

# *Review of The Linguistics Wars: Chomsky, Lakoff, and the Battle over Deep Structure*

John A. Goldsmith

October 19, 2022

## *Randy Harris's second take on the linguistics wars*

Randy Allen Harris: *The Linguistics Wars: Chomsky, Lakoff, and the Battle over Deep Structure*. Second edition. Oxford University Press. 2021.

Randy Harris has written an extremely engaging account of the rise of generative syntax and of some of the linguists who participated in this development, focusing on the scruffy fights that held a lot of people's attention in the second half of the 1960s, and then tracking the trajectories of the linguists after that very belligerent moment. The book is great fun to read—Randy is a terrific writer, the likes of which we rarely see among academics—and along the way, the reader learns a lot of linguistics. (We'll come back to that last point, though, because there are some points, not all of them fine points, which deserve some discussion.) This book is a greatly revised second edition of a book that came out in the early 1990s, and this new edition is longer and covers much more territory. While it is as punchy and pugnacious as the first, it is also more thoughtful and considered. Randy's academic specialization is rhetoric, so you'll learn a *lot* about rhetoric, and rhetoric has a lot to do with this story, which starts off as the story about the first rupture inside the group of young Turks known as generative grammarians back in the mid 1960s, pitting Chomsky and a few of his students, like Ray Jackendoff, against the four horsemen of the Generative Semanticists (Haj Ross, Jim McCawley, Paul Postal, and George Lakoff). Without conflict, there's no story to tell, so conflict is at the center of the book, but it's not an evenly matched conflict: it's CHOMSKY in upper case letters against the others, whose names are in lower case and not set out in neon lights the way a certain other linguist's are.<sup>1</sup>

And because Chomsky is at the center of the story, and because Randy (as a rhetorician) gives himself free rein to talk about what it is that makes Chomsky the extraordinary person that he obviously is, we end up with a very interesting perspective on just who this fellow Chomsky is. We'll get to that in a minute, but you should know that if you read this book, you'll get a pretty perceptive account of how it

<sup>1</sup> After finishing this review, I posted a video review on Youtube, based in part on this review, but different in form; feel free to take a look there. You will notice that in this review, I refer to most people by their first names in this review. In Randy's case, my inclination to call him "Randy" is magnified by the fact there are some other important Harris's right here who we are talking about, and anything that reduces confusion or ambiguity is a good thing. It's only Noam Chomsky who I have called mostly by his last name. Perhaps it's a generational thing, or a sign of respect, or both. I'm aware I'm doing it, not so sure exactly why.

is that Chomsky can be such a creature of contradictions: a man who is gracious, obstreperous, compassionate, contemptuous, courageous, and acrimonious, all of that rolled into one human being.

Because I'm writing a review, I'm obliged to tell you whether I think you should read this book, when all I know about you is that you've come from a place where you encountered this review. I don't know how much linguistics you know: on average, probably not too much, though I imagine that among the readers will be a very few of my colleagues in the field. There's a danger that if you don't know much linguistics and you read this book, you won't be aware of how much real linguistic work you're missing out on. But you know what? I'm going to recommend this book anyway. I'm going to object below to some of the things Randy wrote, but that's OK: you're still going to learn a lot by reading this book. He has a good ear for what's important and for what is suspicious, and I recommend this book both to the linguist who's been around the block and to the neophyte. I've chosen a few points to discuss where I thought Randy's account was not quite right. There were a ton more, but they'll have to wait for another book, another time.<sup>2</sup>

I took the opportunity to send this review to Randy when I had finished the first draft, and got back from him lots of suggestions where he thought I could have done better, mostly in representing what he did and what he intended to do, and I'm grateful for that—and I hope that my comments, when they are critical, will be better formulated than they were originally. Randy was concerned that my suggestion that I had a ton of points to raise with him might suggest an implicit charge of negligence or incompetence, and that's not at all what I intended; disagreement is the soul of conversation, of dialog, and it is what makes the intellectual world move forward—as I see it. So the best I can offer as a reviewer is to be able to say that this book is worth reading: I don't expect to go ahead and say I agree with everything that's in it.

The biggest part of the story that Randy tells involves the evolution in Chomsky's thinking about syntax during the 1960s and the early 1970s and the development of the views of others who cared about what Chomsky was writing, which was certainly most American syntacticians during this period. Chomsky's most influential work at this point was *Aspects of the Theory of Syntax*, published in 1965, and the generative semanticists took the model sketched there and pushed it in a direction that Chomsky found unconvincing. Chomsky's developments of the theory after *Aspects* went in a dif-

<sup>2</sup> I should say a bit about myself and my connections to this story. I know, or knew, almost all of the participants in this story, many of them very well: I was their student, friend or colleague. That includes Chomsky, Halle, Ross, McCawley, Jackendoff, Householder, Sadock, and Lakoff. I started out in the field in 1970 — the first LSA meeting I went to was in 1970, at the Linguistics Institute that summer at Ohio State University. I was a student at MIT from 1972 to 1976, and have great admiration for Chomsky's many talents. I also wrote a book, co-authored with Geoff Huck, on the events and ideas of the 1960s that Randy discusses in the present book. Randy cites it at several points, and he cites it fairly and accurately; the same goes for other places he references things I've written. I say that because this impression cannot help but calibrate my sense of how well he represents people and what they wrote, and I think he does a very good job. I'd also like to mention a book that I published in 2019 with Bernard Laks called *Battle in the Mind Fields*, which also delves into some of the question touched on here.

ferent direction, and there was a lot of interesting work and a lot of heated hostilities that resulted from these disagreements. But it's not just history that Randy looks at; he brings the story up to the present, focusing on how the characters who were active in the 1970s developed in their work in the decades that followed.

There might be some grumbling at this point. "Not just history," someone might mumble. "Yeah, right. This was the linguistics my *grandmother* studied back in the day." In this conflict, the elder statesman (that is, Chomsky) is now well into his 90s (he was born in 1928), and the young Turks are well into their 80s. A lot of the *students* of the young Turks have made it into retirement. So yes, it's history. But there are a lot of connections made to the present, many of the connections made explicitly in the book, and others lurking for the reader—like when Randy cites Lakoff and Ross writing in the 1960s, "we think we can show [that] lexical items are inserted at many points of a derivation" (p. 101), just like the developers of distributed morphology have observed more recently.

Here's an example of what Randy does well, writing about Chomsky's "Remarks on nominalizations" (a paper which was Chomsky's principal contribution in the years just after *Aspects*) and its relationship to three things: Chomsky's earlier work, the earlier work of his student Robert Lees, and the work by Lakoff and Ross, which was cutting edge work at that moment:

Chomsky repudiated successful early work, proposed radical changes to the *Aspects* model, and opened makeshift escape channels for those changes—all on the basis of quite meager evidence—with no more motivation, as far as anyone could see, than to hamstring the work of his most productive colleague (Postal) and of some of the most promising junior scholars in the field, including some of his own former students. Lakoff and Ross were shaken to the bone. (118)

This is linguistic history as no one else would write it. Randy actually attributes *personal* motives to linguists who are proposing *intellectual* positions, and the person who he most often subjects to this analysis—the person whose *views* are most often subject to such scrutiny—is Noam Chomsky. Over and over again Randy describes the scene in such a way that Chomsky's actions have a social meaning that is not hard to discern, but which is at odds with what Chomsky says he is doing, and in that sense, Chomsky's work, Randy implies, is far from transparent. Chomsky is trying to accomplish things in addition to what he *says* he is trying to accomplish, and there are motives to his writing which are different from those which

he acknowledged. And it's pretty obvious that Randy is right.

What makes this psychoanalysis even more charged than it might otherwise be is that Chomsky's reputation outside of linguistics is firmly based on his attacks on defenders of American foreign policies and on pundits who are incapable of seeing that while they justify American imperialism, they fail to identify the true motives of the political actors, and mistakenly take superficial and self-serving description of American motives for the real thing.

Now I, for one, largely agree with Chomsky's political attacks, especially the attacks he published in the late 1960s, those early years in his political career when he wrote like the prophets of the Old Testament, the prophets who shouted to the people of Israel that their God was not going to excuse their sins just because they were the chosen people. God would judge them by the same standards He would judge their enemies, the prophets intoned, and the pride of the Israelites was in no way justified by their acts. You might think that someone who could see through the thin veneer of self-serving hypocrisy in the world of politics would be able to apply those lessons to linguistics—but you'd be wrong. Alas, we are all human, and we are far too often mysteries to ourselves. And so a great deal of the story that Randy has to tell involves the mismatch between what Chomsky did and what he says he did.

Of course it is galling to be subject to such analysis, galling if you're Noam Chomsky or if you are someone who identifies intellectually with Chomsky's position. But Randy nowhere descends to what could be labeled as an attack. What he writes, in fact, is very different from the personal attacks that the young Noam Chomsky and the young Paul Postal launched against the linguists of the older generation, attacks where they said that the older generation simply were not able to understand the issues (or not able to understand the issues as well as Chomsky and Postal did, in any event).<sup>3</sup> Randy certainly doesn't *attack* any of the linguists whose work he describes and analyzes, and there is much that is striking (interesting, infuriating, novel, inappropriate) about what Randy is doing. He's not saying there are no intellectual arguments at hand; he largely keeps his thumb off the scale (if you don't get the metaphor, ask someone) as arguments go, but his criticisms hold water regardless of the validity of the arguments. He is saying that the personal quirks and animosities that all of us recognize in ourselves and in others are at play in linguistic research even when the players are doing their level best to deny it, to themselves and to others. "Doing their best to deny

<sup>3</sup> Randy gives an example of this on page 121, citing Chomsky:

I will not consider Reichling's criticism of generative grammar here. The cited remark is just one illustration of his complete lack of comprehension of the goals, concerns, and specific content of the work he was discussing, and his discussion is based on such gross misrepresentation of this work that comment is hardly called for.

it” means not talking about the animosities, most of the time, and only on rare occasions denying anything with explicit words. But doing one’s best to deny feelings of animosity is far, far different from actually liberating oneself from them.

There are a couple of times when Randy oversteps the line, I’d say, and comes out with a statement that the scientific arguments were not at the core. At one point (p. 128), he writes about the effect that Postal’s presence on the side of generative semantics had, and he says, “But his mere presence in the camp was almost enough on its own. Postal gave the movement its greatest source of credibility.” That comes very close to saying that it really didn’t matter what reasons or arguments Postal brought to the discussion, and that’s a really wrong view to hold; if Randy were to try to defend that, he’d undermine the worth of what he has to say. But, of course, he’s saved by his “almost.” Well, almost saved.<sup>4</sup>

Anyone who knows the syntax that was being done during the 1970s in the United States will be able to point to a lot of work that is not mentioned at all, or at best in passing; the three that come to mind first are the work inspired by Joe Emonds, that inspired by Joan Bresnan, and that inspired by Gerald Gazdar (along with Geoff Pullum, Ivan Sag, Ewen Klein, and others). But it would be foolish to criticize Randy for not including everything that anyone was doing: that’s not how history is written.

I mentioned earlier that Randy took objection to some of the comments I have made in this review, and he noted that<sup>5</sup>

many of the misunderstandings, I believe, stem from you losing sight at times that I am writing a rhetorical history—a history of appeals, currents of influence, attributions of credibility, argument structure, and the like—not a history of technical ‘accomplishments’ or disciplinary ‘truths.’

I’m not at all sure that the project that Randy defines in this way is possible. By that I mean that I am skeptical that one can cover the sort of things that he wants to discuss without getting into the details and the meanings of the concepts and the claims. I would be happy, in fact, if this review were read—at least in part—as a set of comments on why one can’t do a rhetorical history without relying on a complete intellectual history and analysis. Randy went on to say that the non-rhetorical part would have to be handled by linguists. “My concern is the process by which these accomplishments and truths are entered into the record, where the consensus and dissensus

<sup>4</sup> Defending himself against Chomsky’s charge that he’s a post-modernist, some sort of Foucaultian, Randy writes that in his book “there are no claims that science is a mere collection of power plays.” Of course that’s not what Foucault saying, but Randy told me that this wasn’t really his view of what Foucault said either; he said that he offered it as a paraphrase of Chomsky’s view, but the reader can’t guess that. This comment about Postal comes pretty darn close to supporting such a claim, as I see it. In his own defense, Randy objected that “nowhere do I say that arguments don’t matter,” and that is certainly true.

<sup>5</sup> October 8, 2022.

of the practitioners forms. What you seem to take as my judgement on linguistic technicalities are rather my judgement on the relation between their cogency and their uptake.”<sup>6</sup>

<sup>6</sup> *ibid.*

### *The main story line*

The story that Randy tells goes like this. Back at the end of the 1940s, a young student of linguistics at the University of Pennsylvania, working with a prominent linguist there named Zellig Harris, began to develop a new way of thinking about language and linguistics, and over a period of several years as a visiting scholar at Harvard as a Junior Fellow, he wrote a massive book called *The Logical Structure of Linguistic Theory* in which he set out his view. This young man’s name was Noam Chomsky. Up in Cambridge, Mass, he struck up a great friendship with Morris Halle, a former student of another prominent linguist, Roman Jakobson, who Chomsky met at Harvard. Halle, some five years older than Chomsky, landed a job teaching at the Massachusetts Institute of Technology, and he was instrumental in Chomsky landing a job at MIT when his fellowship came to an end. Together they set up a doctoral program in linguistics in the early 1960s, and it attracted a good number of enthusiastic and smart graduate students. Chomsky’s star rose rapidly in the linguistic sky, resting in part on the great success of his 1957 book, *Syntactic Structures*, and his 1965 book, *Aspects of the Theory of Syntax*. The theory that he offered was one that saw sentences as having two different structures: a deep structure and a surface structure. The deep structure was closer to the structure of the meaning the sentence expressed, and while one could study surface structure, doing so was pretty pointless if surface structure was not understood to be a poor and derivative reflection of the deep structure, mediated by a set of rules called transformations.

The generative semanticists—Haj Ross, Jim McCawley, Paul Postal, George Lakoff—proposed a version of this account in which deep structure was as close to the meaning of a sentence as its logical form can be. (If you’re not exactly sure what a logical form is, it may be of some succor that you’re not alone in that; what counts as logical form varies a lot from one linguist or philosopher to another.) And transformations might well be the tool needed to explore what that deep structure was.

Haj/Jim/Paul/George thought they were pulling in the same direction that Chomsky was, but Chomsky didn’t see it that way at all. At this point in the story, it’s helpful to introduce a distinction that

Randy discusses that comes from Huck and Goldsmith: the difference between studying syntax as distributional system, and studying it as a mediational system. The first approach tells us to figure out why words appear in sentences in particular orders, nearer or farther away from other words, sometimes even disappearing completely (disappearing? like when we say “Kim is interested in baseball, but I’m not”—i.e., but I’m not interested in baseball). The mediational approach to studying syntax, on the other hand, focuses on the surface structure relates to the deep structure, and in particular to the meaning associated with the word. Obviously—I think it’s obvious—both are valid and essential approaches to the study of syntax, but most linguists privately think that one of them is important, while the other is there to be of service to the one. Yet linguists disagree as to which is the really important one, and they rarely if ever make their convictions in this regard public.

Haj/Jim/Paul/George set off on a chase for the gold prioritizing the mediational view of language: we can understand language best if we recognize that the deep structure that linguists had been groping towards was the logical form of the sentence. Chomsky didn’t seem to agree at all, and rejected all the arguments that they came up with.

All of this disagreement led to a very disagreeable moment in the second half of the Sixties, and by around 1970 people gave up on the disagreements out of simple exhaustion. Did good things come out of this, good in a scientific sense? That’s one of the questions that Randy tries to answer, and certainly the answer has to be at the very least a qualified Yes, though as Randy shows<sup>7</sup> part of what was good was that Chomsky integrated the notion of logical form into his conception of language, a shift that was obviously the result of his taking Haj/Jim/Paul/George, and their work, seriously, though (Chomsky being Chomsky) he was never prepared to say so.

<sup>7</sup> And as Geoff Huck and I document as well.

But none of these people stopped doing linguistics, and Randy goes on to discuss the particular paths that each of these linguists has followed. He’s most interested in George Lakoff’s work, which he sees as central to the rise of cognitive linguistics since the 1970s, and in the work of Noam Chomsky, who went on, after the generative wars of the 1960s, to develop a series of models of grammar that continued to attract young linguists to work with him.

Randy discusses the rise of interest in pragmatics among some of the generative semanticists—George and Robin Lakoff for sure, and

Haj Ross to some extent. (It is notable that Robin Tolmach Lakoff and her work play a larger role in this second edition than in the first, and that is a welcome addition.) Pragmatics had been knocking on the door of linguistics for quite some time: the University of Chicago philosophers in the 1930s, only minimally influenced by Leonard Bloomfield also present there at the time, were strong believers that the student of language needed to be divided into syntax, semantics, and pragmatics, and it seems to me quite natural to read the English ordinary language philosophers as hoping to maintain that linguists' grammar could be replaced (everywhere but in the classroom) by statements about what speakers did in order to achieve certain results, which is to say, linguistics is reduced to pragmatics. But Lakoff and others working with him took up the cause that the study of pragmatics could not be left at the door when trying to figure out how the syntax and semantics of a language work. (Randy refers to "Lakoff's cheery arrogance about pragmatics" at one point (p. 344).)

The generative semanticists seemed to exude an ethos, one that was even more evident when one did not focus just on the four leaders. Randy tries to give a sense of what it was, and he does a good job. The ethos began with the fun that was at the center of doing linguistics, even if that fun came after studying lots of languages and doing a lot of work. The fun could be done by poking not so innocent fun at the leading politicians of the day, the Nixons and their Kissingers, who served to take the pratfalls of the example sentences generated by the generative semanticists. And sometimes the fun comes along with a disarming honesty, when the author acknowledges not having really solved the problem that they started off with.

Chomsky continued to develop his own approaches—a bit during the 1970s, when he continued to ask what his system would look like if there were very few transformations in each language. Perhaps even just one. And if one, then it was surely going to be the same one in every language. Like just, "Move!" —a proposal made in Chomsky's class one day by Mark Liberman.<sup>8</sup> By the end of the 1970s, he rolled out principles and parameters, and a division of the theory of syntax into semi-autonomous domains, like an account of thematic roles assignment, of abstract case-assignment (exploring an idea of Dorothy Siegel and Jean-Roger Vergnaud), and of constraints on movement expressed over representations with traces.

<sup>8</sup> I say that because I was there, as of course was Mark.

While Chomsky was exploring these reconstructions of grammatical theory, other linguists were sticking closer to the familiar (or so it seems to me), including Joan Bresnan, and Postal and David Perlmutter.



ter, and Ivan Sag, Geoff Pullum, and Gerald Gazdar.

The appeal of LFG [Brenan's model] and GPSG [Sag et al] was tinged with nostalgia. Their "attempts to preserve certain attractive features of the earlier phrases of generative grammar" [citing Tom Wasow] harkened back to the mathematical precision and formal rigor of early Generative grammar, to the "Three Models" and *Logical Structures* era. (280)

I don't think that's quite right, and I remember long discussions of this around 1983-84. There were many linguists—good friends of mine, even one spouse—who were appalled at the hand-waving that they perceived as coming from the Chomsky camp. They wanted, rather, to work on a model in which (to take one example) perfectly clear facts about gender and number agreement in a range of European languages would be handled by one and the same theory. The people working on this weren't at all interested in the logic or the mathematics of it; what they wanted was to see was the development of a framework in which all of the details of French and Romanian agreement (for example) could be adequately described. Not a beautiful picture of language, mind you, but a framework in which one could be faithful to the facts before one was faithful to the cause. Chomsky's Big Picture was magnificent, but the implementation lagged, for those who cared about the details of language (which tends to be a lot of people, when we're talking about linguists).<sup>9</sup>

<sup>9</sup> Randy is not so certain that there is any disagreement between what I've written and what he intended. "We seem to be in agreement here, but perhaps the word *nostalgia* frames it in a way you find unhelpful," he wrote. "I am making no claim that everyone was stampeding to math and logic. I am just saying that there was a desire for more precision and rigour than was apparent in the approach Chomsky was then advancing...."

Randy follows George Lakoff's path into the heart of metaphor in the years after this—the 1980s, and as a scholar of rhetoric, he lets the reader know what he thinks of all this. "Lakoff and Johnson's scholarship on this matter is disgracefully negligent (with the greatest guilt clearly falling on Lakoff, by far the senior scholar and the lead author)." (295) Randy sees George's engagement with metaphor in the book *Metaphors We Live By* as "the clearest transmutation of Generative Semantics into a new and vibrant framework." (298)

George, along with other linguists (many in California, like Ron Langacker and Gilles Fauconnier, but many not there too), had moved into what he called Cognitive Grammar, leaning as often as not on models in cognitive psychology, which had a historical root in gestalt psychology.

At the point where the book is halfway over—*only* halfway over, I mean, and page 301, if we want to be precise—the story moves into the 21st century. And each of the four horsemen of generative semantics gets his moment.

McCawley, in his post-bellum work, much more recognizably followed the early Generative Semantics paths than any of the other horsefolk, right up until he was unfortunately felled by a massive heart attack on the University of Chicago campus, in 1999, at the age of sixty-one.

Randy sketches a deft picture of the linguist that McCawley was, willing to engage in writing in disputes with Chomsky but never showing any irritation at being misrepresented, or at anything else, for that matter. He was in some central respects like Chuck Fillmore: Jim was a working grammarian, both pleased and proud to be uncovering the mysteries of English, Mandarin, Korean, and Spanish.

Randy offers a sympathetic account of Haj Ross's professional trajectory after the rocky years that were the linguistics wars, a trajectory that took him into the heart of poetry, in a way that he said was shown to him when he was a student by Roman Jakobson. Haj was indelibly the student of Zellig Harris, of Roman Jakobson, and of Noam Chomsky, and trying to bring the ideas of those very different souls into one package is probably a super-human challenge. At times he feared that by not creating or developing a theory, he was condemning his work to the margins of linguistics, but his work, which appeared less and less often in written form was always brutal in its honesty about what he found with his own methods of exploration.

Paul Postal is harder to characterize. One of Paul's ideas that Randy tries to explain is the idea that natural languages are abstractions in the same domain as mathematical objects, a notion very far from the view almost universally espoused by linguists today that grammar is a cognitive faculty of some sort. (p. 312) I'm not at all sure that Randy actually gets Postal's perspective, but then I'm not sure how many linguists do. I think that Postal's view is best understood by comparing it to the semantic movement of 19th century philosophy, which rejected what came to be known as *psychologism*, the idea that psychology is the foundational discipline for questions regarding logic, mathematics, and perhaps other disciplines. Despite the name, psychologism is not a way of doing psychology, but rather a view on what the import is of psychology for other disciplines and fields of thought. At the center of that view is the notion of a proposition, which is an abstraction and which is what you and I can both express if we choose to do so. A proposition is expressed (or, crudely, used) in an individual's statement, but two people can express the same proposition, showing that it, the proposition, is not the same as the utterance of it.<sup>10</sup>

<sup>10</sup> Randy is not convinced that he doesn't get Postal's position, which he described in an email as "pointless." In the email, he said that he takes propositions to be abstract objects, but sentences have "direct material representation." This is obviously not the place to delve into this question further, but it strikes me as a great example of something that we have to come to grips with at a deep conceptual level, and where a rhetorical approach only cannot stand on its own.

Randy presents two other developments with considerable enthusiasm: Raj Jackendoff's work over the last 30 years, showing a big-hearted willingness to accept ideas from other linguists regardless of their ideologies, and the work of cognitive grammar and, in particular, construction grammar, and Adele Goldberg's work comes in for special mention.

All of this is very interesting, but the best part of Randy's book, his whole book, is the 35 or so pages at the end, where he tries to put the pieces together and try as best he can to explain who Noam is, really. He talks us through Noam's *humanity*, the ways in which he's a normal guy, one who plays video games with his grandchildren. It made me smile, in a nice sort of a way, and I think this really is Noam, or at least one important part. And then Randy turns to the one important—nay, the one *essential*—aspect of his personality from which everything that's important flows: his dead-certain conviction that he knows the truth (and with that, alas, an inability to imagine that someone else can legitimately see things differently and still be right).

So Randy goes on to survey both the positive and the negative, and summarizing at one point, writes,

How can someone who is so utterly sincere also be so utterly reckless with facts, and especially with the reputations of others, that it becomes indistinguishable from malice? And, let's not forget: he's not a stupid man. Does he not see what he is doing? Unfortunately, it is virtually impossible to raise the embarrassment of this paradox without immediately contributing to the polarization... [395]

Randy expands on this, and then says, "I now come down more on the side of reckless negligence than on the side of calculated deceit. Unreliability with the truth certainly does not give Chomsky a halo, and I'm not saying there aren't gray areas, but neither does it fit him for horns." (395).

Randy concludes that Chomsky "has, in other words, (1) a hermeneutic disorder and (2) an expressive disorder. On the first count, he apparently cannot read (or listen) openly; on the second, he is apparently unaware (or just does not care) that his own sometimes idiosyncratic meanings are not shared by all. Both problems would seem to follow from that blinding arrogance." (396)

Many pages follow in which Randy looks at Chomsky's blistering responses to people he took to be attacking him, when it is so hard

to see why Chomsky works so hard to misrepresent others' positions and lets fly with what what Randy (and I, and doubtless you) see as unwarranted hostility. And Randy does it in about as kindly a fashion as I can imagine. He really carries his respect for Chomsky even through the moments when it is hard to feel sympathy for him.

I've tried, but failed, to pull out a few sentences that illustrate Randy's quite successful attempts at balance in trying to understand Chomsky's misreading of other people's work, and even of Chomsky's own earlier work. Pulling out a few sentences doesn't do it, because the essence of balance is to take both sides seriously: making a statement, illustrating what supports it, considering some possible criticisms of the statement, responding to those possible criticisms, and offering a new and improved version. So you'll have to read these 35 pages or so for yourself. In fact, maybe you should read the last chapter first, so you'll understand where the book is going.

Who is Noam, really?

### *Writing this review*

In writing this review, I have tried to stick to reviewing Randy's book, but the fact is that I was not able to hold myself back from talking about the historical reality that he describes. It's so hard that it's impossible to summarize Randy's opinionated re-stylings of linguistic history without starting to express my own beliefs and values even before I'm halfway through my sentence. (And it's not just me: Geoff Pullum showed me how strong this compulsion is, when he published his review of Harris's book in the *National Review* (February 22, 2022), under the title "Chomsky's Forever War", which is much more about Pullum's take on things ("things" here mainly meaning Chomsky's style of doing linguistics) than it is about Randy's book. That doesn't make Geoff's review any the less interesting—Geoff's review is loud and vituperative, as only he can be!—but it does show the kinds of forces that rain down on the reviewer of this book. I'm just warning you.)

I lived through most—almost all—of the story that Randy recounts in this book, and looking back on it through the eyes of the narrator of this book was not a pleasant pastime for me. (I wasn't completely forthcoming with you when I said that the book is a lot of fun to read. I did find it fun in some places. I think it will be fun for *you* to read. But I also found myself regretting the time and effort expended in the field on aggressive hostilities that were counterproductive.) An

awful lot of the personal interactions were loud, and offensive too. As I read Randy's book, I kept asking myself, And where were the adults when all this was happening? Was there no one who could stand up and tell these guys (all guys...) to wise up, calm down, and act like reasonable, civilized people?

That's a matter of civility. It's important, but it's not the very most important concern. Reading Randy's book I also found myself having doubts about the more important question, whether these linguists were engaged in an effort that made serious, important progress in better understanding the nature of language, though those doubts didn't last long, and disappeared like a child's bubbles in the garden. Yet too many of the linguists in this story judged their own work by one of two measures, neither of which is quite right. Sometimes linguists would justify their work by saying that their methods allowed them to ask new and interesting questions, maybe questions that had not been asked before. Unfortunately the creation of those new questions served as a stand-in in many cases for a measurement of actual success, and that is not just risky, it is a sign of trouble in a field of research.<sup>11</sup> The second measure was in the aesthetics of the new theories that would arise: is not the formulation of an even more beautiful theory a sure sign of scientific advance? But that's not a rhetorical question; it's a real question, and the answer is No. A beautiful theory is not a sign of scientific advance. At best, it's a sign to the scientist who wants to place a bet on a theory being right (or on that theory being a scientific advance). But the beauty is only a hint of things to come; it's not a measure of success. And no scientist is obliged to bet on the most elegant theory, even holding aside the fact that my aesthetics and yours are likely to be different.

<sup>11</sup> It's encouraged by work on the philosophy of science by Imre Lakatos, as that work is often interpreted by linguists. Asking new questions is a good thing, but it is not a stand-in for producing real scientific progress, or measuring it.

So both of those ways of measuring scientific progress seem to me to be misguided, and both develop out of a self-indulgence that is counter-productive. By self-indulgence, I mean a style of working which allows us to float a new idea because it sounds interesting, without trying to understand how it relates to other people's real accomplishments in the past, and how well it deals with complex data that linguists (and other researchers) have established in the past and in the present.

It's quite astonishing how little linguists feel obliged to study what has been accomplished in the past. I have friends a bit older than me (the ones I'm thinking of are also characters in Randy's book) who have told me many times that back in the early days of generative grammar, there was very little to read. But of course that wasn't true;

there was a lot of linguistics, and even a lot of syntax; a lot of it was published in English, and a lot was published in other European languages. Otto Jespersen, for starters, or Lucien Tesnière (who wrote in French), not to mention Charles Hockett. As Bernard Laks and I point out in *Battle in the Mind Fields* (2019), even a familiarity with Wundt and Husserl would have been very helpful to understand what was new in early generative grammar too, and what was not. Jim McCawley was the only character in Randy's book who understood this, and from early on in Jim's career he studied the work of non-generative linguists, and made a big effort to make that work accessible to American linguists (partly by encouraging publishers to republish older works that deserved to be studied).

### *A few points to explore*

*The Linguistics Wars* is not addressed primarily to linguists, and its principal goal is not to describe or evaluate any of the theories that it looks at along the way, though it does some of both along the way. It's a study of the professional dynamics among the groups I've mentioned, exploring personalities and styles of work among the linguists, and the evolution of the questions that were asked and the answers that were provided.

Popular accounts of science don't tend to focus on long, drawn out battles, though some do, like those recounting the animosity between string theorists and the skeptics in contemporary theoretical physics (I'm thinking, for example, of *Not Even Wrong* (2007) by Peter Woit); they tend to focus on how an idea that eventually wins the day arose and eventually overcame the earlier ideas that stood in its way. Most books of this sort are not judged by their science: it's not really possible to explain quantum theory, for example, without talking about matrices and linear algebra to some extent, and there's a tacit understanding that a person can write a good book about quantum mechanics for the intelligent layman that does not require those mathematical prerequisites. We don't think that a popular book on quantum mechanics is worthless just because it doesn't begin to do justice to the theory.

But I found a number of times I was not so generous in my reading of Randy's accounts of linguistic theorizing because of how the linguistics was treated. Perhaps I should have been more generous. I'd like to take a few minutes and discuss some of the areas where I think Randy's account is misleading, on technical grounds.

The most important theoretical question, in my opinion, is also one that has lost the attention of most linguists—in part because of its abstractness, and in part because Chomsky disconnected himself from it towards the end of the 1970s, despite the fact that it was Chomsky himself who had brought it to the center stage during the first part of his career. This question involves the notion of *simplicity*. I'll turn to that now, and then to a few other, less important points.

### *Simplicity*

Perhaps the most difficult concept for people to understand in the classical heyday of generative grammar was the notion of simplicity and the role that it played. I think even a lot of Chomsky's early coworkers didn't really get it, and as far as I know, Chomsky did not talk much about it in his classes (certainly he didn't when I was there). And as I just noted, Chomsky himself long ago gave up on the idea that he was trying to push—alas. I think it was the most interesting idea in that early phase of his work, and I've discussed it at length in several places, but most of all in "Towards a new empiricism for linguistics" (Goldsmith 2015, chapter 3 in Chater et al, 2015).

Here's something that Randy wrote, and I fear it is misleading.

With the central, virtually defining role of simplicity in Chomskyan linguistics, one would have thought (Postal surely thought) that the "Best Theory" case would be enormously appealing. It is a straightforward minimalist argument that the grammar with the fewest theoretical devices is the simplest, and therefore should be the most highly prized. [p. 172]

Chomsky's use of simplicity (or, equivalently, the appearance of an evaluation metric in linguistic theory) was the intellectual descendant of Zellig Harris's concern (stay with me now, you may already be shocked by what I have just said) for finding *compact descriptions of data*. In this, Zellig's goal was not unrelated to the insights of Nelson Goodman regarding the problem of induction in philosophy.<sup>12</sup> Chomsky held out hope that the theory of human grammar could be encapsulated in a two-pronged attack: first, we spell out a formal descriptive device—exactly like a programming language—and we do it in such a way that we make it easy, or simple, to express things that real languages actually do, and we make it hard (or impossible, it doesn't matter which), which is to say, we make it necessary to write out in great detail any sort of generalization that languages use rarely or not at all. Then we *use* that formal descriptive device to *justify* our

<sup>12</sup> I'm going to refer to Zellig Harris as Zellig, just as I refer to the other Harris as Randy.

analysis of individual languages. There's a circular aspect to this, yes, but if it's done right, it's a virtuous rather than a vicious circle.

Why is this called simplicity? It's certainly *not* simplicity in the every day sense of the term, or in the way that Randy used in the citation above. Chomsky was not looking for a simple theory, and he didn't care if any given grammar was simple in the everyday sense of the term. Well, maybe he cared like we all care, but that was not what his theoretical notion of simplicity was about. He could just as well have said he was interested in *complexity*, and then said that he wanted his theory of grammar to evaluate candidate grammars by choosing the least complex.

So Randy makes it look like something changed when Chomsky wrote, "Notice that it is often a step forward when linguistic theory becomes more complex." Randy wrote, "The grounds of theory comparison changed almost overnight: simplicity was out, restrictiveness was in, and progress was to be found through increases in complexity. There is an aspect of rhetoric known as *kairos*, the opportune moment. Some arguments get a better hearing at one moment than another, and the moment was ripe for restrictiveness." (173) Now, it is true that starting around 1965, Chomsky stopped talking about the evaluation metric and seeking the simplest grammar as defined by the evaluation metric in one's theory of grammar (and he actually gave the whole idea up in the late 1970s, a tragic fact for some, like your reviewer). So if you don't know the big picture, it may be hard to see where Chomsky was coming from in his response to Postal. But it wasn't a rhetorical shift; he was trying to make a point about theory evaluation.

Here's another way to describe this early conception of generative grammar, the one that was prominent in Chomsky's thinking from the mid 1950s up until the late 1970s: a theory of grammar may be complex or not, and that's not at issue. What the theory of grammar does is this: it fixes very clearly what the grammar is for any given set of data from a specific language, and we will judge our theories to be adequate if and only if (sorry!) the grammars they provide for languages are simple.<sup>13</sup> Those grammars will be simple *because* the inherent complexities they describe have been extracted from the particular grammars and placed into the theory of grammar. Chomsky's hope, then, was that most or all of the complexities that would be pulled out of the individual grammars and put into the larger theory would be common to most or all languages, and would not stand out as obvious repetitions of generalizations that are found

<sup>13</sup> Said a bit more accurately: A theory of syntax is judged correct to the extent that for each language, the grammar that it provides as its simplest description is the one that we believe is the correct one, on empirical grounds.



in other domains of human experience. (That larger theory would later come to be called *Universal Grammar*.)<sup>14</sup>

<sup>14</sup> If you want to read more about this, see Goldsmith 2015.

The reader at this point can tell that this general point is of great interest to me, and I've already said that it's not widely understood, even though in his early work Chomsky does lay it out for anyone who wants to read what he wrote. Randy objected to me "but [Chomsky] also uses the word, rampantly, either in the ordinary-language sense of simple (uncomplicated, elegant, 'minimal,') or, in any case, indistinguishable from the ordinary language use."<sup>15</sup> That may be true (I'm not at all sure), but I think that misses my point, which is that there are some really deep and non-obvious ideas here that are at the heart of generative grammar of the 1960s ilk.

<sup>15</sup> Ibid.

Sometime during the fraught years of the Linguistics Wars Chomsky stopped believing in the ultimate validity of the project of writing (or discovering) grammars of languages, and that loss of faith led him to make outrageous remarks like there is only one human language (with the apparent differences between Urdu and Swahili being reduced to some unimportant place, like their vocabulary). Of course that's not a scientific statement, or a claim about the world; it's just a methodological remark, akin to the Neogrammarians' decree that there are no exceptions to sound change. It's a statement of how Noam wants to work, and an encouraging nudge to others who are interested in viewing things the same way. It's a hint pointing to how he'd like to compartmentalize work on language, leaving to proper linguistics only that part which is universal.

### *Bloomfieldians*

I got the feeling at several points that the character of pre-Chomskian American linguistics was not well presented. Randy more or less rolls them all the pre-Chomskian linguists up into a ball of "Bloomfieldians." On page 18 he refers to Zellig Harris as a "major Bloomfieldian." Zellig did not think of himself as a Bloomfieldian, and he certainly went way beyond Bloomfield in his linguistics. But he did not reject Bloomfield, as he did not reject Sapir. It is Chomsky, and those who embraced his vague use of the term "Bloomfieldian," who allowed Randy to use the term Bloomfieldian as a blanket term – a vague term – for anything pre-Chomskian. On p. 65, Randy writes, "the Chomskian universe was unfolding as it should in the middle of that optimistic and captious decade [what a great phrase!], the 1960s. The Bloomfieldians had been driven to the margins."

What the heck is that supposed to mean? At times it seems that anyone who became a linguist before Chomsky came around was a Bloomfieldian for Randy, but that's not quite right; he does call Fred Householder, at Indiana University, an "early-adopter" of generative grammar (p. 67). But even that phrase misses the point, or better, illustrates the lack of finesse in Randy's characterization of those senior scholars. There were quite simply people in the field before Chomsky, and most of them were perfectly capable of reading the young Chomsky's work and realizing that he had some very important things to say. They were also capable of reading it and finding it of little interest to them. It is true that a grad student in Cambridge, Mass (a generativist! — me, let's say, before I moved to Indiana University), could well think of Fred Householder as a benighted structuralist, after reading what Chomsky and Halle wrote in rebuttal to Fred's famous 1965 paper on generative phonology. But Fred, I think, could hardly have cared less, and those who knew him and his work knew that he took generativists' work very seriously, and took non-generativists' work just as seriously. He was a linguist and a scholar, with independence of mind, and that's a scientific stance that just doesn't fit into Randy's slippery set of categories fine-tuned to the ins and outs of Cambridge politics and irrelevant to Bloomington, Indiana, just as it is to many other places. Fred, of course, was not unique. I think of Dwight Bolinger in the same category, and Randy refers to him as "decidedly unBloomfieldian older generation scholar." Bolinger was deeply interested in the subtleties of the meanings of English sentences, something that would certainly have made him anathema to Bloomfield, but is that what Randy is leaning on in not calling Bolinger another Bloomfieldian? We don't find that out. And there are other traditions in American linguistics, too—like the tradition embodied by Bill Labov, who was himself a student of Uriel Weinreich, who brought a European conception of dialectology to the United States. It would make no sense to ask whether Weinreich or Labov were Bloomfieldians: they, like anyone else, read Bloomfield, and moved on from there.

On page 188, Randy brings up the Bloomfieldians again. He writes,

Chomsky's routing of the Bloomfieldians had been so complete that by the late 1960s any of the synonyms for that school (*taxonomic, descriptive*—even *structuralist*, which described Chomsky as well as anyone at the time, better than some) were code words for *misguided, unscientific*, and *blockheaded*.

Randy is certainly not wrong at all about that. But I hope that

in the third edition he will stop with the label “Bloomfieldian.” Or should it be used to emphasize the contrast with the “Sapirian” position, much closer to the Chomsky/Halle view in the world of phonology? That would be reasonable, too.

Randy told me<sup>16</sup> that his comment about structuralists being driven to the margins meant that to some extent they were getting less of a hearing in the organs of the discipline, producing proportionally fewer students in their like, and so on; the standard metrics of disciplinary influence, as he put it. “But surely ‘unfolding as it should’ telegraphs my voice here as channeling generative attitudes rather than strict facts.” Perhaps he’s right, but I didn’t read it that way, and most likely different readers will read him differently. The problem, as I see it, is that the reader will either have no idea who the Bloomfieldians were, or they’ll only know the Chomskian caricature; who is in charge? Who is expected to say that there was a big world of linguistics that the Chomskian picture misrepresents? In my opinion, it’s the author of the book who’s in charge.

<sup>16</sup> *ibid.*

*Plans...*

p. 19 Randy mentions the book *Plans and the Structure of Behavior*, which George Miller, Eugene Galanter, and Karl Pribram wrote during a year at the CASBS in Palo Alto – and he refers to it as a textbook, which is astonishing. A textbook is a book that flattens and simplifies for undergraduates; this book was cutting edge and way out there, written by three leaders of the early field of cognitive psychology. And he calls it “a version of Chomsky,”<sup>17</sup> based on nothing that I can see (or that he presents); it *isn’t* Chomsky, it’s early cognitive psychology.

<sup>17</sup> p. 19

Let’s take a quick look at *Plans*—that’s how people referred to this book. On page 2, the authors write,

The notion of a Plan that guides behavior is, again not entirely accidentally, quite similar to the notion of a program that guides an electronic computer. . . In this survey we were especially fortunate in having at our disposal a large mass of material, much of its still unpublished, that Miller had obtained from Allen Newell, J.C. Shaw, and Herbert A. Simon in the course of a Research Training Institute. . . Newell, Shaw, and Simon inspired us by their successes, but they should not be held responsible for our mistakes or embellishments. Nor should Weiner, Ashby, von Neumann, Minsky, Shannon, MacKay, McCulloch, Chomsky, or any of the other authors whose work we studied.

“Nowhere, outside of linguistics, is the influence of Chomsky

more pervasive,” Randy writes (p. 19). I don’t see it; I certainly agree that discussing *Plans* is entirely relevant to understanding what was happening at this period, the end of the 1950s, but I don’t see the influence of Chomsky as pervasive.<sup>18</sup>

### *Computer science, philosophy, and who knows*

p. 20 Randy writes that “In computer science research *Chomsky* was among the biggest names.” I don’t know where Randy got that idea, and the only citation he offers is to a quotation from David Golumbia in 2009 (and I would not choose David Golumbia’s work to provide support for who were the biggest names in computer science). Chomsky’s name is remembered in computer science for his language hierarchy, but a rock star he was not (22: “Meanwhile, back in computer science, Chomsky was a rock star.”) Computer scientists did lots of things, but work on natural language barely moved the needle.

Randy disagreed with my observation, and wrote back that

It is wrong, and highly misleading, to say that my only source is Golumbia, and then to wave away that source as someone who doesn’t know anything about computers. Golumbia is a good social historian, and he is firstly talking more broadly than just about CS, and secondly talking about the energy (and funding) that began to grow around the possibility of speaking computers, which absolutely did very prominently (often, exclusively) feature Chomsky and/or TG in the 1960s and 1970s.

There are histories of research on speech recognition, but the generative grammar does not play a major role in it. The money came from the Air Force, who wanted, after World War II, to find a way for pilots to communicate more successfully with their airplanes, and using speech was a hugely appealing way to do this. Success came with the stage after dynamic time warping, which was hidden Markov models, beginning in the 1970s.

And see this footnote.<sup>19</sup>

Randy says about Chomsky in philosophy what he said about Chomsky in computer science: “he is now widely acknowledged as one of the most important philosophers of the late twentieth century.” [p. 21] I can’t imagine why Randy thinks that. Chomsky does like to cite 17th and 18th century philosophers, the ones that undergrads read in college courses (Hume, Locke, Descartes), but I don’t think that Chomsky has made any contributions to philosophy. He

<sup>18</sup> Randy wrote back to say that “my remark that I utterly misrepresent *Plans* is based mostly on a misreading. The not-a-textbook part, I’ll have to take your word for, though saying that it is “astonishing” that I could construe it as a textbook seems pretty extreme. It reads very much like a textbook, summarizing accepted, field-defining positions. Unquestionably, the TG chapter, which I read most closely, is flat-out textbook-grade exposition. But if the book is full of arguments (delivered authoritatively) about a bunch of open questions meant only as theoretical suggestions, I guess I’ll have defer to you. I don’t know the history precisely enough, though your rationale is not fully convincing.” He added, “‘Astonishing’ is misleading [and] ‘a version of Chomsky’ is mistaken.” [October 8, 2022, email.]

<sup>19</sup> Randy objected, saying that “immediately after the “super star” remark, I quote Don Knuth, who certainly knew something about computer science, saying “Here was a marvelous thing” about TG, “a mathematical theory of language in which I could use a computer programmer’s intuition.” I wasn’t trying to suggest Chomsky was ever actively involved in computer science . . . but that his reputation was very substantial in that community during the TG heydays, even if it rested ‘only’ on the Chomsky hierarchy.

has insisted that philosophers should use the word “knowledge” in ways that are different from how they do in fact, but that’s not doing philosophy or contributing to it. On p. 22, Randy cites Dan Dennett who found Chomsky’s work exciting or perhaps shocking, and Randy says that “philosophers were lining up around the block.” Hard to know what that means. Of course John Searle wrote several times about Chomsky, taking seriously some of the things that Chomsky wrote, and there was some back and forth between Chomsky and Searle, but it didn’t produce much light on the subject. Searle thought that Chomsky had to demonstrate that Chomsky’s grammar had to enter into a causal relationship with human linguistic acts, and Chomsky never got back to him on that requirement—certainly Chomsky would not be willing to accept that challenge, in my view. Searle’s challenge—to show a causal relationship between a grammar and a human action—was a reasonable reaction from a 20th century American philosopher, but it was not at all the sort of challenge that Chomsky wanted to take up.<sup>20</sup>

Randy writes, “Rationalism had fallen largely into obsolescence by the early twentieth century, and Chomsky’s most renowned contribution to contemporary philosophy is its resurrection.” (188) This is an odd thing to say—I’m focusing first on the first half. If by rationalism we refer to the school of thought encompassing Descartes and Leibniz, towards the end of the 17th century, it’s a view that is many, many generations behind us, and has about as much claim to our attention as a modern approach to science and truth as Newtonian physics does, which is to say, not a whole lot, without casting any aspersions on its importance and the insights of its leading lights. By the time of Kant, no one in European philosophy was a rationalist. And no one has, or could, resurrect rationalism, not even Chomsky. Linguists may talk about rationalism and think of it as a view that allows for something like knowledge that is brought by the child to the process of acquiring language, but the connection to rationalism is very, very tenuous.

Now it is true that linguists and psychologists have used the term *rationalism* to describe Chomsky’s views on innate linguistic knowledge, and used it a lot, and Randy wrote that it was this usage that justified his remarks (that’s my paraphrase of his comment, of course).<sup>21</sup> No one (and least of all, I) can diminish Chomsky’s impact, but the question here is his impact on philosophy, and I don’t think that he has in any sense brought rationalism back to philosophy; no one could, and no one should try.

<sup>20</sup> Randy reminded me that Chomsky “shows up very consistently in philosophy of the period – Quine, Putnam, Stalnacker, and so on; Montague’s whole model seems to have been provoked by his antipathy to Chomsky. I’m baffled that you find it so easy to dismiss the observation that philosophers engaged his work at a highly significant level.” [October 8, 2022] The subject is too important to settle it in a footnote, to be sure, but in most cases, philosophers wrote about Chomsky to say that Chomsky’s work did not engage with the problems as philosophers understood them, and in few cases, as Randy points out, they wrote to say that Chomsky had misrepresented them. I think that the Searle-Chomsky dialog was the closest followed of all of these conversations, because it came to life in the *New York Review of Books*, and as I noted, the key was Searle’s demand that Chomsky show a causal relationship between his grammar and human behavior, a demand that Chomsky replied to only with silence.

<sup>21</sup> October 8, 2022.

*The source of kernel sentences*

Randy writes, "Sentences 3–6 are basic sentences – *kernel* sentences, Chomsky calls them." [p.24] Well no: *Zellig Harris* called them kernel sentences, and he had a reason for doing so. Harris was interested in algebra and developing an algebraic approach to language. Harris imagined a function that would map sentences to the transformations that were engaged in creating them. Some sentences would be mapped by this function to the null element, which is to say that some sentences would involve no transformations in their creation. In algebra, the term *kernel* is used to mean the inverse image of the null element, and that's why these sentences form the kernel. To my knowledge, that was never a part of Chomsky's derived use of the term.<sup>22</sup>

<sup>22</sup> Randy wrote to me that he did not think that his wording meant that he was saying that the term originated with Chomsky, but rather simply that Chomsky used the term.

*Data and intuitions*

Randy discusses Chomsky's predilection for using examples that he, or any native speaker, might create on the fly, and he discusses concerns that some other linguists had with this, taking the case of Anna Granville Hatcher as an example.<sup>23</sup> Chomsky says that you cannot grammatically "perform leisure," because "the verb perform cannot be used with mass-word objects: one can perform a task, but one cannot perform labor. Hatcher says, No, you can perform magic, and Chomsky agrees, allowing as how his generalization was wrong (well, it wasn't a generalization; it was more like a stab in the dark, surely). But Randy is barely willing to give Hatcher the win for the round, because "her counterexample is the generalization of a native speaker, Anna Granville Hatcher, not the product of diligent corpus research." And, Randy concludes, "the moral of the story is clear: everyone in the discussion is using intuition to do linguistics." In fact, he takes the pulpit himself and says, "Intuitive data is a perfectly reasonable way to do some linguistic work. . . the shift toward intuition, away from corpora, was liberating."

<sup>23</sup> p. 57.

Well, yes it was, but there's liberating and there's liberating, and the rise of internet search engines like Google that can tell us what people have written (tens of thousands of times) has been equally liberating. But what's liberating got to do with it? Is *liberating* just a funny way of saying *easier*? What we have learned in the last twenty years is that a lot of the ungrammatical sentences that linguists used to test their models, and others models, weren't ungrammatical. And a big part of the goof, the methodological goof, derived from the idea that linguists had that if you wanted to test a grammar, it was OK to look at the tree the grammar would generate, and then stick words

onto the terminal nodes (*giggle* into the verb positions, *duckling* into the noun positions, and so on, just like we were playing Madlibs), and then if the sentence sounded good, we were done. Well, that is a terrible way to do linguistics, and a lot of people are still doing it. One of the most thoughtful reflections on this way of doing bad syntax was the work of Nicolas Ruwet, a French syntactician, in the 1980s, and he looked carefully at how word choices (and the cognitive and cultural niceties that go along with them) have to be thoroughly considered if you want to understand how French or English work (the languages he worked on).<sup>24</sup>

<sup>24</sup> He published a book on this, called *Syntax and Human Experience*, and I highly recommend it.

Let's stay with this a little while longer. A lot of syntacticians know very well how rich the work of Otto Jespersen is, and how valuable his work is for syntacticians. It was, I think, Ed Klima (one of the earliest generative syntacticians in Cambridge MA in the 1960s) who emphasized to all who would hear how important Jespersen's work was (the *Modern English Grammar*, or *The Philosophy of Grammar*, to mention two of the most important). Jespersen would read English texts and think about the sentences, and see how interesting usage could and would teach us about how the language works (and how it plays). If you haven't spent time being amazed at what Jespersen's discovered with his method, you simply can't understand what it means to learn about language by studying a "corpus." A corpus? Jespersen never studied a corpus: he read books. A lot of them, I imagine. And that's what great grammarians of other languages do; it's not hard to find them. I spent many years learning how French worked in a similar way by simply reading Grevisse.<sup>25</sup>

<sup>25</sup> M. Grevisse, *Le bon usage*.

So that's the point: intuition is fine as far as it goes, but it doesn't go far. I think honestly it should not be taken as reliable data: it is useful for pilot studies (so to speak) and that's all. I think we've learned that. We knew that before Chomsky, we forgot it for a while with Chomsky, and now we remember it again: English isn't just what I say it is; English is the work of a community, and we need to understand what that community is creating.

### *And the burden is on who?*

Here is an occasion on which Randy steps into the fray to play referee, giving himself a certain authority:<sup>26</sup>

But Chomsky about taxonomic phonemics—and, in his footsteps, McCawley and Lakoff about Deep Structure—has it exactly backwards. Presumption always falls on the side of established scientific principles. The burden of proof *was* on Halle and on Generative phonology,

<sup>26</sup> Randy wrote to me after reading this review that he viewed himself as presenting others' views on where the burden of proof lies, not his own views: they "are not mine at all. They are established principles of how those notions enter into legal and other argumentation." That's an interesting remark, but I can't read the discussion in his book that way. Caveat lector.

and it was met by providing a model of phonology that worked efficiently without “the phonemic level.” Lakoff’s claim that no specific arguments were advanced in *Aspects* is strictly true. But so what? Deep Structure is the linchpin of a model that the entire community (including, of course, all of the budding Generative Semanticists) found very compelling. Or, look at it from the other end: the *Aspects* model is itself an elaborate argument for Deep Structure. [p. 155]

Looking back on this period, I am amazed at how much time was wasted on arguing about who the burden was on to make their point. But I don’t think the presence of Randy’s own views here are helpful. The most important point, I’d say, about Halle’s argument is that Halle (and Chomsky, too, when he addressed this question in the early 1960s) fail to note the really important scientific question that needed to be joined: can each language be associated with an inventory of sounds that are used to describe and identify the sound-side of words (a “phonemic inventory,” as the term was understood), and if so, are there upper bounds on what sounds can appear in the phonemic inventory that are directly, or indirectly, related to overt contrasts in the language? Saying that a bit less technically, the question is whether we can identify a set of sounds in a language which is rich enough to describe all of the words, keeping apart words that sound distinct to native speakers, but no richer: that is the phonemic inventory. Chomsky and Halle, in their generative careers, never addressed that question, though other generative phonologists very obviously did, notably Paul Kiparsky, and Chuck Kisserberth and Michael Kenstowicz. Halle’s argument was based entirely on the idea that the only way to distinguish parts of a phonological grammar was to split up analyses by looking at them as sequential derivations, and organizing those derivations by which phonological rules related the various successive stages of the derivation. Halle’s argument was that you can’t say whether the effect of voicing assimilation in Russian is a derivational effect within the “morphophonology” of Russian, or within the “phonemics” of Russian; in some cases it’s the one, in other cases it’s the other. There are many assumptions in Halle’s argument that can reasonably be objected to, and even within a strictly derivational view of phonology, such as in the 1980s lexical phonology model of Kiparsky 1985, the force of the argument can completely vanish: all that needs to happen is that the theory permit a rule appearing in two components, or some variant on that—as we saw in lexical phonology. Halle never viewed the developments in phonology that way, and I don’t think Kiparsky did either, but at this point in time and looking back, it seems pretty obvious to me.

What Randy’s passage that I just cited gets wrong from an em-



pirical point of view is that sometimes presumption does not fall on the side of the established view: sometimes the new kid on the block simply points to an old question and says, Who needs to worry about that question? It never helped us get anywhere anyway. That, in effect, was what Chomsky and Halle said about the search for phonemic inventories, and it allowed them the privilege of creating inventories of underlying segments in English (in their SPE analysis) that were wild, hairy, and unconstrained in the worst of ways. Right or wrong—they got away with it, for the most part. There were phonologists who immediately called them on it, and they flew under the banners of natural phonology and natural generative phonology. Well, and there were concerned critics strictly within generative phonology, who I mentioned just above.

### *Conclusion*

I fear that some of my comments may be misunderstood. Randy remarked, for example,

I guess my principal objection to the areas of the review I have singled out is that they often dismiss my observations with the suggestion that they are wholly unreasonable. None of them are, in my estimation; certainly none of the ones you identify.

I hope that my disagreements will not be reduced to the statement that Randy was (or was being) wholly unreasonable, and I think that what I've been doing in this review is what is expected of a reviewer: to provide some context and often a different perspective on the material that is covered in a book. I'm sure that many of my colleagues, those who feel more sympathy for the overall position on linguistics that Chomsky has taken over his career, would object to many *other* things that Randy wrote but which seem to me to reflect sound judgment on Randy's part. It's not the role of the reviewer to take upon themselves the status of the ultimate arbiter; it's to continue the conversation that begins in the book and which might in turn interest the potential reader.

This is one of those books which delivers the story with warts and all. It does it really well, and I trust that my disagreements with Randy's presentation here and there will not discourage the potential reader. Read the book. Maybe starting with the last chapter.

## References

Chater, Nicolas, Alexander Clark, John Goldsmith, and A. Perfors. 2015. *Empiricism and Language Learning*. Oxford: Oxford University Press.

Huck, Geoffrey J. and John A. Goldsmith. 1995. *The Ideological Structure of Linguistic Theory*. London: Routledge.

Goldsmith, John A. 2015. Towards a new empiricism for linguistics. Chapter 3 of Chater et al.

Goldsmith, John A. and Bernard Laks. 2019. *Battle in the Mind Fields*. University of Chicago Press.

Grevisse, Maurice. 2016 [1936]. *Le bon usage*. Duculot.

Kiparsky, Paul. 1985. Some consequences of lexical phonology. *Phonology Yearbook* 2, 85-138.

Ruwet, Nicolas. 1991. *Syntax and Human Experience*. Chicago: University of Chicago Press.