was interested in the theory of how this works, what conceptual representations the monkeys need, what they must know. And I said, You know, this is part of their conceptual representation, too. It's part of what the monkeys see in the world (as Cheney and Seyfarth put it). They see social relations, which are totally abstract. So that became another aspect of semantics that we had in advance of having language. Which eventually led me to do all the stuff on conceptual structure of social predicates, which is in *Language, Consciousness, and*

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[53] Jackendoff, Ray, *Patterns in the Mind. Language and Human Nature*, Basic Books, New York 1994.

[54] Jackendoff, Ray, *Meaning and the Lexicon: The Parallel Architecture 1975 – 2010*, Oxford University Press, Oxford 2010.

[55] Cheney, Dorothy L. & Seyfarth, Robert M., *How Monkeys See the World: Inside the Mind of Another Species*, University of Chicago Press, Chicago 1992.

[56] Richard Wrangham is Professor of Biological Anthropology at Harvard University and author of the book *Catching Fire. How Cooking Made us Human*, Basic Books, New York 2009.

[57] Thomas Struhsaker is an evolutionary anthropologist at Duke University.

[58] Barbara B. Smuts is an anthropologist and psychologist who does research with dolphins, monkeys, and apes; she is the author of the book *Sex and Friendship in Baboons*, Aldine, New York 1985.

Culture.[59] It took a long time for that to mature into part of the theory. But I kept it in mind over the years. It's also present pre-linguistically, along with the spatial stuff. That means you can do all of that [or at least a lot of that] *without* syntax. That means semantics must be autonomous of syntax. You don't need syntax to generate meanings, to generate concepts that are combinatorial and maybe recursive. At some point this somehow interfaced with a theory of lexical insertion. It was a very complicated story that I can't reconstruct now, but somehow it led to this idea of parallel architecture. I said, Well, look, phonology also can't be generated from syntax. The phonologists have all these crazy structures that are not syntactic. An intonational phrase is not a verb phrase. It's not a noun phrase, and sometimes it's not a syntactic phrase at all.

MS: We saw that in "yes, today" and "yesterday."[60]

RJ: Yes, that's right, in Hubert's papers especially. And that suggested to me that we should see language as these three components, each of which might have in addition tiers that articulate them more fully – certainly phonology does and I thought maybe semantics does, and if you believe LFG, then syntax does as well, right?

MS: Yes, where you have c-structure and f-structure.[61]

RJ: Exactly. And Jerry Sadock was doing syntax that way with his Auto-Lexical Syntax[62] and Van Valin was doing it that way in Role and Reference Grammar,[63] where he separated syntax and morphology, and then had information structure and functional structure in the semantics, and interfaces going every which way. And it became clear that it was possible to write [35:00] interface rules, rules that say, If you have this structure in this component, then it can be linked with this other structure in this other component -- and now we can set up a grammar that incorporates that sort of rule. And this turned out to be completely counter-intuitive to standard generative grammar, where the syntax generates everything.

MS: When you were doing this work, in the beginning, the late seventies, when you had that exchange with Chomsky, Chomsky's and other people's new model came around, the Principles and Parameters Theory, in the guise of being Government and Binding as everybody called it. How did you relate to that?

RJ: I didn't really trust it, but I didn't object to it. It was syntax, and at that point in time I thought syntax could work any way it works, that's fine with me. I was trying to figure out the meaning and how much it determines the syntax. And what made syntax come out differently for me? It was the issue of lexical insertion. I thought that if we really want to be pure about these different components, we don't want to insert words into syntax, because words have

[59] Jackendoff, Ray, *Language, Consciousness, Culture. Essays on Mental Structure*, MIT Press, Cambridge, MA, 2007.

[60] Reference is to the paper that the German linguist Hubert Truckenbrodt gave at the March 6/7, 2012 Konstanz Conference on "Complex Sentences, Types of Embedding, and Recursivity," where RJ and MS were both present. The interview was done directly after the conference.

[61] These two abbreviations from the theoretical inventory mean "constituent structure" and "functional structure," respectively.

[62] With regard to this, see Sadock, Jerrold M., *Autolexical Syntax: A Theory of Parallel Grammatical Representations*, University of Chicago Press, Chicago 1990, and more recently, Sadock, Jerrold M., *The Modular Architecture of Grammar*, Cambridge University Press, Cambridge 2012.

[63] For Van Valin, see Van Valin, Jr., Robert D. & LaPolla, Randy J., *Syntax. Structure, Meaning and Function*, Cambridge University Press, Cambridge 1997.

phonology and semantics, and phonology and semantics don't belong in syntax. Rather we want to see words as interfaces that *connect* phonology and semantics with syntax. But then if you do that, then you can't have derivations in syntax in the usual way – you might be able to do Distributed Morphology kind of syntax, I'm not sure. And I was getting more attracted to models like HPSG[64] and LFG that are constraint-based. And a lot of people were asking, "Can you do without movement?"

MS: Yes, and which are syntactically leaner. For example, you don't have PRO, you don't have movement. Assume only as much structure basically as is visible, and don't have all these – I mean there's a nice formulation, there's a book co-edited by Grewendorf and a few other people which came out at the end of the 1980s, and they had the phrase in it, well, "Even a simple sentence looks like a Swiss cheese."[65]

RJ: That's right, yeah, and that didn't seem right. But a lot of people were thinking along those lines, and I'd been collaborating with Culicover by then for quite a while. We're old friends back to graduate school, and all of the things we chose to work on were kind of weird constructions, where it seemed like there were things going on that should have been syntactic according to the then current understanding of syntax, but you couldn't do them syntactically. You couldn't get the things to move to the right places, there were things you couldn't delete, there were things that are single words, but they looked like they had binding structure inside of them, all kinds of things like that.

MS: Can you give an example for this?

RJ: Oh gosh! The first paper we did together was about "someone else."[66] It behaves like it's anaphoric, but instead of "same reference" or "same sense" it means "distinct reference" or "distinct sense," right? For example, in "John bought an apple and Bill bought something else," "something else" means 'something other than an apple.' I can't remember how it all worked, but it had for instance crossover properties like a pronoun, and it was behaving as though it had the structure "something other than," with internal structure, syntactic structure. But it was silly to derive "else" syntactically from "other than that". But suppose you say the binding is really going on in the semantics and "else" *means* "other than." Then we can try to put some of the binding conditions over in the semantics and see what happens. We ended up showing this is a lexical item that has internal *semantic* structure, and it's getting its binding behavior by virtue of the internal semantic structure, while it's syntactically simplex.[67] So this analysis proved that binding, which everybody had assumed is a syntactic relation, is actually mostly a semantic relation, and most of it doesn't belong in syntax at all.

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[64] Head-Driven Phrase Structure Grammar, like LFG, a non-transformational model of generative grammar; two of its leading proponents are Carl Pollard and the late Ivan Sag; see, *inter alia*, Sag, Ivan & Pollard, Carl, *Head-Driven Phrase Structure Grammar*, University of Chicago Press, Chicago 1994.

[65] See Günther Grewendorf, Fritz Hamm & Wolfgang Sternefeld, *Sprachliches Wissen. Eine Einführung in moderne Theorien der grammatischen Beschreibung*, Suhrkamp 1987, p. 227: "One might perhaps find it offensive that the representation of a sentence such as *Hans singt* quite a bit resembles a Swiss cheese, since in our theory there have to be at least three instances of movement." (The authors then argue that the topological model often applied to German syntax must also in many cases postulate empty slots in certain topological units.)

[66] Culicover, Peter & Jackendoff, Ray, "Something Else for the Binding Theory," *Linguistic Inquiry* 26 (1995), pp. 249-275.

[67] The topic discussed in the previous paragraph is also taken up in chapter 11 of Culicover, Peter & Jackendoff, Ray, *Simpler Syntax*, Oxford University Press, Oxford 2005.

MS: As far as I understand Culicover and you, you would argue that having a simpler syntax and having interpretive rules in the semantics still makes the overall system simpler than what you said about baroque trees.

RJ: Yes, in the sense that you need the semantics anyway, as I said earlier. You can't get away without the semantics, because that's what you're running your inferences on. All the semantic structure has to be there for inference [40:00] to take place, to understand the sentence. You also know the phonology has to be there, because you pronounce the sentence. So if you want to minimize the complexity of the grammar, the place to look is the syntax. How much syntax can you eliminate? And so we said, Let's see what we can do, and we got as far as we could, and maybe someone could get further in simplifying it, I don't know. But that book was our attempt, and it built on all the earlier work we had done on quite a lot of different constructions.

MS: We are now almost in the present, and so, while we are at the present, let's take a big leap backwards and let me pose the question I couldn't pose yesterday, namely, how would you frame this stuff you talked about yesterday[68] and we were talking about now, in evolutionary terms? I mean, with the understanding, of course, that we don't know.

RJ: Right, I mean I agree with Chomsky on this, that, you know, there is no evidence.

MS: Yes, just more or less well-informed speculations –

RJ: Right. I think the best way we can approach it is try to reverse engineer. What parts of it could we strip out and still have a communicative system that worked? You couldn't strip it down to government[69] if you didn't have any words, right? You have to have something like words very early on, otherwise the system isn't doing any good. You can't strip it down just to syntax, because syntax doesn't have any adaptive use except for connecting meaning and phonology. But you could have meaning and phonology connected without any syntax, and that would be a communicative system. It might be a crappy communicative system –

MS: For example what you call the one- and two-word stage.

RJ: Well, especially the two-word stage and the concatenation grammars. Sometime in the early '90s, Bickerton made his proposal about proto-language as an early stage in the evolution of the language capacity,[70] and my work sort of elaborates his idea and tries to refine it a bit. I don't think Bickerton accepts my interpretation of his work [laughs], but, whatever, I give him credit for a really great idea. If you're trying to reverse-engineer language, basically what you want first is words and phonology connected to meaning, and then syntax comes along to help you deal with larger utterances and be more expressive and precise.

MS: Where then do you get the interpretive rules from?

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[68] Reference is to RJ's paper at the Konstanz conference the previous day: Jackendoff, Ray &Wittenberg, Eva, "What You Can Say without Syntax: A Hierarchy of Grammatical Complexity," to appear in Newmeyer, Frederick & Preston, Laurel (eds.), *Measuring Grammatical Complexity*, Oxford University Press Oxford 2014. For an abstract, see, http://www.wittenbergkuypers.de/Eva/publications_files/Even%20Simpler%20Syntax.pdf, for the full text, http://www.wittenbergkuypers.de/Eva/publications_files/JackendoffWittenberg_Complexity.pdf.
[69] What's meant here is the technical term *government* from the Government and Binding (GB) stage of the Principles and Parameters Theory.
[70] Bickerton, Derek, *Language and Species*, University of Chicago Press, Chicago 1992.

RJ: I don't know. That's a *problem*. I mean, …

MS: As Jan-Wouter said: "I like problems."[71]

RJ: Well, I mean in the sense that these rules that I talked about yesterday, that apply to very primitive kinds of grammar, do scale up to the rules of full complex languages. Now, are they coming from some cognitive biases? Are they coming from a general, very primitive universal proto-paleo-grammar? I don't know. That's worth asking. But part of my message when I talk about this stuff is that your theory of the evolution of language depends on your theory of language (and I have a paper called this).[72] So if you have a Minimalist view of language, you are forced to quite a different view of language evolution than if you have a Parallel Architecture, Simpler Syntax view of language.

MS: Yes, because you always have to ask the question, "What is it that evolved?"

RJ: That's right. And it's at least my contention that you get more interesting stories from my view than from the Minimalist view, but you know, that seems to be a matter of opinion.

MS: OK, that's one small jump back in time, because that would seem to very much relate to the work Chomsky later on did, and here the topic would be X-bar theory. What's your memory about that, how that evolved? You had a lot to do with it, [45:00] and Noam.

RJ: Well he proposed it basically in "Remarks on Nominalization."[73] The lectures were 1967, and it's interesting, because he proposed a way to bypass some of the arguments for transformations. He pointed out that nouns, like verbs, also take complements,[74] and that there is something sort of like a passive in noun phrases, say "the city's destruction by the enemy." So the general idea is that the same sort of argument and modifier structure occurs in noun phrases as in sentences. That means you don't –

MS: And there is also this earlier parallelism in generative grammar between sentences and nouns where one thing was transformed into the other.

RJ: That's right, and that was developed especially by Lees,[75] but also in *Syntactic Structures* and probably also in [Zellig] Harris, right? So the only way to get arguments of nouns was to transform them from *sentences*. This was a little problem for nouns that take arguments like "friend" and "part" and "boss" that are not verbs. So, the idea was to capture these generalizations not by transforming one from the other, but by saying syntactic categories have a feature structure like phonology, and you can leave features unspecified in rules that apply to both nouns and verbs. And that was a –

MS: Yes, and quite obviously you would also have a relation between "destroy" and "destruction," but that would be in the lexicon.

RJ: Right. Now, he left open how that relation works. His theory of the lexicon in that paper

[71] This is a quote from Jan-Wouter Zwart at the March 2012 Konstanz conference.

[72] Jackendoff, Ray, "Your Theory of Language Evolution Depends on Your Theory of Language," in Larson, Richard, Déprez, Viviane & Yamakido, Hiroko (eds.), *The Evolution of Human Language: Biolinguistic Perspectives*, Cambridge University Press, Cambridge 2010, pp. 63-72.

[73] Chomsky, Noam, "Remarks on Nominalizations," first circulated in 1967 but published only much later, *inter alia* in *Studies on Semantics in Generative Grammar*, Mouton, The Hague 1972, pp. 11-61.

[74] As in, e.g., *the enemy's destruction of the city*, which is in fact Chomsky's example in "Remarks."

[75] Lees, Robert B., *The Grammar of English Nominalizations*, Mouton, The Hague 1960. Chomsky's "Remarks" were in large part a critique of Lees' 1960 (and his own earlier) theories about nominalizations.

was really very rudimentary. He never elaborated it. One much later elaboration is Distributed Morphology. I tried to take lexical relations seriously in a paper I published in '75, "Morphological and Semantic Regularities in the Lexicon,"[76] which I'm now coming back to. There were all these things we didn't know how to do in those days, that we might know how to do now a little better. But X-bar theory in some ways undermined a lot of the work that Lakoff and Ross were doing, and that might have been one reason why Chomsky did it, you know [both laugh]. My book *X-Bar Syntax*[77] was just really trying to work out the consequences for the phrase structure of English. And there were points where I ran into *terrible* problems that I don't think anybody has solved since.

MS: Yeah, I was posing that question to Luigi Rizzi also, and I think everybody who really bothers to deal with it will say that very same thing, it's not so clear. It's clear that something like it is there, but how it really works is difficult. For example if I have structures, and I was always very dissatisfied to have to teach that to my students, structures with like "right into the barn." Why do you say this "right" is a specifier instead of an adjunct? I never understood that.

RJ: Well, I had two chapters on specifiers, and they are full of crazy little constructions.[78] And there was no way to capture these constructions. Now we have Construction Grammar, which helps. Around the late '80s I got interested in Construction Grammar. I was coming to it from issues of argument structure, asking if you can simplify the lexical entries of verbs by saying that not all the frames that they occur in are specified by the verbs. I was thinking about things like "He buttered the bread with rancid butter," where you're adding an adjunct that's supplementing the meaning of the verb. And then I got into these examples like "He belched his way out of the restaurant." It turned out that Adele Goldberg,[79] who was a graduate student at the time, was working on the very same thing. That's when we got to be friends.

MS: Or "sneezed the handkerchief off the table" –

RJ: Exactly. And so in my 1990 book *Semantic Structures*[80] I was looking at these sorts of things and saying Well, there are different ways you could treat them. You could do it by movement, but nah, that doesn't work. You could do it in the lexicon, which was sort of Rappaport-Hovav and Levin's approach:[81] you say there is a lexical rule that changes "sneeze" into "sneeze NP prepositional phrase," and that's [50:00] a new lexical item. And then there's the constructional approach that says that a structure consisting of a verb plus "his way" plus a prepositional phrase is itself a lexical item that composes with a verb. And that led to a view of the lexicon first of all as highly redundant, and second, as containing not only words but also idioms and constructions, and eventually phrase structure rules – but all of it is in a uniform format. So it's very congenial to the Construction Grammar approach. I have some differences with the Construction Grammarians, but I needn't go into them.

MS: But the general idea that you would have about this would be that there is a whole range

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[76] Jackendoff, Ray, "Morphological and Semantic Regularities in the Lexicon," *Language 51* (1975), pp. 639-671.
[77] Jackendoff, Ray, *X-Bar Syntax: A Study of Phrase Structure*, MIT Press, Cambridge, MA, 1977.
[78] *X-Bar Syntax*, chapter 5: "NP Specifiers" and chapter 6 "Specifiers of X'"."
[79] Adele Goldberg was also present at the 2012 Konstanz conference and gave a paper there.
[80] See note 46.
[81] See, *inter alia*, Levin, Beth & Rappaport Hovav, Malka, *Argument Realization*, Cambridge University Press, Cambridge 2005.

of things. Like you have constructions and even phrase structure rules within the lexicon, but you also have phrase structure rules outside of the lexicon.

RJ: No, no. The way it's coming out is that your knowledge of language consists of pieces of structure you can use to put together sentences. They're clipped together by Unification rather than Merge,[82] but who's counting, right? So if the focus is, What do you store, and what can you build online, that makes it very psychologically concrete. Call the stuff that you store the lexicon or whatever you like. In what form do you store it? What are the relations among those items such that you can explain experimental results in lexical access and processing and so forth? So it can come very close to a performance theory.

MS: Yeah, I was just going to ask that. How does that relate to that ancient and honorable distinction between competence and performance?

RJ: Well, I mean it's interesting, the competence-performance distinction has been used as a firewall to protect linguistic theory from experimental results: people say, We're just studying competence. But on the other hand you *should* want a theory of competence that can embed in a theory of performance: How do you use this stuff that you know? And with this notion of the lexicon as your knowledge of language, where there are just pieces of structure that you can assemble in working memory, now it comes very close to a competence theory that tells you what the possible structures are, while the performance theory says how you construct those structures in real time.

MS: Yes, so there is still a difference and a gap, but no longer a yawning gap.

RJ: That's right. Now we can say, Here are some potential structures that the competence theory allows, but they are really hard to process, so we still have that same rhetoric available if we want it.

MS: Yes, but of course you also want to explain things like center embedding and garden path sentences and so on and so forth.[83]

RJ: That's right. And all the notions that the psycholinguists have now are found directly in the linguistic theory. Psycholinguistics for the most part has been operating with this sort of watered down view of syntax, a sort of introductory syntax, Syntax 101, which is what *Simpler Syntax* thinks there actually *is*!

MS: Yeah, I taught a course in psycholinguistics with which had the minimal attachment

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[82] *Merge* is the core operation in Chomsky's Minimalist Program: Syntactic structure is built by putting syntactic objects (either lexical/functional items or more complex objects that are already the result of the operation Merge) together in a binary fashion. In Unification-based approaches, entities are represented by feature structures, and they can only be combined through the unification of these features. A useful introduction to is Shieber, Stuart M., An Introduction to Unification-Based Approaches to Grammar, Microtome Publishing, Brookline, MA, 1986, 1988, 2003, http://langlex.com/tezu/IntroToUnificationBasedGrammar.pdf.

[83] Actually, self-embedding, where structures of the same type are embedded within each other, as in *The cheese the rat the cat chased ate stank*, where a reduced relative – ~~that~~ *the cat chased* – is embedded within another – ~~that~~ *the rat [reduced relative] ate*. The problem is also discussed in Chomsky, Noam, "Poor God. Must Be Bored to Death!," Interview with Michael Schiffmann, Cologne, June 7, 2011, pp. 15-16.

A typical garden path sentence (where sentence processing is "led down the garden path") is the famous *The horse raced past the barn fell*, where the passive form of the verb *race* is spontaneously interpreted as the homonymous past active form, leading to a breakdown in processing when the real matrix verb *fall* is encountered.

thing,[84] and that would be watered down syntax, of course, that's definitely not a baroque tree, it's a very simple one.

RJ: Yeah, right, that's the point. With this theory we really make contact with processing and we can ask, What are the semantic effects, and through what routes does priming take place and so on? And those questions now all make a great deal of sense. Now, in order to think this way, you really have to kind of turn your mind inside out if you're used to the usual way of thinking about generative grammar, and it took me a couple of years to get used to it.

MS: Yes, that's quite visible.

RJ: Because you really are changing the foundations of the way you think sentences are made and what sentences are and what meaning is and so on.

MS: Sure, right.

RJ: And so for example, you know, when I accosted Hubert [Truckenbrodt] yesterday, about how to think about prosody and syntax and semantics, he says: Well, that's really strange, I don't get it. But well, I've been doing it this way for five years, so it seems like a snap [makes a gesture] –

MS: For him it was a different language.

RJ: That's right, so you know, there is this communication gap, shall we say. [55:00]

MS: Yes. Do you perceive more communication going on like I think that happened in this conference,[85] that people at least talk to each other and even talk to each other in an accommodating way, like I work in a different framework, but you may be right after all.

RJ: Well, I think Adele [Goldberg] and I were the only ones who are really outside the mainstream tradition, don't you think?

MS: Oh, probably so. But what I felt was really that there was a lot of respect for you. Mutual, I felt the same from you of course. I also watched you like this [making a face], but that's normal after all.

RJ: Well, you know, it's a field where there is often a lot of not very nice behavior, and I lived through an era of that and was part of it and I'm trying to not repeat that [laughs], while at the same time making my differences clear.

MS: Yes, and since our planned time is almost up, I want to steer, as I told you yesterday, I want to go, also go a little bit into that difficult era. I suppose you were talking right now about what became to be called "linguistic wars,"[86] which probably really wasn't very nice. How did that go? What are your memories of that?

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[84] The "minimal attachment principle" says that the parser should attach a newly incoming phrase in such a way that a tree with a minimal number of nodes results.

[85] Again, MS refers to the March 6/7, 2012 Konstanz Conference on "Complex Sentences, Types of Embedding, and Recursivity."

[86] The debate between Chomskyan syntax and generative semantics in the late 1960s and early 1970s was often bitter and acrimonious, and this was true to such an extent that it gave rise to a book called "The Linguistic Wars"; Harris, Randy Allen, *The Linguistic Wars*, Oxford University Press, Oxford 1993. Another book that deals with the subject is Huck & Goldsmith, *Ideology and Linguistic Theory. Noam Chomsky and the Deep Structure Debates* (see note 22).

RJ: Well, you know the generative semanticists were all from the immediately previous generation of people out of MIT, right? Lakoff didn't get his degree at MIT, but he was there, teaching at Harvard. And Ross was a couple of years ahead of me, and McCawley was a couple of years ahead of me, and Postal[87] had been teaching at MIT and had just left. And out in the world, everybody who disliked Chomsky's version of generative grammar just thought generative semantics was great.

MS: Yes.

RJ: You know, everybody wants to shoot holes in his theory, and if you say he made a mistake, they say, Oh, good! I really wanted him to be wrong!

MS: That's the moment I've been waiting for …

RJ: I had this experience actually in my very first course as an undergraduate when we were reading Chomsky's mathematical linguistics papers with George Miller.[88] I was presenting them in class and I made a mistake and somebody pointed it out. And I said Oh, that's my mistake, I didn't get it right. And the professor said Oh, that's too bad, I was hoping *they* were wrong. [Both laugh.] You know, "I hoped they hadn't done it right."

MS: What was it like to study with George Miller?

RJ: I didn't study with George Miller.[89] I knew George Miller, but I didn't study with him. I met him because he was around MIT a lot. I don't remember if he was still at Harvard when I came to MIT. Anyway, getting back to the linguistic wars, "Remarks on Nominalization" was Chomsky's first response to the abstract syntax position, and all his then current students took up his side, for instance, Adrian Akmajian and Ray Dougherty and Janet Fodor and Peter Culicover and Joe Emonds as well as me. And when the generative semantics guys couldn't attack Chomsky, they attacked me.

MS: OK.

RJ: There was a lot of very nasty stuff, and here I was, a 23-year-old graduate student being attacked by guys who were big deals in the field. But on the other hand the one who was really a gentleman was McCawley. He was [60:00] really interested in my work and we talked and were very friendly despite our theoretical disagreements, throughout his whole life. We sent papers to each other, and I would get wonderful comments from him. But in general, it was very tough, though the fact that I had Chomsky backing me of course gave me confidence to keep going. I remember once showing Morris some paper and he said, You've got to remember you're not writing this paper for George, you know [laughs]. I mean there were a lot of hard things to learn, and it took many years of therapy getting through that.

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[87] Paul Martin Postal (* 1936) is the author of numerous linguistic works and teaches at New York University. He was one of the main proponents of generative semantics.

[88] Chomsky, Noam & Miller, George A., "Introduction to the Formal Analysis of Natural Languages," in Luce, Duncan R., Bush, Robert R. & Galanter, Eugene, *Handbook of Mathematical Psychology Vol. II*, John Wiley and Son, New York 1963, pp. 269-321; Chomsky, Noam, "Some Formal Properties of Grammars," *ibid.*, pp. 323-418; and Miller, George A. & Chomsky, Noam, "Finitary Models of Language Users," *ibid.*, pp. 419-491.

[89] George Miller (1920 – 2012) was a psychologist and mathematician whose work had an enormous influence on the development of cognitive science since the 1950s. He started out as a behaviorist but moved to a cognitive perspective on human behavior around the mid-1950s; in the late 1950s and early 1960s he co-authored a number of articles on language design with Chomsky.

MS: Did the fervor die down at some point?

RJ: Well, it died down because –

MS: You know, when I read in that *Ideology and Linguistic Theory* book,[90] when I read Lakoff or Postal, and by the way still when I do today, I have the feeling the fervor did *not* die down.

RJ: That's probably true.

MS: How did Chomsky relate to that? I mean he was also attacked a lot. How did he react to that?

RJ: I don't really know. Mostly he just kept attacking back. You know there were these classes, his Thursday afternoon lectures, and everybody in Cambridge would come. Haj [Ross] would be there and George [Lakoff] would be there and various other people and they would heckle him.

MS: It would be a free for all.

RJ: Right, it was a free for all. Well, you know, Chomsky's thrives on combat, right? I mean look at his politics.

MS: Yes.

RJ: You know, I don't know if Chomsky's combativeness at this time was any different from what he had done before, in dealing with the structuralists. But it was certainly a shock to the field. There was this one guy who attacked me at a talk I was giving, and he was asked later, Why did you do that? And this guy said, Well, you know, it's like we're a family and the members of the family are disagreeing with you.

MS: Those are the bitterest conflicts you can fight.

RJ: Yeah, that's right, and I think that was the way it felt to him. All of a sudden instead of this triumphalism, there was all this internal strife. And it had a very bad effect on the influence of linguistics outside.

MS: Oh sure.

RJ: All the psychologists and philosophers and artificial intelligence people thought this was great stuff. First of all, they really wanted generative semantics to be true (so Chomsky would be wrong). And second, they said, You guys have been telling us you have this theory, and we're supposed to test it – and all of a sudden you change the theory on us. So now you're saying meaning is *not* Deep Structure? What are we supposed to do?

MS: Exactly. It took us a year to set up this experiment, and you changed all the preconditions.

RJ: Yeah, and I've heard this complaint both in print and also in person from various psychologists and philosophers, and that sort of began the alienation of linguistics from the rest of the cognitive sciences. Linguistics is off on its own, doing its thing and saying, We're doing competence, you're doing performance, that's fine. This has not been good for the

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[90] See note 22.

influence of linguistics. For instance, I was once told [65:00] by some very prominent neuro-scientists, "When we were being trained, we were told, Oh don't read any of that generative grammar stuff." Or maybe it was don't read any of that Chomsky stuff, I don't remember.

MS: I can imagine. When I interviewed him at the MIT, Morris Halle complained in the other direction. These guys are not paying attention to us anymore.[91]

RJ: That's right. And there are some good sociological reasons and some good theoretical reasons, because I think mainstream generative grammar has really lost touch with psychological reality[92] – at least from my point of view. I'm really trying to get a theory that is closer to the psychological grounding. So now we have bio-linguistics, and it talks about biology, but it almost never talks about psychology.

MS: Alright, let me close off with a final question once more about Chomsky's work. What would be your preliminary assessment of what he has done? Let's hope he'll be around for another couple of years and start new revolutions. What would be your assessment now?

RJ: Look, I think he is definitely one of the great intellectuals of the 20th century. He is up there with Einstein and Freud and Bertrand Russell. What he created was a way of thinking about language that on one hand has this psychological, biological grounding and on the other hand is formal, and it's the combination of those two that gave generative grammar its cachet. Who can tell, but I think he will probably be remembered sort of like Freud. Nowadays, everybody says that Freud didn't know what he was talking about, he was totally nuts. But the foundations of Freud's work, everybody takes for granted now. If you go to a therapist, your work is based on the notion that you have repressed ideas and ways of seeing the world that you are not aware of, and you should be aware of them. We take all that stuff for granted. The content of his theory was probably not right. The fact that it's all about sex was partly a product of his milieu – turn-of-the-century Viennese women, right?

MS: Later he added Thanatos when things really went wrong in Germany and elsewhere.[93]

RJ: That's right. In a sense I think his theorizing gradually lost touch [70:00] with the grounding in actual practice. Going back to Chomsky, I think the ultimate thing is that, even if all the machinery he's proposed over the years is wrong, we now all think about language as a mental object that has to be acquired, that has to be processed in the brain, that there is this creative grammar that allows us to do it, that it's telling us something about what to expect in typology. And more generally, the cognitive revolution is partly due to him. I'm trying to

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[91] Cf. Halle's remark: "We speak in real-time. I mean there is no question about it. But how can we do it with such speed? And really, I think it's a failing on [the part of] the whole field of psychology that they don't take [this] seriously." See Halle, Morris, "Jakobson, Chomsky, and the Quest for Linguistic Rules," Interview with Michael Schiffmann, Cambridge, August 5, 2010, p. 12; a little further down the line, his response to the closely related question by MS "When you started this ‑ interpreting what you were doing as part of psychology ‑ you were a pretty isolated bunch of a few young people, is that right?" is also quite pessimistic: "It never took [roots], it didn't penetrate. You know, the way we felt – we really didn't succeed." *Ibid.*, p. 15.

[92] On this question, see Jackendoff, Ray, "Reintegrating Generative Grammar," chapter 2 of his *Language, Consciousness, Culture. Essays on Mental Structure*, MIT Press, Cambridge, MA, 2007, pp. 25-77; Jackendoff, Ray, "What is the human language faculty? Two views," *Language* 87 (2010), pp. 586-624,; Pinker, Steven & Jackendoff, Ray, "The Faculty of Language: What's Special about it?," *Cognition* 95 (2005), pp. 201-236.

[93] In Freud's later theory, there were two fundamental drives, Eros and Thanatos (the "death drive"), struggling for the upper hand in the psychological make-up of the person and the life of man.

think what it would have been like if it had been just George Miller and Marvin Minsky –[94]

MS: Right, that would make a difference.

RJ: It would have been really different, you know, without the third member of the triad. So I think he's made a major change in the way we think about language and the way we think about the mind and the way we think about people. And he deserves every bit of honor for that, and whether one likes his implementation or not, that part, that core really remains, and now the question is to find the right implementation. That's what I'm trying to find, an implementation that satisfies me more and makes more sense psychologically than his. But I wouldn't be in this business if it weren't for him, you know?

MS: OK, Ray Jackendoff, thank you very much.

RJ: You're welcome.

Transcript by Iwo Iwanov

Revised by Michael Schiffmann and Ray Jackendoff

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[94] Marvin Lee Minsky (* 1927) has for many decades been a very prominent researcher in the realm of AI (Artificial Intelligence), on which he has published a number of scientific and popular works.