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From 'scientific revolution' to 'unscientific revolution': an analysis of approaches to the history of generative linguistics

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Abstract

This paper is devoted to the challenge that generative linguistics poses for linguistic historiography. As a first step, it presents a systematic overview of 19 approaches to the history of generative linguistics. Second, it analyzes the approaches overviewed by asking and answering the following questions: (a) To what extent and how are the views at issue biased? (b) What central topics do the approaches discuss, how successfully do they tackle them, and how do the various standpoints converge and diverge? (c) How do the approaches relate to general trends in the philosophy and history of science? The concluding step summarizes our findings with respect to Chomsky's impact on linguistic historiography.

Keywords: Chomsky's revolution; Generative linguistics; Historiography of linguistics; Philosophy of linguistics

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1. Introduction

Besides linguistics, Chomsky's impact on particular subfields of psychology, philosophy, cognitive science and other disciplines is well known and widely discussed. In the present paper we will examine some consequences of his activity which, though unquestionably relevant, have not been discussed as intensively and as systematically as his impact on the fields mentioned: we will analyze his influence on linguistic historiography.

In order to highlight the significance of the way generative linguistics challenges historiography, let us start by quoting Joseph who, in his seminal article entitled 'The structure of linguistic revolutions' – the title of which makes reference to Kuhn's famous book – describes linguistics in the following way:

„Rare is the linguist who does not consider himself part of a linguistic revolution accomplished in recent memory, or in progress, or both. [...] Revolution is the master plot for linguistic history, *what gives sense to our work and careers, what makes it worth getting out of bed in the morning*” (Joseph, 1995, p. 379; emphasis added).

He characterizes the Department of Linguistics and Philosophy at the Massachusetts Institute of Technology as follows:

„The department is unique in having fostered over the last forty years not one, but easily *a dozen* large-scale or small-scale *linguistic revolutions*. Most of them have been revolutions against earlier revolutions spawned there as well. More amazingly still, many of them have been led by the same man, revolting against his own program of a few years before: *Noam Chomsky, Serial Revolutionary*., (Joseph, 1995, p. 380; emphasis added).

This is a witty illustration of the challenge that generative linguistics poses not only for linguistics but also for its historiography. In particular, from the mid-1960s on, Chomsky’s activity has been closely interwoven with Kuhn’s idea of scientific revolution. Nevertheless, the association of the notion ‘revolution’ with generative linguistics raised heated debates. Some of the historiographic approaches to generative linguistics accept the thesis of its revolutionary nature along with Kuhn’s framework for the history of science. Others use significantly different categories, ranging from revolution in a non-Kuhnian sense, through ‘evolution’, ‘coup’, ‘erosion’, ‘spurious science’ to ‘unscientific revolution’. Thus, the current state of the art of the historiography of generative linguistics consists of a complicated network of very different views and its systematic analysis is not a trivial task.

The *aim* of the present paper is to overview and to analyze approaches to the history of generative linguistics. We will confine the overview and the analysis merely to approaches which explicitly define themselves as historiographic; the linguistic debates raised by Chomsky’s works are outside the scope of the present study. Because of the extreme richness of the literature, we do not strive for completeness; rather, we will focus on those relevant approaches which seem to play a pivotal role in the historiographic debates.

Our *line of reasoning* consists of three steps. In Section 2 we will give a systematic overview of 19 different approaches to the history of generative linguistics. As the notion of ‘revolution’ is an indispensable point of departure, we will use it as the main criterion of systematization and describe the views to be discussed in comparison to this notion. With respect to each major stage of the development of generative linguistics, we will start from the assumption of its revolutionary nature and – in so far as we gradually depart from this – we will discuss the rival views. Thereby we will point out important differences and similarities between particular claims maintained by the authors. Accordingly, the order in which we present the approaches will be *systematic and not chronological*.

As the next step, in Section 3 we will systematically analyze the approaches we have surveyed in Section 2 from three points of view: their biased nature, the topics they discuss and their relation to current trends in the history and philosophy of science.

The concluding step will be taken in Section 4 where we will summarize our findings with respect to Chomsky’s impact on linguistic historiography.

2. A systematic overview of approaches to the history of generative linguistics

2.1. Syntactic structures: revolution, evolution, metalinguistics, coup d'état or rhetoric?

2.1.1. Kuhnian revolution and paradigm

Since the second half of the 1960s, numerous works have been published which call *Syntactic Structures* 'revolutionary' in Kuhn's sense. To our knowledge, Thorne (1965, p. 74) was the first to claim explicitly that "[...] a revolution of the kind Kuhn describes has recently taken place in linguistics – dating from the publication of Chomsky's *Syntactic Structures* in 1957".¹ (For further examples, see [Leiber, 1975], [Yergin, 1994], [Searle, 1972], [Sklar, 1994] and [Vidanovič, 2006] and many others.)

In order to demonstrate an important circumstance, we illustrate this stance with Koerner (1978), among the numerous works which claim that *Syntactic Structures* led to a new Kuhnian paradigm in linguistics. The reason why it is instructive to review Koerner's argument is that – as we will see later – in several further papers he considered other possibilities as well, so his contribution to the discussions demonstrates the complexity of the problems raised (see e.g. Koerner, 2004, p. 2).

After analyzing the work of Schleicher, Saussure and Chomsky, Koerner (1978) concludes that the conditions that enable us to talk about a Kuhnian paradigm are met in all three cases.² As for generative linguistics, Koerner supports this claim with two considerations. First, while Chomsky's generative linguistics and Saussure's structuralism share common features, Koerner also enumerates those features of the theory that was introduced in *Syntactic Structures* that significantly differ from the latter. Koerner's second argument is that although *Syntactic Structures* could not remain unaffected by American structuralism, it reflected *the 'climate of opinion'* which was characteristic of the natural sciences in the 1950s, but which American structuralism was not ready to embrace. The main characteristic of this climate of opinion was that mathematics and physics provided the scientific model to be followed for all disciplines that intended to have themselves recognized as viable fields of science (Koerner, 1978, p. 42).³ Accordingly, Koerner claims that the reason why *Syntactic Structures* resulted in a new paradigm is not that Chomsky created something *ex nihilo* (Koerner, 1978, p. 44), but rather, that he understood and adapted the 'climate of opinion' in a creative and genuine manner. Koerner's approach was criticized for example in Hymes and Fought (1981).

2.1.2. Revolution as a change of research program

Hacken introduces the notion of 'research program', which differs from Kuhn's 'paradigm' in that it emphasizes the intellectual aspects and abstracts from the social ones (Hacken, 2007, p. 18).⁴ He argues that Chomsky's work has been a coherent and continuously developing research program since the fifties without revolutionary breaks.

In analogy to the fact that in Kuhn's framework 'paradigm' and 'revolution' are closely connected, Hacken defines 'scientific revolution' with the help of 'research program': "A revolution is a change of research programme" (Hacken, 2007, p. 25). Accordingly, 'scientific

revolution' captures only certain intellectual aspects of inquiry in generative linguistics and does not extend to the social.

These two notions are the pillars of Hacken's approach which yields the following findings:

„The emergence of Chomskyan linguistics *was a revolution* because it is based on a *different research programme* from Post-Bloomfieldian linguistics and gradually replaced the latter.

The Chomskyan revolution can be seen as *progress* because it replaced the emphasis on procedures applied to the set of data by a productive interaction of hypotheses and tests in the empirical cycle.” (Hacken, 2007, p. 179; emphasis added)

For the detailed discussion and the critical analysis of Hacken's approach, see Kertész (2010).

2.1.3. Laudanian revolution

That there is enormous uncertainty regarding the notion of 'Chomsky's revolution', is well illustrated by the change of Newmeyer's views. Newmeyer – who has been in argument with Koerner for decades – called Chomsky's appearance 'revolutionary' in the first edition of his book *Linguistic Theory in America*, without referring to Kuhn (Newmeyer, 1980), though he cited Kuhn (1970) in the second edition (Newmeyer, 1986a, pp. 36–37). Subsequently, Newmeyer modified his approach in the paper in which he reacted to Koerner (1983) (Newmeyer, 1986b; for more on this, see also Section 3.2.1).

Newmeyer (1986b) claims that there was a Chomskyan revolution both intellectually and sociologically. His first argument is that Chomsky's *Syntactic Structures* has brought *intellectual* innovation into linguistics. When attempting to justify the intellectually revolutionary nature of *Syntactic Structures*, Newmeyer partly relies on the same factors as Koerner (1978), namely the pattern of theories in the natural sciences. As further factors he mentions: (a) the revolutionary importance of the central role of syntax; (b) in connection with the central role of syntax, the interdisciplinary nature of the 'revolution', which has brought primarily psychology and philosophy within its scope; as well as (c) the successful adaptation and synthesizing of the progressive elements of American structuralism.

According to his second argument, *Syntactic Structures* was revolutionary from a *sociological* perspective as well, but it is problematic that historiographers use conflicting criteria to separate revolutionary from non-revolutionary scientific achievement. By making reference to Percival (1976), Newmeyer rejects the application of Kuhn's framework, emphasizing that “if Kuhn is correct, *no* (scientific) revolutions have occurred in linguistics” (Newmeyer, 1986b, p. 6, emphasis added; see also Percival, 1976, p. 289). Namely, generative linguistics does not meet two necessary requirements of the Kuhnian notion of revolution: it has not become generally accepted in the discipline, and its appearance had not been preceded by a crisis.⁵ Based on these facts, however, Newmeyer's conclusion is not that *Syntactic Structures* did not trigger a revolution, but that it did, although in a *non-Kuhnian sense*. In answer to the question of what sociological criteria we can base the notion of scientific revolution on, he claims Laudan (1977, pp. 137–138) has provided the correct criterion. Laudan uses the notion of 'research tradition', which is analogous to the Kuhnian notion of 'paradigm'. In his view, it is not at the point at which the entire discipline or at least a definitive part of it accepts a certain

research tradition that a scientific revolution takes place. Rather, it takes place when a research tradition attracts enough attention to make scholars within the field take it seriously, in the sense that they either accept it or consider it a rival. With reference to Hymes (1964, p. 25), Newmeyer points out that transformational generative grammar had become the reference point of research within linguistics by the beginning of the 1960s, and thus it completely conforms to Laudan's criterion. It has become an accepted routine to explicitly reject Chomsky's views when alternative approaches are introduced. Therefore, if Laudan's view is correct, then the Chomskyan revolution has taken place in linguistics in the Laudanian sense (Newmeyer, 1986b, pp. 7–8).

Newmeyer's third argument is that the number of Chomsky's followers radically increased and these followers gained significant positions at an increasing rate. He maintains, however, that contrary to appearances, Chomsky and his followers did not have real influence, since: (a) there are numerous opportunities for students at American universities to continue their studies within non-generative linguistics; (b) generative linguists receive only a modest amount of funding; (c) the *Linguistic Society of America* was not controlled by the generativists. For the criticism of Newmeyer's historiographic attitude in general see (Koerner, 1989), (Koerner, 2002), (Koerner, 2004), (Murray, 1981), (Murray, 1982), (Murray, 1989), (Murray, 1999), (McCawley, 1980), (Tomalin, 2008), (Lakoff, 1989) and (Huck and Goldsmith, 1995), etc.

2.1.4. Evolution

In his later work Koerner realized that “the term ‘revolution’ does not properly apply to TGG” ([Koerner, 1989], [Koerner, 2002] and [Koerner, 2004]) – and thereby entered into a heated debate with Newmeyer. He formulated the hypothesis ([Koerner, 1989], [Koerner, 2002] and [Koerner, 2004]) according to which the movement that had been triggered by *Syntactic Structures* was not a revolution but the evolution of the former state of linguistics.⁶ He claims to prove that generative linguistics could not have become as strong as it did by the 1960s if factors other than its intellectual innovations had not played a part in this process.

First, while making reference to Maher (1980), Koerner points out that the impact of *Syntactic Structures* may have been influenced by *fashion*. However, he is not in full agreement with Maher, because in his opinion there were other relevant factors, too.

Second, the *considerable amount of financial support* which, in Koerner's view, Chomsky and his circle received, also contributed to the success of generative linguistics. Therefore, Koerner's standpoint is the opposite of Newmeyer's (1986b) which we summarized at the end of the previous section. (Koerner, 1989), (Koerner, 2002) and (Koerner, 2004) cites McCawley's summary, according to which the peculiarities of American University administration favor situations where smaller groups increase the number of their followers through funding while their intellectual achievement does not justify this. McCawley's statement leads Koerner to conclude that the rapid growth of Chomsky's influence could not be attributed exclusively to the content of *Syntactic Structures*, but “it may have had something to do with a fad”, too ([Koerner, 1989], [Koerner, 2002] and [Koerner, 2004]).

Third, (Koerner, 1989), (Koerner, 2002) and (Koerner, 2004) stresses that the representation of Chomsky's initiative as a revolution is basically not rooted in the innovative nature of the content of *Syntactic Structures* but is of an *ideological* nature.

Fourth, according to (Koerner, 1989), (Koerner, 2002) and (Koerner, 2004) the *revolutionary rhetoric* and the intensive *propaganda activity*, emphasizing the radical innovative nature of generative grammar, also played a role in the fact that Chomsky's early work became known as revolutionary. This propaganda was related to the constant, aggressive, rude, often *ad hominem* attacks against the representatives of neo-Bloomfieldian structuralism. Koerner presents compelling examples of this.⁷

Fifth, since the claims summarized in the previous paragraphs lead us to conclude that the above mentioned circumstances surrounding the appearance of *Syntactic Structures* – propaganda, fashion, ideology, funding – may have made it *seem* revolutionary, Koerner examines to what extent the views elaborated in the book were innovative in comparison to the findings of neo-Bloomfieldian structuralism. He enumerates and analyses the factors which he claims prove that the appearance of *Syntactic Structures* – contrary to the propaganda – did not represent a radical break with the latter. Rather, it was deeply rooted in its findings and methods, with special regard to the impact Hockett and Zellig S. Harris had directly on the linguistic theory and methodology suggested in *Syntactic Structures*: it “is basically post-Saussurean structuralism” ([Koerner, 1989], [Koerner, 2002] and [Koerner, 2004]).⁸

Sixth, Koerner claims that the proponents of generativism were striving to *rewrite the history of linguistics* in accordance with their interests.⁹ We illustrate this statement with two examples. Koerner (1989, p. 102) considers Newmeyer's work to be ‘partisan historiography’ – also called ‘Whig historiography’¹⁰ – implying that it represents the interests of the generative camp and gives a one-sided account of the facts while disregarding the methodology of historiography. Newmeyer (1986a) cites Voegelin's review of *Syntactic Structures* (Voegelin, 1958) and claims that Voegelin evaluated the appearance of Chomsky's work as a Copernican revolution. However, the cited expression occurs in a context in which Voegelin evaluates *Syntactic Structures* negatively (Voegelin, 1958, p. 230; cited in Koerner, 1989, p. 130.). That is, Voegelin, in fact, stated the opposite of what Newmeyer attributes to him and therefore, Newmeyer's method is extremely biased historiography.¹¹ We mention Chomsky's *Cartesian Linguistics* (Chomsky, 1966) as a second example of the rewriting of the history of linguistics. According to Koerner, this book has little to do with the historical facts and – similarly to (Newmeyer, 1980) and (Newmeyer, 1986a) – is a ‘partisan historiography’, whose only aim is to legitimize generative linguistics ([Koerner, 1989] and [Koerner, 2004]).¹²

We highlight three characteristics of the historiographic method as outlined, among others, in (Koerner, 1995) and (Koerner, 2002). First, it strives for impartiality, objectivity and the avoidance of ‘partisan historiography’.¹³ Second, it contains careful philological analyses of a wide range of written sources and well-documented interviews (Koerner, 2002, pp. 165–157).¹⁴ Third, it demonstrates familiarity both with the linguistic theory at issue and the extra-linguistic factors (such as sociological, and even political aspects) which is an important prerequisite for linguistic historiography (Koerner, 2002, p. 155).

2.1.5. The impact of the formal sciences

Tomalin (2008) motivates his investigations with the criticism of historiographic approaches to generative linguistics. In his view, they are contradictory with respect to the treatment of the relationship between the formal sciences and generative linguistics. Namely – as was emphasized as early as 1957 in Lees’s famous review of Chomsky (1957), a review which played a decisive role in the latter’s reception – there is no doubt that generative linguistics has been under the influence of the formal sciences from the very beginning. Therefore, it has been claimed that it “was a *superior* linguistic theory primarily because it was *more ‘scientific’* (whatever that means) than the syntactic theories that preceded it” (Tomalin, 2008, p. 1; emphasis added). However, the state of the art in the historiography of generative linguistics does not reveal this relationship properly. Tomalin’s aim is – as the title of his book suggests – to resolve this contradiction by arguing for the claim that *Syntactic Structures* originates in the formal sciences (Tomalin, 2008, pp. 4, 183, 186).

By ‘formal sciences’ Tomalin means disciplines which take their methods from mathematics and formal logic, with special emphasis on the application of the axiomatic-deductive method. In this respect, they rest on a common scientific basis (Tomalin, 2008, p. 3). Tomalin supports his central claim mentioned with two lines of reasoning. First, he carries out precise comparative analyses of the mathematical and logical literature of the first half of the twentieth century, the literature of Bloomfieldian and neo-Bloomfieldian structuralism, and Chomsky’s writings before the publication of *Syntactic Structures*. Thereby, Tomalin obtains three new findings. One concerns Bloomfield’s attitude to the formal sciences. Namely, Bloomfield’s mistrust of semantic considerations was rooted in Hilbert’s proof-theoretical techniques. Tomalin characterizes Bloomfield as an early proponent of the axiomatic-deductive method, and therefore one of the latter’s achievements is, as Tomalin calls it, “the rehabilitation” of the mathematical aspects of Bloomfield’s structuralism. The result is:

„The conclusion offered in this book is that the formal sciences and post-Bloomfieldian linguistics appeared to approach their different tasks in a similar fashion partly because the methodologies used in both disciplines were directly influenced by Formalism during the first half of the twentieth century.” (Tomalin, 2008, p. 183)

Furthermore, Tomalin re-evaluates the role which Bar-Hillel played in shaping Chomsky’s thinking. He shows that Bar-Hillel continuously and profoundly influenced Chomsky’s intellectual development. Further, Tomalin makes similar observations with respect to Goodman’s influence which proved to be much more complex and effective than the historiographic literature has assumed so far. For example, the way Chomsky made use of the criterion of simplicity in his early writings was the result of Goodman’s influence.

Tomalin’s second line of reasoning focuses on the claim that those components of generative grammar which are rooted in the formal sciences remained constant through the whole history of generative linguistics and are still present in the Minimalist Program (Tomalin, 2008, pp. 188–200).¹⁵ Thus, he seems to refer to the fact that the scientific superiority of generative linguistics remained the same throughout its history despite changes in the theoretical apparatus. Such a constant component is the axiomatic-deductive character of Chomsky’s theory. For example, derivations in the Minimalist Program start with primitive elements such as lexical and formal features and result in phonological form and logical form pairings (Tomalin, 2008, p. 188). Another example is that, as is well-known, recursion is one of the

pillars of minimalism. Finally, ‘economy’ (or ‘simplicity’, or ‘elegance’ or ‘compactness’ and the like) is a crucial aspect of minimalism closely related to Goodman’s influence.

We remark that Seuren (2009) sharply criticizes Tomalin’s analyses for their one-sided and unbalanced nature as a result of which it overemphasizes the impact of the formal sciences on the rise of generativism. See Nevin’s (2009) evaluation of both Tomalin’s stance and Seuren’s criticism.

2.1.6. One of the neo-Bloomfieldian trends

(Hymes and Fought, 1981), (Matthews, 1993) and (Matthews, 2001) belong to those historiographies which question the historical account attributing a revolutionary relevance to *Syntactic Structures*. The authors interpret the latter as an organic part of neo-Bloomfieldian structuralism. Hymes and Fought (1981) was the first among these works, and is representative of this view. The authors claim, first, that Neo-Bloomfieldian structuralism was not a coherent school, but consisted of a variety of alternative conceptions and individual initiatives (Hymes Zellig S. and Fought, 1981, p. 156). Second, within neo-Bloomfieldian structuralism, *Syntactic Structures* contributed to the victory of Zellig S. Harris’s approach over that of Trager and Smith (Hymes and Fought, 1981, p. 155). Tomalin (2008, p. 11) calls these claims “provocative”.

The authors emphasize that the “climate of opinion” did not change immediately after the appearance of *Syntactic Structures* (Hymes and Fought, 1981, p. 154).¹⁶ Chomsky’s work was evaluated as one of the standpoints within neo-Bloomfieldianism. It was not considered as a challenge that might lead to the redefinition of the discipline.

In their book on American structuralism – in which Chomsky’s activity is only one episode – the authors devote much space to the methodology of linguistic historiography. They suggest the simultaneous consideration of a series of factors. Among others, referring to Percival (1976) they reject the application of Kuhn’s approach to linguistics (Hymes and Fought, 1981, pp. 24–25, 239–241, 245–246) and criticise Koerner (1978). They also reject building linguistic historiography on one basic concept such as ‘paradigm’ and claim that “a single term will not suffice” (Hymes and Fought, 1981, pp. 25). Much more complex and differentiated notions are needed in order to capture “the rich range of materials actually forming the data to be explained” which every “serious historiography” is expected to use (Hymes and Fought, 1981). These materials also include the personal and institutional dynamics as well as unpublished documents. The authors discuss the range of data which they believe to be potentially relevant for historiographic research. Naturally, these principles apply to the historiography of linguistics in general and, as element of this, to that of generative linguistics in particular.

2.1.7. Chomsky as a metalinguist

Seuren (1998) carefully analyses the relationship between Chomsky’s and Zellig S. Harris’s early views. He argues that they were thinking along the same lines and that Chomsky’s work is deeply rooted in the way Zellig S. Harris practiced neo-Bloomfieldianism (Seuren, 1998, p.

248). However, in spite of this, it is possible to reveal in what respect Chomsky's early work was original (Seuren, 1998, 249 ff.). The main achievement of Chomsky (1957) is that it (a) systematized and critically evaluated the notions and methods of neo-Bloomfieldian structuralism (b) attributed a greater significance to the formal properties of rules than the latter did (c) made the concept of transformation more precise (d) destroyed behaviorism and (e) placed linguistics in the context of the philosophy of science. It is the latter point that is especially relevant in Seuren's view, because "most of Chomsky's work in theoretical linguistics deals with *metatheoretical* questions, which makes Chomsky a *metalinguist rather than a linguist*" (Seuren, 1998, p. 252; emphasis added).

By raising and answering pivotal questions of the philosophy of science, Chomsky established the relationship between the latter and linguistics. He called attention to issues which had fallen outside the scope of neo-Bloomfieldianism: the nature of linguistic data, realism vs. nominalism, the criteria of adequacy, formalization, explanation, causality etc. (Seuren, 1998, p. 252 ff.).¹⁷

Seuren (1998) analyzes the history of Western linguistics from the point of view of methodology. Therefore, his approach focuses on synthesizing aspects of the history and the philosophy of science. Nevertheless, he does not specify the details of his metascientific background assumptions he takes for granted as his point of departure. For the evaluation of Seuren's historiographic approach see e.g. Droste (1999) and Falk (1999).

2.1.8. Rhetoric

Harris's book (1993a) provides an unvarnished account of the cruelty of the 'linguistics wars'. It gives a detailed analysis of the war triggered by the generativists against neo-Bloomfieldian linguists and that between interpretive and generative semanticists. Nonetheless, the manuscript was finished before the rise of the Minimalist Program. The impact of the book exceeded the tight boundaries of both linguistics and the historiography of linguistics. On the one hand, it instilled the notion of 'linguistics wars' into the public mind by reaching a relatively wide range of readers due to its style (which Joseph, 1995, p. 397 called "jaunty"). On the other hand, several of its main characters were active at the time of its publication (and many of them, the heroes of the battles between interpretive and generative semantics as well as members of the younger generation of the generativists, are still active). Chomsky, for instance, completely rejects the picture Harris paints of the history of generative linguistics in the interview given to Grewendorf (1995, pp. 231–232). He expresses a similar view when interviewed by Barsky in that he describes Harris's story as belonging to the world of fantasy (Barsky, 1998, pp. 56, 151). Barsky (1998, p. 56) evaluates Harris's work as Foucauldian historiography (in agreement with Chomsky, cf. Grewendorf, 1995, p. 232), which provides a psycho-social explanation for the changes surfacing in the research while emphasizing the power struggle between the participants. Tomalin (2008, p. 14) maintains that Harris's work cannot be taken seriously because it was written in the popular science tradition. In contrast, Joseph (1995) does not question that Harris's book is a competent scientific achievement, nor does Langendoen (1995), although they criticize it in different respects.

Similarly to Murray (1994), Harris attributes the success of generativism not to a scientific revolution, but to rhetoric (Joseph, 1995 analyzes the similarities and the differences between the two views; see also Section 2.1.9).

First of all – in agreement with the claims made by (Newmeyer, 1986a), (Newmeyer, 1986b) and (Murray, 1994) and (Koerner, 1989), (Koerner, 2002) and (Koerner, 2004) – American structuralism was not in a Kuhnian crisis when and after *Syntactic Structures* was published. For this reason, one of the prerequisites of Kuhnian revolutions is not met. Harris (1993a, pp. 35–37) demonstrates – through the examples of (Trager and Smith, 1957) and (Harris, 1951) – that in the 1950s “not the slightest hint of frustration at the Bloomfieldian program” could be noticed and “there is little indication in the literature of the period that there was a crisis on any front” (Harris, 1993a, p. 36). Therefore, he concludes that “measured dissent, pluralism, and exploration [...] represent *the exact opposite* of Kuhn’s definition of crisis” (Harris, 1993a, p. 37; emphasis added).

Second, and also in harmony with Murray’s (1994) and (Koerner, 1989), (Koerner, 2002) and (Koerner, 2004) views, Harris claims that there is continuity between *Syntactic Structures* and neo-Bloomfieldian linguistics. He stresses that in the 1950s most linguists conceived of *Syntactic Structures* mainly as the propagation of Zellig S. Harris’s theories and that Chomsky’s program appeared to be a consistently improved version of neo-Bloomfieldianism (Harris, 1993a, pp. 37, 39). In this respect, Harris seems to accept Hymes and Fought’s (1981) position.

Third, Harris (1993a, p. 33) believes that it was Chomsky who turned against his supporters by destroying the optimistic atmosphere of peaceful development (see also Murray, 1994 and Lakoff, 1989 for the same view to be discussed in Sections 2.1.9 and 2.2.3, respectively).

Finally, according to Harris, Chomsky and his followers did not apply substantial arguments against the claims of neo-Bloomfieldians and later those of generative semanticists by responding to arguments with arguments. Rather, they based their argumentation on humiliating and ridiculing their opponents in the debate with the help of rhetorical tools. Chomsky’s opinion reflected the contempt of those whose views differed from his and he continuously emphasized that except for himself and his small circle “virtually everyone else in linguistics is intellectually and emotionally and even morally callow” (Harris, 1993a, p. 78) and thus not suited to conduct scientific research. Harris (1993a, pp. 61, 69–73) provides a compelling description of the rhetorical arsenal reflecting the “missionary zeal”, with which Chomsky’s adherents led their war against neo-Bloomfieldians.

In order to provide a coherent description of Harris’s view, we have to mention at this point – although this refers to a later stage in generative linguistics (see Section 2.2) – that according to Harris, the development of Chomskyan generative linguistics after *Aspects* was not motivated by intellectual goals with respect to Chomsky’s reaction to the appearance of the generative semanticists. Rather, it was motivated by his objective of retaining power: “Attacking generative semantics [...] occupied virtually all of his linguistic energies for several years” (Harris, 1993a, p. 143). Harris discusses a number of episodes in retrospect and demonstrates the intrigues and rhetorical tools which Chomsky and his immediate circle used to achieve this goal ([Harris, 1993a] and [Harris, 1993b]). Harris (1993a, pp. 160–161) considers three of these as especially effective: Chomsky’s *ad hominem* and straw man arguments¹⁸ as well as his vagueness, which he himself frequently chose to employ while at the same time reproaching his opponents for that very fault.

2.1.9. Revolutionary rhetoric and coup

In the literature, the thesis has been widely discussed according to which after the publication of *Syntactic Structures*, Chomsky and his group carried out a palace coup followed by revolutionary rhetoric ([Anttila, 1975], [Gray, 1976], [Murray, 1980], [Murray, 1994], [Murray, 1989], [Murray, 1999], [Koerner, 1989], [Koerner, 2002] and [Koerner, 2004]). It was Murray who elaborated this view very thoroughly and clearly. Therefore, in what follows we discuss Murray's arguments in a relatively detailed way.

Murray adapts a sociological approach to the historiography of linguistics which is, on the one hand, based on a significant modification of Kuhn's approach, and on the other hand, on the direct application of the sociological view of (Griffith and Mullins, 1972) and (Griffith and Miller, 1970).

In his view, the notion of 'revolution' is narrowed down *exclusively* to rhetoric in the sense that the terminology of the given group stresses the revolutionary nature of innovations as opposed to their continuity. Thus, in this respect, "the notion of scientific revolution has been relativized here to consideration of 'revolutionary rhetoric'" (Murray, 1994, p. 23). This means the following.

First, the generativists' rhetoric of revolution – in contrast to (Newmeyer, 1986a) and (Newmeyer, 1986b) view discussed above – was not associated with revolutionary innovations either intellectually or methodologically. Murray (1999, p. 343) argues that there is a crystal clear continuity between the content of *Syntactic Structures* and neo-Bloomfieldian structuralism. In this aspect, he agrees with Hymes and Fought's (1981), (Koerner, 1989) and (Harris, 1993a) position. He concludes, however, that for this reason *Syntactic Structures* can only be described through the exploration of sociological factors surfacing at the level of rhetoric.¹⁹

Second, instead of revolution, it was rather a *coup d'état* as a result of which Chomsky and his group captured power. The coup had been thoroughly prepared and then organized by Halle and others at the 1962 international linguistics conference held in Cambridge (Murray, 1994, pp. 239–240). For the contrary interpretation of these facts (see Newmeyer, 1986b, pp. 8–9). In Murray's view, the intellectual and organizational leader embodied in Chomsky's personality played a significant role in capturing and retaining power as, with Chomsky's leadership, the group was able to dominate and alter the institutional, organizational, and financial structure of linguistics (Murray, 1994, p. 445; see also Section 3.2.4).

Third, according to Murray, one of the tenets of the Kuhnian approach to scientific revolutions is that the representatives of the 'old paradigm' reject new ideas. Seemingly in accord with this, Chomsky stated in several interviews, memoirs, and letters that his early publications attempts were received adversely and his papers were constantly rejected by established linguistic forums (for quotes, see Murray, 1994, pp. 230–234). Newmeyer (1986b, p. 8, footnote 7) claims the same. (Murray, 1994) and (Murray, 1999) proves the opposite of this by making reference to original documents that he thoroughly described and explored: (a) Bernard Bloch's correspondence with Chomsky documents that Bloch supported all the four papers that Chomsky submitted to *Language* and he paid special attention to Chomsky even when no one else did ([Murray, 1994] and [Murray, 1999]). (b) Only one of Chomsky's papers was rejected. Chomsky submitted it to *Word*, whose editor-in-chief was André Martinet at that time. Martinet did not belong to the neo-Bloomfieldian school. Therefore, it is

not the case that the emergence of Chomsky's views was hindered by a member of the old paradigm which was to be replaced by the new ([Murray, 1994] and [Murray, 1999]). (c) It is well known that Lees' (1957) review played a significant role in the positive professional reception of *Syntactic Structures*. However, it appeared under rather unusual circumstances: Lees was Chomsky's student and thus he was not an unbiased, external expert; moreover, the review was published *prior to* the appearance of the book²⁰; Bloch placed the review of a book by a then unknown author above the reviews of the books of two leading linguists (Murray, 1999, p. 345). (d) The documents explored by Murray witness that they are in sharp contrast with Chomsky's and Newmeyer's claims regarding the publication of Chomsky's dissertation, entitled *The Logical Structure of Linguistic Theory*. The manuscript of *The Logical Structure of Linguistic Theory*, in sharp contrast with Chomsky's and Newmeyer's claims, was not rejected but accepted by Mouton, and two other Dutch publishers also expressed their interest in it. However, as Murray shows, Chomsky did not send back the contract, which had already been prepared, to the publisher ([Murray, 1994] and [Murray, 1999]).²¹

Consequently, according to Murray (1994, pp. 244; 486–487), the reception of generative linguistics in the 1950s and 1960s is in sharp contrast with Kuhn's model:

„[...] a major anomaly is that the conflict between generations was not instigated by the older generation rejecting the younger generation's innovation. Chomskyan work was published, sought out and taken seriously by major neo-Bloomfieldians. The most central of them *actively fostered Chomsky's and Lees careers*. In terms of aggression, *the Chomskyan struck first*. Their revolutionary rhetoric was *not* a reaction to the incomprehension of the 'establishment', nor a defense against neophobia or persecution by angry elders" (Murray, 1994, p. 244; emphasis added).²²

In his review of Murray's book, Joseph (1995, p. 382) evaluates this as an "*unparalleled*" finding, which was "*a big bombshell*" (Joseph, 1995, p. 388). Newmeyer (1986a, p. 36), in contrast, calls these conclusions also discussed in Murray (1980) into question.

Murray's approach makes the following crucial generalization, which sheds new light on the historiography of generative linguistics:

„There *does not seem to be any relationship* between the novelty of ideas and the claims to novelty. Major changes in assumptions may be made by changes wrapped in the mantle of tradition [...], while minor changes may be heralded as epoch-making breakthroughs after 'dark ages'. [...] The causal direction posited by Kuhn to explain scientific revolution – viz. that rejection of new ideas leads to counter-community – *needs to be reconsidered*. In the case of transformational grammar, *the younger generation attacked elders* who had been facilitating the diffusion of TGG work, rather than the elders attacking those daring to put forth new ideas" (Murray, 1994, pp. 486–487; emphases added).

2.2. Aspects of the theory of syntax: Kuhnian revolution, ideology or untenable methodology and the clash of two kinds of personalities?

2.2.1. Kuhnian revolution

Although it follows from (Newmeyer, 1986b), (Koerner, 1989), (Koerner, 2002), (Koerner, 2004), (Murray, 1980), (Murray, 1994), (Murray, 1999), (Seuren, 1998) and (Harris, 1993a) claims that the appearance of *Syntactic Structures* did not trigger a Kuhnian scientific revolution in linguistics, it does not follow that later stages of generative linguistics could not have been revolutionary. As early as the 1970s the assumption was considered that it was *Aspects* (Chomsky, 1965) that bore revolutionary characteristics in the Kuhnian sense rather than *Syntactic Structures*.

McCawley (1976, p. 4) argues for this thesis by making reference to three factors: (a) by the mid-sixties, the number of adherents of generative transformational grammar – originally a small minority of linguists – had greatly increased; (b) generative transformational grammar developed from an avant-garde movement into a leading and institutionalized field; (c) the nature of generative publications had significantly changed, because, while, at the end of the 1950s and at the beginning of the 1960s, they were primarily motivated by polemics against neo-Bloomfieldians, after the appearance of *Aspects* they became more research-oriented. According to McCawley, the first two factors amount to a scientific revolution; and the third indicates the beginning of normal science in Kuhn's sense.

(Koerner, 1989), (Koerner, 2002) and (Koerner, 2004) cites McCawley (1976) and highlights that in his opinion it is the following components of the content of *Aspects* which primarily support its revolutionary nature: (a) semantics was included in *Aspects*; (b) the systematic nature of *Aspects* exceeded that of *Syntactic Structures* and thus the findings that could be pursued through the application of the theory became relatively transparent; (c) the theory brought the investigation of syntactic universals to the foreground.

2.2.2. Ideology

As the rise of the generative semantics movement initiated by James D. McCawley, George Lakoff, Paul Postal and John Robert Ross is closely related to the appearance of *Aspects*, we now briefly discuss the conflict between the movement and Chomskyan interpretive semantics. The evaluation of the war between them is similar in (Harris, 1993a), (Harris, 1993b), (Harris, 1993c), (Huck and Goldsmith, 1995), (Murray, 1994) and (Seuren, 1996), and Seuren (1998); we exemplify this conception by Huck and Goldsmith (1995).

The 'official' chronicle generally accepted as a result of (Newmeyer, 1980) and (Newmeyer, 1986a) works – which was propagated in widely used textbooks such as Riemsdijk and Williams (1986), too – represents the process that led from the appearance of *Syntactic Structures* to later developments as continuous progress. Accordingly, generative semantics appears as a regressive episode which justly lost the battle against interpretive semantics, because it was both theoretically and empirically inferior to the latter.

Huck and Goldsmith reject this "received view". They ask the following question: if – according to the received view – the generative semantics movement fell apart because its

tenets were refuted by the proponents of interpretive semantics, then how could it happen that the very same tenets were integrated into the later development of generative linguistics? They answer this question by pointing out that, first, the received view according to which Chomsky's interpretive semantics refuted the tenets of generative semantics is "essentially ideological in character and scientifically unjustifiable" (Huck and Goldsmith, 1995, p. 93). Second, the demise of generative semantics was rooted not in its empirical and/or theoretical shortcomings, but rather, in ideological and personal factors as well as the argumentation strategies applied in accordance with the latter. Third, as a historiographic framework the authors choose Lakatos (1970). They believe that within the framework of Lakatosian 'scientific research programs' it is not possible to identify clear empirical, theoretical and conceptual grounds in the arguments put forward in the debate on which a decision between the two approaches could be based (Huck and Goldsmith, 1995, p. 7). Namely, the arguments put forward against the claims of generative semanticists were not strong enough to refute them, and what is more, the tenets of the two approaches are compatible. Huck and Goldsmith devote special attention to argumentation, because they "understand that scholarly argument forms a crucial part of scientific life and that the acquisition of knowledge depends vitally on the questioning of dogma" (Huck and Goldsmith, 1995, p. viii).

2.2.3. Untenable methodology and the clash of two kinds of personalities

We illustrate the evaluation of the rise and fall of generative semantics from the point of view of generative semanticists by Lakoff's memoir.²³ Lakoff's (1989) main claim is that the intellectual differences between interpretive and generative semantics rooted in *Aspects* originate from the different evaluation of the scientific method, and the different personalities involved in the debate.

Lakoff believes that one of the reasons why the war on the notion of D-structure found in *Aspects* started is that Chomsky attempted to force the methods of the natural sciences onto linguistics for the sake of the legitimacy of his theory, the prestige of his work, and the authority of his group. In her view this was fundamentally mistaken and harmful to the development of linguistics. With regard to this, she points out the following.

First, as opposed to the natural sciences, in the humanities – to which linguistics belonged and belongs, according to Lakoff – there were no uniform paradigms, but different views held simultaneously due to the fact that the object of investigation could be approached from various perspectives. Therefore, in the humanities, the style of argumentation was gentlemanly, acknowledging *the pluralism* of views (Lakoff, 1989, p. 964). As the methodology of the natural sciences which Chomsky forced on linguistics is unsuited to an examination of the variety, complexity, and contradictory nature of the investigated data, two consequences followed immediately: Chomsky introduced "the tradition of contentious and acrimonious adversarial argumentation" (Lakoff, 1989, p. 967) into linguistics, and along with this, "there was *no place for pluralism*" (Lakoff, 1989, p. 968; emphasis added).

Second, Lakoff does not dispute that it is possible to imitate natural sciences via formalizing, quantifying and the application of discrete categories. Nevertheless, the findings will be different from those of the natural sciences (Lakoff, 1989, p. 965). She makes a distinction between "doing science" and "playing science", where the latter imitates the former, and she places Chomsky's approach in the latter category. If one mixes the two types of activities, one

may apply *the surface manifestations* of scientific behavior for their own sake. For instance, quantifying, formalizing, and the replication of the findings, etc. may convey the impression that one is a responsible, real scientist (Lakoff, 1989, p. 965, footnote 8).²⁴

Third, the variety and complexity of linguistic data is not compatible with the adherence to the exclusive use of the formal methods represented in *Aspects* (Lakoff, 1989, pp. 595–596):

„To abjure non-discrete theories because they are unsettling, or because they conflict with the kinds of formalism we currently feel comfortable with, *is antiscientific in the most dangerous way*: analogous to the Church’s determination that Galileo’s claims were heretical because they were antithetical to current established wisdom.” (Lakoff, 1989, p. 956, footnote 5, emphasis added)

Finally, in accord with this observation and in stark contrast with the vulgarized linguistic application of Kuhn’s approach, she rejects the exclusive attitude of Chomsky’s group and believes that the pluralism of linguistics stemming from the constitutive properties of data contributes to a fuller and more precise understanding of the object of investigation.²⁵

Another reason why the war between generative semantics and the Chomskyan approach started is that two kinds of personalities were present in the group (Lakoff, 1989, pp. 944–946). One group tended to think formally and in terms of discrete notions; while the other consisted of humanists. These two types got along well since they had been forged together against the neo-Bloomfieldians, whom they had declared their common enemy. However, this war had ended with the victory of the Chomskyan group by the time *Aspects* was published (Lakoff, 1989, p. 968). At that time, although both kinds of personalities thought they would proceed with their research in accord with *Aspects*, the community split into two groups (Lakoff, 1989, p. 946). The two types of theoretical habits originating from the distinct personalities were closely related to the structure of the groups. Whereas formal thinking requires an autocratic and hierarchical organization, humanist personalities accept equality and the pluralism of ideas. Lakoff demonstrates vividly the autocratic system that Chomsky has established around himself and the increasing antipathy with which generative semanticists rejected not only Chomsky’s views but also his personality, as a result of which “once Chomsky was seen not to be an idol, he was recast as satanic, the Enemy” (Lakoff, 1989, p. 970).

The conflict that arose between the two groups by virtue of the distinct interpretations of the notion of D-structure in *Aspects* “was not a theory-internal conflict, such as can be resolved in terms of a Kuhnian paradigm shift” (Lakoff, 1989, p. 946). Therefore, *Aspects* cannot be considered to be revolutionary, and generative semantics was not a counter-revolution against it – it is simply the case that the history of generative linguistics either before or after *Aspects* cannot be evaluated in terms of the Kuhnian framework. For a detailed discussion of Lakoff’s memoir see Harris (1993b).

2.3. Generative phonology as Kuhnian revolution

As an alternative, Murray (1994, p. 238) proposes the hypothesis according to which generative phonology triggered a Kuhnian scientific revolution. In their argumentation discussed in the previous sections Murray as well as Harris and Koerner claim – amongst

other things – that *Syntactic Structures* as the direct continuation and syntactic extension of Zellig S. Harris’s work has not yielded significant intellectual innovations, and therefore has not provoked resistance from the neo-Bloomfieldians. The fights of the 1950s and 1960s culminated around phonemics and not syntax. Based on his sources, Murray (1994, p. 238) concludes that “[w]hat outraged neo-Bloomfieldians was not the theory of syntax, but the dismissal of phonemics.” According to Murray, the dismissal of phonemics in linguistics was a much more provocative step than anything that Chomsky could have said about syntax or the conflict between behaviorism and mentalism (Harris is of a similar opinion, cf. Harris, 1993a, pp. 59–61). He makes reference to Archibald Hill’s famous memoir in which Hill writes that he had got on well with transformational grammarians until his “darling, the phoneme” (Hill, 1980, p. 75) was attacked. Murray’s conclusion is that if this interpretation is correct and if one accepts Kuhn’s approach, then it would be better to deem Halle (1959) and Chomsky and Halle (1968) revolutionary and not *Syntactic Structures* or *Aspects* (Murray, 1994, pp. 238–239).

2.4. Lectures on Government and Binding: Kuhnian revolution or the decline of linguistics and the erosion of its object of investigation?

2.4.1. Kuhnian revolution

Although Murray (1994, p. 244, footnote 20) and Koerner (2004, p. 51) remark that Chomsky himself has never used the term ‘revolution’ when describing his own theories, his statements can lead us to conclude that he considers particular stages of his work to be revolutionary in a Kuhnian sense. For instance, he describes the Theory of Government and Binding (Chomsky, 1981; called later the ‘Theory of Principles and Parameters’, cf. Chomsky and Lasnik, 1993) in a way that is reminiscent of a *Copernican revolution* compared to previous versions of generative linguistics (see, for instance, Chomsky, 2002a, p. 95).²⁶ In an interview with Grewendorf, when asked which stage of generative linguistics he finds revolutionary, he explicitly mentions the Theory of Principles and Parameters (Grewendorf, 1995, pp. 219–222). The Theory of Principles and Parameters is assumed to be ‘revolutionary’ even in recent publications by his adherents.²⁷ Thereby the notion of revolution is not explicated, but tacitly understood in a Kuhnian sense.

However, it is not the above references that make this view really interesting, but the debate that was initiated by Lappin et al. (2000a) in *Natural Language and Linguistic Theory*, because it raised the issue of the revolutionary nature of the Theory of Government and Binding in comparison to the Minimalist Program. Therefore, we summarize the essence of this debate when discussing Lappin, Levine and Johnson’s approach in Section 2.5.4.

2.4.2. Decline and erosion

In 1993 and 1994, a fierce debate was published in *Zeitschrift für Sprachwissenschaft*. The target of Jäger’s article (Jäger, 1993a) was Chomsky’s cognitivism which had become powerful in the Theory of Government and Binding and its widespread applications by the beginning of the 1990s. Jäger’s central claim is that the dominance of the Theory of

Government and Binding significantly contributed to the decline of linguistics and the erosion of its object of investigation.

Jäger points out first that linguistics used the same procedure at each stage in the course of its history in so far as it redefined its object of investigation, i.e. ‘language’, by importing methodological principles and theoretical schemes in accord with the current development stage of the natural sciences. To the challenge posed by the natural sciences, linguistics responded by identifying itself with the ‘aggressor’ and took over the approach that appeared to be progressive (Jäger, 1993a, p. 92). In Chomsky’s case this was cognitivism. However, Jäger claims that the ‘real’ object of linguistics can be grasped by the hermeneutic-functional tradition, which examines ‘human’ communication, as embedded into the complex system of its social and historical environment. Therefore, the extreme scientism of the Chomskyan theories, which excluded that kind of linguistics that belonged to the hermeneutic-functional tradition, leads to the decline of linguistics.

Second, Jäger finds it paradoxical that while, on the one hand, cognitive science, according to its own judgment, has made much progress regarding the analysis of particular aspects of language, on the other hand, as a result of this ‘progress’, linguistics has lost control over its object of research, namely, the definition of ‘language’ (Jäger, 1993a, p. 90). As a consequence of the rigid adjustment to the methodology of the natural sciences, referred to as the ‘aggressor’, the history of the Chomskyan theories is nothing but the history of the *erosion* of ‘language’, as the object of linguistics (Jäger, 1993a, p. 79). In the course of this process, the object of research has narrowed down to such an extent that linguistics is not even capable of retaining its disciplinary identity.

Third, similarly to (Koerner, 1989), (Koerner, 2002) and (Koerner, 2004), he claims that generativists rewrite the history of linguistics in a biased manner. They highlight only the scientific views by glorifying them and hiding their shortcomings. They also distort or simply deny the progressive elements of theories belonging to the hermeneutic-functional tradition without which, however, modern linguistics could not have come into existence.

(Bierwisch, 1993) and (Grewendorf, 1993) responded to Jäger’s claims – by defending the progressive nature of the Theory of Government and Binding with *ad hominem* and straw man arguments (cf. Schnelle, 1994), thereby criticizing the unscientific nature of the hermeneutic-functional tradition. Jäger’s (1993b) response did not contain new arguments in comparison to his keynote article.

2.5. The Minimalist Program: revolution within a revolution, Lakatosian research program, spurious science or the result of authority?

2.5.1. Revolution within a revolution

As every stage of generative linguistics, minimalism (Chomsky, 1995) has also been the object of fierce debates and divided the professional community. According to one extreme view, “[...] the Minimalist Program is indeed *a revolution within a revolution*” (Piatelli-Palmarini, 1998, p. xxii; emphasis added).

This view is motivated basically by the following characteristics of minimalism ([Piatelli-Palmarini, 1998] and [Boeckx, 2006], etc.): (a) the leading principles of grammar are economy, virtual conceptual necessity, perfection and symmetry; (b) the analytic and explanatory apparatus has been radically simplified; (c) grammar has been provided a firm biological foundation; (d) the Galilean style in science has been applied; (e) as minimalism is not a well-established theory, but only a program, it enables us to evaluate such alternative solutions to various problems that a mature theory would not tolerate.

Piatelli-Palmarini's argumentation is strikingly euphoric: Minimalism

„[...] is well on its way to becoming a *full-blown natural science*, offering a serious promise of an advanced field of scientific inquiry whose idealizations, abstractions, and deductions will eventually match in depth and subtlety those of the most advanced domains of modern science. Generative grammar is turning into a natural science already, *because of what it is now*, not because of what it might one day turn into, when neural imaging and neurobiology will have provided dramatic new refinements.” (Piatelli-Palmarini, 1998, p. xxv; emphasis added.)

Nevertheless, as Joseph (1995, p. 380) mentions in a different context, the idea of serial revolution is not compatible with the Kuhnian framework. Therefore, Piatelli-Palmarini's stance cannot be supported by Kuhnian considerations, either. For fierce criticism, see Lappin et al. (2000a).

2.5.2. Lakatosian research program

Boeckx (2006) attempted to provide a historiographic overview that presents minimalism as the climax of the continuous and organic development of generative linguistics, and, at the same time, takes the wind out of the sails of counter-arguments questioning the progressivity of the latter.

Boeckx's argumentation revolves around the view that the basic features of minimalism are identical to those of 'scientific research programs' in the sense of Lakatos (1970) and this is exactly what weakens the objections to minimalism. This idea is based on the fact that such research programs cannot be falsified or verified, but are fecund or sterile and are only related to positive or negative heuristics (Boeckx, 2006, pp. 6–7, 89–91). They are not finalized, mature theories, but keep changing and develop during a long maturing process (Boeckx, 2006, pp. 6, 91–93). Furthermore, they comprise an accepted core and further auxiliary hypotheses which complete the core differently version by version. This is in harmony with the development and simultaneous existence of the various approaches within minimalism (Boeckx, 2006, p. 88). For a critical analysis of Boeckx's approach see Kertész (2010).

2.5.3. Spurious science

However, in other views, minimalism is the opposite of what Piatelli-Palmarini claims. According to Seuren (2004, p. 3), the Minimalist Program is not the glorification but rather the destruction of the development that Chomsky started with his early work. What is more,

„[...] Chomsky’s book *The Minimalist Program* is a sad example of *spurious science*, as it *fails to satisfy basic scientific criteria*, such as respect for data, unambiguous formulations, falsifiability, and also, on a different level, simple good manners.” (Seuren, 2004, p. 4; emphasis added)

Seuren bases his judgment on methodological criteria taken from the philosophy of science (Seuren, 2004, pp. 8–10). Referring to Popper (1959) and others, he maintains that according to the analytic philosophy of science there are generally accepted criteria of scientific rationality which are rooted in common sense knowledge and are timeless. However, he also notes that this picture of science has been questioned by recent post-Kuhnian approaches which highlight the role of social factors in scientific inquiry. Seuren chooses the middle course between these extremes. He does not deny that social factors may influence scientific inquiry, but at the same time he assumes that scientific values have universal features. Thus he holds that

„[...] work that presents itself as in any way scientific or academic should satisfy certain judiciously applied elementary criteria of common rationality: clarity of expression; faithfulness to the relevant facts that are, or can be made, available, acceptance of whatever can be concluded on the basis of these facts; and a mandatory preference for the ‘best theory’ – that is, for the theory that explains more facts with less machinery than rival theories on the market.” (Seuren, 2004, p. 10)

In Seuren’s view, the Minimalist Program fails to meet these criteria. For the criticism of Seuren’s stance, see Grohmann (2005) and Hacken (2006).²⁸

2.5.4. Authority

In Section 2.4.1, we mentioned the debate which was initiated by Lappin et al. (2000a). In their paper which bears the ironic title *The Structure of Unscientific Revolutions*, the authors claim, first, that although the development of generative linguistics including the Theory of Government and Binding can rightly be called revolutionary, minimalism is a *step backwards*. Second, the retention of the Minimalist Program is motivated exclusively by the *authoritative* organization of the generative community, and not by its scientific significance.

When arguing for the first part of this claim, (Lappin et al., 2000a), (Lappin et al., 2000b) and (Lappin, 2001), acknowledge the fact that generative linguistics has been revolutionary since the end of the 1950s. However, they cannot provide a rational explanation for the fact that the progression of this revolutionary change came to halt as a result of the uncritical acceptance of minimalism.

The authors analyze the main categories of minimalism, which we listed in (a) in Section 2.5.1, and they draw the conclusion that minimalism is built upon the vague metaphor of ‘perfectness’ instead of a firm empirical basis (Lappin et al., 2000a, p. 666). As opposed to (b) discussed in Section 2.5.1 (i.e. that the analytic and explanatory apparatus has been radically simplified), they demonstrate that it is those categories that have been eliminated from the technical apparatus of minimalism that have made the Theory of Government and Binding so successful and widespread (Lappin et al., 2000a, pp. 668–669). As for (c), they consider Uriagereka (1998) to be the propagandistic apology of minimalism and take it as an example

to demonstrate that minimalism “does *not* manage to specify even a *remotely credible connection* between the concepts and methodology of the MP [the Minimalist Program] and those of the natural sciences” (Lappin et al., 2000a, pp. 667–668; emphasis added; see also Levine, 2002). As opposed to (d), they believe that minimalism is by no stretch of the imagination an implementation of the ideal of Galilean science. Instead, “this *groundless aura of scientism* is used to promote the view that minimalist theory has brought the study of syntax to a level of precision and empirical coverage comparable to that of chemistry and physics” (Lappin et al., 2000a, p. 667; emphasis added). Finally, the fact that minimalism is called ‘only’ a program (see Section 2.5.1(d)), from which one cannot expect as mature and rigorous results as from a theory, attempts to turn a shortcoming into a virtue. Namely, the authors express their doubts regarding the statement that minimalism is not a theory, but only a program, since the transition to the Minimalist Program from the Theory of Government and Binding involves a significant theoretical change (Lappin et al., 2000b, p. 902).

As regards the second part of their main claim, the authors find it “mysterious” that the group of generativists that had formerly applied the Theory of Government and Binding enthusiastically and successfully, gave it up overnight, and accepted a less mature program (Lappin et al. 2000a, p. 667). They find it difficult to understand that

„[...] large numbers of researchers should substitute one theory for another simply on the basis of Chomsky’s personal *authority*, without subjecting his assumptions to the sort of critical evaluation that they would normally apply to theoretical innovations proposed under different authorship.” (Lappin et al., 2000a, p. 669; emphasis added)

Thus, in the authors’ view, rational argumentation is of a minor significance in generative linguistics (Lappin et al., 2000a, p. 670), and the solution of the “mystery” is, as the last quotation shows, *authority*.

In their conclusion, the authors question that the Minimalist Program is scientific at all (cf. [Lappin et al., 2000a], [Lappin et al., 2000b] and [Lappin, 2001]), as they say: “We know of no serious scientific discipline where theoretical paradigms are granted large scale acceptance in such a cavalier and uncritical manner” (Lappin et al., 2000a, p. 670). The authors’ conclusion is even sharper than Seuren’s: while Seuren characterizes minimalism as spurious science, Lappin et al. suggest that it is *unscientific*.

Lappin, Levine and Johnson’s article was followed by a heated debate, in which the arguments for and against minimalism, and parallel to this, the opinions on the revolutionary nature of minimalism and that of the Theory of Government and Binding polarized. See Hacken (2007, pp. 107, 122–123) and Boeckx (2006, pp. 101, 122, 154, 158) as more recent defenses of minimalism against Lappin et al.’s arguments

3. Systematic analysis of the approaches overviewed

3.1. Introductory remarks

The analysis to follow is expected to reveal those features of the various approaches which connect some of them with other approaches, which witness their differences, which

motivated the heated debates, and which place these approaches within general trends in the philosophy and history of science.

Three questions will guide our analysis. First, what immediately strikes the reader of the works surveyed is that most of them seem to be extremely biased. Thus, we will ask and answer the question of to what extent and how the views at issue are biased. Second, we will ask what central topics the approaches discuss and how successfully they tackle them. Our third question will be how the historiographic approaches relate to general trends in the philosophy and history of science.

3.2. Biased argumentation

3.2.1. The antagonism of historiographers towards one another

We illustrate the antagonism of historiographers towards one another with some compelling examples.

As a first example, Murray charges the University of Chicago Press with having published Newmeyer (1986c):

„[...] I am surprised to find it [the University of Chicago Press] serving as an organ for the cynical misrepresentation of the past which is characteristic of Stalinist history, whether of political or of academic power grabs self-legitimated as ‘revolutions’. 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.” (Murray, 1989, p. 156–157)

Second, when writing her memoir, Robin Lakoff was directly motivated by (Newmeyer, 1980) and (Newmeyer, 1986a) since, according to her, “Newmeyer’s bias is of the most dangerous kind [...] Newmeyer misleads the reader and distorts the facts” (Lakoff, 1989, p. 940), as he pretends his evaluations are objective, while, in fact, they represent Chomsky’s interests.

Third, Koerner originally submitted his (1983) article to *Language*, but due to Newmeyer’s negative review, it could not be published there. After it appeared in *Language & Communication*, Newmeyer responded to it in Newmeyer (1986b). The first footnote of Koerner (1989) – which is a revised version of his paper published in 1983 –, while referring to this fact, serves as a response. Newmeyer (1986b) criticizes Koerner’s changing views as represented by the difference between (Koerner, 1978) and (Koerner, 1989). He also attacks Murray’s and others’ claims with respect to the Chomskyan coup, which are not, in his view, tenable.

In our final example – out of the many available – we mention the debate which took place in Murray’s startling critique of (Newmeyer, 1980) and (Newmeyer, 1986c) and in Newmeyer’s reaction ([Murray, 1981], [Murray, 1982], [Murray, 1989] and [Newmeyer, 1982]) full of *ad hominem* formulations.

3.2.2. The historiographer's bias towards the object of investigation

The historiographic approaches discussed above are permeated with sympathy or antipathy towards Chomsky's personality and generative linguistics as a scientific approach. Some of the formulations of Murray, Harris, Levine et al. and Lakoff reject Chomsky's personality in an extreme fashion with *ad hominem* arguments, a technique which some of them criticize in Chomsky's case. For instance, Murray (1994, p. 445) compares Chomsky's dictatorial behavior to that of Stalin and Mao Zedong. Or Lakoff even in her historiographic contribution appears to identify with the opinion of former generative semanticists that considers Chomsky to be "satanic" (Lakoff, 1989, p. 970), and in another context, draws a comparison between the Chomskyan dictatorship and Mussolini's regime (Lakoff, 1989, p. 973).

At the other extreme are authors whose historiographic analyses are enthusiastic towards generative linguistics and Chomsky's personality. This group comprises, among the above mentioned authors, Newmeyer's publications, (Grewendorf, 2002), (Grewendorf, 2006) and (Hacken, 2007) approach to Chomsky's research program as well as (Boeckx, 2006) and (Piatelli-Palmarini, 1998) emotionally-laden minimalism-apologies.

Consequently, the lack of objectivity makes such historiographic works unreliable, whether they are extremely rejective or laudative – irrespective of how significant certain details in other respects may be.

3.2.3. Historiography as a legitimizing tool

Certain historiographic approaches serve the legitimization of generative linguistics. One of the consequences of the impact of Kuhn's seminal work was that in a series of disciplines, especially the social sciences, and within these, in linguistics, it became fashionable to argue for their 'scientific' status by proving the existence of 'revolutions' and 'paradigms'.²⁹ For example, Robin Lakoff points out that Popper's and Kuhn's work has "been taken up with passion within a number of the social sciences, where it has been *treated as a litmus test for legitimacy or entry in the club*" (Lakoff, 1989, p. 966; emphasis added). Continuous references to Popper and Kuhn convey the message that "[i]f you can prove that the findings of your field are falsifiable, that your field has paradigms, etc., you are respectable" (Lakoff, 1989, p. 966).

This characterization applies, for instance, to Hacken's (2007) argumentation as well. Although his basic concepts are different from Kuhn's, he makes great efforts to prove the applicability of Kuhn's theory to linguistics exactly in the spirit of the above quotation (see also Kertész, 2010).

Newmeyer (1986b), after realizing that the revolutionary nature of *Syntactic Structures* cannot be legitimized in a Kuhnian framework, is still motivated by the objective of ensuring legitimacy and applies Laudan's notion of revolution.

Interestingly (besides Koerner, Murray and Lakoff), Tomalin (2008, p. 11) also criticizes Newmeyer's one-sided and biased propagandistic historiography. However – although less obviously – it is easy to detect his tacit intention to legitimize Chomsky's linguistics. His legitimizing strategy can be reconstructed as follows: (a) applying the tools of the formal

sciences represents the highest quality of scientific research; (b) generative linguistics is rooted in the formal sciences; (c) components adapted from the formal sciences have remained constant during the history of generative linguistics through its different stages up to minimalism; (d) consequently, the history of generative linguistics meets, from its very beginning on till the present day, this superior scientific standard.³⁰

Boeckx (2006, pp. 84–109) acts similarly. His primary, though undeclared goal is to legitimize the fact that minimalism is uncertain, incoherent, contradictory and empirically unfounded – that is those properties which the critics target. For him, the tool for legitimacy is Lakatos' concept of 'research program'. He argues that, since Lakatos's (1970) notion of 'research program' is capable of describing outstanding achievements of the natural sciences (Boeckx, 2006, p. 88), the application of this notion to minimalism will show that despite its immaturity the latter may also lead to important achievements. (See also Kertész (2010).)

Nevertheless, the opponents of Chomskyan generative linguistics act analogously. Jäger's attack on Chomsky's cognitivism is at the same time an apology for the hermeneutic-functional perspective. Likewise, Lakoff's (1989) memoir defends the standpoint of generative semanticists.

3.2.4. Obtaining conflicting conclusions from identical data

Different authors sometimes draw conflicting conclusions from identical data. For instance, based on the fact that Chomsky's early work was not rejected but supported by neo-Bloomfieldian gatekeepers, Murray (1994, etc.) concludes that a revolution took place only in the rhetoric of Chomsky and his adherents, and not intellectually. The very same fact leads Koerner (1989) to maintain that the relation between *Syntactic Structures* and its direct neo-Bloomfieldian antecedents can be described with the notion of evolution. Another example: while Newmeyer (1986b, p. 12) believes that Chomsky and his followers received only a small minority of the grants allocated to linguistics at the turn of the 1950s and 1960s, Koerner (1989, pp. 108–109) claims that this is not so. In a third example, we refer to the fact that despite the common elements of the neo-Bloomfieldian tradition and those of *Syntactic Structures*, Newmeyer (1986b) claims the revolutionary nature (in the sense of Laudan) of the latter, whereas Koerner (1989) questions it based on the same common elements. A more striking example is, as mentioned before, that whereas (Murray, 1980), (Murray, 1994) and (Murray, 1999) considered the international linguistics conference held in Cambridge in 1962 to be a coup d'état, which had been cunningly prepared, Newmeyer (1986b) regarded it as Chomsky's convincing intellectual success.

3.2.5. Changing views

When evaluating *Syntactic Structures*, Newmeyer hesitated between the Kuhnian (Newmeyer, 1986a) and the Laudanian (Newmeyer, 1986b) revolution. Moreover, he claims that "minimalism [...] represents a big step backwards from GB" (Newmeyer, 2003, p. 589). In his various publications Koerner considers the Kuhnian paradigm brought about by *Syntactic Structures*, its evolutionary relation to neo-Bloomfieldianism, its being revolutionary in Izzo's (1976) sense, its revolutionary rhetoric along the lines of Murray (1994), and the possibility of

the revolutionary nature of *Aspects* as well. He points out the difficulty of choosing between these accounts: “The question of what kind(s) of ‘revolution’ Noam Chomsky’s work has produced appears to be a complex one [...] and it might be safer to let the reader reach his own conclusions, rather than trying to impose a particular interpretation” (Koerner, 2004, p. 4).

3.2.6. The contradictory evaluation of Chomsky’s cognitivism/mentalism

Historiographers who appear to be the adherents of generative linguistics attribute a major significance to Chomsky’s (1959) legendary review of Skinner (1957) and its role in the establishment of cognitive science (see also Chomsky, 1968). For instance, (Otero, 1994a) and (Otero, 1994b) is in agreement with George Miller and Howard Gardner when he calls September 11th, 1956 the birthday of cognitive science. It was when Chomsky proved in a talk that the theory of finite state machines is not suited to describing the structure of natural languages. Boeckx (2006, p. 16) considers not *Syntactic Structures*, but Chomsky’s review of Skinner (1957) as the turning point, because it was the latter that laid the foundations of biolinguistics which, as a result of linear progress, culminated now in minimalism. Hacken argues that Chomsky’s cognitivism had been one of the constitutive aspects of his research program before the appearance of *Syntactic Structures* (Hacken, 2007, pp. 104–105).

The opposite extreme is, as we have seen in Section 2.4.2, (Jäger, 1993a) and (Jäger, 1993b) view which alleges that Chomsky’s cognitivism has brought about the decline of linguistics. Between these extremes we find less radical characterizations. Murray (1994, pp. 232–233) for instance mentions the Skinner-review and the mentalist view against behaviorism as only one of the rhetorical tools of the attack against neo-Bloomfieldians. Murray – just as Seuren (1998, p. 252) – highlights Chomsky’s straw man arguments, with which he attacks a fictitious approach he himself has created and not Skinner’s view itself. While making reference to Miller’s and Gardner’s evaluation, Harris (1993a, p. 55) also considers mentalism to be one of the elements (although not the most significant one) of the war against neo-Bloomfieldians. He claims, for instance, that the publication of Halle (1959) was an even more devastating event (Harris, 1993a, pp. 59–60), because “one of the Bloomfieldians’ most sacred possessions, the phoneme, was tagged as worthless trinket” (Harris, 1993a, p. 61).

Obviously, all these accounts may distort the impact of Chomsky’s mentalism because of their biased nature. To understand its significance, one should go beyond oversimplified evaluations and needs analyses which, by weighing up historiographic and philosophical aspects alike, attempt to clarify in what way and to what extent the cognitive turn and, as part of it, Chomsky’s cognitivism contributed to the restructuring of the system of disciplines in the 20th century.

3.3. The topics discussed

3.3.1. The relationship between the rise of generative linguistics and neo-Bloomfieldian structuralism

Although this is one of the most crucial topics, and although many details have been revealed by the different approaches, no satisfactory account of it has been given as yet. The historiographies either highlight revolutionary innovations (see [Newmeyer, 1980], [Newmeyer, 1986a], [Newmeyer, 1986b] and [Hacken, 2007]; etc.), or vice versa, they emphasize continuity while questioning the relevance of innovations ([Hymes and Fought, 1981], [Murray, 1994], [Harris, 1993a] and [Koerner, 1989]). However, the comparison of the historiographies demonstrates that the continuity and difference between the findings of neo-Bloomfieldian structuralism are much more delicate than these two schematic representations; in this respect, [Koerner, 1989], [Koerner, 2002], [Koerner, 2004] and [Seuren, 1998] make promising attempts to give sophisticated and balanced accounts of this relationship.³¹ It is a highly important but still open question how and to what extent early generative linguistics retrospectively re-evaluated the methods, concepts and tenets of neo-Bloomfieldian structuralism – especially the trend associated with the name of Zellig S. Harris.

3.3.2. The relationship between the differences and the continuity with respect to a certain stage and its immediate predecessor within generative linguistics

Some authors ([Piatelli-Palmarini, 1998], [Grewendorf, 1993], [Grewendorf, 2007], [McCawley, 1976] and [Bierwisch, 1993]) assume a revolutionary break between certain stages of generative linguistics. Others characterize the relation between the stages after the appearance of *Syntactic Structures* as linear progress ([Newmeyer, 1980], [Newmeyer, 1986a], [Hacken, 2007] and [Boeckx, 2006]). There are also views which interpret the history of generative linguistics as decline ([Lakoff, 1989], [Seuren, 2004], [Lappin et al., 2000b], [Lappin, 2001], [Jäger, 1993a] and [Jäger, 1993b]).

3.3.3. The fact that a later stage of generative linguistics adopts problem solutions rejected in an earlier stage

The approaches do not properly analyze the fact that in the new stages of generative linguistics there appear to be problem-solutions, notions, methods and claims that were rejected or sharply criticized in previous stages. This can be exemplified by the fact that some of the ideas formulated in generative semantics and rejected by Chomsky were later included in the Theory of Government and Binding. For example, Harris (1993a, p. 253) claims that Chomsky has “stolen” the findings of his opponents, the generative semanticists. Or: according to some critics of minimalism, the latter, in fact, returned to the original endeavors of generative semantics (see, for instance, [Seuren, 2004] and [Pullum, 1996]). Interestingly, even Newmeyer claims that “[i]ronically, the Chomsky of the 21st century reads like a reincarnation of the Postal of the 20th” (Newmeyer, 2004, p. 418); see also Newmeyer’s (2003) review of Chomsky (2002a). The adaptation of some previously refuted findings of generative semantics in the Minimalist Program can be illustrated by the fact that in his apology of minimalism even Boeckx himself admits: “What is important to note is that there

is nothing odd, irrational, or inconsistent about recruiting old tools for new purposes in a different theoretical context” (Boeckx, 2006, p. 179, footnote 14).

This kind of ‘retrospective re-evaluation’ of concepts, tenets and problem-solutions seems to be an important aspect of the history of generative linguistics, but its mechanism is still unrevealed.

3.3.4. The fact that an earlier stage solved problems which a later stage could not

Let us mention two examples. The first concerns the relationship between Government-Binding Theory, the second that between the latter and minimalism. Lakoff (1989, p. 959–962) mentions as one of the most important and most progressive findings of generative semanticists that instead of the interpretivists’ grammar-internal, theory-internal and theory-extrinsic “false dichotomies” they suggested continua which the interpretivists rejected immediately. According to Lakoff, the thinking underlying this reaction

„[...] is oddly reminiscent of that of the Church in its confrontation with Galileo: If the current conventional wisdom, or scientific method, is in disagreement with the facts, deny the facts. *Eppur si muove.*” (Lakoff, 1989, p. 691).

One of the allegedly false dichotomies was the simple ‘grammatical vs. ungrammatical’ distinction. The problem arose that with respect to many data it could not be decided whether a sentence was clearly grammatical or ungrammatical; rather, judgments could be made only along a continuum. Although Chomsky himself raised the issue of degrees of grammaticalness in *Syntactic Structures* Chomsky, 1957, p. 36, footnote 2) and touched on it later as well, in research practice, interpretivists stuck to the dichotomy till the mid-1990s – that is, through the whole period of Government-Binding Theory.³²

That Government-Binding Theory could handle problems which are beyond the capacity of minimalism is extensively discussed e.g. in (Postal, 2003), (Pullum, 1996), (Lappin et al., 2000a), (Lappin et al., 2000b), (Lappin, 2001), (Levine, 2002), (Newmeyer, 2003) and (Newmeyer, 2004). By way of illustration, we quote Newmeyer (2003, p. 590):

„Does the MP bring phenomena under its explanatory scope that were outside the scope of GB? Quite the contrary. [...] Given the impoverished ontology of the bare theory [...], the empirical coverage of the MP is vastly reduced compared to the GB. As a consequence, just in order to describe the facts in question, virtually every minimalist-oriented paper posits an ancillary set of hypotheses, either directly borrowed from the GB era or posited ad hoc for the analysis in question.”

The topic mentioned in the title of this section is also related to the retrospective re-evaluation of problems and their solutions. While in the previous section we saw examples which witnessed that certain previous findings were successfully reinterpreted in some later theoretical framework, here we encounter the opposite process: the later rejection of previously successful problem-solutions.

3.3.5. The relationship between Chomskyan generative linguistics and its rivals within theoretical linguistics

On the one hand, most of the approaches do not consider this relationship at all. On the other hand, those which do discuss it, propose very different accounts. Newmeyer (1998) draws a dividing line between the ‘formalist’ and the ‘functionalist orientation’ in linguistics, maintaining that the formalist orientation includes two trends. One is Chomsky’s, and the other consists of a series of further theories which in Newmeyer’s view do not differ decisively from Chomsky’s (Newmeyer, 1998, p. 12). (Hacken, 2002) and (Hacken, 2007) argues against these distinctions. His argumentation centres on the notion of ‘incommensurability’ adapted from Kuhn (1970). He tries to show that Chomskyan linguistics on the one hand, and LFG, HPSG, GPSG and Jackendoff’s linguistics, on the other, are incommensurable – thus, the incommensurability effects in the discussions between the proponents of these trends indicate the presence of different research programs (Hacken, 2007, p. 268). Nevertheless, the four research programs mentioned diverge from Chomsky’s in different ways and to a different extent. Hacken’s findings are thought-provoking and may reveal new aspects of current trends in theoretical linguistics, but they are not entirely convincing. In particular, according to Hacken a revolution is a change in research program (see Section 2.2.2). If Chomskyan linguistics, LFG, HPSG, GPSG and Jackendoff’s approach are different research programs which came about because there were perceived crises in Chomskyan linguistics, then these crises should have led to revolutions. However, Hacken seems to have overlooked this consequence (see also Kertész, 2010).

3.3.6. Constant elements in all stages of generative linguistics

The question of what elements have been constant in all stages of generative linguistics is important, because the evaluation of the unity and diversity of generative linguistics will partly depend on the answer. For example, as we have seen in Section 2.1.2, there is such a view according to which Chomsky’s research program was fully in place in 1960 at the latest (Hacken, 2007, pp. 104–105), and this applies to its cognitivism, too. In turn, instead of cognitivism, Tomalin (2008, p. 188) considers formalism as its constant component which had been present before the appearance of *Syntactic Structures* in Chomsky’s thinking and was retained till the present day, thus witnessing the unity of generative linguistics. Seuren (1998) considers the metatheoretical aspect of Chomsky’s work as constant. Accordingly, due to the diversity of the answers, the question is anything but trivial.

3.3.7. The relation between generative linguistics and the natural sciences

The answers given to the question of whether the methodology of generative linguistics is basically the same as that applied in physics or biology, are difficult to interpret, because they are governed by group interests which serve the purpose of legitimization. On the one hand, the affirmative answer is part of the legitimizing strategy of the Chomskyan whose aim is “to exploit the prestige of the natural sciences” (Levine, 2002, p. 326). For example, there is a contradiction between the methodology of biology and Galilean style science. Therefore, in order to justify the claim that minimalism is ‘biology’,³³ without having to give up the desideratum of pursuing Galilean style science, Boeckx (2006, pp. 123–151) tries to resolve

the contradiction between the methodology of Galilean style science and biology in a lengthy discussion based on the fallacy of ‘argument from authority’ (‘argumentum ad verecundiam’). Another recent example is Hacken’s (2007, p. 30) reasoning: since Kuhns paradigms apply to the natural sciences in order to retain the claim that Chomskyan linguistics constitutes a paradigm, one should try to show that it is a natural science. Here the search for legitimization is accompanied by the fallacy of ‘argumentum ad verecundiam’, too (see also Kertész (2010) on both Hacken’s and Boeckx’s for the claim that generative linguistics is natural science).

On the other hand, arguments attacking the claim that linguistics should follow the pattern of the natural sciences seem to serve legitimization, too – although of other approaches, such as generative semantics (Lakoff, 1989) or the hermeneutic-functionalist perspective (Jäger, 1993a) or, within generativism, Government-Binding theory contrasted with minimalism (Lappin et al., 2000a).

Clearly, the question of what relation generative linguistics bears to certain disciplines in the natural sciences can be answered only as a result of impartial, differentiated and competent metatheoretical investigations. No such investigation is available in current historiographic literature.

3.4. The relation of the approaches to current trends in the philosophy and history of science

3.4.1. The source of the method applied

As regards the source of the historiographic method, the approaches we have discussed can be basically grouped into four categories. The first category comprises those that use none of the methods applied and known in the historiography of science and do not work out their own method either. Rather, they provide a primarily *ad hoc* description of things based on their personal experiences, convictions and undocumented sources. For instance, Grewendorf, Bierwisch, Newmeyer or Piatelli-Palmarini (and numerous linguists with similar convictions, who have not been mentioned in this paper) interpret the history of the development of Chomsky’s oeuvre from the perspective of the dedicated generative linguist, while Lappin et al. (and many others) from the perspective of the opposite extreme. Due to their failure to use historiographic methods properly, these works cannot be considered to be competent historiographic pieces, even if they latently presuppose some framework, for example that of Kuhn.

The approaches in the second category are based on an incorrect adaptation of frameworks originally developed with respect to another discipline. This is characteristic of those who take sides for the Kuhnian revolutionary nature of generative linguistics without comparing the Kuhnian criteria with the historical data, and without reflecting on the arguments put forward in the literature against the application of the latter (see e.g. [Percival, 1976] and [Oesterreicher, 1979], etc.). It is not possible to reach reliable findings as long as thorough historiographic investigations are replaced by unfounded declarations. Boeckx’s interpretation of Lakatos (1970) belongs to this category, too. Boeckx appears to assume that Lakatos’s notion of ‘research program’ serves the defence of immature programs as contrasted with mature theories. But Lakatos’s aim was something very different: namely, in that he replaced Kuhn’s ‘paradigm’ by the notion of ‘scientific research program’, he attempted to reconcile

Popper's and Kuhn's ideas to solve certain basic problems of the history and philosophy of science in this way. Although the concepts Hacken introduces differ from Kuhn's, he assumes that proving the applicability of the latter to linguistics is a precondition of the workability of his approach. Thereby he eliminates the social component of Kuhn's concepts and retains the intellectual only. However, 'revolution' deprived of the social factors seems to be a 'contradictio in adiecto'. (See also Kertész, 2010) Huck and Goldsmith (1995) present Lakatos' framework correctly, but then they introduce notions (i.e. 'ideology', 'argumentation', 'rhetoric') whose relation to the Lakatosian framework they do not clarify.

The correct adaptation of frameworks developed with respect to other disciplines makes up the third category. Murray's view is rooted in systematic and competent sociological investigations based on rich and differentiated data as well as the modification of (Kuhn, 1970), (Griffith and Miller, 1970) and (Griffith and Mullins, 1972) accounts.³⁴ Seuren's (2004) approach is also well-motivated in that it builds his evaluation of minimalism on methodological criteria adapted from the philosophy of science.

The approaches belonging to the fourth category strive to develop autonomous historiographic means which reflect the peculiarities of the object of investigation. This category includes (Koerner, 1989), (Koerner, 2002), (Koerner, 2004), (Tomalin, 2008) and (Hymes and Fought, 1981).

3.4.2. The relationship between the intellectual, rhetorical and social factors that shape the history of generative linguistics

It is apparent in our overview that three factors played a significant role in the history of generative linguistics: sociological, intellectual and rhetorical/argumentative. This observation is crucial as one of the central questions of the post-Kuhnian approaches to the history of science is to what extent these three factors are present in scientific theorizing, and in what kind of historiographic framework they can be grasped. That reference to the very relationship between these factors in the approaches discussed here is unsystematic and unreflecting, witnesses the fact that in the historiography of generative linguistics the reception of recent developments in epistemology, the philosophy of science and the history of science has not been satisfactory. Since the appearance of Kuhn's, Lakatos' or Laudan's works important insights were made in epistemology and the philosophy of science which motivated the elaboration of novel theoretical approaches to the history of science. The lack of reflection on these developments makes the applications of Kuhn's or Lakatos' framework appear quite anachronistic today.³⁵ The question concerning the relationship of the three factors mentioned cannot be answered without the evaluation of these new trends.

3.4.3. The exclusiveness of particular aspects

Some of the historiographies attribute exclusive relevance to one particular aspect as well as questioning the workability of others. For example, (Newmeyer, 1980), (Newmeyer, 1986a), (Hacken, 2007), (Tomalin, 2008) and (Boeckx, 2006), etc. treat linear intellectual progress as the exclusively decisive factor while they acknowledge neither the relevance of social factors such as social interests, the structure of social groups, and authority, nor that of rhetorical and

argumentative strategies. In contrast, Murray (1994) and Harris (1993a) regard the social and the rhetorical aspect as pivotal. The one-sidedness of these views results in *circularity* at two levels. First, the reason *why* the approaches at issue have been applied is that they are intended to justify certain theses with respect to Chomskyan linguistics. Consequently, it will indeed be the case that they justify these theses. The self-legitimization of the historiographic approaches themselves is based on circular reasoning, too. Since they have successfully justified their claims, they appear to be workable approaches to the history of generative linguistics. (See also Kertész, 2010.)

3.4.4. Metascientific reflection

A major weakness of the approaches discussed is that they disregard the commonplace thesis of the post-Kuhnian developments, according to which there is a close interdependence between the history and the philosophy of science.³⁶ With a few exceptions, the historiographers of generative linguistics do not explicate this very important issue. The first and most valuable exception is Seuren who attempts to associate historiographic research with aspects of the philosophy of science in a well-motivated way. Schnelle makes reference to this thesis quite vaguely in connection with the debate initiated by (Jäger, 1993a) and (Schnelle, 1994) emphasizes that although the debate appears to revolve around the question of whether linguistics became better or worse in the course of its history, in fact it centers around a related, but still different problem: namely, what kind of philosophy of science should provide the appropriate methodological basis to pursue linguistics. Boeckx applies Lakatos' historiographic theory, which *per definitionem* follows the principle of the interaction between the history and the philosophy of science. Nevertheless, in other respects the way he applies Lakatos' framework is – as we mentioned – problematic.

4. Concluding remarks

Our systematic overview and analysis boil down to the following generalizations:

(a) *Pluralism*. We have highlighted the extreme pluralism of approaches to the history of generative linguistics. On the one hand, this pluralism is fruitful because it provides us with multifaceted information about the rise, the changes and the present state of generative linguistics from very different angles of a prism. On the other hand, this multifaceted information does not result in a single, coherent picture about “what happened”, because the angles of the prism through which historians have viewed the philological data, the content of the works written, the social context of the debates and the personal motivation for choosing or rejecting the methods, concepts and aims of generative linguistic research are in many cases incompatible.

(b) *New insights: research topics, methods, data*. It is at least partly due to this pluralism that the research we have overviewed and analyzed enriched the historiography of linguistics with new topics, new data, and new methods. A rich inventory of *topics* has emerged, has triggered interesting debates and has shaped historiographic thought in linguistics – as outlined in the previous section. A great variety of *methods* have been applied including, among others, ‘conservative’³⁷ historiographic research, postmodernism, sociological research, the

Lakatosian framework, etc. *New data* were detected and interpreted from many different points of view. Therefore, the insights put forward in these studies seem to have significantly contributed to the fact that the historiography of linguistics has become an established field of research during the past four decades.

(c) *Debates*. The historiography of generative linguistics in particular has brought to the surface many methodological characteristics of the historiography of linguistics in general in a pointed and extreme way. On the one hand, the heated debates we reported on, the antagonism between the proponents of particular approaches and the biased nature of many of the approaches have shown that the historiography of generative linguistics has been strongly influenced by group interests, whatever these may be. On the other hand, the fact that this influence manifests itself in such an extreme way may be even useful, for it brings it clearly to the surface and makes it obvious.

(d) *The methodological basis*. On the one hand, as we have emphasized in the last section, some of the approaches realized that it is indispensable to relate the historiography of linguistics to sophisticated methodological self-reflection conforming to the state of the art in the philosophy of science. This is an important advance, because, first, as we have pointed out, one of the most important insights of post-Kuhnian developments is the interaction between the history and the philosophy of science. Second, as we have also indicated, several shortcomings of the historiographies of generative linguistics result from ignoring this relationship between the philosophy and the history of science. On the other hand, it must be said that most of the approaches have not yet realized this. The post-Kuhnian era in the historiography of science – in the broadest sense – has become a successful field which has developed its own methodologies, which consists of different trends and which has its own debates. With few exceptions historiographies of generative linguistics, however, do not consider these developments, findings, trends, concepts and controversies, and are behind the state of the art in this respect.

(e) *The Chomskyan revolution*. Our analysis suggests that the key notions of ‘Chomskyan revolution’ and ‘Chomskyan paradigm’ have become dubious during the debates. It follows from our analysis that the notions ‘paradigm’ and ‘revolution’ have been discredited. For example, if several works of the same author are seen as having triggered different ‘revolutions’ within a few years and having led to a series of new ‘paradigms’ – while these notions are interpreted according to the current group interests of both generative linguists and certain historians –, then they are empty and useless.

(f) *The contested nature of generative linguistics and of historiographic approaches*. One substantial reason (but not the only reason) why approaches to the history of generative linguistics are biased is that generative linguistics itself is contested. The field of the historiography of linguistics has been split by generative linguistics in analogy to the way in which linguistics has been split by generative linguistics. The division of the historiography of generative linguistics seems to mirror the division of linguistics.

Our considerations witness that the historiography of linguistics has not been able to escape being influenced by one of the most frequently praised and harshly criticized scientific achievements of the 20th century. Yet the merits and the shortcomings of the approaches we have surveyed suggest that the question of whether, and if so, how and to what extent the historiography of linguistics has been able to meet the challenge which Chomsky’s contested activity posed is still open.

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Notes

¹ Or, according to Sklar, “What has happened in linguistics since Chomsky appeared on the scene almost perfectly fits Kuhn’s description of how a scientific revolution works.” (Sklar, 1994 [1968], p. 27). Searle states in his famous article which appeared in *The New York Review of Books* that

“[h]is revolution followed fairly closely the general pattern described in Thomas Kuhn’s *The Structure of Scientific Revolutions*: the accepted model or ‘paradigm’ of linguistics was confronted, largely by Chomsky’s work, with increasing numbers of nagging counterexamples and recalcitrant data which the paradigm could not deal with. Eventually the counter-examples led Chomsky to break the old model altogether and to create a completely new one.” (Searle, 1972, p. 16)

Searle’s article deserves special attention because, if a widely-acclaimed scholar such as Searle who, as is well-known, represents significantly different views from those of Chomsky, acknowledges Chomsky’s work as revolutionary and does so in a journal that is so widely read, then his value judgement counts as reliable and affects public opinion. It is worth remarking that the article was published quite late – in the middle of the ‘linguistics wars’ between interpretive and generative semantics – and its appearance was followed by a heated debate between Lakoff and Chomsky, also in *The New York Review of Books*. Lakoff questioned the revolutionary nature of Chomsky’s achievement, and then Chomsky published an answering article in which he attacked Lakoff (Harris, 1993a, p. 156). Three decades later, Searle (2002a) revised his previous opinion and questioned the Chomskyan revolution in the very same journal. This recent article by Searle has also sparked a debate, on which Chomsky has commented, too. For more on this, see Section 2.5.3.

² “The work of all three scholars fully meets Kuhn’s (1970, p. 10) twin criteria for a paradigm: ‘Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve’.” (Koerner, 1978, pp. 45–46).

³ It is worth noting that (Otero, 1994a) and (Otero, 1994b) highlights similar characteristics without either referring to Koerner or mentioning the term ‘climate of opinion’ in the introductory article of a monumental piece of work discussing Chomsky’s reception up until 1994. The same applies to Tomalin (2008).

⁴ This notion is not identical with Lakatos’ (1970) concept of ‘scientific research program’. The scope of Hacken’s notion of ‘research program’ is restricted to generative linguistics (Hacken, 2007, pp. 18–19).

⁵ See also Newmeyer (1986a, p. 1). In very different contexts and with very different arguments, a similar claim was made in (Harris, 1993a), (Koerner, 1989), (Koerner, 2002), (Koerner, 2004), (Murray, 1980) and (Murray, 1994).

⁶ “[...]what is frequently described as a ‘revolution’ in linguistics looks, upon closer inspection of the evidence, much more like a *natural outgrowth*, an ‘*evolution*’, of theoretical discussions and methodological commitments characteristic of the period immediately following the end of World War II.” (Koerner, 1989, p. 129; emphasis added).

⁷ In this respect, Koerner seems to share (Murray, 1980) and (Murray, 1994) and Harris’s (1993a) views, according to which there was no scientific revolution in linguistics in the 1950s and 1960s. Rather, it was the application of revolutionary rhetoric that provided a false image of the importance of generative linguistics in the history of linguistics (for more details, see also Sections 2.1.8 and 2.1.9).

⁸ To distinguish references to Randy Allen Harris from those to Zellig S. Harris, we will mention the latter's first name, too.

⁹ See also (Jäger, 1993a) and (Harris, 1993a) and Murray (1994) on the same issue in different contexts.

¹⁰ After Butterfield (1931), 'Whig historiography' means that the description of a past revolution serves the appraisal of present interests.

¹¹ Newmeyer (1986b, pp. 9–10, footnote 11) rejected this charge.

¹² In Koerner's view, these examples provide evidence for "the attempt of adherents of the TGG school to rewrite and eventually cement a history of American linguistics corresponding to the advantages they see in it for their own current position" (Koerner, 2004, p. 40). See also Aarsleff's (1970) devastating review of Chomsky (1966) and Koerner (1999, pp. 10–14, 178, 210–214).

¹³ As an illustration of Koerner's notion of 'impartiality', cf. e.g.:

"[...] we may say that a historiographer should remain as impartial as he possibly can. Neither distance from the subject matter nor impartiality, however, need necessarily entail the exclusion of what Kuhn (1977, p. 149), invoking Bertrand Russell, called 'hypothetical sympathy.[...] I am not at all in favor of a one-sided preoccupation with the mere 'facts', since, [...] we hardly ever have to do with facts without a certain amount of [...] 'speculation'. The historiographer's ideal, as I see it, may be called 'broad positivism', an approach to the subject which is committed to Ranke's (1795–1886) program [...] namely, that history is neither supposed to judge the past nor instruct the present on how to act for the benefit of the future, but to depict how things really happened" (Koerner, 2002, p. 154).

¹⁴ Koerner (2002, p. 157) rejects Newmeyer's method in this context, too, since Newmeyer "appears to have only communicated with adherents and staunch supporters of one side."

¹⁵ This claim of his differs decisively from Hacken's, who does not consider Chomsky's formalism, but rather his cognitivism, as constant.

¹⁶ They use the term 'climate of opinion' pre-explicatively, and not in the same sense as Koerner (1978)

¹⁷ According to Seuren, in the course of his metatheoretically oriented activity, Chomsky suggested many principles and constraints. However, the analysis of the data was carried out by others who regularly found that Chomsky's suggestions did not work (Seuren, 1998, p. 252, footnotes 27 and 28).

¹⁸ It is well-known that these tools are considered to be classic fallacies.

¹⁹ Murray remarks: "*Certainly no revolution in linguistics was ignited by the publication of SS or by frustration at hostile and uncomprehending reaction of neo-Bloomfieldians to it*" (Murray, 1994, p. 239, emphasis added).

²⁰ According to Murray (1999: 345), *Syntactic Structures* appeared in November-December 1957, whereas Lees' review was published in the July-September issue of *Language* in 1957.

²¹ The book was published only later in a significantly abridged version, cf. Chomsky (1975).

²² Based on these facts, in a later paper Murray also claims that

„[...] it bears stressing that the Kuhnian expectations fostered by Chomsky, Lees and their followers is rejection by the *ancient régime* pushing people to become revolutionaries. The evidence for such rejection by the neo-Bloomfieldian regime of linguists during the 1950s is not merely lacking, but the evidence in the *opposite* direction, i.e., rather than rejection there was encouragement – and even solicitation

from those controlling the means of linguistic publication.” (Murray, 1999, p. 351; second italics added).

²³ For further memoirs see the Appendix of Huck and Goldsmith (1995). See also McCawley (1980) on the evaluation of Newmeyer’s (1980) account of the rise and fall of generative semantics.

²⁴ “It also may be germane to note that the paradigm ‘science’ social scientists like linguists are prone to take as a model is Newtonian physics, with its dichotomies, objectivity, and certainty. But quantum physics has cast doubt on all these vaunted desiderata, and we might ponder the dubious advantages of *modeling our own theory and method on those of an obsolescent field*.” (Lakoff, 1989, pp. 965–966, footnote 8, emphasis added).

²⁵ “We have tried for most of this century to force language into the Procrustean bed of ‘science’, and the chaos and dissension that we have experienced in the field are the result. If we are a science, we must assume that only one paradigm has access to the truth, and it had better be our own. But the impossibility of getting everyone in the field to accept a single paradigm, to settle down to Kuhnian ‘normal science’, demonstrates that we have been seeing things incorrectly. [...] each linguist, or each theoretical perspective, captures a different vision of the linguistic reality, and all, though incompatible as scientific theories, have something to add to our knowledge. But we can no longer require that perspectives be combinable into one single theory: We must settle for different, but equally valid, viewpoints.” (Lakoff, 1989, pp. 985–986, for more on the pluralism of linguistics as desideratum, see Harris, 1993b, p. 432).

²⁶ “The whole history of the subject, for thousands of years, had been a history of rules and constructions, and transformational grammar in the early days, generative grammar, just took it over. So the early generative grammar had a very traditional flair. [...] What happened in the Pisa discussion was that the whole framework *was turned upside down*. [...] the way of looking at things was totally different from anything that had come before, and it opened the way to an enormous explosion of research in all sorts of areas, typologically very varied. [...] In fact I think it is fair to say that *more has been learned about language in the last twenty years than in the preceding 2000 years*.” (Chomsky, 2002a, p. 95; emphasis added) This formulation may remind the reader of the fact that Kuhn’s most thoroughly elaborated example of scientific revolutions was the Copernican revolution.

²⁷ By way of illustration, see e.g. the following recent example: “The Principle and Parameter approach has permitted many subtle and revolutionary discoveries over a broad domain of phenomena in a wide range of languages such that linguistic theory for the first time in its history came close to its crucial objective of explanatory adequacy.” (Grewendorf, 2007, p. 370; emphasis added).

²⁸ At this point it is worth noting that Searle – who declared early generative linguistics as revolutionary in a Kuhnian sense in his famous article published in *The New York Review of Books* in 1972 (see Searle, 1972) – deems the revolution unsuccessful in the same journal 30 thirty years later (Searle, 2002a). See (Chomsky, 2002a), (Chomsky, 2002b) and (Bromberger, 2002) response, as well as Searle’s responses in return ([Searle, 2002b] and [Searle, 2002c]).

²⁹ Nevertheless, Murray’s and Koerner’s claims, namely that *Aspects* and generative phonology may be revolutionary in the sense of Kuhn, do not belong to this category, as their goal is to refute the unreflecting claims regarding the Kuhnian revolutionary nature of *Syntactic Structures*.

³⁰ In the last pages of his book he confronts this conclusion with the views which seriously question the ‘scientific’ nature of minimalism. Nevertheless, this does not change his

standpoint, because he maintains that this tension should be resolved by revealing the scientific basis of generative linguistics as carried out in his book.

³¹ In addition, Allan (2007) might serve as an instructive example for two reasons. First, Allan describes a tradition which extends from ancient Greece to present-day linguistics. Generativism is characterized along with its complex relations to different components of this tradition. Second, as one of his main findings, he suggests the synthesis of inductivism as applied by neo-Bloomfieldian structuralists and hypothetico-deductivism advocated by Chomsky. See also Allan (2003).

³² Lakoff could not know what would happen in the future. That she was right is best witnessed by the fact that about a decade after the publication of her article, in the second half of the nineties – as a result of Schütze's (1996) influential monograph – the Chomskyans realized the relevance of the problems which the dichotomy could not handle. The discussion on the nature of data and grammaticality judgments is still going on ([Sternfeld, 2007] and [Stefanowitsch and Gries, 2007]; for an evaluation of the discussion see Kertész and Rákosi, 2008).

³³ According to the contemporary 'climate of opinion', biology is considered to be one of the most progressive disciplines.

³⁴ However, in spite of the methodological correctness of his research, his formulations are often biased (see Section 3.2).

³⁵ See e.g. Barton (1995, pp. 483–484) on the same issue in a different context.

³⁶ It has become an adage in Lakatos' formulation: "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." (Lakatos, 1978, p. 102).

³⁷ Koerner's term, see e.g. Koerner (2002, p.151).