AKADÉMIAI KIADÓ

BOOK REVIEW

Frederick J. Newmeyer. (2022). *American linguistics in transition: From post-Bloomfieldian structuralism to generative grammar*. Oxford: Oxford University Press. xvii + 412 pp.

Reviewed by *András Kertész**

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1. THE BACKGROUND

The controversies in the historiography of generative linguistics are strikingly acrimonious. Frederick J. Newmeyer's publications attract great interest and trigger extreme reactions. Since his responsibility in shaping the field is beyond doubt, his new book deserves special attention. The significance of the impact which it might have on the historiography of generative linguistics is anticipated by the author's seemingly praiseworthy self-criticism in the very first sentences of the Foreword (p. xi). Although he mentions that his best known book *Linguistic Theory in America: The first quarter century of transformational generative grammar (LTA)* (see Newmeyer 1980, 1986a) – which has counted as **the** standard work of the historiography of generative linguists over the past four decades – had a basically positive reception, he also admits that its critics were right in accusing it of having "treated the structural linguists who preceded transformational generative grammar (TGG) **with little respect and even less understanding**" (ibid.; emphasis added). In addition, he also conceded that *LTA* had a "**triumphalist** tone" in that it "exaggerated both the intellectual success of the theory (as measured by the number of its adherents worldwide) and its organizational success (as measured by institutional dominance)" (ibid.; emphasis added). Therefore, in the book under review he attempts to "**set the record straight**" (ibid.; emphasis added) by analyzing the transition from post-Bloomfieldian structuralism to the emergence of TGG in a novel way.

Nevertheless, the admittance of the flaws he committed in *LTA* is euphemistic at best. The positive reception of the book in generative circles is hardly surprising within the context of its flattering portrayal of the generative enterprise. And the negative reactions to Newmeyer's historiography were in no way limited to mild criticism of its "triumphalist" tone and lack of respect and understanding of post-Bloomfeldian linguists. Rather, the critics charged Newmeyer

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with the greatest sins a historiographer can commit: "Whig historiography", "partisan historiography", or "*pro domo* historiography";[1] bias; the improper handling of data; misleading reasoning; and deficient historiographical methodology.

For example, in his foundational study, Koerner (2006, 2812–2813; emphasis added) illustrates how linguistic historiography **must not** be conducted, by maintaining that "in 1980, Frederick Newmeyer published a book which constitutes the best example to date of this *pro domo*, **Whiggish** type of history writing", and thus proceeding in a "propagandistic, if not ideology-driven" manner. In the same vein, according to Lakoff (1989, 940; emphasis added), in *LTA*, "Newmeyer's **bias** is the most dangerous kind […] Newmeyer **misleads** the reader and **distorts** the facts." Or, *LTA* "does show that true believers of TGG respect the historicity of the past when writing about themselves as little as when writing about others" (Murray 1982, 107). According to Nielsen (2010, 5–6), "Newmeyer, an ardent transformationalist, writes with strong **pro-transformationalist** sensibilities; […] Lacking is a balanced account of the era, informed by **history of science methodology**." Newmeyer's later historiographical works were evaluated similarly. For example, "Newmeyer's claims are **misleading**, and they sometimes seem designed to support a **simplistic, pre-existing interpretation** of linguistic history" (Tomalin 2008, 17; emphasis added).[2] Another verdict by Murray (1989, 156–157; emphasis added) is that the book he reviews "is **wildly biased** and unsuitable as a textbook either for history or linguistics […] There is **hardly a page in it** that doesn't exemplify the **distortions** of a perspective that takes Chomsky's pronouncements as the telos of human thought."[3] Nevertheless, there were also more balanced evaluations, simultaneously pointing out both *LTA*'s flaws and its merits. For example, McCawley (1980, 913; emphasis added) compares *LTA* to "an extended **sermon**, which to a large extent it is, though in the course of it Newmeyer presents considerable historical detail about linguistics in the United States in the 1950s through [the] 1970s that is **valuable** independently of its relation to the sermon".

These examples serve to illustrate that the charges against Newmeyer's previous historiographical work go beyond his self-evaluation quoted. Therefore, the present reviewer feels compelled to ask two questions:

1. The first is, of course, whether this book succeeds in "setting the record straight" by immunizing it from the two kinds of criticism the author mentions with respect to *LTA* (quoted above).
2. However, in the light of the fierce criticism also quoted above it seems essential to raise another question, too, namely, whether the new monograph meets the elementary standards of scientific history writing at all, or is still a propagandistic contribution to the promotion of generative linguistics.

The present review attempts to answer these two questions. In order to answer them, we must clarify what the standards are. In the light of the methodology of the history and

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[1]"Whig history" is Butterfield's (1931, 11) term, which was applied "to the kind of history of science in which past science and scientists were judged in the light of modern knowledge" (Mayr 1990, 301–302). The past is characterized as progressively leading to the glorious present and thus legitimizing it. Whig historians map present beliefs back onto the past. Terms like "*pro domo* historiography" and "partisan historiography" also refer to unscientific history writing.

[2]The quotation pertains to Newmeyer's (1996) collection of essays.

[3]The quotation pertains to Newmeyer (1986b).

philosophy of science in general, as well as the literature in the historiography of linguistics in particular, the following minimal criteria present themselves.

(i)   **Method.** The historiography of scientific knowledge differs substantially from other kinds of historiography, at least in the sense that it is closely related to the philosophy of science.[4] In the post-Kuhnian era the history and the philosophy of science have become closely intertwined. Accordingly, historiographers of any scientific discipline must be familiar with the rich inventory of methods offered by the general history and philosophy of science and are expected to choose from among them the particular framework they need, or, alternatively, they may develop their own. What is not preferred, however, is conducting some kind of "intuitive" or "common sense" or "naïve" historiography that dispenses with a well-defined scholarly method.[5]

(ii) **Data.** Another standard that has to be met is that instead of subjective evaluations the author's claims should be based on well-documented data.[6] Historical data are, of course, as theory-laden as any other kind of data in other disciplines, and have to be interpreted in the methodological framework chosen along the lines of the standard mentioned in the previous paragraph.[7] Therefore, if the standard in (i) has been violated, that is, if there is no well-defined methodological framework conforming to the current state of the art of the history and philosophy of science, then it is not clear what counts as data and what doesn't, and whether there are well defined criteria of data selection at all.

(iii) **Bias.** Historiographers of generative linguistics are inclined to accuse each-other of being biased (see the above quotations). Although "bias" is a complicated term, and the authors do not define it, it is not difficult to find out what its avoidance might involve. Here I mention only two aspects.[8] First and foremost, Whig history writing should be avoided; and second, the metatheoretical perspective of the historiographer must not interfere with the object-theoretical acceptance or rejection of generative linguistics.

(iv) **Fallacies.** The great majority of contributions to the historiography of generative linguistics are misleading in that they make extensive use of informal fallacies such as different versions of *argumentum ad hominem*, *argument from authority*, *false analogy*, etc.

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[4]For an overview of the state of the art see, for example, Mauskopf and Schmaltz (2012).

[5]In this respect, most approaches to the history of generative linguistics are insufficient. Even the few attempts that consider some aspect of the philosophy of science in their historiography of generative linguistics misunderstand the framework they have tried to apply. For example, Boeckx (2006) did not understand Lakatos, and ten Hacken (2007) represents Kuhn fallaciously (Kertész 2010a). Murray (1994) is a competent application of a sociological framework, but the credibility of his findings is undermined by the author's *ad hominem* statements.

[6]*LTA* has also been criticized for being dominated by the author's subjective claims and dispensing with documented data. See, for example, Murray (1982).

[7]"It is impossible to study anything, **history included**, without some a priori **framework** by which phenomena are made meaningful. All observations are **theory-laden** […]" (Miller 2012, 40; emphasis added).
    "The facts of history are **not simply found** but have to be inferred from historical sources through complex inferential and interpretative processes. These processes, in turn, are structured by theoretical presuppositions. Hence, **historical events and facts are theory-laden** in the sense that they are partly constituted by theory" (Kinzel 2016, 127; emphasis added).

[8]For further kinds of bias committed in the controversies on the history of generative linguistics, see Kertész (2010b).

(Kertész 2019). Thus, it is an elementary requirement that such fallacies, which discredit historiographical approaches at the outset, should be avoided.

Having said all this, in Section 2, I will give a brief overview of the book's structure and content. In Section 3, I will infer my answer to the two questions I have raised.

2. OVERVIEW

In Chapter 1, the author intends to show that the process unfolding in linguistics between the late 1920s and the 1940s led to an even more significant change than the publication of *Syntactic structures* (Chomsky 1957) and the subsequent rise of TGG. He evaluates Bloomfieldian linguistics as "extremely modern" and its practitioners as "in some sense revolutionaries" (p. 10, quoted approvingly from Murray 1989, 162). The chapter clarifies the difference between philology and linguistics, outlines how both were present in the work of the founding members of the Linguistic Society of America (LSA), and describes the foundational process of the society as well as of the journal *Language*. The rest of the chapter explores the factors responsible for the transformation of a group of philologically oriented scholars into a structuralist-oriented group. There is special emphasis on the formation of linguistics as "an autonomous field rather than being a branch of psychology, anthropology, language studies, or anything else" (p. 21). Another factor was that leading personalities were convinced of the equality of all languages and their analysis. The LSA's Summer Institutes, which were a great innovation, played a pivotal organizational role. Further, the bulk of American linguists was involved in the war effort in World War II, thus contributing to the coherence of the community. Moreover, the change of leadership in the LSA in 1940–1941 was an important turning point insofar as Bernard Bloch was elected as the Editor of *Language* and Milton Cowan as Secretary-Treasurer of the society. Both of them contributed to the transformation of the field in the next decade with great efficiency.

The aim of Chapter 2 is to overview the reciprocal relationship between American and European structuralism. First of all, leading American structuralists had strong European connections. The chapter carefully surveys the early American linguists' debt to the early Prague School, and to the early Geneva School. However, by the 1940s American structuralists seemed to have lost their interest in the achievements of European linguistics. The reason for this turn was that "the increasingly positivistic outlook of the former" (p. 53) could not be reconciled with the lack of rigor attributed to the European schools, which were ready to make use of abstract constructs as well as hypotheses which did not satisfy "rigorous scientific methodology" in the sense of Greenberg, Gleason and others (pp. 53 ff.). European refugees seemed to threaten the positions of the native linguists and were therefore not welcome. As regards European views of American linguistics, between 1930 and 1940 the European reception was dismissive of the American structuralists' endeavor to construct grammars in which meaning was merely of peripheral interest. However, by 1950 the reception of post-Bloomfieldian works by European linguists became more intensive, in part enhanced by the foundation of the journal *Lingua* in 1949. The foundation of *Word* in 1945 contributed to the positive European reception of post-Bloomfieldian works. In the 1950s the mentality changed and there was "an era of good feeling between American and European structuralists" (p. 75), which was, however, turned upside-down by the publication of Chomsky's (1957) *Syntactic structures*. Although the Prague School

influenced the phonology of generative grammar, many European structuralists could not accept the neglect of semantics in the early contributions to generative grammar.

After the first two chapters have discussed general developments, Chapter 3 is devoted to only one publication, namely, Martin Joos' *Readings in linguistics: The development of descriptive linguistics in America since 1925*, which is considered "the crowning glory of post-Bloomfiel-dianism" (p. 80), and which appeared in the same year as *Syntactic structures*. The relevance of the book lies, of course, in the papers themselves, which had been selected by the editor after eliciting the opinions of more than 100 American linguists about the proposed content. However, due to Joos' Preface and the commentaries that he added to most papers, "the fact remains that his extreme empiricist viewpoint was taken as the mainstream view (point of attack) by the following generations of linguists" (op. cit.). The volume represented the views of the most radical wing of American descriptivism based on a rigid empirical methodology that rejected the search for explanations. Although most of the reviews were negative, the fact that the volume went through four editions and an abridged edition in 1995 bears witness to its impact. Newmeyer seems to attribute an important historical role to the volume. Answering the question of why the volume stood the test of time better than other anthologies, he says that "[i]t is always an easy way out to see only good or only evil, and for most of today's linguists, no idea could be more evil than that the desire for explanation is an infantile aberration. But also, for Americans at least, it provides a fascinating historical record of how linguistics used to be done – not so long ago that the approach documented is a mere historiographical curiosity, but also not so recently as to be no more than a quaint version of current theory" (p. 126; see also p. 124 for a quotation by Chomsky to which this verdict refers).

Chapter 4 is, as the author has already anticipated in the Preface, one of the two central chapters of the book. It focuses on the birth of TGG and discusses a number of factors related to its historiographical evaluation over the decades. One of the most important questions that has been fiercely disputed in the historiographical literature about generative linguistics is whether there was a "Chomskyan revolution", and if so, in what sense and when. In order to appreciate the insight which Newmeyer develops in the present book, we have to be familiar with his previous views. In the first edition of *LTA* he applied the notion of "Chomsky's revolution" to early generative linguistics but without citing Kuhn (1962). In the second edition (see Newmeyer 1986a) he cited Kuhn's seminal book without, however, examining whether the facts which characterized Chomsky's impact did indeed meet the standards of a Kuhnian scientific revolution as well as those of a new paradigm (Newmeyer 1986a, 36–37). In Newmeyer (1986c) he rejected the claim that generative linguistic triggered a Kuhnian scientific revolution and presented a third view, based on Laudan's (1977) approach, according to which "[t]here was a Chomskyan revolution because anyone who hopes to win general acceptance for a new theory of language is obliged to show how the theory is better than Chomsky's. Indeed, the perceived need to outdo Chomsky has led him to be the most attacked linguist in history" (Newmeyer 1986c, 8). In the present book, Newmeyer's fourth view is that "devoting any more space to arguing that linguistics has (or has not) seen a Chomskyan revolution is **a useless exercise**" (p. 143; emphasis added).[9] The key notion of the sequel is "originality" rather than "revolution". In the author's view there are two features that make TGG truly original. First, Chomsky's (1951) MA thesis

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[9] Kertész (2010b) argued for the same insight.

entitled *Morphophonemics of modern Hebrew* provided the insight that the sentences generated can be assigned a full structural description. Second, Halle's (1959) *The sound pattern of Russian* rejected the structuralist phoneme as a result of which the central post-Bloomfieldian findings had to be re-evaluated. The author documents the fact that the reception of *Syntactic structures* by leading structuralist personalities such as Voegelin, Joos, Hockett, and Bloch was very positive, while that of generative phonology was negative. He confronts Chomsky's claims about his own early days with the documents cited. The final finding of this chapter is that the extensive military funding which Chomsky's projects received did not influence them substantially.

Chapter 5, the second central chapter of the book, examines the factors which contributed to the diffusion of generative ideas. It argues against views which claim that the period from the early 1960s to the 1970s was "a decade of private knowledge" and "underground culture" (p. 184) in that papers were distributed as Xeroxes or mimeographs to which people outside the inner circle of the generativists had no access. The author's first argument against the existence of the generativist's underground culture and private knowledge is an overview of the publication lists of four leading personalities, namely, Noam Chomsky, Morris Halle, Robert B. Lees and Paul Postal, with the result that all of them published regularly in prominent journals of the time. As regards the so called "underground literature", Newmeyer tries to refute Nielsen's (2010) claim according to which there were three types of underground literature: manuscripts that were deliberately circulated privately; institutional and laboratory reports; and manuscripts deliberately written in forms inappropriate for publication (see also McCawley (ed.) 1976). He argues that "[…] with a minimal amount of effort, any linguist in the world could have had access to the vast majority of the writings of the vast majority of generative grammarians" (p. 199). In his view the main reason for the fact that the "private knowledge" idea spread rapidly was that MIT students used rhetorical tools with which they distanced themselves from linguists outside MIT. Newmeyer also argues that besides the rapid publication of the findings of the early generativists, there were other means employed for the diffusion of the new theory: textbooks, meetings, teaching at LSA Institutes, Visiting Scientists at MIT, and increasing generative linguistics research outside MIT.

The next three chapters are considerably shorter than the previous ones. They discuss seemingly minor events, which, however, in the author's opinion, shed light on the decline of post-Bloomfieldianism and the rise of TGG.

In Chapter 6, the author overviews the European reception of early transformational generative grammar. He himself evaluates this overview as superficial for two reasons: space limitations and the lack of generalization. In spite of this, the chapter gives a useful bird's-eye-view of European generative linguistics, which witnesses that in the 1970s and 1980s there was increasing interest and systematic research in generative linguistics in most European countries, even in those behind the Iron Curtain like Hungary or the German Democratic Republic. In the concluding section of the chapter the author calls our attention to a paradox which states that "[t]he achievements of European generative grammarians are vastly out of proportion to the number of practitioners, as compared to the United States" (p. 260). He leaves the explanation of this paradox for future research.

Chapter 7 is devoted to the contested LSA presidential election of 1970 because it contributes to our understanding of the transition from post-Bloomfiedieanism to TGG. In the history of the LSA this was the only time when the official candidate for the office of the President was

challenged. Martin Joos was defeated by the challenger, Dwight Bolinger.[10] The chapter first introduces the two candidates, then describes the situation of the LSA before 1970, and documents the circumstances in which the election took place. There were two factors that influenced the conflict and the result alike. One was Joos' unpleasant personality. The other, which seems to motivate Newmeyer's claim that this election played an important role in the decline of post-Bloomfieldianism and the emergence of TGG, was that in the eyes of most younger members of the LSA, including sociolinguists as well as generative grammarians, Joos was a representative of the post-Bloomfieldian establishment. Although they "voted overwhelmingly for Bolinger […] [t]he fact that Bolinger himself was neither social variationist nor generative grammarian was irrelevant" (p. 281). Consequently, the motivation for their votes was the rejection of post-Bloomfieldianism rather than Bolinger's election.

Chapter 8 investigates the situation in which Charles Hockett – in the author's opinion the most prominent linguist of the pre-Chomskyan era in the US – attempted to resign from the LSA in 1982. The first part of the chapter sketches Hockett's career and impact on linguistics, highlighting his pioneering role in the emergence of information theory, his contribution to mathematical linguistics, and his findings that are still cited today in discussions of human language evolution. The relevance of this chapter is that it documents "what was in all probability the closure of the Bloomfieldian era […]" (p. 283). It describes how the last outstanding personality decided to resign because of the new developments in linguistics with which he could not identify himself, thus highlighting the generational shift that took place. The chapter also exemplifies the personal and professional conflicts between prominent members of the LSA that were characteristic of the transition period.

The last chapter of the book documents that although by 1970 generative linguistics had become the most visible linguistic approach not only in the United States but around the world, and Chomsky had become the most famous linguist, "visibility did not engender organizational dominance" (p. 298). It is shown that in the period 1970–1989 only one President of the LSA was a generative grammarian; one of the two Editors of *Language* was a sociolinguist, the other a historical linguist; in *Language* there was no dominance of generativist papers; although the number of generativist talks at the LSA meetings rose between 1980 and 1989, "one could hardly speak of 'generative dominated meeting'" (p. 310); generative grammarians only obtained a small percentage of grants; and Chomskyan linguists did not dominate the job market. Finally, after examining Chomsky's relation to the LSA, the author states that for his former students the rejection of Chomsky and his approach was accompanied by the acceptance of the LSA, and that neither the LSA, nor *Language* were part of the professional identity of generative linguists.

The volume also includes five appendices, the author's Afterword, a References section, an Index of Names, and an Index of Subjects.

3. EVALUATION

The book is an easy read. Its line of reasoning is simple, and no technical knowledge is needed in order to understand the author's train of thought. Every chapter is self-explanatory, begins with an introductory section anticipating its main tenet and structure, and terminates with a brief

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[10]The rule is that in a given year a nominee is elected Vice-President, then in the next year he or she serves as President.

summary of the findings. Some of the chapters (i.e., Chapters 1, 2, 4, 5, 6 and 9) look at the events from a bird's eye view while others (i.e., Chapters 3, 7, and 8) present detailed analyses of seemingly minor, but in the author's view relevant, details.

To conclude my review, I come back to the two questions I raised in the first section.

Has the author succeeded in avoiding the "triumphalist tone" of his previous books and in treating post-Bloomfieldian structuralism with the respect its proponents deserve? My brief answer is: yes. Unfortunately, the book does not contain a summarizing chapter, therefore I recapitulate some of the most important tenets the book argues for:

(a) It is widely believed that the ascent of early generative linguistics was the most dramatic change in American linguistics, and was represented by Chomsky's *Syntactic structures* (p. 1). As opposed to this, Chapter 1 argues that the ascent of American structuralism immediately preceding TGG was a more important change.
(b) The author abandons the notion of "Chomskyan revolution" and argues instead for the "originality" of early TGG.
(c) In the historiographical literature it has been assumed that the success of early TGG was in part due to funding. The book challenges this assumption.
(d) It is a widespread belief that there was an underground publication culture, a kind of private knowledge. In contrast, the book argues against this position.
(e) The author documents his thesis that in opposition to the generally accepted view, generative grammarians were organizationally not dominant in the United States in the period investigated.
(f) The state of the art in the historiography of generative linguistics has been shaped by heated controversies whose rhetoric often violates the norms of the scholarly exchange of ideas. Against this background, the book provides an opportunity for the consolidation of the field.[11] Whether the community is willing to embrace this opportunity will be seen in the near future.

Now I turn to my second question, namely, whether the book meets the minimal standards of scientific history writing sketched in Section 1 of this review.

Ad (i). The book does not apply current developments in the history and philosophy of science, although there is a rich inventory of approaches from which to choose and there are thought-provoking discussions. The author's decision to reject Kuhn's and Laudan's framework is justified. But he replaces the theoretical term "scientific revolution" by the commonsense term "originality", which he does not explicate. His approach does not go beyond naïve historiography that dispenses with a well-defined historiographical framework conforming to the state of the art. As opposed to this feature of the book, in contemporary historiography of science it is widely held that a "historical analysis that does not allow for methodological insights and generalizations is meaningless since there is nothing to learn from it. Thus, suggestions emerging from a direct confrontation of detailed history with larger philosophical claims […] **should be taken very seriously**" (Pietsch 2016, 66; emphasis added).

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[11]However, prominent critics of Newmeyer's previous work on the historiography of generative linguistics – such as James D. McCawley, E. F. Konrad Koerner, Stephen O. Murray – are no longer with us.

Ad (ii). The five Appendices, whose length is more than a hundred pages, include information on LSA members and officers, the Editors of *Language* as well as its contents in certain years, and lists of grant recipients. One of the most valuable achievements of the book is the publication of hitherto unknown archival material gathered from ten archives. But the criteria of data selection have not been defined in terms of a historiographical framework. There are subjective evaluations and guesses in the text (for example, "my feeling", pp. 272, 322, etc.) which are not supported by documents.

Ad (iii). The author seems to have made great efforts to distance himself from the subject of his investigation in order to avoid the interference of the historiographer's perspective and his preference for generative linguistics. However, since he is an outstanding generative linguist himself and a contemporary of the majority of the events described, his commitment to the generative movement cannot be excluded at the outset.[12]

Ad (iv). Those kinds of fallacies in which, as a rule, contributions to the historiography of generative linguistics abound are missing from this book.

Consequently, the answer to my second question is that the book meets the standard (iv), ignores the standard (i), and attempts to meet the standards (ii) and (iii).

In sum, the book is double-faced. On the one hand, Frederick J. Newmeyer has been able to reconsider his previous views. As the insights summarized in (a)–(f) witness, the novelty of the book is that it puts forward claims which are antithetical to both widely accepted common knowledge and some of the author's previous statements. On the other hand, although the book has made some progress towards the improvement of historiographical methodology in linguistics, there is no doubt that much remains to be done.

REFERENCES

Boeckx, Cedric. 2006. Linguistic minimalism: Origins, concepts, methods, and aims. Oxford: Oxford University Press.

Butterfield, Herbert. 1931. The Whig interpretation of history. London: Bell.

Chomsky, Noam. 1951. Morphophonemics of modern Hebrew. MA thesis. University of Pennsylvania, Philadelphia, PA.

Chomsky, Noam. 1957. Syntactic structures. The Hague: Mouton.

ten Hacken, Pius. 2007. Chomskyan linguistics and its competitors. London: Equinox.

Halle, Morris. 1959. The sound pattern of Russian. The Hague: Mouton.

Harris, Randy Allen. 2021. The linguistics wars: Chomsky, Lakoff, and the battle over deep structure. Oxford: Oxford University Press.

Kertész, András. 2010a. Two notions of 'research program' and the historiography of generative linguistics. Historiographia Linguistica 37. 165–191.

Kertész, András. 2010b. From 'scientific revolution' to 'unscientific revolution': An analysis of approaches to the history of generative linguistics. Language Sciences 32. 507–527.

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[12]Ironic as it may be, almost simultaneously with the book under review, Randy Allen Harris published the second edition of his *The linguistics wars*. It may be instructive to note that since it has a very different attitude toward generative linguistics and is based on in part different data, it yields very different conclusions (Harris 2021).

Kertész, András. 2019. Fallacies in the historiography of generative linguistics. Foundations of Science 24. 775–801.

Kinzel, Katherina. 2016. Pluralism in historiography: A case study of case studies. In T. Sauer and R. Scholl (eds.) The philosophy of historical case studies. Cham: Springer. 123–149.

Koerner, E. F. Konrad. 2006. The development of linguistic historiography – history, methodology, and present state. In S. Auroux, E. F. K. Koerner and H.-J. Niederehe (eds.) History of the language sciences: An international handbook on the evolution of the study of language from the beginnings to the present. Berlin: De Gruyter. 2802–2820.

Kuhn, Thomas S. 1962. The structure of scientific revolutions. Chicago, IL: Chicago University Press.

Lakoff, Robin. 1989. The way we were; or; the real truth about generative semantics: A memoir. Journal of Pragmatics 13. 939–988.

Laudan, Larry. 1977. Progress and its problems. Berkeley, CA: University of California Press.

Mauskopf, Seymour and Ted Schmaltz (eds.). 2012. Integrating history and philosophy of science: Problems and prospects. Dordrecht: Springer.

Mayr, Ernst 1990. When is historiography Whiggish? Journal of the History of Ideas 51. 301–309.

McCawley, James D. (ed.). 1976. Notes from the linguistic underground. New York, NY: Academic Press.

McCawley, James D. 1980. Review of Newmeyer (1980). Linguistics 18. 911–930.

Miller, David Marshall. 2012. The history and philosophy of science history. In Mauskopf & Schmaltz (2012, 29–48).

Murray, Stephen O. 1982. Review of Newmeyer (1980). Historiographia Linguistica 9. 107–112.

Murray, Stephen O. 1989. Recent studies of American linguistics. Historiographia Linguistica 16. 149–171.

Murray, Stephen O. 1994. Theory groups and the study of language in North America: A social history. Amsterdam: Benjamins.

Newmeyer, Frederick J. 1980. Linguistic theory in America: The first quarter century of transformational generative grammar. New York, NY: Academic Press.

Newmeyer, Frederick J. 1986a. Linguistic theory in America: The first quarter century of transformational generative grammar, 2nd edn. New York, NY: Academic Press.

Newmeyer, Frederick J. 1986b. The politics of linguistics. Chicago, IL: Chicago University Press.

Newmeyer, Frederick J. 1986c. Has there been a 'Chomskyan revolution' in linguistics? Language 62. 1–18.

Newmeyer, Frederick J. 1996. Generative linguistics: A historical perspective. London: Routledge.

Nielsen, Janet. 2010. Private knowledge, public tensions: Theory commitment in postwar American linguistics. PhD dissertation. University of Toronto, Toronto.

Pietsch, Wolfgang. 2016. Two modes of reasoning with case studies. In T. Sauer and R. Scholl (eds.) The philosophy of historical case studies. Cham: Springer. 49–68.

Tomalin, Marcus. 2008. Linguistics and the formal sciences: The origins of generative grammar. Cambridge: Cambridge University Press.