Chomskyan (R)evolutions. Edited by Douglas A. Kibbee. John Benjamins Publishing Company, Amsterdam and Philadelphia. 2010. 488 pages + xii.

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This book is a festschrift to Konrad Koerner on the occasion of his 70th birthday, and at the same time a series of reflections on the ways in which the modern world of linguistics has fashioned a history OF itself FOR itself, a history that is grounded both in the professional positions that linguists have taken and in the gradual emergence of ideas over time. The editor asked the contributors to reflect on whether there are "'revolutions' in linguistic theory (in the Kuhnian sense) and how ... Chomsky's career support[s] whichever conclusion one might reach to that question."

These contributors generally come to their work with the conviction that things are not all fine in the world of linguistics, and that at least a part of the problem is the result of a sorry understanding of how we got to where we are today—and furthermore, that this results in good measure from the disproportionate influence that Noam Chomsky has had on the field over the last 50 years.

I think it is fair to say that the writers come to the table, too, with the understanding that *all* thinking is historical, in the sense that any discovery made, any assertion uttered, is at the same time the answer to a question that has been previously asked; and so to understand an answer, it is necessary first to understand the question; knowledge emerges from dialog. Science is thus doubly historical: it is historical in that *all* knowledge is dialogic, but it is historical for a quite different reason as well: a science is not characterized by *what* it knows at any given moment, but rather by the *method* that, when all goes right, helps to ensure that scientists are not simply changing their minds over the course of decades, but they are indeed *progressing* in their understanding. There are many things that one can be and still be a scientist, but one cannot be a scientist and believe that changes in scientific thought are merely changes in fashion and style, and nothing more.

There is much food for thought in these essays. Most succeed in placing current linguistics against the backdrop of intellectual history. Some are a bit shrill, or more than a bit.

The first chapter is John E. Joseph's essay, 'Chomsky's Atavistic Revolution,' (1-18), which is for this reader the most interesting chapter of the book, and certainly worth several readings. It focuses on two notions: *modernism*, the intellectual trend that includes the thorough dismissal of all preceding movements, and *ironic distancing*, a much more complex rhetorical relationship developed by an author speaking about his predecessors ("Smith's work was well ahead of its time, though it suffers from the lack of attention to rigor that is characteristic of its age; still she saw what few linguists of her time were able to see..."). This distancing is *ironic* because it is always simultaneously affirming and denying; it praises the previous work, all the while making clear that by today's standards such work would not be considered adequate.

With a few deft examples, Joseph sketches the ways (quite varied, in fact) that the heroes of our field

(Whitney, Saussure, Bloomfield, Sapir) have employed ironic distancing in explaining their own relationship to those who preceded them. But his greater concern is Chomsky, and Chomsky's distinctive stance towards his intellectual predecessors, especially in *Cartesian Linguistics* (1966), Chomsky's long essay on Lancelot and Arnauld's Port-Royal grammar. This book is, Joseph suggests, a "condescension-free zone" for linguists who antedate Hermann Paul; the work of the Cartesian linguists is discussed "as if they were active members of [the MIT] faculty": no ironic distancing asked, none given.

Joseph goes on to give a brief account of the withering criticisms that targeted Chomsky's book, and he notes that after defending himself against these critics, he dropped the subject of the relation of his grammatical theory to that of his 17th century predecessors. Joseph asks—tongue in cheek, I suppose—whether Chomsky might have been more judicious in his claims for his own originality if his critics had not discredited his genealogical claims.

Joseph's chapter is silent on the topic that several other authors in this book consider the most interesting and important: how should we understand the relationship between Noam Chomsky's work and that of his teacher, Zellig Harris? Two ambitious contributions on this question are offered by Bruce Nevin, himself one of Zellig Harris's last students, and by Stephen O. Murray.

Nevin's lengthy and thoughtful chapter ('Noam and Zellig,' 103-168) is the best account I have seen of the intellectual relationship between Harris's work and Chomsky's in the period during which they interacted, the period from 1946 to the mid 1950s or so.[1] It is not everywhere easy to read, for much the reason that Harris himself is not always easy to read—more often than not, because the Harrisian perspective is not the orthodox one, and the reader is challenged to take on new perspectives. Nevin succeeds in countering a number of the misrepresentations that have been made of Harris's program. Some of Nevin's presentation of Harrisian syntax (notably of Operator Grammar) and how it could or should measure up to Chomskian accounts of similar phenomena are a bit obscure to me, but suggestive at the very least.

Towards its end, Nevin's paper includes a presentation of Harris's general perspective on language, as well as a prickly critique of generative grammar as a whole; this critique is likely to put off an average working generative syntactician. The final few pages express a sense, almost a cry—more bitter than angry—that something ethically wrong took place with the ascendancy of generative grammar. There are some suggestions sketched as to what those things might have been, with references to other places in the published literature where these views have been developed at greater length (such as Murray 1994).

It is now exactly fifty years since the beginning of the linguistics program at MIT, and this reviewer has only just returned from a large celebration of that event (December 2011). While there were nearly fifty years' worth of linguists in attendance, some younger and many older, there were also reflections offered by Noam Chomsky on the state of linguistics during the time he came into the field, the time when he was a

[1] Perhaps the latest real interaction between Harris and Chomsky can be seen in Chomsky's (1955) public rejoinder to Bar-Hillel (1954), which Chomsky has noted several times he wrote and published because Harris had asked him to do so.

student of Harris's and the time that Nevin discusses in his paper. One thing is certain: Chomsky to this day believes that there was little or simply nothing of intellectual value happening in the field in the first half of the 1950s.

Stephen O. Murray's "'Scientific revolutions' and other kinds of regime change" (75-101) left this reader quite dissatisfied. The first half of this chapter reviews how little emphasis there is on syntax in the published work of Boas, Sapir, Bloomfield, Whorf, Jespersen, or de Saussure (his order, not mine). The Jespersen he describes does not sound too much like the Jespersen I have read, and my impression is (*contra* Murray) that Jespersen's writings did indeed influence early work in generative syntax, notably through the teaching of Edward Klima and the work of James McCawley (the term *extraposition*, for example, comes from Jespersen's analysis of the phenomenon in English). But the reader finds no awareness of the growing work on syntax already appearing in the 1940s and into the 1950s, such as the work by Rulon Wells (e.g. Wells 1947) and C.C. Fries (e.g. Fries 1952), not to mention the work by Bar-Hillel (1954) (inspired by Carnap 1937 and Ajdukiewicz 1935) on formalizing the combinatorics of words—their permitted hierarchical combinations—in English and other languages. If I understand him correctly, Murray believes that there could be no Chomskian revolution to speak of because there was no previous paradigm to overthrow (a view with which I think Chomsky would heartily concur, but I am not sure that *I* would!). He concludes his chapter with a sketch of the work of Chomsky and his students that is rife with charged vocabulary ('ritualized humiliation,' 'generation after generation of zealots [who] attack their elders') and gratuitous—some might even say snarky—remarks.

Peter Daniels ("Chomsky 1951 and Chomsky 1951b," 169-214) is a careful and illuminating study of Chomsky's earliest work that eventually became generative grammar, notably in his master's thesis on Hebrew morphophonology, and it is a must-read for anyone interested in the intellectual origins of generative grammar, with useful discussion of the role played by traditional Semitic studies and by the comments of Bar-Hillel in fashioning Chomsky's early work.

R. Allen Harris ("Chomsky's other Revolution," 237-264) presents a good thematic account of the relationship between Chomsky's work in the 1950s and 1960s and the work that eventually came to be described as the "cognitive revolution," most notably in psychology. While some of Chomsky's work played an important part in this—notably his review (Chomsky 1959) of Skinner's *Verbal Behavior*—and while Chomsky was supportive of early work by such psychologists as George Miller, Chomsky by the 1970s for all practical purposes had lost interest in cognitive psychology, as far as the documentary history goes. While little in the chapter is new, it is a good overview of the subject, though its primary weakness is the way in which it hews to the version of the story of the birth of cognitive science that was laid down in *The Mind's New Science* (Gardner 1985), a story that begins with a self-conscious coalition of researchers who met in Dartmouth and MIT in 1956; unfortunately, it fails to see the essential ways in which this movement was a continuation of

the cybernetics movement (see, notably, Dupuy 2000). Like many of the authors in this book, Harris seems totally amazed by Chomsky's magesterial dismissal of entire subdomains: "Machine translation [MT] is a very low level engineering project," Chomsky told Barsky (Barsky 1997), a remark cited again by Harris (p. 258). I seriously doubt that Chomsky has read (Brown et al. 1993) or followed any of the literature based on it, and Chomsky's off-hand comment is probably not worth very much (except perhaps in a biography of Chomsky, though even there it's not clear to me that it's particularly relevant).

Malcolm D. Hyman ("Chomsky between revolutions," 265-298) offers an insightful account of the evolution of Chomsky's efforts to align generative grammar first with cognitive psychology and then later with biology, notably with evolutionary biology. This is the first serious discussion to my knowledge exploring Chomsky's remarks in the last few years that he has long viewed generative grammar as a part of biology, a plank in the biolinguistic platform. In a footnote (p. 273), Hyman observes that "just how much Chomsky owes to Lorenz is open to doubt," referring to a suggestion in Agassi (1997) linking Chomsky's views about grammar to the widely discussed experiments in the mid 1950s on animal imprinting. This reviewer recalls a conversation with Burton Dreben in 1973: Dreben (who died in 1999) was a Harvard junior fellow with Chomsky during the period when Chomsky was writing LSLT, and he spent his career as a professor of philosophy at Harvard. Drebin said to me that Chomsky's interest in Konrad Lorenz's work, and others, on imprinting in ducks and other species was very important in Chomsky's thinking at the time, and that they had discussed this often during the time when Chomsky was writing his review of Skinner.

Margaret Thomas ("What do we talk about, when we talk about 'universal grammar', and how have we talked about it?" 301-314) offers an illuminating survey of the use of the term *universal grammar* and various closely related variants, most notably *general grammar*, with useful observations both on usage in the period from the thirteenth century to the twentieth as well as on the change in usage by Chomsky-oriented linguists, most notably in the late 1980s.

Marcus Tomalin ("Migrating proposition and the evolution of Generative Grammar," 315-336) attempts to apply contemporary models of scientific programs and theories to better understand the origins and growth of relational grammar, a view that was developed by linguists (notably Paul Postal and David Perlmutter; see Perlmutter 1983, for example) whose work emerged directly out of work that had been unquestionably mainstream generative syntax. Although this reviewer found the contribution made in the chapter by philosophy of science meager, the choice of subject and the attention to the evolution of the notion of unaccusativity (to take just one of several examples) is both timely and significant.

Jacqueline Leon ("British empiricism and Transformational Grammar: A current debate," 423-444) provides a useful summary of the divergent views of Firthians, neo-Firthians, and generativists, over the last 50 years, an area of disagreement that has seen considerable fracturing in the United States under the impact of easy accessibility to large corpora, notably the internet.

Giorgio Graffi ("The 'linguistic wars': A tentative assessment by an outsider witness," 395-420) offers an overview of seven lengthy studies of the disagreements between the generative semanticists and the interpretive semanticists—these coming from Newmeyer (1986), Tolmach Lakoff (1989), R. Harris (1993), S. Murray (1994), Huck & Goldsmith (1995), Seuren (1998), and Koerner (2002). He looks at the situation in Italy during the 1970s in hopes of using it as a social experiment: a place where the institutional structure was different from that in the U.S., but where the ideas were as available as they were in the United States, so to speak. I find the analysis not convincing (coming, as I do, with a strong set of differing opinions, this should not be too surprising)—because, I would argue, Graffi's account is not yet ready for public consumption. As someone reading the chapter very closely, I found myself reacting in almost every important sentence to the author's abuse of SCARE QUOTES ("those who were essentially 'neutral' " (417), "an 'external' member of the group", "one or another 'party' " (408)). There is a deep problem with academics' use of scare quotes (that is, quotes that are not obviously being used as citations from another publication): they indicate a desire on the part of the author to not make clear which aspects of the meaning of the word (or phrase) he agrees with and adopts, and which parts he is uncomfortable with and wishes to distance himself from. In the case of a discussion of a highly charged and virtually ideological topic such as this one, it is essential that the author present a very clear account of how he thinks that words like "winning" and "war" take on a useful meaning. And this I do not believe Graffi makes a sufficient effort to accomplish.

Camiel Hamans and Pieter A. M. Seuren ("Chomsky in search of a pedigree," 377-394) also discusses the disagreements between the generative semanticists and the interpretive semanticists, but with an interesting slant: they discuss the (for me, unexpected) relationship between that controversy, on the one hand, and, on the other, Chomsky's work on what he called Cartesian linguistics and its critique by a number of scholars, including Aarsleff (Aarsleff 1970) and Tomach Lakoff (Lakoff 1989), which I referred to above in connection with John Joseph's contribution.

Other chapters offer suggestions about what is fundamentally lacking in generative grammar. Christopher Beedham ("The equivocation of form and notation in generative grammar," 19-42) suggests that generativists "believe you can do science by assumption or postulation alone; the assumptions are never tested, because that is not the point." Joanna Radwasńska-Williams ("Chomsky's paradigm: What it includes and what it excludes," 43-71) explores poetry and metaphor to show that a dynamic view of language, in which speakers are "agents of . . . evolution" (62) and variation is an essential characteristic of speech communities. T. Craig Cristy ("The evolution of meaning and grammar: Chomskyan theory and the evidence from grammaticalization," 353-375) explores the differences between generative grammar and views of language which put grammaticalization at the center of the dynamic of language change. Christopher Hutton ("Universalism and human difference in Chomskyan linguistics: the first 'superhominid' and the language faculty," 337-351) emphasizes the gap between Chomsky's view of how biology can play a role in support of generative lin-

guistics and what biologists actually do. In a different direction, Pierre Swiggers ("Grammar and language in Syntactic Structures: Transformational progress and structuralist 'reflux' ", 215-233) places Chomsky's early work in the context of the distributionalist approach to syntax which was both explicit and dominant during the 1950s.

Julie Tetel Andresen's contribution, "Historiography's contribution to theoretical linguistics" (445-471) brings the book to a close with a rousing call for a rethinking of what linguistics is:

> we need a twenty-first-century linguistics that is inspired not by Descartes but by Darwin... we need to abandon the idea that linguistics needs an object of study. Whatever uses the constructs of langue and competence may have had, they have outlived their usefulness. They were never intended to have anything to do with neurology or psychology, and so it is no wonder that linguists have inherited a theoretical framework that is psychologically and neurologically uninformed and, worse, implausible. (453)

We can agree that linguists should construct their theories with an awareness of what our colleagues in psychology and neurology (and mathematics and computer science, I would add) do. I hear you say: but life is short, and we cannot do all that—and that is true. In the meantime, I think it is just as important (perhaps more important) that linguists make sure that when psychologists and neurologists speak about language, it is not in a way that is linguistically uninformed. I do not think that the study of the history of linguistics draws us to the conclusion that linguistics is ready to fold up its bags and its sails.

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