

# LINGUISTICS AND REVOLUTION WITH PARTICULAR REFERENCE TO THE ‘CHOMSKYAN REVOLUTION’

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## **1. Opening Remarks**

Let me begin this paper by stating that I am concerned here with Linguistics, the science of language, not Language and Revolution. The difference is important as I deal with a meta- if not, by discussing the views of others on the subject, a metalevel. That social, political, and ideological upheavals affect the use of language is a well-known fact, but it is not the subject of the present paper.

In a searching review article, characteristically entitled “The structure of linguistic revolutions”, John E. Joseph critically analyzed recent scholarship in 20th-century historiography concerning American linguistics, in particular the books by Harris (1993) and Murray (1994). Joseph (1995) suggests, furthermore — undoubtedly speaking against conservative historians of linguistics such as myself (e.g., Koerner 1989; cf. Joseph 1991) — that the concept of ‘revolution’ may not only have to be seen as central to linguistic history-writing but would also have to be taken as something which occurs much more frequently in the development of linguistics than I would have assumed, though perhaps on a much more modest scale. As a result, there may be a variety of small-scale revolutions to be accounted for, ‘counter-revolutions’ against previous revolutions, even ‘serial revolutions’, as witnessed in Chomsky’s work over the past forty or more years. Indeed, Joseph suggests that, in the understanding of the nature of linguistic revolutions, at least, there may well be four distinct stages in our assessment of such changes, namely, the Popperian type, the Kuhnian type, and the two exhibited to some degree in the two books he was reviewing, i.e., Murray (‘sociology of science’) and Harris (‘rhetoric of science’).

In this paper, I shall say comparatively little about the concept of ‘revolution’ in terms of the various philosophies of science (Kuhnian, Popperian,

etc.). Here, I shall consider, however, John Joseph's position that "[m]ost revolutions are essentially rhetorical, with the substantive change being one of personnel" (Joseph 1995: 384n.5), while at the same time not ignoring Stephen Murray's 'three factors' defining what he believes to be all coherent scientific groups: good ideas, intellectual leadership, and organizational leadership (cf. Murray 1994: 22–23). However, I will first offer some of my own thoughts on the issue of 'revolutions' in general and in linguistics in particular (Section 2) and also refer to a few points in 19th and 20th century history of linguistics for illustration (Section 3). The question of what kind(s) of 'revolution' Noam Chomsky's work has produced appears to be a complex one (see Section 4), and it might be safer to let the reader reach his own conclusions, rather than trying to impose a particular interpretation.

## **2. *Remarks on the term 'revolution' generally and specifically***

As is to be expected, there are a variety of factors that would determine whether a particular 'revolution' in linguistic theory and practice is recognized and widely accepted. More often than not, certain works are regarded as turning-points post rem (e.g., Bopp 1816; Chomsky 1957) where one is hard pressed to discover the locus of such a claim, whether by analyzing the text itself or its original reception. Extra-linguistic factors, both social and political, would have to be taken into account to explain the success or failure of important proposals and indeed advances made by an author. As the record shows, rhetorical, at times even polemic, aspects have played a not insignificant role in the acceptance or rejection of a particular 'paradigm', and this not only in 'modern linguistics'.

In this paper, I shall try to address the question of 'revolution' in linguistics; not so much from the point of view of the philosophy of science or any other particular framework — I presume that I should heed John Joseph's (1991) advice to follow 'common usage' — but more from the point of view of what has actually happened in linguistics over the past two hundred years. I should add that here I confine myself to the main currents in 19th and 20th century linguistics, namely, comparative-historical grammar and structuralism, respectively, for must it be remembered that linguistics is in no way exhausted by these 'mainstream' activities: a lot of linguistic work continues to be done outside these perhaps more general concerns, whether it deals with lexicographic, phonetic, didactic, dialectological, or any other work, even though within those activities, too, fairly sudden changes of method, important advances and other events may occur that their practitioners may liken to revolutions within their particular domain.

### 3. *Illustrations of continuities, discontinuities, and possible revolutions*

When compared with the case of the breakthrough or breakthroughs associated with Noam Chomsky's name, earlier instances appear to have been much less complex, but this may be due to our ignorance of many of the circumstances that would explain to us the successes or failures of certain publications in earlier periods of linguistics. They certainly involved changes in research methodology, generational differences, and polemical exchanges, too (cf. Koerner 1999, for details).

#### 3.1 *Initial methodological considerations*

Writing in 1980, István Bátori suggested that it was still too early to evaluate Chomsky's contribution to linguistics in a historical perspective, largely because 'the waves of his revolution (in the sense of Thomas Kuhn) have not yet come to a standstill' (Bátori 1982: 103). A similar sentiment was expressed five years later by Herbert Penzel (1987: 418). However, in light of the fact that the history of the school associated with Chomsky's name is currently being written in a less than objective manner, it appears desirable to raise the question as to the proper method of treating the subject now, before certain misconceptions and, indeed, myths are cemented as facts. I am thinking of such erroneous claims as the one that Chomsky's *Syntactic Structures* was 'turned down by numerous established publishers' (as found for instance in the "Geleitwort der Herausgeber" to No.95 of *Linguistische Berichte* of February 1985, p.1),<sup>1</sup> or that "the publication of *Syntactic Structures* radically changed the goals, the methodology, and the research questions of the field" (Fromkin 1991: 78).

Ideally, the historian should be at a certain distance from his subject, in the sense that he should have no personal stake in the outcome of his research but be guided by a desire to set the record straight.<sup>2</sup> Of course this is not the

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<sup>1</sup> It appears that Chomsky himself may have had something to do with this myth. For instance, in conversation with Herman Parret, he asserted that "*Syntactic Structures* was not written for publication. It is basically a set of lecture notes for an undergraduate course at M.I.T." (Chomsky 1974: 27). This is hard to believe when looking at the publication itself. More importantly, accounts concerning the publication of *Syntactic Structures* suggest that the typescript was handed by Morris Halle for exactly that purpose to C. H. van Schooneveld (b.1921), the editor of the Mouton series. It was indeed carefully prepared for publication (cf. Murray 1999, Noordegraaf 2001).

<sup>2</sup> It seems clear from this point of view that accounts such as in Hymes (1972, 1974) may be vulnerable to criticism. However, if the historian states his commitments clearly, allowing

only prerequisite for a historian, but it seems that one of the main prerequisites for any historical work is not to approach a subject with preconceived ideas, trying to establish a particular point which may be of importance to his immediate interests. In a word, 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: 149), invoking Bertrand Russell, called ‘hypothetical sympathy’.

Certainly, I am not advocating a narrow positivistic approach interested in little else than what Comte called ‘les petites choses vraies’. Indeed, I am not at all in favour of a one-sided preoccupation with mere ‘facts’, since, as was clear long ago even to empiricist theorists of linguistics such as Hermann Paul (1880: 6), we hardly ever have to do with facts without a certain amount of — what he termed — ‘speculation’. The historiographer’s ideal, as I see it, may be called ‘broad positivism’, an approach to the subject which is committed to analyse, describe, and present historical events in line with Leopold von Ranke’s (1795–1886) program first announced in his *Geschichten der romanischen und germanischen Völker* of 1824 — several years before the appearance of August Comte’s (1798–1857) 6-volume *Cours de philosophie positive* (Paris, 1830–1842). That this ideal is hardly ever reached may be evident to the reader of the present account as in most other instances, including Ranke’s own post-1824 work. Still, I would like to refer to Ranke’s frequently-quoted affirmation — usually associated with his much later voluminous work — 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.<sup>3</sup> To some this suggestion may appear excessively conservative, but those who are interested in the history of linguistics in the 20th century cannot escape the conclusion that in the wake of partisan

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the reader to draw his own conclusions, we are still already much better served than in the partisan accounts that present themselves as objective history.

<sup>3</sup> Since this statement is usually quoted out of context and without proper reference to its original source, I am supplying both in the following: “History has been assumed to serve the task of passing judgment on the past and to teaching one’s contemporaries for the benefit of the future: the present essay does not pretend to serve such lofty goals: it simply wants to show what really happened” [“Man hat der Historie das Amt, die Vergangenheit zu richten, die Mitwelt zum Nutzen zukünftiger Jahre zu belehren, bey gemessen: so hoher Aemter unterwindet sich gegenwärtiger Versuch nicht: er will bloß sagen *wie es eigentlich gewesen.*”] See Leopold Ranke, “Vorrede”, *Geschichten der romanischen und germanischen Völker von 1494 bis 1535* (Leipzig & Berlin: Georg Reimer, 1824, i-xi, on pp.v-vi; emphasis mine: EFKK).

accounts published over the years a return to basic historiographic principles appears to be called for.

### *3.2 Some additional prerequisites*

I have discussed, on various other occasions, the prerequisites for linguistic historiography (e.g., Koerner 1976[1972], 1982) and do not intend to repeat them here at any length. It needs hardly be emphasized that familiarity with the particular linguistic theories at issue is of prime importance: a historian of linguistics should have formal training in linguistics. Less obvious perhaps but of equal importance is general knowledge of the various extra-linguistic factors, intellectual, sociological and possibly even political, which may have had an impact on the course of events in a given field of scientific inquiry at particular periods of its development. Without this knowledge of the extra-linguistic ‘context of situation’ it would be difficult to understand changes of emphasis in linguistic theory or ‘revolutions’ within the discipline (for instance the increased importance attached to syntax, over and above morphology and phonology in the early 1960s). It is important that we distinguish between intra-linguistic developments (i.e., those specific to the particular discipline that tend to be picked up where the preceding generation of researchers left off, often coupled with the desire to overcome the enduring problem of dealing with semantics in an adequate manner (cf. Seuren 1998: 474–527, for details), and various extra-linguistic factors. The latter have nothing to do with the operation of the craft, its methodology, its specific data, or its findings per se; however, they may have, and in many instances have had, a significant impact on the wide-spread acceptance of a particular framework or philosophy of science as well as on the foci of attention in research, and this more often than not with social ramifications of some consequence.

### *3.3 Further methodological considerations*

So far, I have referred to general attitudes on the part of the historiographer (i.e., that he should be capable of treating his subject matter with a certain detachment) and the fundamental distinction between what may be called the intra-disciplinary requirements of and the extra-disciplinary influences on the field. For anyone interested in undertaking historical research these generalities can only suffice as the most rudimentary guidelines. The historiographer must know how to ascertain the relevant data, material which cannot simply be obtained by consulting the textbooks of a given period or school of thought. No doubt these texts have their value too; they usually

present the accepted doctrine in a pragmatic fashion. (For instance, the number of editions of any such book may give an indication as to its popularity, and the extent to which it is receiving the attention of linguistic practitioners.) However, textbooks constitute secondary sources only, for they tend to dilute the theoretical issues in order to make them accessible to a wide audience. More importantly, their authors try to depict what they believe to be the general consensus, and usually don't take a critical stance: after all, they want to sell as many copies as possible.

In an early state-of-the-art account of the history of linguistics, Yakov Malkiel provided a list of what he regarded as source material for the historian of linguistic science. The list includes autobiographies, memoirs, prefaces, correspondence, Festschriften, book reviews, summations at symposia, institutional records, and other material (Malkiel 1969: 641–643). In addition to these sources, it has become more widely accepted that unpublished writings and especially correspondence between scholars conducted without the public in mind, may well constitute important documentary evidence for certain events. Thus Stephen Murray (1980) has been able to establish — something which many may have suspected but were unable to prove beyond doubt — that Bernard Bloch, editor of the journal of the Linguistic Society of America, *Language*, from 1941 until his death in 1965, played an important, if not decisive, role in the promotion of Noam Chomsky and his linguistic theories during the late 1950s and early 1960s. Bloch's role was certainly much more crucial than chroniclers of the 'Chomskyan paradigm' (e.g., Newmeyer 1980: 47–48) are willing to concede.<sup>4</sup> Perhaps such oversight occurs simply because the Bloch papers deposited at the Sterling Library of Yale University were not consulted. However, judging from more recent publications (Newmeyer 1986a, b, 1996), the impression made by his 1980 book, namely, that Newmeyer does not seem interested in presenting anything close to objective history, is confirmed (cf. Murray 1989, Huck & Goldsmith 1998).

One other source, where contemporary linguistic historiography is concerned, has so far remained largely untapped. I am referring to direct interviews with persons who participated in the events and, more generally, to what is nowadays termed oral history (cf. Davis & O'Cain 1980, for the first such undertaking in North-American linguistics which has become available

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<sup>4</sup> Still in 1998, Julia S. Falk, herself a generativist de la première heure, reviewing Murray (1994), found that “there is no evidence that he [Bloch] did anything more than any reasonable and responsible editor and teacher might do” (Falk 1998: 446).

in print). Murray (1980, 1994), a sociologist, made extensive use of interviews as well as correspondence with both Chomsky and his associates and with scholars not following or opposing transformationalist theories, whereas Newmeyer (1980; 21986) appears to have only communicated with adherents and staunch supporters of one side.<sup>5</sup> Newmeyer (1980: xii), however, maintains that his own participation in the events of the 1960s and early 1970s has given him “a real advantage” and that it has permitted him “an inside view of the field that would be denied to the more displaced historian”. It remains to be seen whether a critical reading of his book bears out this claim.

#### 4. *The ‘Chomskyan Revolution’ in Linguistics*

It has become common-place to talk about a ‘Chomskyan Revolution’ in the study of language, with the result that few, if any, would pause to think about what the term ‘revolution’ implies or is taken to imply. It is interesting to note that it is non-linguists in particular (e.g., Sklar 1968; Searle 1972)<sup>6</sup> who referred to ‘Chomsky’s revolution in linguistics’. Interestingly, no such term can be found, for example, in Bierwisch (1971), the noted linguist and very early and steadfast proponent of transformational-generative grammar. This appears all the more surprising when we note that Malkiel (1969: 539) spoke of Kuhn’s *The Structure of Scientific Revolutions* (1962) as a “sensationally successful book”. Yet the absence of the term in accounts of transformational theory by Chomsky’s followers during the 1960s and 1970s does not imply their rejection of the use of the Kuhnian morphology of scientific revolutions. Bach (1965: 123), interestingly enough, refers to ‘revolution’ without mentioning Kuhn, whose name is also conspicuously absent from Newmeyer’s (1980) book (but compare the second edition of 1986, pp.38–39, where explicit references to Kuhn are made). Others, usually European-trained linguists, though with direct exposure to transformational grammar (e.g., Meisel 1973; Anttila 1975; Weydt 1976), cast doubt on the actual occurrence of a ‘Chomskyan revolution’ in the study of language in the regular sense of the term.

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<sup>5</sup> McCawley (1981: 911), who is otherwise quite critical of Newmeyer’s account, gives the misleading impression that Newmeyer did indeed make much use of interviews.

<sup>6</sup> A fairly early statement about a “transformationalist revolution in Linguistics” came, typically, from participants in the ‘revolution’ (see Katz & Bever 1976[1974]: 11).

#### 4.1 *A few remarks on the concept of 'revolution'*

Our first association with the term 'revolution' is political in nature; we think of governments being overthrown in a coup d'état and one system of government being replaced by another. Herbert Izzo (1976: 51) has given the following characterization of what he refers to as 'successful social revolutions':

[They] rewrite history for their own justification [...]. The Soviet example, though not the first, is the most familiar and one of the most thorough. First the old order must be condemned *en bloc*; everything about it must be shown to have been bad to justify its overthrow and prevent its return. Then any changes of direction of the new order must be consigned to oblivion. [...] Finally, it becomes desirable to show that the new order is in reality not so much new as a return to the correct, traditional ways, from which only the immediately preceding regime had been a deviation and a usurpation. Along the way there may have been a return to many features of that same preceding regime. These will not, however, be represented as *regressions* but as *new developments*.

For those who have observed the history of transformational-generative linguistics in North America unfolding during the mid 1960s and early 1970s, Izzo's description of a 'social revolution' appears to apply quite well to what actually happened. (For some examples of propaganda emanating from the centres of this movement, see below.)

4.1.1 *Fashion?* Hymes (1974: 48–49) and others (e.g., Murray 1980) have suggested that the so-called 'Chomskyan revolution in linguistics' may be largely due to social factors which have little to do with the theory and its inherent value, its 'explanatory adequacy', the 'power' of its 'generative' device, etc. Maher (1982: 3ff.) goes so far as to associate the success story of transformational-generative linguistics (henceforth: TGG) with fashion, referring to the following statement made by Bertrand Russell — in his 1959 preface to Ernest Gellner's criticism of the Wittgensteinians at Oxford — according to which "the power of fashion is great, and soon the most cogent arguments fail to convince if they are not in line with the trend of current opinion" (Gellner 1959: 13). To support his claim Maher (1982: 4) refers to observations made more than fifty years earlier by the American sociologist William Graham Sumner (1840–1910) who noted at the beginning of the last century:

Fashion is by no means trivial. It is the form of the dominance of the group over the individual, and it is quite often as harmful as beneficial. There is no arguing with fashion. [...] The authority of fashion is imperative as to everything which it touches. The sanctions are ridicule and powerlessness. The dissenter hurts himself ... (Sumner 1906: 194).

While a consideration of the effects of fashion in linguistics (as in any other human affair) is not to be ignored, I believe that this aspect may cloud some of the issues rather than elucidate them. It is certainly difficult to believe that it was the particular theoretical proposals of TGG exclusively which appealed to the young students of language who entered university during the sixties and early seventies. Newmeyer (1980: 52ff.) presents statistics, of which, in particular, the table showing the growth of the membership in the Linguistic Society of America (LSA) indicates the tremendous academic population explosion of the period: 1950: 829 members; 1960: 1,768 members, and 1970: 4,383 members, with the peak having been reached in 1971 (4,723 members). For Newmeyer, this growth reflects the appeal and strength of the ‘Chomskyan paradigm’; however, when this development levels off and shows a decline, he explains this as the result of the bleak employment picture in linguistics (Newmeyer 1980: 53). Here one is constrained to ask ‘Why not a reflection of a widespread disenchantment with TGG?’, since Newmeyer earlier (p.52) regarded the membership increase in the LSA as being “considerably above the average [compared with which other discipline?], suggesting that it was the appeal of transformational generative grammar rather than economic growth”. Murray (1981: 109) saw the reasons for this dramatic expansion (in addition to the general growth of institutions of secondary and post-secondary education) in what he describes as

the *zeitgeist* of a rebellious generation coming along at the time of rapid expansion of the academic sector in North America. The channeling of so much of the available money to an institution [i.e., the Massachusetts Institute of Technology, in particular the Linguistics Department there] where it was astutely used by accomplished academic warriors further enhanced the attractiveness of a perspective in which the elders were dismissed just when generational rebellion was particularly prominent in the general culture.

In other words, TGG would not and could not have gained in strength to the extent that it did during the 1960s and early 1970s if there had not been other, major, factors helping to bring the ‘Chomskyan revolution’ about.

*4.1.2 Funding?* We have mentioned the question of funding, which Newmeyer (1980: 52 and n.8) has reduced to a few lines in a 250-page account of the first 25 years (1955–1980) of TGG, but which, I believe, was of distinct importance in the furtherance of the transformationalist cause. Writing about how government spending on research and education significantly advanced the diffusion of this particular linguistic doctrine, James McCawley, who did his doctorate with Chomsky at M.I.T. in 1965, and who has always remained an adherent of generativism — albeit taking a critical point of view on par-

ticular issues, philosophical or otherwise (cf. Koerner 2002, chapt. 6), noted the following:

I maintain that government subsidization of research and education, regardless of how benevolently and fairly it is administered, increases the likelihood of scientific revolutions for the worse, since it makes it possible for a subcommunity to increase its membership drastically without demonstrating that its intellectual credit so warrants. The kind of development that I have in mind is illustrated by the rapid growth of American universities during the late 1950s and 1960s, stimulated by massive spending by the federal government. This spending made it possible for many universities to start linguistics programs that otherwise would not have been started or would not have been started so early, or to expand existing programs much further than they would otherwise have been expanded. Given the situation of the early 1960s, it was inevitable that a large proportion of the new teaching jobs in linguistics would go to transformational grammarians. In the case of new programs, since at that time transformational grammar was the kind of linguistics in which it was most obvious that new and interesting things were going on, many administrators would prefer to get a transformational grammarian to organize the new program; in the case of expansion of existing programs, even when those who had charge of the new funds would not speculate their personal intellectual capital on the new theory, it was to their advantage to speculate their newfound monetary capital on it, since if the new theory was going to become influential, a department would have to offer instruction in it if the department was to attract students in numbers that were in keeping with its newfound riches. And with the first couple of bunches of students turned out by the holders of these new jobs, the membership of the transformational subcommunity swelled greatly. (McCawley 1976b: 25)

Such a long quotation is justified for a number of reasons, especially since it provides readers not familiar with the mind-set and operational modes of North-American university administrators with at least some insight. Naturally, the informed reader would like to underscore particular passages in the citation, comment on certain points of detail, and draw further conclusions from the observations made; but in general it characterizes well both the mentality of administrators (frequently *académiques manqués* eager to be seen as progressive, by their superiors and their former colleagues) and the particular situation they found themselves in, just at the time when Chomsky's ideas began to gain ground, if not fame — though not exclusively for reasons directly related to linguistics, as I shall try to argue in this paper. In McCawley's account there seems to be a lurking suspicion that the rapid growth of TGG may have had something to do with a fad (cf. Maher's observations in section 4.1.1 above), a suspicion I had during my graduate years in linguistics at a North American university in the late 1960s.

4.1.3 *Ideology?* Robert A. Hall, himself a staunch Bloomfieldian ‘structuralist’, reviewing Newmeyer’s (1980) book, mentions another reason for the apparent success of TGG, namely, that it had more to do with ideology and less to do with the honest attempt of a group of linguists to provide a more adequate theory of LANGUAGE — in contrast to a theory of LINGUISTICS. Hall (1981: 185) notes the particular choice of vocabulary on the part of Newmeyer, for instance when relating in paper 2 of his book titled “The Chomskyan revolution” how this turn of events was brought about. Expressions suggesting military and political conflict, e.g., ‘campaigner’, ‘old guard’, ‘rebellion’, ‘revolution’, ‘struggle’, ‘tactic’, ‘defend’, ‘confront’, and ‘win victories’ abound. Politico-religious terms are not rare either, e.g., ‘charisma’, ‘convert’, ‘hegemony’, ‘win over’ (cf. Newmeyer 1980: 45 and elsewhere). Newmeyer’s chapter thus fits Maurice Cranston’s (1974: 196) characterization of ‘ideology’ very well indeed:

It is characteristic of ideology both to exalt action and to regard action in terms of a military analogy. Some observers have pointed out that one has only to consider the prose style of the founders of most ideologies to be struck by the military and war-like language that they habitually use, including words like *struggle*, *resist*, *march*, *victory* and *overcome*; the literature of ideology is replete with martial expressions. In such a view, commitment to an ideology becomes a form of enlistment so that to become the adherent of an ideology is to become a combatant or partisan.

Especially during the early 1970s, many enthusiasts of TGG spoke of a revolution in linguistics (cf. in addition to those mentioned at the outset of section 4 above: Dingwall 1971: 759; Greene 1972: 189; Yergin 1972). It is interesting to note that more recent publications that maintain the same argument (e.g., Smith & Wilson 1979: 10; Newmeyer 1980: 20) no longer make an explicit reference to Kuhn’s (1962) book on scientific revolutions, perhaps because the ideas therein appear to them as a chose acquise that need no longer be demonstrated. As a matter of fact, I suggested the existence of something like a ‘Chomskyan Paradigm’ as early as 1972 (cf. Koerner 1976: 703) because I was of the opinion (and still am) that with Chomsky and his circle a definite shift of emphasis in the goals of linguistic theory was brought about which superficially at least seemed dramatic enough to resemble Kuhn’s concepts of disciplinary ‘paradigm’ and ‘revolution’. These changes in the general approach to language and, concomitantly, the philosophy of science, were probably not in all respects beneficial to linguistic studies as a whole. Yet it cannot be denied that a number of proposals, procedures of analysis and concepts of theoretical argument have become part of the linguist’s tool-kit and general outlook, which no one seriously interested in the-

ory construction can any longer ignore<sup>7</sup> (though linguistic practitioners, i.e., those conducting empirical research instead of selecting data from the work of others that might confirm their theoretical claims, may well have been able to do without them). In other words, whether we like it or not, we will have to agree that noticeable changes, in the linguist's attitude towards language and within the linguistic discipline itself, did take place during the past forty or so years, changes which a number of people have likened to a 'revolution' in the Kuhnian sense of the term (cf. Pearson 1978, for a discussion).

However, we may ask ourselves whether such changes of focus and emphasis, this introduction of new terminology (frequently replacing traditional terms describing the same phenomena), and this 'idealization' — which Newmeyer (1980: 250) invokes to support his claim that "more has been learned about the nature of language in the last 25 years [i.e., 1955–1980] than in the previous 2500" — have indeed produced something like a revolution in the field necessitating, as it were, not just a new outfitting of every linguist's operating kit but also a relearning of the trade. In fact, a closer analysis of what was really done by linguistic practitioners (not by armchair theoreticians who tend to ignore data that could disconfirm their hypotheses) in North America and in Europe during the same period may well bring to light the following:

(1) A number of linguistic schools continued to survive (e.g., Tagmemics, largely associated with the work of Kenneth Lee Pike and his collaborators, and Systemic Grammar, a neo-Firthian approach headed by Michael A. K. Halliday, as well as Stratificational-Cognitive Grammar, introduced by Sydney M. Lamb during the 1960s); indeed, several of these schools have been thriving in recent years, suggesting not only that there has not been one all-embracing theoretical framework operating in North-American linguistics during the past 40 or more years (as Newmeyer and others would like us to believe), but also that the PARADIGM fostered by TGG has long since lost its attraction for, and grip on, the minds of many present-day linguists.

(2) TGG provoked to no small degree the development of approaches to language which have tried to account for specifically those aspects of language study (e.g., human communication, social conditioning, and actual language use — Chomsky's talk about the latter notwithstanding), which the Chomskyan model consistently eliminated from its list of 'interesting' phenomena.

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<sup>7</sup> On this, cf. Neil V. Smith's (b.1939) advice in his Foreword to a recent collection of Chomsky's papers: "You may not agree with Chomsky's work, but it would be short-sighted and unscholarly to ignore it" (Chomsky 2000: v).

Thus the revival of interest in discourse analysis, speech act theory, pragmatics, and various sociolinguistic approaches since the late 1960s would probably not have been as pronounced had the Chomskyan ‘paradigm’ not focused so much on abstract ‘data’ (usually made up by the analyst in support of a theoretical argument) far removed from actual speech, or what Labov has called ‘realistic linguistics’.

In short, as will become still clearer from what follows, it seems that, upon closer inspection, the term ‘revolution’ does not properly apply to TGG, if this was to mean that one framework of how to conduct research replaced previous or competing frameworks, as Lavoisier’s New Chemistry replacing Stahl’s Phlogiston Theory. Despite many disclaimers, TGG is basically post-Saussurean structuralism,<sup>8</sup> although Joos (1961: 17) characterized the movement, which he associated with the work of both Harris and Chomsky, “as a heresy within the neo-Saussurean tradition rather than a competition to it”. TGG is still basically, in Joos’ view, excessively concerned with ‘langue’, the underlying grammatical system, to the detriment of ‘parole’, the actual speech act; or, in other terms, with an abstract formalism claiming to represent the essence of language structure instead of the analysis of the function and use of human language. (It is often forgotten that formalization by itself does not lead to new insights about the nature of language.) However, it cannot be denied that many young men and women in linguistics during the 1960s and 1970s BELIEVED that they were witnessing a revolution in the field, and it appears that this widespread belief (and the associated enthusiasm that young people tend to generate) has been, I submit, at the bottom of the ‘Chomskyan revolution’. (Some of the participants in the ‘revolution’ I have talked over the past twenty or more years still today get a gleam in the eye when they recount their recollections of linguistics in the 1960s.)

To do justice to historical fact, it should be remembered that — like Curtius, who in 1885 FELT that the Neogrammarians had embarked on a course that constituted a break with the past (cf. Koerner 1981: 168–169) — there were scholars of the post-Bloomfieldian generation who, at least during the early 1960s, conceived of TGG as a ‘breakthrough’ (Hockett 1965: 196; although he associated it with the name of Sydney M. Lamb as well!). Earlier, in 1963, Rulon S. Wells (b.1919) expressed a similar apprehension of change when he spoke of “some neglected opportunities in descriptive linguistics”. Wells (1963: 48), however, approached the subject somewhat more cautiously:

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<sup>8</sup> Indeed, Joseph (1999) has suggested that true structuralism begins with Chomsky’s work.

Whether the change that actually took place — the advent of and eager reception of the approach called transformation-theory — should be described as internal or external, as a revision and rehabilitation of D[escriptive] L[inguistics] or as a displacement of it, is no simple one, for which reason I save it for another day. Some major change did take place; the episode ended; and the present paper is a historian's attempt to explain the change. It does not, however, purport to explain the advent of transformation-theory (TT), but only the reception of it. Given the TT-approach was put forward when it was, why was it taken up in the way it was?

It would be laborious beyond the ambitions of my paper to describe this way with any great accuracy; it must suffice to say that there arose a very widespread belief that TT, the successor to DL, could lead linguistics to fruitful successes where its predecessor had proved unable to do so. My own judgment as a linguist about such a belief is that mixed in with a solid core of truth there is much that is false, gratuitous, or misleading. But in the present paper I try to set aside my own views as a linguist, and to speak only as a historian of linguistics, without taking sides.

Wells, whose own paper on 'constituent analysis' of 1947 may be credited for having gone beyond the mere descriptive stage of post-Bloomfieldian linguistics, feels the "norm of pure description [which] was the Zeitgeist in the thirties and forties" (p.49) was to blame for the abandonment of the merely descriptive in favour of a more explanatory approach in the 1950s and 1960s, and the switch from DL to TGG. Sydney M. Lamb (b.1929), a theory-oriented linguist of Chomsky's age, found that one of the shortcomings of the post-Bloomfieldians was their excessive concern "with trying to specify procedures of analysis" (Lamb 1967: 414) — Zellig Harris' *Methods in Structural Linguistics* of 1951 immediately comes to mind here. It seems however that extra-linguistic matters (i.e., what may be called changes in the intellectual climate) had more to do with the rise of TGG in the period than the problems that beset the, at times, extreme positivist tendency of linguistic analysis among Bloomfield's successors. (We shall see in Section 4.4 examples of how several post-Bloomfieldian linguists anticipate many basic ideas later associated with Chomsky alone.)

#### *4.2 Concrete factors contributing to the Chomskyan 'revolution'*

I have already referred to the 'climate of opinion' during the 1960s and the sociological aspects of the relationship between 'old guard' and the 'young Turks'. A conflict normally exists between generations but can be heightened and intensified by socio-economic and political causes. For example, the civil rights movement of the Kennedy and Johnson years, the opposition to the American involvement in the Vietnam war, and other issues polarized the diverging views of the old and the young. These are external factors meriting the attention of the historian of any discipline, though proba-

bly more in the humanities and social sciences than the so-called ‘hard’ sciences, that is, the natural sciences as well as mathematics (although the introduction of the ‘new math’ into the educational system during the 1960s was probably not exclusively motivated by the superiority of the new approach over the traditional one). Yet I believe that the Geisteswissenschaften generally are more likely to be influenced by intellectual currents of any sort than the Naturwissenschaften as Dilthey, Rickert and others noted more than one hundred years ago. Notwithstanding that it is impossible to map out all these spheres of influence within the confines of one exploratory essay, these external factors have so far been largely neglected by historians of most disciplines, and certainly those dealing with the history of linguistics.

There is however at least one factor that can be fairly easily identified. It is related to the widespread acceptance of TGG during the 1960s and early 1970s — the funding of university programs during that period. We have already referred to this subject (see 4.1.2 above), and quoted from a 1976 statement made by James McCawley concerning the impact of the National Defense Education Act (passed by the United States government in late 1958) on linguistics (cf. also Mildenberger 1962). As a matter of fact, Newmeyer — who tends to downplay the role of the large sums of money that were poured into all kinds of linguistic research during the 1960s — documented, in a paper done with his partner Joseph Emonds in 1971, that these monies in effect constituted “a great shot-in-the-arm to the field of linguistics” (Newmeyer & Emonds 1971: 287). But since Newmeyer wishes us to believe that the success of Chomskyan linguistics is exclusively due to its scientific merits, the subject of funding is mentioned only in a single footnote in his 250-page *Linguistics in America*.<sup>9</sup> In his 1986 *Linguistics and Politics* no reference to this quite revealing paper can be found (Newmeyer 1986b)

In what follows, I will try to illustrate the point with the help of just three examples, though they could be multiplied almost ad libitum. One is the statement made by Chomsky himself in an interview in 1971; the other two are public acknowledgements of funding. All three suggest the extent the fi-

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<sup>9</sup> It reads: “Newmeyer and Emonds 1971 have discussed at length the funding of linguistic research in the United States. The point is made that while, of course, the source of funding is irrelevant to the ultimate CORRECTNESS of a theory, this is by no means irrelevant to a (partial) explanation of one’s ACCEPTANCE. It is tempting to speculate on the speed with which transformational grammar would have won general acceptance had Chomsky and Halle’s students had to contend with today’s more austere conditions, in which not just military, but ALL sources of funding have been sharply curtailed, and the number of new positions has been declining yearly.” (Newmeyer 1980: 52n.8; emphasis in the original).

nancial aspect played in the expansion of linguistics in general, and the success of TGG in particular.

Asked about the question of funding and the reason why *Syntactic Structures* and many other works of his contained acknowledgements of support from agencies of the U.S. Defense Department, Chomsky replied:

Ever since the Second World War, the Defense Department has been the main channel for the support of the universities, because Congress and society as a whole have been unwilling to provide adequate public funds [...]. Luckily, Congress doesn't look too closely at the Defense Department budget, and the Defense Department, which is a vast and complex organization, doesn't look closely at the projects it supports — its right hand doesn't know what its left hand is doing.<sup>10</sup> Until 1969, more than half the M.I.T. budget came from the Defense Department, but this funding at M.I.T. is a bookkeeping trick. Although I'm a full-time teacher, M.I.T. pays only thirty or forty per cent of my salary. The rest comes from other sources — most of it from the Defense Department. But I get the money through M.I.T. (Mehta 1971: 193)

I am not quoting Chomsky's account to 'raise the moral index finger' (as we say in German) but to give an idea of the tremendous non-academic involvement in the funding of research, including work not visibly (at least to an outsider) connected with military interests. (Interestingly, Newmeyer & Emonds [1971: 301] noted that a "result of the reliance on outside funding agencies is the occasional deliberate falsification of the nature of linguistic work".) It should be remembered that one of the major projects of the Defense Department during the 1950s was machine translation, and that M.I.T. had a major stake in it (cf. Locke & Booth 1955; Yngve 2000). Morris Halle, Chomsky's longtime supporter and ally, for instance, acknowledged the kind of support that existed there at the time:

During the past eight years [i.e., since 1951: EFKK] it has been my great and good fortune to be associated with the Research Laboratory of Electronics, M.I.T. This unique research organization has been an ideal environment in which to carry on investigations that overlap a number of traditional boundaries between disciplines. (Halle 1959: 15)

Needless to add that Halle, like Chomsky, was in a comparatively sheltered position during the 1950s. (Who, with only a Master's degree to his credit, would nowadays obtain a four-year fellowship with no other strings attached than to pursue independent research, and who would be employed, several

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<sup>10</sup> One may doubt this assumption and instead be inclined to believe that Chomsky's reductionist approach to language and the highly operationalist nature of his theory may have appealed to certain administrators in the Pentagon (and elsewhere) who prefer to deal with diagrams and program sheets rather than with the untidiness of much of regular linguistic work.

years before completing one's Ph.D., in a research position at M.I.T.?) That the funds which were received by the Research Laboratory of Electronics and later also by the Department of Linguistics, founded at M.I.T. in 1961, were used for proselytizing purposes as well, may be deduced from the number of acknowledgements of support by workers in linguistics. That at least part of these funds was intended to convert young students to the new faith may be surmised from the acknowledgement printed at the bottom of Robert Lees' widely acclaimed 'review' of *Syntactic Structures* (Lees 1957: 375), which was written and published while Lees was a close associate and, for all practical purposes, still a doctoral student of Chomsky's at M.I.T. (Lees 1960 constitutes his dissertation, published shortly after its completion.) Owing to the godfatherly attitude that Bernard Bloch displayed (cf. Murray 1980), Lees' propaganda piece for Chomsky's ideas appeared in *Language* (still today the most widely circulated linguistics journal in the world) almost at the same time *Syntactic Structures* itself was published.<sup>11</sup> Under normal circumstances, a review would take at least a year to appear in print following the publication of a book; also one may wonder if Lees was indeed the sole author of the 'review', considering his employment situation at M.I.T. at the time. But even if the arguments were all Lees' own, as Chomsky emphatically maintained in a letter to the present writer commenting on Koerner (1984b), it can be at least assumed that Chomsky — and probably Halle too — had seen and approved the text before it was sent to Bloch. (That Lees had published a paper in *Language* as early as 1953, and thus established previous contact with Bloch, cannot serve as a convincing counter-argument of collusion.)

The question of 'revolutionary rhetoric' will occupy us in section 4.3 (below); however, in the present context we may refer to Jerrold J. Katz's (1964) apprenticeship piece in this area entitled "Mentalism in linguistics". Together with Paul M. Postal's *Constituent Structure* of the same year, it set the stage for the transformationalists' polemics against the so-called taxonomists (a term created by Chomsky [1964: 11]) or, as Voegelin & Voegelin (1963: 12–13) characterized the phenomenon, Katz's paper embarked on the 'controversial stance' with a view to establishing the 'eclipsing stance'. Chomsky had

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<sup>11</sup> Chomsky (1975: 3) noted himself that "there would have been little notice in the profession if it had not been for a *provocative* and extensive review article by Robert Lees that appeared almost simultaneously with the publication of *S[yntactic] S[tructures]*" (emphasis added: EFKK). Naturally, Chomsky does not indicate how this came about; for details, see Murray (1980: 79–81, and especially footnote 55 on p.87).

given the signal for this kind of attack in 1957 (cf. Voegelin 1958: 229). It is interesting to note that in Katz's piece it was not the LINGUISTICS of the older scholars that was attacked, but rather what Katz made out to be their particular view of science. In other words, ideological questions appear to have offered a more promising forum for his attack than actual linguistic analyses of the Bloomfieldians from whom Chomsky himself had learned his craft.<sup>12</sup> Katz's paper on "Mentalism in linguistics", which Bloch, the Bloomfieldian stalwart, accepted for publication in *Language*, though it contains little that may be termed research, has the following acknowledgement:

This work was supported in part by the U.S. Army, Navy, and Air Force under Contract DA36-039-AMC-03200(E); in part by the U.S. Air Force, ESD Contract AF 19(628)-2887; and in part by the National Science Foundation (Grant G-16526), the National Institutes of Health (Grant MH-04737-03), and the National Aeronautics and Space Administration (Grant NsG-496). This paper, although based on work sponsored in part by the U.S. Air Force, has not been approved or disapproved by that agency. (Katz 1964: 124, n.\*)

In addition to public acknowledgements such as these, other documents (e.g., the annual report of the National Science Foundation in Washington, D.C.) could be cited to show the magnitude of the financial support received by major universities and in particular by the Massachusetts Institute of Technology which can be fairly said to have built its flourishing Linguistics Department from a rather modest Department of Modern Languages on the strength of the tremendous sums of money that flowed into its coffers during the 1960s and early 1970s. While it would be unfair to say that money alone has made the success story of TGG possible — to maintain such a view would mean to deny the existence of human resourcefulness and creativity (not in the Chomskyan sense, *nota bene!*) — nevertheless every researcher knows the importance of funding for any project s/he might conceive.

#### 4.3 *The rhetoric of revolution*

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<sup>12</sup> In this context, it is almost curious to see Chomsky's debt to Harris' work acknowledged in a history of linguistics by a one-time adherent of TGG (cf. Sampson 1980: 134-138 *passim*). Indeed, Chomsky himself (1975: 41-45), writing on Harris' concept of 'grammatical transformation' and of his attempts at discourse analysis, acknowledges his introduction to linguistics through Harris on this and other occasions (e.g., Mehta 1971: 187-188), though always stressing the differences between his and Harris' views. In another interview (Sklar 1968: 215) Chomsky indicated that his introduction to linguistics began by proofreading Harris' *Methods of Structural Linguistics*, a manuscript edition of which was circulating at least since 1946. (It had been completed early in 1947, but was published in Chicago only in 1951.)

## LINGUISTICS AND REVOLUTION

All who have lived through the period of the 1960s and early 1970s in North American linguistics will recall instances — at professional meetings, national or international conferences, at the Linguistic Institutes sponsored by the Linguistic Society of America as well as those of other associations and institutions — where propaganda of one kind or another was made for the ‘radically novel’ approach to linguistic analysis provided by TGG. Indeed, I believe that many students in linguistics, if not the majority, were glad to see what was regarded as establishment scholars being attacked by members of the younger generation (see below for illustration). Many students having come from Europe during the mid- or late 1960s, usually after having completed at least their first university diploma there, tended to embrace the new brand of theory; they could never have warmed up to the models of language analysis provided by Bloch, Harris, Trager, Smith, and others. But they felt they could easily associate with ideas that seemed to hark back to Descartes, Port-Royal, and Humboldt. One may doubt that these young Europeans regarded TGG as particularly revolutionary; indeed, many of them soon detected that for all practical purposes the alleged ‘mentalist’ view of language had little effect on the actual practice which retained much of the earlier kind of data-manipulation in accordance with prescribed rule. To them it probably did not really seem that much different from earlier procedures stigmatized as ‘taxonomic’, ‘mechanistic’, and ‘uninteresting’. Many of them abandoned TGG a few years after their return to Europe. The more critical attitude of many European students (e.g., Anttila 1975, Meisel 1973) suggests that, in order to understand the success story of TGG during the 1960s and 1970s, we must go beyond the technical framework of the theory and recapture, as much as possible, the general atmosphere within which it was proposed. (On ‘linguistic rhetoric’, see also Paul Postal’s [1988] rather revealing analysis.)

In order to map out this intellectual climate fully, the historiographer would have to interview the participants in the discussions held during the period (as was done by R. A. Harris 1993b, Murray 1994, and Huck & Goldsmith 1995), especially at those public meetings which were regarded as important by the strategists of ‘modern linguistics’ (a term dear to TGG discourse; cf. Smith & Wilson 1979). These professional meetings include the Ninth International Congress of Linguists held in Cambridge, Massachusetts in August 1962, and various other meetings in North America thereafter, especially the (until the 1980s still) semiannual meetings of the Linguistic Society of America, which, as we know, provided handy forums for public debates and even attacks on the views of others not bowing to the new theory. This is admitted by adherents of the Chomsky school (cf. the references to

Newmeyer's accounts below), and needs no further documentation in the present paper; instead, I would like to raise some questions concerning the 1962 International Congress held at Harvard and M.I.T (for the first time in the history of this organization outside Europe).

Was it really "sheer coincidence", as Newmeyer (1980: 51) claims, that the Congress was held at Cambridge, Mass., with Morris Halle and William N. Locke, then chairman of the M.I.T.'s Modern Languages Department, on the local arrangements committee? (In fact, Locke also held the position of Secretary General of the Congress and Halle the post of secretary of the Executive Committee according to the information supplied in Lunt [1964: v].) And what happened to Joshua Whatmough (1897–1964) of Harvard, who "was the chief figure in securing the invitation for the 9th International Congress to meet in the United States, and who was instrumental in obtaining two substantial grants for support of that congress" (as Eric P. Hamp reports in *Language* 42.622, 1966)?<sup>13</sup> And why did Zellig Harris turn down the offer to present one of the five major papers to be given at the Congress' plenary sessions? (The other four scholars, Jerzy Kuryłowicz, Émile Benveniste, André Martinet, and Nikolaj D. Andreev, were between 52 and 66 years old.) The fact is that Chomsky, less than 35 years of age and without any prior international exposure, was given the spot not taken by his former teacher. Was it an accident that Roman Jakobson, with whom Halle had collaborated on phonological research since the late 1940s and completed his doctorate at Harvard in 1955, presented Chomsky to the Congress participants as the rising star?<sup>14</sup> (An indication of how much Chomsky owed Jakobson may be gathered from his own testimony in *A Tribute to Roman Jakobson* published in 1983.)

Chomsky's "Logical basis of linguistic theory" presentation was by far the longest of these five plenary papers; it was given as the fifth and last of the plenaries (in seeming deference to the international standing of the other four speakers), but it had 62 pages in the printed Proceedings in comparison

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<sup>13</sup> As a matter of fact, Whatmough, professor of comparative philology at Harvard, had originally been selected to serve as President of the Congress, but as the 1964 Proceedings indicate, he was replaced prior to its tenure by Einar Haugen (who at the time was still at the University of Wisconsin). Whatmough's name does not even appear in the list of Congress participants (cf. Lunt 1964: 1145-1171). He thus was effectively written out of the historical record.

<sup>14</sup> Professor Johann Knobloch, who participated in the 1962 Congress, told me when I gave a paper on the present topic in 1982 at the University of Bonn, that he had felt at the time that he was witnessing the 'enthronization' of Noam Chomsky.

to between 22 (Kuryłowicz's paper) and 10 pages (for each of the three remaining plenary speakers). Likewise, the discussion of Chomsky's paper took up 30 pages in contrast with between 5 and 10 pages for the four others. (Comparison between the *Preprints* of the Congress — edited by no other person than Morris Halle — and the *Proceedings* edited by another former student of Jakobson's, Horace Gray Lunt (b.1918), reveals that Chomsky was given unlimited opportunity subsequent to the Congress to expand on his views and to answer any of the objections raised in these discussions that he considered relevant.)<sup>15</sup>

It is also interesting to note that it was at this Congress, which was attended by some 950 scholars from all over the world, especially from Europe,<sup>16</sup> that Chomsky talked for the first time about Saussure, Humboldt, and the Port-Royal grammar, all the time trying to demonstrate how much his own theory had in common with these hallowed traditions of 17th to 19th century Europe. I believe that it was at this well-orchestrated Congress where Chomsky's appeal to a 'rationalist' tradition underlying his linguistic ideas first attracted the attention of many Europeans to his work. (Before 1962 — the year when *Syntactic Structures* was reprinted for the first time, evidently for the International Congress — few Europeans had taken note of Chomsky.) Murray (1980) appears to have been one of the first scholars to devote particular attention to the socio-political manoeuvres of the TGG group around Chomsky and his early and enduring ally, Morris Halle. It is from him (Murray 1980: 88, n.85) that I took the idea of 'rhetoric of revolution', about which I would like to say a few things in what follows. Indeed, Halle's role in the promotion of Noam Chomsky and TGG should be thoroughly examined (cf. Koerner 2002, chapt. 9); his talents as organizer and administrator are acknowledged by Newmeyer (1980: 39), who unfortunately says nothing about Halle as an academic politician. However, as one visiting fellow at

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<sup>15</sup> Note that Chomsky's paper at the Congress was by no means the only one promoting TGG; papers by William S-Y. Wang, Samuel R. Levin, Paul M. Postal, Emmon Bach, Paul Schachter, and others too (cf. Lunt 1964: 191-202, 308-314, 346-355, 672-677, 692-692, in that order) had their share in it.

<sup>16</sup> Following my paper on the present subject at the University of Vienna on 16 December 1982, Prof. Wolfgang U. Dressler, who served as the president of the 1977 International Congress, commented that, according to his information, there had never been as much money available for a congress as for the one held at Cambridge, Mass., in 1962, and that there would probably never again be so much money available in the future. According to him, hundreds (!) of foreign scholars had their travel expenses paid by the congress organizers.

M.I.T. at the time recalls, in the spring and early summer of 1962, prior to the tenure of the International Congress (which took place on 27–31 August), he was “watching Morris Halle plot as if he were Lenin in Zurich” (John Gumperz in a 1977 interview with Stephen Murray).

We may forego here an analysis of what Murray has termed Chomsky’s ‘publishing woes’ (on which see now Murray 1999) and the standard myth of young Chomsky’s intellectual isolation during the 1950s, a claim he never tires of reiterating (cf. Sklar 1968: 214; Chomsky 1979: 131; 1982: 42–43). As a matter of fact, and contrary to what Newmeyer (1980: 34–35) and others have been saying, Murray (1980, 1981) has convincingly established that only one paper by Chomsky was ever rejected, and this by the then editor of *Word*, André Martinet (1908–1999), despite a strong recommendation by the late Uriel Weinreich (1926–1967), the journal’s associate editor at the time (cf. Murray 1980: 77) and Jakobson’s pleading with Martinet to reverse his decision.<sup>17</sup> But then neither the journal nor the editor subscribed to the Bloomfieldian type of structuralism that lays at the bottom of Chomsky’s linguistics. *Language*, the official organ of the Linguistic Society, and with it its long-time editor, Bernard Bloch (1907–1965), supported Chomsky in every possible way. Similar observations could be made about the publication of Chomsky’s books; consider Murray’s (1980: 76–77; 1999) account of the fate of *The Logical Structure of Linguistic Theory*, which the author released for publication some twenty years after it had been written, although previous offers to publish it had been made (see also Chomsky’s own [1975: 3] account of this). As can be gathered from Chomsky’s bibliography, he published papers and reviews in all recognized outlets in the field, especially in *Language* and the *International Journal of American Linguistics* (usual acronym: *IJAL*), but also in *Word* (cf. Chomsky 1961) during 1954–1961 (cf. Koerner & Tajima [1986: 3–13] for details).

Another important aspect of the success story of TGG during the 1960s had little to do with scholarship. Newmeyer (1980), who regarded it as a

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<sup>17</sup> The Roman Jakobson Papers at M.I.T. (Box 44, folder 12) contains a copy of the letter from Jakobson to Martinet, dated 28 October 1953, which carries the following passage (Jakobson was serving as an associate editor of *Word* at the time):

I’d like also to bring to your attention Noam Chomsky, who has the high tribute of being Junior Fellow of Harvard. Both Harris and the outstanding logician Goodman (Penn), as well as our Quine, consider him as a remarkable thinker in linguistics and logic. He was very unhappy about your rejection of his paper, which on my recommendation he submitted to you for *Word*. I think, however, that for the sake of understandability to the average linguist, it was useful, as you suggested, to retouch this indeed valuable piece of work. Now that he has done it, may I again bring his study to your attention. I am sure that Quine and Harris will fully support my recommendation and I know that you in your turn find these problems as important to be raised.

commendable feature on the part of the young adherents of TGG, describes it in the following terms (p.50):

The missionary zeal with which “the other guys”<sup>18</sup> were attacked may have led some linguists, along with Wallace Chafe (1970), to be “repelled by the arrogance with which [the generativists’] ideas were propounded [p.2],” but overall the effect was positive. Seeing the leaders of the field constantly on the defensive at every professional meeting helped recruit younger linguists far more successfully and rapidly than would have been the case if the debate had been confined to the journals. [Robert Benjamin] Lees and [Paul Martin] Postal, in particular, became legends as a result of their uncompromising attacks on every structuralist [i.e., non-TGG]-oriented paper at every meeting.

Newmeyer hints that both Chomsky and Morris Halle encouraged students to engage in this type of aggressive and openly polemical activity which not infrequently turned into ad-hominem attacks (cf. also Chomsky & Halle 1965); he concedes that there may have been some excesses:

The combative spirit may have gotten a bit out of hand at times, as even undergraduate advocates of the theory such as Thomas Bever and James Fidelholtz got into the act, embarrassing their teachers as they ruthlessly lit into linguists old enough to be their grandparents. (Newmeyer 1980: 50–51)

It was in the publications and, in particular, in the public debates of the followers of TGG that the rhetoric of revolution, the claim to novelty, ‘creativity’, and originality, came to the fore, coupled with the claim of a lack of comprehension and support on the part of the older generation of linguists. Murray (1980; 1994: 228–235) has shown, on the contrary, that support from the older academics was indeed forthcoming. For instance, Chomsky was invited twice, in 1958 and 1959, to expound his theories at conferences on the structure of English held at the University of Texas at Austin. If we are to believe Newmeyer (1980: 46), however, Archibald Hill (1902–1992), the organizer and host of these conferences had invited Chomsky for the express purpose of “confronting it [i.e., TGG] directly with the intent of snuffing it out before any serious damage could be done [to Bloomfieldian structuralism]”. Anyone familiar with Hill as a person would find this hard to believe, and everyone interested in verifying what happened at the 1958 conference

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<sup>18</sup> Sampson (1980: 252n.12) reports that the “course which Halle’s and Chomsky’s department offers on non-Chomskyan linguistics [...] is popularly known, by staff and students alike as ‘The Bad Guys’. Obviously the name is not intended [to be taken] too seriously, but it is indicative [of their general attitude towards the ideas of others displayed at MIT].” (I am completing here Sampson’s elliptical sentence: EFKK.)

may read the faithfully transcribed discussion following the presentation of each paper. “Here”, according to Newmeyer (1980: 35),

we can see the history documented as nowhere else — Chomsky, the *enfant terrible*, taking on some of the giants of the field and making them look like rather confused students in a beginning linguistics course.

Personally, I do not notice any ‘giant’ in the roster of speakers, but it is clear from the proceedings (Hill 1962) that Chomsky was little interested in compromise; instead, he sought ways to make his ideas look controversial, because in his words “they go to the root of the problem and give radical answers”, as he later claimed in an interview, where he expounded on his general attitude as follows:

Even before I came to M.I.T. [i.e., in 1955], I was told that my work would arouse much less antagonism if I didn’t always couple my presentation of transformational grammar with a sweeping attack on empiricists and behaviorists and on other linguists. A lot of kind older people who were well disposed toward me told me I should stick to my own work and leave other people alone. But that struck me as an anti-intellectual counsel. (Mehta 1971: 190–191)

It is clear from this statement (as well as others made by Chomsky publicly and privately) that the new theory was to be presented in a polemical fashion. However, during the 1950s and even until the mid-1960s, most American linguists of the older generation were well disposed not only toward Chomsky as a person but also toward his theory. The Bloomfieldian descriptivists felt that Chomsky’s syntactic theory was extending their own endeavours, and the fact that he had done his doctorate with Zellig Harris<sup>19</sup> at the University of Pennsylvania persuaded them to believe that he was one of theirs. Despite the attacks on the Old Guard by Chomsky and his associates, the fairly posi-

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<sup>19</sup> Actually, this statement requires modification. Chomsky had left for Harvard shortly after completion of his M.A. in 1951, and it cannot properly be said that Harris supervised his dissertation. What actually happened has recently been recounted by Chomsky himself. In April 1955, he had received a draft notice from the U.S. Army:

I was 1-A. I was going to be drafted right away. I figured I’d try to get myself a six-week deferment until the middle of June, so I applied for a Ph.D. I asked Harris and Goodman, who were still at Penn, if they would mind if I re-registered — I had not been registered at Penn in four years. I just handed a chapter of what I was working on for a thesis, and they sent me some questions via mail, which I wrote inadequate [sic] answers to — that was my exams. I got the six-week deferment, and I got my Ph.D. (Hughes 2001: 41)

As a result, Chomsky was freed from military service. The particular handling of Chomsky’s thesis defence also explains why the dissertation carries only Zellig Harris’ signature, both as thesis supervisor and as committee chair, and no else’s as would have been regular procedure.

tive attitude of the older generation of scholars (which included not only the ‘Bloomfieldians’ but the ‘Sapirians’ as well) did not noticeably change until Halle and Chomsky began attacking their work in phonology, an area typically ignored in Newmeyer’s (1980) survey of TGG.<sup>20</sup> We may refer to the exchange between Householder (1965) and Chomsky & Halle (1965), as well as Hockett’s verdict about “Chomskyan-Hallean ‘phonology’”, which, in his opinion (Hockett 1968a: 3), was “completely bankrupt”. Hockett had earlier (1965: 187) indicated his reactions to the style of Young Turks like Lees:

We do not enjoy being told that we are fools. We can shrug off an imprecation from a religious fanatic, because it does not particularly worry us that every such nut is sure he holds the only key to salvation. But when a respected colleague holds our cherished opinions up to ridicule, there is always the sneaking suspicion that he may be right.

Although Hockett was referring to Lees’ review of *Syntactic Structures* and the introductory remarks Lees had made in his *Grammar of English Nominalizations* (Lees 1960), the real bone of contention was phonology and the phoneme concept, as Murray (1981: 110–111) has pointed out; compare Archibald A. Hill’s observation:

I think that if one can speak of partial survival [in the revolution of Chomskyan and post-Chomskyan linguistics], I have partially survived it. [...]. I could stay with the Transformationalists pretty well, until they attacked my darling, the phoneme. I will never be a complete transformationalist because I am still a phonemicist. (Hill 1980: 75)

Hill’s statement is an important document for the historian of linguistics since it dispels the widely accepted myth that it was the early work on syntax that had revolutionized linguistics (and antagonized the older generation). Note Bierwisch’s (1971: 45) affirmation: “When Chomsky published *Syntac-*

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<sup>20</sup> In the preface to his book Newmeyer (1980: xi) states: “In fact, there is no discussion of developments in phonology since the early 1960s.” Apart from one of his colleague’s (at the University of Washington, Seattle) suggestion that Newmeyer would not know enough about the subject to write about its evolution, it is a simple fact that volumes of collective articles on ‘generative linguistics’, at least those published during the 1970s, are heavily tilted toward phonology, with comparatively few contributions devoted to syntax. This may have changed somewhat since the early 1980s when the Government-and-Binding approach became popular among the new generation of linguists trained at MIT, Amherst, UCLA, USC, and a few other places (e.g., the University of Arizona). – The history of phonology by Anderson (1985), while not free from generativist bias, has been judged as much more balanced than Newmeyer’s (1980) treatment of syntax (cf. Howell 1986).

*tic Structures* in 1957, structural linguistics entered a new phase”<sup>21</sup>. Newmeyer goes a few steps further, trying to establish the view that in fact a revolution was taking place, and that it began in 1955, when Chomsky had completed his “truly incredible work of the highest degree of creativity”, i.e., his study *The Logical Structure of Linguistic Theory* (henceforth: *LSLT*), which “completely shattered the prevailing structuralist conception of linguistic theory” (Newmeyer 1980: 35). Newmeyer does not adduce much evidence to support his claim, something which would be difficult to do since this bulky work was published only twenty years later (Chomsky 1975). In his 1986 paper on ‘the Chomskyan revolution’ Newmeyer (p.8) now concedes that Bernard Bloch, “arguably the most influential linguist of the period, concretely abetted Chomsky and his theory in a number of ways”, as Murray (1980) had clearly documented earlier (see also Newmeyer [1980: 47–48] for an early indication of Bloch’s support of TGG).

As a matter of fact, by the mid-sixties the North American linguistic scene was much like the characterization that Sydney Lamb gave it in his review of *Current Issues in Linguistic Theory and Aspects of the Theory of Syntax* (Chomsky 1964, 1965):

The prevailing attitudes are of two different types. Older-generation linguists, upon encountering some of these pages [in Chomsky 1964 and 1965], will stare with incredulity and no little irritation at the distortions and misunderstandings of their ideas and practices and those of their colleagues; while students who never knew what neo-Bloomfieldian linguistics was really like, and those from fields outside linguistics, are led to the false impression that all linguists before Chomsky (except, of course, Humboldt, Sapir, and a few other candidates for canonization) were hopelessly misguided bumbling, from whose inept clutches Chomsky has heroically rescued the field of linguistics. (Lamb 1967: 414)

No doubt the fact that a great many, if not most, of the Ph.D. students that arrived at M.I.T. during the mid-1960s came from fields outside linguistics such as chemistry (e.g., Robert B. Lees, James A. Foley), mathematics (e.g., James D. McCawley, Barbara Hall Partee, Joseph Emonds), and other sciences (e.g., D. Terence Langendoen, Sc.B., M.I.T., 1961) and, as a result, had no prior exposure to, and no previous theoretical commitment within, linguistics, fostered this view of things as described by Lamb.

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<sup>21</sup> In view of the attempt of some to characterize *Syntactic Structures* as the work that ushered in Chomsky’s revolution of the field, Bierwisch’s observation is important.

#### 4.4 *Continuity and/or discontinuity*

It is interesting to note that Newmeyer, who has tried so hard to establish something like a ‘rupture épistémologique’ (Bachelard) between Chomsky’s theories and those of his immediate predecessors, refers to two papers by Harris and Hockett published in 1954, which contain statements which sound very ‘Chomskyan’ to me. However, according to Newmeyer (1980: 37), these statements must be regarded as uncharacteristic of the work of these two theorists. I presume he means to say that they were intellectual Entgleisungen, accidental slips of the pen, which, as Newmeyer maintains, “clashed head-on with their usual methodological assumptions” and that therefore, “it is not surprising that they did not develop them”. While it is true that neither Harris nor Hockett developed the generative model now associated with Chomsky’s name, nevertheless the context in which these ideas were put forward indicate clearly that they were anything but mental lapses. It is obvious, however, that those stressing discontinuity rather than continuity in the development of American linguistics during the later 1950s would like to see it that way. In order to answer this question about their theoretical outlook, let us inspect the two 1954 papers by Harris and Hockett separately as well as earlier statements by these two scholars in view of Newmeyer’s attempt to push the date of the origin of TGG back to the year 1951, i.e., Chomsky’s M.A. Thesis (Newmeyer 1986a: 5n.4). In this connection, it may be interesting to read that George Lakoff, himself an early adherent of ‘modern linguistics’, regarded at least the earlier phase of TGG as “a natural outgrowth of American structural linguistics” (1971: 267–268).

4.4.1 *Harris.* Zellig S. Harris’ 1954 paper is entitled “Transfer grammar”. (The terminological change from ‘transfer grammar’ to ‘transformational grammar’ appears to me comparable to the terminological pair ‘evolution theory’ and ‘evolutionary theory’; Wells, writing in 1963, still spoke of ‘transformation theory’.) In his paper Harris was concerned with developing a model of language transfer, i.e., the construction of methods by which phonological, morphological, and also syntactic structures of one language could be transferred to those of another language. In short, Harris was working on a theory of language translation which could be used by a machine. As mentioned earlier in this paper, machine translation was one of the major interests of theoretical linguists at the time (cf., e.g., Bar-Hillel 1954, Casagrande 1954, Locke 1955) and received considerable financial support from various U.S. government agencies, including the CIA (cf. Hutchins

2000, for details). Harris (1954a: 259) believed that one should begin the task of mechanical translation by

defining difference between languages as the number and content of the *grammatical instructions needed to generate the utterances of one language out of the utterances of the other*. (Italics mine: EFKK)

He subsequently defines ‘grammar’ as “a set of instructions which generates the sentences of a language” (p.260), and this definition is repeated in the paper — in other words, it was not meant to be a remark à part but a definition, at least an operational one. Section 5 of Harris’ paper (pp.267–270) is devoted to syntax, an area which is said to have been neglected, if not totally ignored, by linguists before Chomsky (cf., however, Bloomfield 1942a,b; Nida 1966 [1943]; Bloch 1946). Interestingly, Harris proposes a transfer of sentences from English to Modern Hebrew, a language whose morphophonemic system occupied Chomsky for a number of years (1949–1951; cf. Koerner 2002, chapt. 9 for details).<sup>22</sup> The chart on page 268 of Harris’ paper, its explanation and the discussion deserve particular attention, since they show quite clearly his tendency toward formalization. This penchant for mathematical formulae and algebraic expression, which characterizes Chomsky’s approach to syntax in *Syntactic Structures* several years later, is also very obvious in Harris’ *Methods in Structural Linguistics*, a book which Chomsky read in proof in 1947. Chomsky (1975: 25) in fact acknowledged that this reading was his “formal introduction to the field of linguistics”. In the early 1950s, Chomsky (p.29) was “firmly committed to the belief that the procedural analysis of Harris’ *Methods* and similar work should really provide complete and accurate grammars if properly redefined and elaborated.” But before quoting an interesting passage from Harris’ book *Methods*, which Norman McQuown (b.1914) called ‘epoch-making’ in his 1952 review (p.495), let me refer to an important statement by Harris in his 1954 paper (which Chomsky may well have seen in manuscript a year or two prior to its publication), as it shows that Harris had a definite purpose in mind when he distinguished between ‘transfer grammar’ and ‘transformational grammar’:

Even in the grammar of a single language by itself, it is possible to generate some of the sentences of the language out of other sentences of the same language by particular grammatical transformations. However the conditions for these grammatical transformations are quite different from those that carry us from the sentences of one language to those of another [as in transfer grammar]. (Harris 1954: 260n.2)

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<sup>22</sup> To suggest, as Newmeyer (1980: 34) does, that Harris never “even looked at it [i.e., Chomsky (1951)]”, is at best gratuitous.

Statements like this speak for themselves and refute suggestions that “such views clashed head-on with (Harris’) usual methodological assumptions” and that it required Chomsky to come along and develop them (Newmeyer 1980: 37). Note also Harris’ formulation of a principle of formation rules in his *Methods* completed in 1947, if not earlier:

The work of analysis leads right up to the statements which enable anyone to synthesize or predict utterances in the language. These statements form a deductive system with axiomatically defined initial elements and with theorems concerning the relations among them. The final theorems would indicate the structure of the utterances of the language in terms of the preceding parts of the system. (Harris 1951: 372–373)

That an approach like this was important for his development of the theory of transformational grammar is acknowledged by Chomsky when he reports on his early research:

When I began to investigate generative syntax more seriously a few years later [i.e., after completion of Chomsky (1951)], I was able to adopt for this purpose a new concept that had been developed by Zellig Harris and some of his students, namely, the concept of “grammatical transformation”. It was quickly apparent that with this new concept, many of the inadequacies of the model that I had used earlier could be overcome. (Chomsky 1975: 40–41)

Seen in this light, it is no longer surprising when McQuown of Chicago found Harris’ emphasis on following basic methodological assumptions to their logical conclusion “wholly admirable”, and considered Harris’ contribution to linguistics

epoch-marking in the double sense: first in that it marks the culmination of a development of linguistic methodology AWAY from a stage of intuitionism, frequently culture-bound; and second in that it marks the beginning of a new period, in which the new methods will be applied ever more rigorously to ever widening areas in human culture. (McQuown 1952: 495; emphasis in the original)

Chomsky was unquestionably the most important developer of key ideas first formulated by Harris (cf. also Seuren 1998: 248–249). Regarding this we have Chomsky’s own account (1975: 41–45), where he delineates the basic lines of argument made in Harris’ 1955 Presidential Address to the Linguistic Society of America, “Transformation in linguistic structure” — published two years later with a different title (Harris 1957).<sup>23</sup> More or less the same ideas were published in a much later paper (Harris 1965), by which time

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<sup>23</sup> It is interesting to note that, as late as 1964, three papers by Harris, including this LSA Presidential address, were republished in a volume edited by Fodor & Katz and evidently intended to promote TGG.

Chomsky's and Harris' views had visibly diverged. However, it should not be forgotten that Chomsky was also familiar with Harris' earlier papers on 'discourse analysis', which clearly paved the way for the study of syntax (Harris 1952a, 1952b — mentioned only in a footnote in Chomsky's account [1975: 46n.6].) One could go back to even earlier statements by Harris (especially his *Methods* whose preface [p.v] is dated 'January 1947') to show that his concern with the subject of syntax did not only date from 1951 onwards. The contrary view would ignore the fact that the post-Bloomfieldians had been struggling with the problem for some time, at least on the level of what was later called 'phrase structure' (see the long article by Rulon Wells on 'immediate constituents' of 1947 as evidence of this effort). In this context it is interesting to note that Daladier [1980: 59n.1], who otherwise is at pains to show that Chomsky and Harris are worlds apart, affirms that Chomsky took the distinction between 'acceptability' and 'grammaticality' from Harris.

To sum up, it appears that the more closely we look into the discussion going on in American linguistics during the late 1940s and early 1950s, the more obvious it becomes that what many people today want to call a 'revolution', namely, the movement said to have been initiated by the publication of Chomsky's *Syntactic Structures*, was at most an evolution of then current work (cf. Anders 1984). As late as 1973, reviewing Hockett's volume of selected papers by Leonard Bloomfield, Harris points to this continuity in American linguistics when he states (p.255):

The work of Bloomfield can be looked at as paving the way for the later methods of transformational analysis. But his work is not only of historical relevance. It created the apparatus for a certain type and degree of linguistic analysis, and the body of analytic concepts which are a necessary part of any theory of grammar.

It can be seen that Newmeyer's attempts to establish the priority of Chomsky over Harris (and Hockett — see 4.4.2 below) by referring to "Chomsky's undergraduate thesis and his 1951 master's thesis" as antedating "the [1954] Harris and Hockett papers by several years" (1986a: 5n.4) is simply not born out by the facts. Indeed, in his 1980 book Newmeyer himself (p.36) mentioned Bloomfield's 1939 paper on Menomini morphophonemics as well as Roman Jakobson's 1948 paper on Russian conjugation as clearly exhibiting the spirit "of a generative phonology". It is therefore not surprising to find references to these two publications in the printed version of Chomsky's *Logical Structure of Linguistic Theory (LSLT)* of 1955 (Chomsky 1975: 571, 572), even though a number of other revealing references contained in the original typescript, notably those to Hjelmslev's 1953 *Prolegomena*, had been removed. Also noteworthy is Henry Kuãera's claim that Jakobson's

“Russian conjugation” of 1948 constitutes “a full generative description on the morphological level” (1983: 878). Its publication in *Word*, the only other major linguistic journal of the period, besides *Language* and *International Journal of American Linguistics (IJAL)*, makes it highly unlikely that Chomsky was not aware of this paper in 1949.

At least until the 1960s, when Chomsky began introducing the concepts of ‘deep’ or ‘underlying structure’ in contrast with ‘surface structure’ — cf. Chomsky [1965: 198–199n.12] for the ancestry of this distinction — the difference in Chomsky’s approach to syntax as found in *LSLT* and *Syntactic Structures* (compared to Harris’ approach in his 1954 paper for example) seems to be that Chomsky was concerned with transfers (and transpositions) within a single language only (e.g., Chomsky 1957: 61–84 *passim*).

Regarding the background to his work in a more general way, it is interesting to note that Chomsky consistently denied that it had anything to do with “attempts to use electronic computers” (e.g., Chomsky 1964: 25; cf. also Chomsky 1982: 63). It seems to me, however, that Chomsky is engaged in rewriting his own past, seemingly in an attempt to widen the difference between his work and Harris’ and to suggest discontinuity and novelty of his own approach. Thus in a 1979 interview Chomsky tried to explain away as simply a concession to the prevailing fashion of the times that *Syntactic Structures* contained a discussion of automata (Chomsky 1982: 63). Given the fact that he had been employed since the fall of 1955 at the Research Laboratory of Electronics at M.I.T., one would indeed expect such contemporary references. Thus in a 1958 paper (not mentioned in Newmeyer 1980 or its revised 1986 edition), Chomsky suggested, among other things, that

the study of this intermediate area between full scale Turing machines [cf. Turing 1950] and absolutely bounded automata is however quite important, not only for linguistics (it is, in a good sense, the general theory of grammar), but also [...] of intellectual processes. (Chomsky 1958: 437; also cited in Maher 1982: 18)

That the reference to computer work cannot be discounted as a passing remark may be gathered from a 1971 interview (Mehta 1971, cited in Maher 1982: 17), in which Chomsky said much the same. This is not at all surprising when we note that his collaborator Morris Halle stated in the 1959 preface to the publication of the revised version of his 1955 thesis:

I have assumed that an adequate description of a language can take the form of a set of rules — analogous perhaps to a program of an electronic computing machine — which when provided with further special instructions, could in principle produce all and only well-formed (grammatical) utterances in the language in question. This set of rules, which we shall call the grammar of the language and of which phonology

[i.e., Halle's special interest: EFKK] forms a separate paper, embodies what one must know in order to communicate in the given language ... (Halle 1959: 12–13).

Halle's statement, in which he clearly aligns himself with Chomsky's work (as is evident from the two immediately preceding paragraphs in his foreword) leads us back to the other important 1954 paper, namely, Charles Hockett's celebrated "Two models of grammatical description", to which Chomsky refers frequently in his writings during 1955 and 1964, and to which his 1956 paper is a kind of response.

*4.4.2 Hockett.* Since Newmeyer (1980: 37) refers to Charles F. Hockett's "Two models of grammatical description" as one of the two 1954 papers that 'uncharacteristically' contained the seed of generative grammar, this well-known, programmatic article merits somewhat closer inspection. Hockett (1954: 210) himself said, the "bulk of the [...] paper was written between 1949 and 1951"; but because of the fact that he recognized, in 1951, that it gave the "erroneous impression that there were principally just two archetypes [of grammatical description] to be dealt with", he withheld the paper from publication for a number of years. However, the typescript version was circulating among Hockett's colleagues as early as 1951 (cf. Voegelin & Voegelin 1963: 25), and it appears that Hockett made use of it when the editors of *Word*, specifically André Martinet, asked him for a contribution to their special volume celebrating the tenth anniversary of the journal which they entitled *Linguistics Today*. (The volume features, among others, a paper by Benoît Mandelbrot on "Structure formelle des textes et communication", one by Zellig Harris on "Distributional structure", and one by Rulon Wells on "Meaning and use".) In his paper Hockett makes, as I read it, a strong argument in favour of a dynamic — in his terminology 'Item and Process' (IP) — approach, in contrast to the more usual 'Item and Arrangement' (IA) approach characteristic of most of the work done until then in North American linguistics, although, as Hockett (1954: 210–211) himself remarked, the IP model was the older, though it had largely been confined to historical linguistics.

Hockett's paper is intended as an important theoretical statement; indeed, we see him grappling with problems which Chomsky attacked soon after more successfully, and it is not difficult to see the importance the paper had for Chomsky (cf. also his 1956 paper, whose title echoes Hockett's). In his argument, Hockett makes a series of theoretical statements and definitions, first with regard to IA analysis (211–227), giving particular attention to the problems arising from various definitions. Then, parallel to the preceding

discussion, he presents the various definitions basic to a descriptive analysis within a process framework (227–228), before making a comparison between the two approaches (229–232). The final page (232–233) consists of a discussion of more general considerations in ‘grammatical description’. I shall return shortly to this last-mentioned issue; before doing so, however, I would like to quote one of the statements made by Hockett with regard to IP analysis, the one pertaining to ‘derived forms’. Hockett says:

A derived form consists of one or more UNDERLYING FORMS to which a process has been applied. The underlying forms and the process all recur (save for occasional uniqueness) in other forms. The underlying form or forms is (or are) the IMMEDIATE CONSTITUENT(S) of the derived form, [...] (Hockett 1954: 227–228; small capitals in the original).

When we are told by Chomsky that his first interest in language derived from his acquaintance during childhood with his father’s historical work on medieval Hebrew and that his “original interest in generative grammar was based on a perfectly conscious analogy to historical Semitic linguistics” (quoted in Koerner 1978: 44; see also Yergin 1972: 112), it is not surprising to find terms and concepts such as ‘derivation’ and ‘underlying form’ in Chomsky’s non-historical work. Indeed, as Hockett indicates (1954: 210–211), Chomsky’s teacher Harris referred to this historical analogue in his work as early as in 1944.

If the above theoretical considerations are little other than common knowledge in the field at the time, a number of Hockett’s general stipulations regarding the criteria “for the evaluation of a grammatical description” were probably not. Apart from the criteria of generality, specificity, and what he terms ‘efficiency’ of a model, the requirement of ‘productivity’ deserves particular attention, especially since it is related to another observation to which I shall turn in a moment:

(4) A model must be PRODUCTIVE: when applied to a given language, the results must make possible the *creation of an indefinite number of valid new utterances*. This is the analog of the ‘prescriptive’ criterion for descriptions. (Hockett 1954: 232–233; italics added: EFKK)

This criterion is preceded by one of ‘inclusiveness’, by which Hockett means that when a model is “applied to a given language, the results must cover all the observed data and, by implication, at least a very high percentage of all the not-yet-observed data.” That this is not simply an unimportant passing remark is clear from the earlier general requirement of a satisfactory grammatical description:

The description must also be prescriptive, not of course in the Fidditch sense, but in the sense that by following the statements one must be able to *generate any number of utterances in the language, above and beyond those observed in advance by the analyst — new utterances most, if not all, of which will pass the test of casual acceptance by a native speaker.* (Hockett 1954: 232; italics mine: EFKK)

It is clear that Hockett means something like ‘predictive’ when he uses the term ‘prescriptive’ (see also the preceding quotation). Moreover, Hockett’s 1954 paper was the result of a number of years of reflection, especially on the importance of ‘prediction’ in linguistic theory.

That these observations are by no means isolated in Hockett’s thinking during the late 1940s and early 1950s, when Chomsky was a young student of linguistics, can be shown by two other important theoretical statements of his, published in 1948 and 1950 (not mentioned by Newmeyer in his 1980 book on the history of TGG nor in its second edition of 1986). Both papers are short; the first was reprinted in Martin Joos’ 1957 *Readings in Linguistics*, included in Newmeyer’s (1980: 263) bibliography and therefore accessible to him; the other appeared in George L. Trager’s working-paper-type journal *Studies in Linguistics* (1943–1973). I am tempted simply to reproduce in full Hockett’s 1948 “A note on ‘structure’”, but a few salient passages will have to suffice here to show how much the Cornell linguist — arguably the most interesting general theorist of his generation — was ahead of his time. Outlining the “task of the structural linguist, as a scientist”, Hockett emphasizes that it must go much beyond classification and the simple accounting for all the utterances which comprise the corpus of a language at a given time; he states,

the analysis of the linguistic SCIENTIST is to be of such a nature that the linguist can account also for utterances which are NOT in his corpus at a given time. That is, as a result of his examination he must be able to predict what OTHER utterances the speakers of the language might produce ... (Hockett 1948: 269; small capitals in the original).

And as if to anticipate much of Chomsky’s later argument about (the Bloomfieldians’ aversion to) ‘mentalism’ and his proposal of a (rather abstract) ‘language acquisition device’, Hockett continues in the next paragraph:

The analytical process thus parallels what goes on in the nervous system of a language learner, particularly, perhaps, that of a child learning his first language. The child hears, and eventually produces, various utterances. Sooner or later, the child produces utterances he has not previously heard from someone else. (Hockett 1948: 269–270)

The essential difference between the child’s acquisition of the language and the analyst’s procedure is described by Hockett in the same paper as follows:

[...] the linguist has to make his analysis overtly, in communicable form, in the shape of a set of statements which can be understood by any properly trained person, who in turn can predict utterances not yet observed with the same degree of accuracy as can the original analyst. The child's 'analysis' consists, on the other hand, of a mass of various synaptic potentials in his nervous system. The child in time comes to BEHAVE the language; the linguist must come to STATE it. (Hockett 1948: 270; emphasis in the original)

In the final analysis, a 'linguistic scientist' must "determine the structure actually created by the speakers of the language", not impose one, for "a language is what it is, it has the structure it has, whether studied and analyzed by a linguist or not" (Hockett 1948: 270–271).

Referring to what he believes is the unquestionable promise of 'immediate constituent' analysis, Hockett in his 1950 paper observed that it is "not an analytical technique, but a hypothesis about the nature of talking and hearing language"; at the same time he admitted:

The problem is to develop techniques by which the hierarchical structure of the utterances of a language can be revealed and stated. A child learning to speak has such a technique; our objective techniques are as yet quite faulty, but at least they are good enough to reveal this very important feature of linguistic structure. (Hockett 1950: 56)

*4.4.3 Preliminary conclusions.* From what has been presented in the two preceding subsections, we may be allowed to ask, some fifty years later, how far our insights into human language have advanced since then. Seen in this way, what is frequently described as a 'revolution' in linguistics, upon closer inspection of the evidence, looks 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. True, neither Harris nor Hockett carried through on several of their proposals, but the further development of certain aspects of their theoretical statements by someone else, and especially by someone who grew up within their tradition, does not make that person's theory revolutionary — and it certainly was not seen that way by the generation of Harris (1909–1992) and Hockett (1916–2000), neither during the 1950s, nor the early 1960s — unless we make allowances for a variety of other, non-linguistic factors, generational, ideological, and political, to have played their part in fostering this view.

#### *4.5 Rewriting the history of TGG*

Parallel to the "eclipsing stance" (Voegelin & Voegelin 1963: 12) that Chomsky and his associates had adopted fairly early in the development of TGG, various efforts were made from the beginning of the 1960s onwards to

rewrite the history of North American linguistics. Attempts by others (e.g., Hymes & Fought 1981[1975]: 154–157) to redress the one-sided picture were “categorically rejected” (Newmeyer 1980: 5n.4). Such an attitude, which refuses to read primary sources — and interpretations thereof — in an unbiased manner, cannot result in a proper historical account. What this leads to may be illustrated by two examples from Newmeyer’s manner of presentation, although many other such instances could be cited.<sup>24</sup>

On p.46 of his 1980 book, Newmeyer states that Hockett, in his 1964 LSA Presidential address (Hockett 1965: 185), “actually characterized the publication of *Syntactic Structures* as one of ‘only four major breakthroughs’ in the history of modern linguistics”. It is clear that at the time Hockett, aware of a possible rift separating the old and the young, was making friendly overtures towards Chomsky and his followers. Nevertheless, in the opening paragraph to his address, Hockett does not exactly say what Newmeyer is claiming he said; rather, when he comes to talking about what he terms ‘the accountability hypothesis’, Hockett in fact states the following (p.196):

We are currently [i.e., in 1964] living in the period of what I believe is our fourth major breakthrough; it is therefore difficult to see the forest for the trees, and requires a measure of derethesis on my part to say anything not wholly vague. Instead of a long list of names, I shall venture only the two of which I am sure; and since the two are rarely linked I shall carefully put them almost a sentence apart. I mean Noam Chomsky on the one hand and, on the other, Sydney M. Lamb. The order is intentional: Chomsky is unquestionably the prime mover.

No doubt this statement is much more measured than what Newmeyer would like us to believe; indeed, Sydney Lamb is not mentioned only in passing in Hockett’s paper but is referred to several times thereafter in conjunction with Chomsky and Halle’s (morpho)phonology (cf. Hockett 1965: 200). Newmeyer’s affirmation quoted earlier may simply have been the result of a young writer’s impatience with the judicious observation of an intellectual. However, when one finds several more such extrapolations of the statements of others that tend to say more than what was actually said, one is no longer sure whether Newmeyer’s accounts are indeed to be relied on. To cite yet another example from his 1980 book. When he begins talking about the ‘Chomskyan Revolution’, Newmeyer, after having highlighted the importance of Lees’ ‘review’ of Chomsky (1957), seeks further support for his

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<sup>24</sup> Cf. the exchange between Newmeyer and his reviewer, Stephen Murray, in *Historiographia Linguistica* 9.185-186 and 187 (1982) for additional examples, and also what I say in section 4.4 (above).

view that a revolution in linguistics had taken place at that time by referring to a statement made by a scholar of the older generation, Charles ('Carl') Frederick Voegelin (1906–1986), a former pupil of Albert Louis Kroeber (1876–1960) and also of Edward Sapir (not Bloomfield) and actually a good friend of Zellig Harris. Newmeyer writes (p.19):

And C. F. Voegelin (1958), in another review, noted that even if *Syntactic Structures* managed to accomplish only part of its goals, "it will have accomplished a Copernican revolution [p.229]."

Unfortunately it is impossible to reproduce Voegelin's argument in full, something which would be desirable in a detailed history of TGG, but I shall cite at least two passages from his two-page review, one from which Newmeyer has lifted the phrase he cites, another giving quite a different interpretation of Chomsky's accomplishments.

Having stated that "immediately after reading Chomsky" he "had formed a rather strong positive impression, and developed an equally strong negative bias", Voegelin (1958: 230) noted on 'the negative side',

I would not accept the strategy of criticism adopted by Chomsky and his explicator [i.e., Robert Lees in his 'review' of *Syntactic Structures*: EFKK] — putting the burden of justification on anyone who would maintain the validity of pre-transform grammar. Some would (almost) accept this; thus, one of my western friends says that Chomsky (almost) convinced him that morphemics was a poor old dead dog. And if transform grammar also persuades linguists to relegate phonemics to a preliminary stage of analysis (called 'discovery'), and to operate in final analysis (called 'description') exclusively with morphophonemics, it will have accomplished a Copernican revolution.

I submit that this sounds quite different from the interpretation that Newmeyer tries to give. As we know, Chomsky had moved from morphophonemics<sup>25</sup> (Chomsky 1951) to syntax by 1955 at the latest. Moreover, it is clear for Voegelin that Zellig Harris was the inventor of this approach and that the "application of the principle of transformation to grammar" was "certainly not new" (Voegelin 1958: 230n.1). Finally, Voegelin replies to his own rhetorical question "Will they [i.e., Chomsky, Lees, and perhaps others] start a Copernican revolution within linguistics?" with the following footnote:

A palace revolution, perhaps, in contrast to the interdisciplinary revolutions plotted by David Bidney, *Six Copernican Revolutions, Explorations I: Studies in Culture and Communication* pp. 6–14 (1953). (Voegelin 1958: 230n.2)

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<sup>25</sup> On this subject, and the manner in which Chomsky and Halle have engaged in rewriting the history, see now Koerner (2003).

Little needs to be added to suggest that Newmeyer's quotations are at best unreliable and at worst say virtually the opposite of what the authors have said. Voegelin's reference to a 'palace revolution', however, gets us back to our theme, namely, 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. (See Newmeyer's later [1986a: 9–10n.11] defense of his 'selective' interpretation of Voegelin's review.)

We have already mentioned Noam Chomsky's reiterated claim that he had not been understood by his older colleagues during the 1950s. The suggestion not to be lost on his audience of course is that a kind of Kuhnian phenomenon of incommensurability of theoretical views about language existed in American linguistics which ultimately had to lead to a 'scientific revolution'. We have already referred to Chomsky's repeated, though less than 'candid', remarks about the lack of publication possibilities for his 'radical' views of linguistic theory — note that he did not make any of his political views known to the public before 1966 (cf. Koerner & Tajima 1986: 91), i.e., after *Aspects* (1965) and *The Sound Pattern of English*<sup>26</sup> had in fact been written.<sup>27</sup>

Earlier in this paper, I referred to Chomsky's attempts (from 1962 onwards) to rewrite the history of TGG by claiming, for one thing, 'Cartesian' ancestry for his theory of language. In regard to this let me cite just one such example. The absence of "any discussion of mentalism in Syntactic Structures" was pointed out to Chomsky by interviewers in 1979, but — as the published transcripts indicate, Chomsky made no reply except for a reference to the 'MIT-context' and the purpose of the book (i.e., to serve as teaching material for an undergraduate course at M.I.T.) which, one supposes Chomsky felt, sufficed to explain the omission (see Chomsky 1982: 63). However, it appears from other sources that statements concerning the mentalism idea — touched upon in his attack on Skinner (Chomsky 1959) — were played up

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<sup>26</sup> This work, though published only in 1968, had been available in typescript form by 1964, two years after Halle (1962) had 'opened up the field' for the inclusion of phonology in TGG. It is not quite correct to say, as Newmeyer (1980: 40) does, perhaps in hindsight, that Halle's *The Sound Pattern of Russian*, published in 1959, though largely derived from his dissertation completed under Jakobson's supervision in 1955, constitutes the "first major work of generative phonology".

<sup>27</sup> A recent selection of Chomsky's political writings contains only a few newspaper articles dating from the late 1960s (see Chomsky 1980).

only from the early 1960s onwards (cf. Katz 1964).<sup>28</sup> Yet Chomsky, intent on rewriting his intellectual development, does not want to have others see things this way. Thus Iain Boal, a linguist (who taught the history of science at Harvard and was later working for California University Press), comparing the 1975 printed version of *LSLT* with the 1955 manuscript, in which he found “no claims about making grammars psychologically valid”,<sup>29</sup> noted the following:

Indeed, in the original mimeograph he [= Chomsky] said that “the introduction of dispositions (or mentalistic terms) [e.g., mind, belief, meaning – IAB] is either irrelevant or trivializes the theory”, and he ruled out all talk of mind for “its obscurity and general uselessness in linguistic theory”. In the version published in 1975, these passages are expunged and he writes that the “psychological analogue” (i.e., the radical idea that a grammar models knowledge that is actually incorporated in our heads) “is not discussed but it lay in the background of my thinking. To raise this issue seemed to me, at the time, too audacious.” This has brought from an old colleague of Chomsky the wry comment that “it is hard not to be skeptical about Chomsky’s claim that timidity prevented a thought of his from becoming known.” (Boal 1984: 15)

There is no doubt in my mind that a careful comparison of the 1975 publication of *LSLT* with the original typescript would yield many such instances where Chomsky has revised his intellectual past. (I have already mentioned the deletion of all references to Hjelmslev’s *Prolegomena*— the English translation of which had appeared early in 1953 — where there are many metalinguistic considerations that we find discussed in Chomsky’s work from 1955 onwards, and one would expect to discover other such instances of deletion as well as revision in of earlier positions in *LSLT*.) However, writers of partisan histories of TGG, of which Newmeyer’s *Linguistic Theory in America* of 1980 is the most successful example, tends to rely on Chomsky’s personal depiction of the origins and development of TGG as if these accounts could be taken at face value without further corroboration. On other occasions, Newmeyer treats his sources much more selectively,<sup>30</sup> and

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<sup>28</sup> On this issue, compare Steinberg (1999), which is a devastating review of Chomsky’s theories and their applications from the point of view of a linguistic psychologist. He shows first that Chomsky was an anti-mentalist formalist before 1959, and that when he adopted mentalism in 1965, his grammars were useless for psycholinguistic purposes because they are centered on syntax rather than semantics.

<sup>29</sup> For a thorough analysis of Chomsky’s transition from a fervently formalist and anti-mentalist stance during the 1950s to his thorough-going mentalist advocacy in *Aspects*, see Steinberg (1999).

sions, Newmeyer treats his sources much more selectively,<sup>30</sup> and presents one particular line of thought in American linguistics as if it reflected the entire development of the discipline. For him the paradigmatic nature of *Syntactic*

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<sup>30</sup> In his review of Newmeyer (1980), Fought (1982: 317) noted that Newmeyer's treatment of Zellig Harris' role in the development of TGG was insufficient and faulty. It is true that Newmeyer, quite in line with his attempt to emphasize the 'revolutionary' nature of Chomsky's proposals, virtually eliminates the question of Harris' influence on Chomsky, suggesting instead that Chomsky did just what his teacher tried to persuade him not to do. Typically, we would search in vain in Newmeyer for references to documents that could weaken the image of TGG as the theory that was 'winning over' (Newmeyer's term) the brightest linguists of the 'revolutionary' period. I am referring to the 1962 debate on "The advantages and disadvantages of transformation grammar" held in the framework of the 13th Annual Round Table Meeting at Georgetown University, Washington, D.C., and published in the following year (Woodworth & DiPietro 1963: 3-50) as just one example. The discussion was chaired by Eric P. Hamp; Paul M. Postal was the main speaker. (Postal, although officially enrolled at Yale for his doctorate, actually worked at MIT's Laboratory of Electronics at the time, and had served as a crusader for TGG since 1961, especially at the LSA summer and winter meetings.)

Anyone reading the 48-page proceedings of the debate will understand why Newmeyer has conveniently overlooked this important piece of historical evidence. To be sure, this encounter does not show TGG winning in the way that Newmeyer depicts the march of the revolution in linguistics: On every theoretical point or claim made by Postal at the symposium, he was very effectively knocked down by Paul Garvin – a scholar whose career could be said to never have quite come off, possibly, if not probably, because he saw too early the flaws of transformational theory and could not be won over to the TGG camp (like Sol Saporta or Robert Stockwell). It is probably not surprising that Garvin's name does not appear even once in Newmeyer's 250-page account of American linguistics.

From the exchange between Postal and Garvin, let me present just one excerpt to illustrate how far transformationalists may go if pressed for explanations. Postal has just outlined what a generative grammar could do in the analysis of sentences of a given language, when Garvin states his objections (Woodworth & DiPietro 1963: 36-37):

*MR. GARVIN:* I would disagree for one very serious reason. One way of verifying the validity of a theory is by writing a recognition routine based on this allegedly correct, and allegedly *only* correct grammar, and then by seeing whether it indeed does "recognize". I deliberately mentioned the Washington Post and Times Herald, because to a large number of speakers of English, it contains grammatical sentences.

*MR. POSTAL:* Most of the sentences would not be sentences at all.

*MR. GARVIN:* What a preposterous claim! On behalf of the *Washington Post* I protest! This is a very common brand of English.

*MR. POSTAL:* I would say it is a very common brand of non-English, that is, not complete English sentences.

*MR. GARVIN:* Then, of course, you are in the marvellous position where whenever you can't analyze something you simply say, "this is not English."

Observers of the linguistic scene of the 1960s and early 1970s will no doubt remember the debate over 'grammaticality' (cf. Hill's early critique of 1961, and Chomsky's aggressive rebuttal of the same year) and related notions, and realize that Garvin's hunches were correct.

*Structures* remains in force: “A truly alternative theory with any credibility has yet to emerge” (p.20).

A historian of linguistics, however, knows that although certain hints may be found (usually in hindsight) in the early works of a scholar or scientist who is important in a field, it is usually a later work that becomes to be regarded as paradigmatic for subsequent research. We might mention, for example, Bopp’s *Conjugationssystem* of 1816, which traditional histories of linguistics regard as the beginning of comparative linguistics (as if Schlegel’s work of 1808 had not mapped out the field in which Bopp and others were to harvest thereafter); however, it was Bopp’s *Vergleichende Grammatik* appearing in successive volumes from 1833 onwards which provided the framework for the subsequent generation of comparative-historical linguists. Similarly, it was with his *Compendium der vergleichenden Grammatik* of 1861–1862 (4th ed., 1876), not with his earlier books, that Schleicher’s work became the point of reference for linguistic research of much of the next two and more decades (cf. Koerner 1982). In the case of Saussure, the situation is somewhat more complicated because the *Cours* was published posthumously and did not have the author’s imprimatur.<sup>31</sup> In addition, a number of factors external (but also internal) to linguistics delayed the impact of his synchronic theory of language.

From these observations it is not surprising that the ‘revolution’ in ‘modern’ linguistics should be associated with Chomsky’s later synthesis rather than with his early writings. In this connection, I may refer James McCawley’s opinion. In his view (note that McCawley takes Kuhn’s morphology of scientific revolutions for granted), it was *Aspects of the Theory of Syntax* (1965) rather than *Syntactic Structures* (1957) that provided the basis for a ‘revolution’, for several reasons: (1) *Aspects* “brought semantics out of the closet” (McCawley 1976b: 6), which “increased the inherent interest in doing transformational syntax, as well as making it relatively easy to come up with analyses that stood a chance of being right” (p.7); (2) its ‘greater systematicity’ made the theory more appealing and “relatively easy to determine what the grosser implications of a given analysis were” (pp.7–8), and (3) the separation of syntactic category from ‘various factors that affect what co-occurs with what’ (p.7) made it “relatively easy to formulate transformational analy-

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<sup>31</sup> Interestingly enough, Calvert Watkins told me that in his view scholars who do not fully grasp the significance of Saussure’s *Mémoire* of 1878 are unable to understand the meaning of his *Cours* either. See his paper, “Remarques sur la méthode de Ferdinand de Saussure comparatiste”, *Cahiers Ferdinand de Saussure* 32.59-68 (1978).

ses in general terms without any loss of precision, and to start dealing seriously with syntactic universals” (p.8).

McCawley had Kuhn’s idea of a ‘scientific paradigm’ in mind when he formulated his views on the status of *Aspects*, especially Kuhn’s (1970: 10) suggestion concerning the relative open-endedness of those ‘paradigmatic’ works which “leave all sorts of problems for the redefined group of practitioners to resolve”. In other words, if we are going to talk about something resembling a revolution in syntax during the past thirty or more years, it should be associated with Chomsky’s work of the 1960s, and in particular with the introduction of the concept of ‘deep structure’ and associated notions, which were absent from his earlier writings, i.e., with *Aspects* rather than with *Syntactic Structures*, despite the impression that Chomsky and his associates have tried to create, and which at times succeeded in impressing on certain post-Bloomfieldians of the earlier 1960s. As we may gather from the history of the neogrammarian school (cf. Koerner 1981), the propaganda distributed by adherents of a particular view of linguistic theory and the impression it produces on the minds of many of their contemporaries is one thing; the actual story of how it really was — “wie es eigentlich gewesen” (Ranke 1824: vi) — is quite another.

##### ***5. Further aspects of a historiography of American linguistics***

The preceding discussion suggests that we are still far removed from an adequate history of linguistics in North America for the past fifty years or so, in particular where the sources and the development of transformational-generative grammar are concerned. An effort has been made to identify several issues which need to be clarified and areas which ought to be investigated more closely. In my opinion, the task is not an easy one for a number of reasons, including that of the vested interests of what has been called ‘institutional linguistics’ in holding the camp together and in fighting off ‘heresies’ as well as ‘counter-revolutions’ (cf. Newmeyer’s [1980: 167ff.] account of the ‘collapse of generative semantics’). But there are basic problems of scholarship as well, including that of outlining an exact work chronology — which in a history of TGG is of vital importance if an accurate picture of the ongoing theoretical discussion is to emerge — which Newmeyer, perhaps for reasons of convenience, choose to ignore. Anyone even the least superficially familiar with TGG and the behaviour of generative grammarians knows, among other things, that many of their products circulate only among members of the ‘in’-group, with a number of papers never being printed or published only many years later, by which time many positions therein de-

fended have long been discretely abandoned (cf. Grunig's [1982: 290] account of this traditional strategy.)<sup>32</sup> However, Newmeyer (1980: xii–xiii), for his part, announces: “Throughout the text, I cite books and articles by the year of their first publication, not by the year that they were written.” For example, McCawley's (1976b) edition of a significant number of papers dating from between 1960 and 1967, published under the title of *Notes from the Linguistic Underground* is tucked away in Newmeyer's bibliography (1980: 268) under the innocuous series title *Syntax and Semantics*, vol.7; besides, there is no indication that any of the papers published therein has actually been used in Newmeyer's account of the history of TGG.

The situation is quite frustrating for the historiographer of linguistics trying to establish what really happened in order to present an adequate picture of the history of linguistics in North America during the past forty or more years. Polemics, even if written in masterly manner with the insight and humour that Maher (1982) achieves, proves ineffective. Those who believe Maher is right do not belong to the TGG camp, and those who do belong to it, stonewall his challenge: they will not read his (or anyone else's) work (unless it subscribes to the basic tenets of TGG); there is a general agreement among them to keep silent about such non-TGG work, and students are asked by their teachers to ignore it. Polemic exchanges, it appears, are valuable only when both sides are in search of truth, but there are few signs that those who associate themselves with the ‘Chomskyan Revolution’ are in any non-trivial way interested in that. Newmeyer isn't, and Chomsky and his associates have consistently shown themselves to only want to win the fight, and in such a manner that no rematch will take place.<sup>33</sup>

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<sup>32</sup> That this technique of referring to either still unpublished or not readily accessible papers and dissertations (so well displayed in Chomsky's *Syntactic Structures*) in support of one's particular theory or claim is still practiced among members of the TGG camp, I witnessed myself in Spring 1982, when a doctoral student from M.I.T. gave a paper at the University of Ottawa. (Indeed, a similar event took place here as recently as November 1987 on the occasion of another paper given by an M.I.T. Ph.D.) – For just one example from a printed source, the reader may refer to *Linguistic Theory and Natural Language* 6.128 (1988), where altogether 14 references can be found, of which 7 are to unpublished writings (mostly MIT dissertations) and an eighth – by the author of the paper – to a forthcoming article.

<sup>33</sup> As a typical example of the tactics employed by Chomsky's associates, one can refer to the well-documented exchange between Uriel Weinreich and Jerrold J. Katz. The latter incorporated many corrections to faults in his theory to which Weinreich had alerted him in his criticism, pretending that they had been his own initiatives. Cf. Katz's “Recent issues in semantic theory”, *Foundations of Language* 3.124–194, and Weinreich's brief response, in which he expressed his astonishment about such a procedure, “On arguing with Mr. Katz”.

### 5.1 Organizational linguistics in the U.S.A.

Something should be said about what is referred to as ‘organizational linguistics’, i.e., the influence on, if not control over, access to publication outlets and research funding for example. It appears that early on in the generativist movement leaders saw to it that this kind of support was forthcoming. How else could it be explained, for instance, that within less than a year of the publication of Newmeyer’s 1980 book, a glowing review appears in *Language* (the journal with the widest circulation of all linguistics periodicals in the world, no less). The review was written by Donna Jo Napoli, who, like Newmeyer, was serving as an associate editor of *Language* at the time, by the way. Napoli sees a particular benefit of *Linguistics in America* in that “the structuralist [!] who stopped reading generative work sometime soon after Chomsky’s *Aspects* can [now] follow more recent developments” (Napoli 1981: 456).<sup>34</sup> No doubt the question of ‘The Politics of Linguistics’ needs to be addressed; but in a manner much different from Newmeyer’s recent book by that title (Newmeyer 1986b; cf. Murray 1989). In that book no attempt is made to lay bare the operation of social networks in the manner of, for instance, Murray (1983). Newmeyer instead published a paper defending the ‘Chomskyan Revolution in Linguistics’ (Newmeyer 1986a), where he argued that it occurred ‘sociologically’ and ‘intellectually’, while at the same time denying that there was any ‘power grab’ (p.9) on the part of the TGG school, unexpectedly claiming that “their influence [in American linguistics] is disproportionately small” (p.12). In a footnote (p.12n.14) Newmeyer acknowledges that “Paul Chapin, the National Science Foundation Director for linguistics, has a doctorate from MIT”, but that the “1983 advisory panel contained only one generativist”. What he does not mention is the important fact that Chapin — Chomsky’s seventh Ph.D. student (cf. Koerner & Tajima 1986: 196) — was the first incumbent in this position, which was established

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ibid., 284-287 (1967). – For Chomsky’s manner of conduct when challenged, see Koerner 2002, chapt. 6.

<sup>34</sup> To select just a few further statements from the review: “This book is astounding for its information, intelligence and insight” (p.456); “[...] the greatest value of *LTA* [= Newmeyer 1980] lies not so much in the material it covers, but how it covers that material” (p.457), “This is a major contribution to our knowledge of the history of linguistic theory [as if there was only one on the market of ideas]” (p.459). Where one can agree with the reviewer is when she states that the book is devoted to the history of “syntactic theory” — of a particular kind, of course — not of the history of American linguistics generally (p.456).

## LINGUISTICS AND REVOLUTION

on 31 October 1975,<sup>35</sup> and that he held on to it for about 25 years, retiring only in 1999, though still associated with the National Science Foundation (NSF) until early 2001 in another capacity.<sup>36</sup> While no suggestion is made that Chapin may not have acted properly in his position, it is only natural to assume that he would have looked favourably upon grant applications from persons with generativist credentials. The fact remains that, of the many millions of dollars distributed by the agency's Linguistics Program, M.I.T. and its associate institutions have received — and I am referring to the 1960s and 1970s especially — a considerable, and at times a rather disproportionate amount (as may be gathered from the NSF's annual reports).<sup>37</sup>

Another important aspect not mentioned by Newmeyer in his 1986 paper on the 'Chomskyan Revolution' is the fact that Chomsky's first (official) doctoral student, D. Terence Langendoen, served as Secretary-Treasurer of the Linguistic Society of America for a five-year term (1984–1989),<sup>38</sup> and that he had been preceded by Victoria A. Fromkin (from 1979), who can surely be included in the TGG camp, too. If indeed we were to accept New-

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<sup>35</sup> Prior to this date the Special Projects Program in NSF's Division of Social Sciences would have processed grant applications; from summer 1973 onwards, Alan E. Bell of the University of Colorado's linguistics department served as staff associate to handle these requests.

<sup>36</sup> In his detailed e-mail to the author of 15 January 2002, Dr Chapin kindly provided me with these (and other) details; his last NSF position was that of Senior Program Director for Cross-Disciplinary Initiatives.

<sup>37</sup> I recall that, since I had been asked by Dr Bell in that year to serve as one of the referees for the project of the LSA to organize a Third Golden Anniversary Symposium in 1974, this time devoted to 'The European Background of American Linguistics' (cf. Koerner 2002, chapt. 1, for details), I received the annual report issued by the NSF Linguistics Program early in 1974. From it I could gather that while the major scholars of the day (like Charles Ferguson of Stanford for the Phonological Archive) received a grant of \$30,000 or \$40,000, none other than Morris Halle of MIT received an amount of many times that much, \$120,000 or more, for a project entitled "The study of language". It would be interesting to check all these annual reports in order to obtain an idea of how heavily TGG-type research proposals were funded. (Paul Chapin, in his e-mail to the author of 17 Jan. 2002, promised "the next time I have occasion to go through the boxes in my storeroom, I'll keep an eye out for the lists, and will let you know promptly if I find them", but had not yet done so by late May 2002.)

<sup>38</sup> It may seem ironic to some that no other than Newmeyer should have been chosen at the LSA December 1988 meeting to replace Langendoen who resigned from this position following his acceptance of a position at the University of Arizona in 1988.

meyer's claim that there were "many major universities [...] dominated by non-generativists" [1986a: 12], suggesting at the same time that the number of generativists at the time were fairly small, one cannot fail to notice that they are disproportionately overrepresented in the important LSA committees. For instance — as may be gathered from the *LSA Bulletin* No.117 of October 1987 — the Nominating Committee proposed two candidates for the 1988–1990 Executive Committee, one an M.I.T. Ph.D., the other a distinguished generativist, with a third candidate, who did his doctorate at M.I.T. in 1976, being nominated by more than ten LSA members.

Unlike the LSA president (note, for instance, that Chomsky's third doctoral student, Barbara Hall Partee, was president in 1986, preceded by Victoria Fromkin in 1985, and followed by Elizabeth Traugott, also an early associate of the TGG school, in 1987), who usually does not exercise much influence during his/her one-year tenure,<sup>39</sup> the Secretary-Treasurer, who is an ex officio member of most of the important committees (e.g., those distributing travel grants, fellowships, delegate positions), plays an important role in American linguistics. Besides, we should not forget that the LSA is by far the largest professional organization of linguists in the world. But 'organizational linguistics', i.e., the power and influence exercised by people who, whenever an associate of the 'TGG paradigm' is criticized, rush to his/her defence, does not stop there. It would be interesting to find out how many other linguistics organizations that deal with fellowships, decide on visiting appointments and the like are effectively controlled by people who at least in a broad sense belong to this generativist movement. Likewise, one would like to know how many of them are in positions of political power in the universities as chairmen, deans, etc. Besides, if there was no 'power grab', how could anyone claim that a 'revolution' took place? Yet this is just another aspect (though probably a very crucial one) that requires thorough investigation.

### *5.2 Effective access to and control over linguistics journals*

I have already mentioned the subject of access to publication as an important part of organizational linguistics. Following the death of Bernard Bloch (who we have already seen as very sympathetic to and supportive of Noam Chomsky) in 1965, William Bright, an anthropological linguist at the Uni-

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<sup>39</sup> Although it should not be forgotten that subsequent to their tenure former presidents often sit on important LSA committees and are called upon by the administration to serve as (informal) advisors. The 2002 LSA presidency went to Frederick J. Newmeyer; the one for 2003 has gone to Ray Jackendoff.

versity of California in Los Angeles (UCLA), was selected as editor of *Language*, largely as a result of Robert Stockwell's recommendation who since 1961 had been busy building a fledgling linguistics program into strong TGG department with clearly generativist agenda (cf. Hill 1991: 128 and the note by Martin Joos; Stockwell 1998: 236–239). Bright, who served as the editor of *Language* for some twenty years (1966–1987), was by no means an adherent of TGG, but he soon moved from the Anthropology Department at UCLA to its Linguistics Department (which probably was the biggest such department in the US at the time) and he obviously was amenable to this school.<sup>40</sup> Bright's successor until 1996, Sarah Gray Thomason, likewise was by no stretch of the term a follower of Chomskyan linguistics, but the evidence shows that she bent over backwards to accommodate the work of linguists of this persuasion. The turn toward generativist linguistics became more obvious during the five-year tenure (1997–2001) of Mark Aronoff (PhD, MIT, 1976) as editor of *Language*.

However, focussing on *Language* gives a distorted picture of the North American scene as far as publishing papers in linguistics journals was concerned, as I had to realize when I was trying to place a paper which I eventually published in Europe (Koerner 1983), because of the hold that TGG held over the most important outlets, *Linguistic Inquiry* (launched at MIT in 1970), *Linguistic Analysis*, *Linguistics and Philosophy*, and other periodicals. Not unlike the Neogrammarians during the 1870s, who either started new

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<sup>40</sup> As an example of this I may refer to a personal experience. In summer 1982, I submitted what was to become Koerner (1983) to Bright for possible publication in *Language*. It was largely a critique of the manner in which Newmeyer (1980) had depicted the history of American linguistics. I recall that at the International Congress of Linguists held in Tokyo that year, I announced that I had submitted a paper on the subject of the 'Chomskyan Revolution' to *Language*, but that I expected it to be rejected. Subsequent to this announcement, I was stopped in the corridor by Victoria Fromkin of UCLA's linguistics department who assured me that Bright would give it a fair treatment. Bright chose three referees, Charles Hockett (who had previously encouraged me to send the paper to *Language*), Dell Hymes (who complained that I had not sufficiently considered his work on the subject), and none other than Frederick Newmeyer (whose scholarship I had questioned). Essentially on the advice of the latter the paper was rejected. But the story does not end here. Several years later Newmeyer was given the opportunity to respond to my paper in *Language*, although it had not appeared there. (That it was a reply to Murray's 1980 paper [as Newmeyer (1986b: 159n.18) tries to make his readers believe] can be easily disproved by simply counting the frequent references to my 1983 article in his 1986 paper.) That he should refer in the same paper to the editor of *Language* as being "scrupulously fair in his handling of submissions to the journal", adding that he knows "from personal experience that he [William Bright] is a model of impartiality" (Newmeyer 1986a: 14n.17), strains credulity.

journals (*Beiträge zur Geschichte der deutschen Sprache* in 1874 and *Morphologische Untersuchungen* in 1878) or redefined the goals of established ones (like *Zeitschrift für vergleichende Sprachforschung* in 1876), once they had gained editorial control over them, linguists at MIT and those allied with them did much the same thing, adding *Natural Language and Linguistic Theory* (which was launched at MIT in 1983) to their arsenal.

## 6. Concluding observations

Returning to observations made by Stephen Murray and John Joseph at the outset of this paper, we may attempt a kind of résumé. If we accept Murray's (1994: 22–23) 'three factors' defining scientific groups which ultimately decide who drives the agenda — good ideas, intellectual leadership, and organizational leadership — one cannot deny that TGG, from the late 1960s onwards, and more clearly during the 1970s, could lay claim to all three: Chomsky's ideas, notably from *Aspects* onwards, provided what could be called the 'good ideas'; together with Morris Halle, he provided 'intellectual leadership', and one could say that Halle provided 'organizational leadership', at least beginning with the preparations for the 1962 International Congress. If indeed, if "[m]ost revolutions are essentially rhetorical, with the substantive change being one of personnel — who is in charge of the government, who defines the mainstream", as Joseph (1995: 384n.5) has it, we would have come to the conclusion that there was a 'Chomskyan Revolution'.

That this revolution did not occur overnight, and that it took about a decade after the publication of *Syntactic Structures* to carry the day, may be gathered from the fact that even in departments with a fairly strong generative bias like UCLA, we could have witnessed the following canon of post-Bloomfieldian literature to be required reading:

Prior to the mid-1960s, the typical MA student, [...], was required to have a "theoretical" background based on Joos's (1958[recte: 1957]) *Readings in Linguistics*, including Bloomfield's (1939) *Linguistic Aspects of Science* and Bloch's (1948) *Postulates*. A major topic in seminars concerned "item and arrangement": vs. "item and process" [Hockett 1954] analysis. Bloomfield's (1933) *Language* and Hockett's (1958) *A Course in Modern Linguistics* were the texts for the prerequisite courses for graduate study. (Fromkin 1991: 78)<sup>41</sup>

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<sup>41</sup> When I entered graduate school at Simon Fraser University in Vancouver in September 1968, Fromkin's depiction of the required readings still applied, together with writings by Sapir, despite the fact that several staff members espoused strong TGG persuasions.

## LINGUISTICS AND REVOLUTION

It is not only because of this, but also because of the documented evidence provided in this paper (and also in Koerner 2002, chapt. 9) of the indebtedness of Chomsky to his predecessors that I have tended to argue in favour of ‘evolution’ rather than ‘revolution’ when referring to the changes that occurred in American linguistics during the 1960s and 1970s.

But perhaps we should give Noam Chomsky the last word. As far as I know, he never claimed himself to have produced a revolution in linguistics, at least not in his writings or interviews during the 1960s through 1980s that I am aware of, although he may not have objected to others attributing to him having caused one. This appears to have changed during the 1990s. Whereas in a 1994 interview with the editors of *Linguistische Berichte*, he merely hinted that his Government & Binding (GB), also referred to as Principles & Parameters (P&P) theory, constituted an important departure from the earlier frameworks he had proposed (Chomsky 1994), he came out much more strongly in an interview he gave in Brazil in November 1996. There he said about the GB theory first outlined in Chomsky (1981):

It was the first genuine theory of language that had ever been produced in 2500 years because it showed how you could, in principle and to some extent even in practice, overcome the conflict between descriptive and explanatory adequacy. (Chomsky 1997: 169–170)

Chomsky (p.171) added in all seriousness (and as if to echo almost verbatim Newmeyer [1980: 250] concluding statement): “Probably more was learned about language in the 1980s than in the entire preceding 2500 years.”

While Chomsky did not use the term, he surely meant to say that the GB/P&P approach did produce a revolution, in fact one of staggering proportions. It then must seem at least ironic, if such an insightful framework for the analysis and understanding of language should become obsolete after a shelf life of only a decade. As Chomsky explains further to his interviewers (1997: 171):

That brings us to the Minimalist Program [Chomsky 1992, 1995], which is an attempt to try to show that these great successes [of BG/P&P] are based on sand. That is, they are based on descriptive technology that works but is wrong because it is unmotivated and should be taken apart.

Seen in the light of these pronouncements, one cannot but agree with Joseph (1995: 380), when he spoke of “Noam Chomsky, Serial Revolutionary”.<sup>42</sup>

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<sup>42</sup> It is interesting that the Berkeley philosopher John R. Searle (b.1932), who in 1972 hailed “The Chomskyan Revolution”, now speaks of the “End of the Revolution” thirty years later in his review of Chomsky (2000). There Searle (2002: 33) writes:

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After such a long time it would seem appropriate to assess the results of the revolution. This article is not by itself such an assessment, because to do an adequate job one would require more knowledge of what happened in linguistics in these years than I have, and certainly more than is exhibited by Chomsky's new book. But this much at least we can say. Judged by the objectives stated in the original manifestoes, the revolution has not succeeded. Something else may have succeeded, or may eventually succeed, but the goals of the original revolution have been altered and in a sense abandoned. I think Chomsky would say that this shows not a failure of the original project but a redefinition of its goals in ways dictated by new discoveries, and that such redefinitions are typical of ongoing scientific research projects.

It is telling to see Chomsky's colleague at MIT, Sylvain Bromberger (b.1924), rushing to Chomsky's defense in a letter to the editor of *The New York Review of Books* 49: 7.60 (25 April 2002) characterizing Searle's review as "seriously misleading" and claiming, in the face of the evidence provided by Chomsky himself, that "None of these 'revolutionary' conjectures have been abandoned by Chomsky or by those who work within the framework he created". See also Searle's reply in the same issue (pp.60–61).

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