American Linguistics in Transition

From Post-Bloomfieldian Structuralism to Generative Grammar

FREDERICK J. NEWMEYER
Contents

Foreword xi
List of Abbreviations xv
Timeline xvi

1. The structuralist ascendancy in American linguistics 1
   1.1 Introduction 1
   1.2 American linguistics in the mid-twentieth century 2
   1.3 The Linguistic Society of America 6
      1.3.1 The founding of the Society 6
      1.3.2 Linguistics, philology, and their relationship to science 11
      1.3.3 The leadership and composition of the early LSA 12
   1.4 The changes in the field from the 1920s to the 1940s 17
      1.4.1 The LSA Executive Committee in 1936 and in 1946 18
      1.4.2 The changing contents of the first quarter-century of Language 20
   1.5 Some reasons for the dramatic changes in American linguistics from the 1920s to the 1950s 21
      1.5.1 The sense of a distinctive field making rapid progress 21
      1.5.2 The leading American linguists' commitment to the equality of all languages and their analysis 24
      1.5.3 The LSAs summer Linguistic Institutes 27
      1.5.4 American linguists' involvement in World War II 32
      1.5.5 The change of leadership in the LSA in 1940–1941 35
   1.6 Summary 42

2. American structuralism and European structuralism 43
   2.1 Introduction 43
   2.2 The early American linguists' debt to Europe 44
      2.2.1 The European background of many American linguists 44
      2.2.2 American linguists and the early Prague School 45
      2.2.3 American linguists and the early Geneva School 46
   2.3 The American structuralists' turn away from Europe 53
      2.3.1 The American structuralists' view of science and its consequences 53
      2.3.2 The Second World War and the two-dollar bill conspiracy 58
   2.4 European views of American linguistics 60
   2.5 The American rediscovery of European linguistics 65
2.5.1 Roman Jakobson, the École Libre des Hautes Études, and the founding of Word 65
2.5.2 Increasing American appreciation of European linguistics 71
2.6 The European reaction to early generative grammar 75
2.7 The Prague School influence on American functional linguistics 77
2.8 Summary 79

3. Martin Joos's Readings in Linguistics as the apogee of American structuralism 80
3.1 Introduction 80
3.2 Background to Joos's Readings in Linguistics 81
  3.2.1 Bernard Bloch's idea for a collection 81
  3.2.2 The American Council of Learned Societies 83
  3.2.3 Martin Joos 84
  3.2.4 Why Joos was chosen as editor of the Readings 86
3.3 Joos takes on the editor's role 87
  3.3.1 Joos's letter of September 12, 1955 87
  3.3.2 Reactions to Joos's letter 91
3.4 The first edition of Joos's Readings in Linguistics 96
  3.4.1 The contents of the first edition 96
  3.4.2 Joos's editorial commentary in the first edition 100
3.5 The later editions of the Readings 103
  3.5.1 The second and third editions (1958 and 1963) 104
  3.5.2 The fourth edition (1966) 105
  3.5.3 The first four editions: A summary comment 107
  3.5.4 The abridged edition (1995) 108
3.6 A visual display of the progression of the book of readings 109
3.7 The reviews of Joos's Readings in Linguistics 113
  3.7.1 The 'non-reviews' in Language and Word 114
  3.7.2 The Voegelin review in IJAL 115
  3.7.3 The Hymes review in American Anthropologist 116
  3.7.4 The Uhlenbeck review in Lingua 117
  3.7.5 The MacQueen review in Quarterly Journal of Speech 118
  3.7.6 The Trager review in Studies in Linguistics 118
  3.7.7 The Pei review in Modern Language Journal 119
  3.7.8 The Lightner review in General Linguistics 119
  3.7.9 The reviews: A summary 120
3.8 Further remarks on the Readings 121
  3.8.1 The Readings in the classroom 121
  3.8.2 On the 'staying power' of Joos's Readings 122
3.9 Conclusion 126
4. Early transformational generative grammar: Some controversial issues

4.1 Introduction

4.2 Early transformational generative grammar: A whirlwind overview

4.2.1 Syntactic Structures

4.2.2 What came before Syntactic Structures

4.2.3 Generative phonology

4.2.4 From Syntactic Structures to Aspects of the Theory of Syntax

4.3 On the question of a ‘Chomskyan revolution’

4.3.1 On scientific revolutions

4.3.2 The originality of transformational generative grammar

4.3.3 The roots of transformational generative grammar in earlier work

4.4 The mainstream reaction to early transformational generative grammar

4.4.1 The reaction to Syntactic Structures and other early syntactic studies

4.4.2 The reaction to early generative phonology

4.4.3 The reaction to Aspects of the Theory of Syntax

4.5 Chomsky in the spotlight: Examining some of his claims about his early days

4.5.1 ‘I have never heard of the alleged offers to publish LSLT’

4.5.2 ‘My MMH was written, I regret to say, in ignorance of Bloomfield’s study’

4.5.3 ‘It’s next to inconceivable, for example, that Harris looked at my Ph. D. dissertation or LSLT’

4.5.4 ‘In editing [LSLT] for publication now, I have made no attempt to bring it up to date or to introduce revisions in the light of subsequent work’

4.6 Linguistics, MIT, Chomsky, and the military

4.6.1 Linguistics and the military

4.6.2 MIT and the military

4.6.3 Chomsky and the military

4.7 Summary

5. The diffusion of generativist ideas

5.1 Introduction

5.2 The beginnings of generative grammar at MIT

5.3 Accusations of ‘private knowledge’

5.4 The early generative grammarians were committed to publication

5.5 On the so-called ‘underground literature’

5.5.1 Polished manuscripts ‘deliberately circulated privately’

5.5.2 The institutional and laboratory reports
5.5.3 The third type of ‘underground literature’ and McCawley’s edited volume 196
5.5.4 More informal publication outlets 197
5.5.5 A little historical perspective 198
5.5.6 Summary 199
5.6 Further examples of MIT outreach 200
  5.6.1 Textbooks 200
  5.6.2 LSA and other meetings 201
  5.6.3 Linguistic Institutes 202
  5.6.4 Visitors to MIT 203
  5.6.5 Generative grammarians outside of MIT 205
5.7 Some explanations for the spread of the ‘private knowledge’ idea 208
  5.7.1 The consequences of a rapidly evolving discipline 209
  5.7.2 Aggressive and insular public behavior 209
  5.7.3 MIT student mentality 211
5.8 Conclusion 215

6. The European reception of early transformational generative grammar 216
  6.1 Introduction 216
  6.2 TGG in Europe: Some general remarks 216
  6.3 TGG in Europe: A country-by-country breakdown 218
    6.3.1 Western Europe 218
    6.3.2 Nordic Europe 235
    6.3.3 Eastern Europe 242
    6.3.4 Southern Europe 253
  6.4 Concluding remarks 260

7. The contested LSA presidential election of 1970 261
  7.1 Introduction 261
  7.2 The antagonists: Martin Joos and Dwight Bolinger 262
    7.2.1 Martin Joos 262
    7.2.2 Dwight Bolinger 263
  7.3 The prelude to the 1970 election 266
    7.3.1 The LSA in early 1970 266
    7.3.2 The 1970 Nominating Committee 268
    7.3.3 Bolinger challenges Joos 270
  7.4 The internal debate over the contested election 274
  7.5 The election results and their aftermath 279
  7.6 Concluding remarks 281

8. Charles Hockett’s attempt to resign from the LSA in 1982 282
  8.1 Introduction 282
  8.2 Charles Hockett 283
  8.3 The LSA’s Fund for the Future of Linguistics 288
  8.4 Hockett takes action 289
8.5 Hockett’s perception of the field and reality 296
8.6 Concluding remarks 297

9. The generativist non-dominance of the field in the 1970s and 1980s 298
9.1 Introduction 298
9.2 Generativist predominance (or not) in LSA elected offices 303
  9.2.1 The Presidents of the LSA 303
  9.2.2 The Secretary-Treasurers of the LSA 304
  9.2.3 The Editors of Language 305
9.3 Generative predominance (or not) in the pages of Language 305
9.4 Generative predominance (or not) at LSA meetings 308
9.5 Generative predominance (or not) at LSA summer Institutes 311
9.6 Generative predominance (or not) in obtaining grants 312
  9.6.1 National Science Foundation grants 312
  9.6.2 American Council of Learned Society grants 315
  9.6.3 Guggenheim Foundation grants 315
  9.6.4 National Endowment for the Humanities grants 316
  9.6.5 Fulbright-Hayes (and other Fulbright-related) awards 316
  9.6.6 National Institute of Mental Health grants 316
  9.6.7 Ford Foundation grants 317
9.7 Generative predominance (or not) in departmental composition 317
  9.7.1 PhD programs in linguistics in 1987 317
  9.7.2 Job postings in 1987 318
9.8 Discussion 320

Appendix A: The members of the LSA Executive Committee in the 1970s and 1980s 324
Appendix B: The members of the LSA Nominating Committee in the 1970s and 1980s 326
Appendix C: Associate Editors of and example articles in Language in the 1970s and 1980s 328
  C1: The Associate Editors of Language in the 1970s and 1980s 328
Appendix D: The members of the LSA Program Committee in the 1970s and the 1980s 333
Appendix E: Grant recipients in American linguistics in the 1970s and 1980s 335
  E1: National Science Foundation, Division of Social Sciences, Grants in support of linguistics research awarded during fiscal years 1966 through 1972 335
X  CONTENTS

E5: National Institute of Mental Health grants in 1973 356

Afterword 358
References 359
Index of Names 394
Index of Subjects 409
Foreword

I am probably best known for my book *Linguistic Theory in America: The First Quarter Century of Transformational Generative Grammar* (LTA), published in 1980, with a second edition appearing six years later (Newmeyer 1980, 1986b). While the reviews and general reaction to the book were largely positive, there were two criticisms that were raised by more than one reader. First, there was a general feeling that I treated the structural linguists who preceded transformational generative grammar (TGG) with little respect and even less understanding. Second, there were objections to the ‘triumphalist’ tone that pervaded the book. A number of reviewers felt that I exaggerated both the intellectual success of the theory (as measured by the number of its adherents worldwide) and its organizational success (as measured by institutional dominance). Over forty years of reflection have led me to conclude that these critics were essentially correct. This book is in part an attempt to set the record straight, as far as these two issues are concerned. But more basically, my goal here is to explore in depth the transition from post-Bloomfieldian structuralism, which dominated the American linguistics scene from the 1930s to the 1950s, to TGG, which had largely eclipsed it by around 1970.

As far as the post-Bloomfieldian structuralists are concerned, I devote the first chapter, ‘The structuralist ascendancy in American linguistics’, to a thorough examination of the sources of their ideas and, even more importantly, to how, for a generation or more, they managed to be so successful. I argue that several factors contributed to their success: the conviction of its leading practitioners that they were doing ‘scientific linguistics’, the feeling that knowledge and understanding were growing by leaps and bounds, the ‘egalitarian’ message that all languages could be analyzed by the same methods, the Linguistic Society of America (LSA)-sponsored summer schools that helped spread knowledge of structuralist theory, the government-sponsored work in linguistics during the Second World War, and the change in the LSA leadership around 1940.

As far as my misguided triumphalism is concerned, I devote the final chapter, ‘The generativist non-dominance of the field in the 1970s and 1980s’, to documenting and attempting to explain why generative grammarians, despite the high visibility of their theory and the celebrity of Noam Chomsky, were so relatively unsuccessful in achieving organizational power in the United States during those decades (I take it for granted that the question would not even arise in most other countries). As measured by their occupation of LSA elected offices, articles in the
pages of *Language*, presentations at LSA meetings, positions at LSA Institutes, receipt of grant awards, and representation in departmental composition, generativists were quite minoritarian. Many factors combined to cause this state of affairs, including the inward-looking attitudes of many prominent generativists, the immaturity, as well as the diversity, of the field of linguistics, and Chomsky’s sense of isolation from the field.

My story begins with the founding of the LSA in 1924. It was at that point, and not earlier, that one could speak of a distinct field of linguistics in the United States. My ending point—better 'points'—might seem a bit ragged. The interplay between structuralists and generativists was still ongoing in the early 1980s, so I document an interesting event from that period with respect to that interplay. However, I ignore developments internal to TGG later than the 1960s. For one thing, I have already covered these developments in *LTA*, and for another, they have little to do with the main theme of this book, namely the transition from structuralism to generativism. The most noteworthy development from this period, the 'linguistic wars' fought between interpretive semanticists and generative semanticists, will be passed over here. These wars are treated not only in *LTA*, but also in books by Randy Harris and by Geoffrey Huck and John Goldsmith (Harris 1993, second edition 2021; Huck and Goldsmith 1994).

A major gap in *LTA* was the almost complete lack of discussion of the European influence on both American structuralism and TGG, as well as the attempts of early generativists to 'internationalize' the theory. This book attempts to fill that gap. Chapter 2, ‘American structuralism and European structuralism: How they saw each other’, describes American-European interactions during the structuralist period and later. The Americans owed a great debt to the Europeans, a debt that they tended to downplay in the 1930s and early 1940s. At the same time, the Europeans tended to be highly critical of most post-Bloomfieldian work, given its extreme empiricist foundations. But by the late 1940s, partly as a result of the presence of a large number of European refugee scholars in the United States, a rapprochement took place between the Americans and the Europeans. This rapprochement accelerated with the advent of TGG, where the results of European theorizing played a major role from the beginning. Ironically, however, European structuralists, who had become more and more admiring of the work of their American colleagues, almost uniformly rejected TGG, a fact which I attempt to explain.

Chapter 6, ‘The European reception of early transformational generative grammar’, surveys how linguists in each European country greeted the advent of TGG in the 1960s and 1970s. There is no overarching generalization—the situation differed from country to country. A partial generalization, however, is that the stronger a home-grown structuralist tradition in a country, the less likely the linguists there would be to accept TGG. So the theory made few inroads in Czechoslovakia and Denmark, given the predominance of the Prague School and the Copenhagen School respectively, but did quite well in the Netherlands and Norway, where no
indigenous approach was predominant. There are exceptions in both directions, however. The United Kingdom warmly greeted TGG, despite the importance of the structuralist model associated with J. R. Firth, and in any number of European countries, despite the absence of a structuralist presence, TGG fell upon deaf ears.

The central chapters of the book, both physically and thematically, are Chapter 4, 'Early transformational generative grammar: Some controversial issues' and Chapter 5, 'The diffusion of generativist ideas'. The former chapter treats a wide variety of topics related to the transition from post-Bloomfieldian structuralism to TGG, such as the key features that distinguished the latter from the former, the perennial question of whether we can speak properly of a 'Chomskyan revolution' in linguistics, and the reaction of leading structuralists to the advent of TGG. The chapter also delves into related issues, such as Chomsky's own remarks about his interactions with his teacher Zellig Harris and other structuralists, as well as the role that military funding played in the success of the early theory.

Chapter 5 is devoted to combatting the popular idea that Chomsky and his colleagues eschewed publication, preferring to disseminate their work in a murky 'underground press'. We will see that the early generativists used every means at their disposal at the time to diffuse their ideas: publishing books, journal articles, anthology chapters, and technical reports; aiding the writing of textbooks; giving conference talks; teaching at LSA Institutes; and hosting numerous visitors to MIT.

The remaining chapters focus on the last days of the structuralist ascendancy in American linguistics. Chapter 3, 'Martin Joos's Readings in Linguistics as the apogee of American structuralism', documents the fascinating publication history of this volume and how it came to be regarded as the 'official document' of post-Bloomfieldian structuralism. The publication date, 1957, coincided with that of Chomsky's Syntactic Structures, a curious coincidence which in the ensuing decades gave generativists an easy whipping boy to hold up as a symbol of how linguistics 'used to be done'. Chapter 7, 'The contested LSA presidential election of 1970', documents Dwight Bolinger's successful challenge to Martin Joos for the LSA presidency. Generativists saw Joos as the arch-representative of the linguistics of the past and hence sided with Bolinger, even though the latter was hardly one of their co-thinkers. And Chapter 8, 'Charles Hockett's attempt to resign from the LSA in 1982', covers an interesting topic, not particularly of value for his unsuccessful resignation attempt per se, but because it brought into play some stunning interactions between the prominent linguists of the day, whatever their theoretical orientation.

I wrestled for some time over whether to organize the chapters thematically or chronologically, and ended up opting for the latter. It would have been confusing for the reader, I think, to be presented too often with events that took place, say, in the 1960s before those that took place in the 1940s. Let me give one example of what I mean. Two chapters deal primarily with events in Europe, so at first thought it might seem natural to place them one immediately after the other. Instead, I have
opted to present Chapter 6, ‘The European reception of early transformational generative grammar’, at some distance from Chapter 2, ‘American structuralism and European structuralism: How they saw each other’. I felt a full understanding of how TGG was greeted in Europe in the 1960s and 1970s to be contingent upon an understanding of its development and reception in the United States in the 1950s and 1960s (which are treated in Chapters 4 and 5). Furthermore, following the suggestion of Margaret Thomas, I have constructed a timeline of some of the more important events discussed in the book, which I hope will further aid the reader to connect the dots between the chapters.

One unique feature of this book, if I may be permitted to say so, is its copious use of archival material. It incorporates letters and other documents from ten different archives in the United States and the Netherlands, and would have drawn from even more if COVID-19 had not put the kibosh on international travel. Luckily for posterity, the majority of prominent scholars in the mid-twentieth century saved their correspondence and ended up bequeathing it to libraries, generally in universities where they spent their careers. Little did they know that what they wrote to their colleagues would be used, and in some cases used against them, in a publication to appear three-quarters of a century later.

Literally dozens of colleagues deserve acknowledgement for their role in providing information and advice to me as I prepared this volume. My greatest debt is to Margaret Thomas, who reviewed the entire pre-final manuscript and passed on to me many pages of critical commentary, all of it quite useful. I also had the opportunity to discuss much of the material with colleagues around the world. The following stand out as particularly worth of mention in that regard: Stephen Anderson, Hans Basbøll, Noam Chomsky, B. Elan Dresher, Julia Falk, Camiel Hamans, Brian Joseph, John Joseph, D. Robert Ladd, Geoffrey Pullum, and Sarah Thomason. Many other colleagues are thanked in the appropriate chapters and sections. And special thanks to Julia Steer, my editor at Oxford University Press, for steering me (pun intended) in the right direction from the conception to the realization of this project.
List of Abbreviations

<table>
<thead>
<tr>
<th>Abbreviation</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>AHA</td>
<td>The Archibald Hill Archive, at the University of Texas, Austin Texas</td>
</tr>
<tr>
<td>BBA</td>
<td>The Bernard Bloch Archive, at Yale University, New Haven, Connecticut</td>
</tr>
<tr>
<td>CHA</td>
<td>The Charles Hockett Archive, at the Smithsonian Institution, Washington, D. C.</td>
</tr>
<tr>
<td>CVSC</td>
<td>The C. H. van Schooneveld Collection in the Leiden University Library, Leiden, Netherlands</td>
</tr>
<tr>
<td>DBA</td>
<td>The Dwight Bolinger Archive, at Stanford University, Stanford, California</td>
</tr>
<tr>
<td>FBA</td>
<td>The Franz Boas Archive, at the American Philosophical Society, Philadelphia, Pennsylvania</td>
</tr>
<tr>
<td>LSAA</td>
<td>The Linguistic Society of America Archive, at the University of Missouri, Columbia, Missouri</td>
</tr>
<tr>
<td>LSAB</td>
<td>Linguistic Society of America Bulletin</td>
</tr>
<tr>
<td>MJA</td>
<td>The Martin Joos Archive, at the American Philosophical Society, Philadelphia, Pennsylvania</td>
</tr>
<tr>
<td>RJA</td>
<td>The Roman Jakobson Archive at MIT, Cambridge, Massachusetts</td>
</tr>
<tr>
<td>TSA</td>
<td>The Thomas Sebeok Archive at Indiana University, Bloomington, Indiana</td>
</tr>
</tbody>
</table>
Timeline

1924  The founding of the Linguistic Society of America (§ 1.3.1)
1925  The first issue of the journal *Language* appears (§ 1.3.1, 1.4.2)
1926  The Prague Linguistic Circle is founded (§ 2.2.2)
1928  The first LSA-sponsored Linguistic Institute is held (§ 2.5.3)
      The first International Congress of Linguists takes place in The Hague (§ 2.5.3)
1933  Publication of Leonard Bloomfield’s book *Language* (§ 1.2, 1.3.3)
1940  Bernard Bloch becomes editor of *Language* (§ 1.5.5)
1941  J Milton Cowan becomes LSA Secretary-Treasurer (§ 1.5.5)
      Roman Jakobson arrives in the United States (§ 2.5.1)
1942  The LSA becomes heavily involved in supporting the war effort (§ 1.5.4)
1945  The journal *Word* is founded (§ 2.5.1)
1951  Noam Chomsky submits his MA thesis *The Morphophonemics of Modern Hebrew* (§ 4.2.2)
1955  Noam Chomsky’s *The Logical Structure of Linguistic Theory* appears (§ 4.2.2, 4.5.4)
1957  Martin Joos’s edited volume *Readings in Linguistics* appears (§ 3.4)
      Noam Chomsky’s *Syntactic Structures* is published (§ 4.2.1)
1961  The PhD program in linguistics at MIT is founded (§ 5.2)
1965  We find the first reference to a ‘Chomskyan revolution’ (§ 5.1)
1970  Dwight Bolinger challenges Martin Joos for the LSA presidency and defeats him (Chapter 7)
1974  Morris Halle is the first generative grammarian to take office as LSA President (§ 9.2.1)
1977  Generative Linguistics in the Old World is founded (§ 6.2)
1982  Charles Hockett submits his resignation letter to the LSA (Chapter 8)
1
The structuralist ascendancy in American linguistics

1.1 Introduction

I did an informal survey of a few dozen of my colleagues, all faculty members in departments of linguistics in the United States, and asked them what they would identify as the most dramatic change in American linguistics in the past century. I was careful to specify both intellectual developments and sociological ones. Without an exception they pointed to the ascent of generative grammar, which originated with the publication of Noam Chomsky’s *Syntactic Structures* (Chomsky 1957a) and, in their view, had swept the field by the 1970s (if not earlier). I outline in this chapter a more important change in the field, starting in the late 1920, picking up steam in the 1930s, and completed by the mid-1940s. Before this period an autonomous field of linguistics barely existed in the United States. Linguistic studies were viewed as an auxiliary discipline to studies in other fields, most prominently literature, but also to a degree anthropology and language pedagogy. By the mid 1940s linguistics had broken free of all of these other disciplines. There had developed a consensus that one could make important claims about how an individual language worked and how language in general worked without any appeal to the cultural and societal context in which language is spoken. Concretely, this meant that descriptive-analytical studies of linguistic structure and as societal studies of language change had come to carry the day. The purpose of this chapter is to outline how and why this profound change took place.

The chapter is organized as follows. § 1.2 outlines the state of American linguistics in the mid-twentieth century, followed by § 1.3 on the early Linguistic Society of America. § 1.4 presents the changes in the field from the 1920s to the 1940s and § 1.5 attempts to explain why these changes occurred. § 1.6 is a brief summary.
1.2 American linguistics in the mid-twentieth century

American linguistics in the late 1940s and early 1950s was dominated by the intellectual heirs of Leonard Bloomfield and Edward Sapir. As far as synchronic studies are concerned, the majority of linguists based in the United States were ‘structuralists’, or ‘structural linguists’. Put simply, their goal was to elucidate the structural system at the heart of every language. Bloomfield’s classic work *Language* (Bloomfield 1933) set the tone for most mainstream American linguists in the mid-twentieth century. The book dealt with each level of grammatical structure, outlining its properties and its relationship to other levels, and at the same time serving as both a major contribution to linguistic theory and as an introductory textbook. Bloomfield himself was by the 1930s quite anti-mentalist and in touch with the logical empiricist philosophers of the Vienna Circle. He contributed a monograph on linguistics to their *International Encyclopedia of the Unified Sciences*. This monograph, *Linguistic Aspects of Science* (Bloomfield 1939b), is the clearest statement in print on the intimate relationship of empiricist philosophy, behaviorist psychology, and structural linguistics. Bloomfield united all three in the following famous passage:

If language is taken into account, then we can distinguish science from other phases of human activity by agreeing that science shall deal only with events that are accessible in their time and place to any and all observers (strict BEHAVIORISM) or only with events that are placed in coordinates of time and space (MECHANISM), or that science shall employ only such initial statements and predictions as to lead to definite handling operations (OPERATIONALISM), or only such terms that are derivable by rigid definition from a set of everyday terms concerning physical happenings (PHYSICALISM).

(Bloomfield 1939b: 13)

Given such strictures, it follows that ‘the only useful generalizations about language are inductive generalizations’ (Bloomfield 1933: 20). That in turn led Bloomfield to be skeptical that meaning, ‘the weak point in language-study’ (p. 140),

---

1 There are a great many publications that treat developments in American linguistics in the first half of the twentieth century. I particularly recommend Murray (1994: chapters 3–8); Goldsmith & Laks (2019: Chapter 6); and Anderson (2021: chapters 10–14).

2 Many American linguists in the 1940s and 1950s preferred the self-designation ‘descriptivist’ to ‘structuralist’. Confusingly, however, not all linguists whose goal was to describe languages saw them as integrated structural systems. Franz Boas, for example, was certainly a descriptivist, but less clearly a ‘structuralist, as defined here (see § 1.3.3).

3 Much earlier, however, his work was grounded in Wundtian (mentalist) psychology, which is reflected in his book *Introduction to the Study of Language* (Bloomfield 1914). Sources agree that his turn to behaviorism was in large part a result of discussions with his Ohio State colleague, the psychologist A. P. Weiss.
could play a central role in grammatical analysis. But Bloomfield felt quite conflicted here. While he was adamant that "The study of language can be conducted without special assumptions only so long as we pay no attention to the meaning of what is spoken" (p. 75), he felt that 'as long as we pay no attention to meanings, we cannot decide whether two uttered forms are "the same" or "different"' (p. 77) and hence inevitably 'phonology involves the consideration of meanings' (p. 78).

Nevertheless, 'linguistic study must always start from the phonetic form and not from the meaning' (p. 162).

Sapir was no less a structuralist than Bloomfield; indeed papers such as Sapir (1925a) and Sapir (1963 [1933]) probably did more to lay the foundations for structural linguistics in the United States than did Bloomfield's Language. In the opinion of Zellig Harris, one of the leading mid-century linguists, 'Sapir's greatest contribution to linguistics, and the feature most characteristic of his linguistic work, was [...] the patterning of data' (Harris 1951b: 292). Unlike Bloomfield, however, Sapir was not an empiricist. One has to describe him as more 'intuitive' than Bloomfield, whereby flashes of insight led him to a brilliant analysis of some linguistic phenomenon, but without some particular philosophy of science that gave that analysis a theoretical and methodological underpinning. A. L. Kroeber, one of the most distinguished anthropological linguists of the early twentieth century, wrote that 'Edward Sapir, I should say, is the only man I have known at all well, in my life, that I would unreservedly class as a genius' (Kroeber 1984: 131). Sapir's bucking the empiricist tenor of the times and his untimely death at the age of fifty-five resulted in his having less influence than Bloomfield over the next generation of linguists.

The most influential tendency within American structural linguistics at mid-century followed Bloomfield's theoretical pronouncements, though often not his actual practice. Linguists customarily included in this group are Zellig Harris, George Trager, Bernard Bloch, Martin Joos, Henry Lee Smith, Jr., and (at least at as far as his earlier work is concerned) Charles Hockett. Bloomfield's view of science, which members of this group adopted enthusiastically, pointed to

---

4 In a survey article it was written that "The study of meaning does not have a place within descriptive linguistics, at least in its American variety [...]" (Lounsbury 1959: 191).

5 Sapir and Bloomfield had deep respect for each other (Harris 1973: 255), but with certain reservations. Sapir admired Bloomfield's ability patiently to excerpt data and to file and collate slips until the pattern of the language emerged, but spoke deprecatingly of 'Bloomfield's sophomoric psychology.' Bloomfield was dazzled by Sapir's virtuosity and perhaps a bit jealous of it, but in matters outside of language referred to Sapir as a 'medicine man' (Hockett 1970: 540; Jakobson 1979: 170). Yakov Malkiel has written: 'Such young Americans as I used to meet [around 1940] referred to the unique blend of scientism and mysticism in Bloomfield's personality; perhaps they should have substituted hard moral core for mysticism. Sapir, in contrast, was remembered as a magician' (Malkiel 1980: 83).

6 See Hymes & Fought (1981: 128); Murray (1983: 173); Hall (1987: 59); Fought (1995); and Koerner (2002a) for an (often conflicting) breakdown of American structural linguists into various categories. The views of Hockett and many others evolved over the years, making it sometimes difficult to pigeonhole particular individuals as being in particular 'camps.'
linguistic descriptions that were essentially catalogues of observables and generalizations extractible from observables by a set of mechanical procedures: ‘The overall purpose of work in descriptive linguistics is to obtain a compact one-one representation of the stock of utterances in the corpus’ (Harris 1951a: 366), that is, the requirement that all distinctive elements in a corpus be analyzed in the most efficient economical way. Given their subjective nature, informants’ judgments were looked upon with suspicion (except perhaps for the judgment as to whether two words or utterances were ‘the same’ or ‘different’). Analyses embodying underlying representations and derivations involving rule ordering were quite common in morphophonemic analysis (see Swadesh & Voegelin 1939; Bloomfield 1939a; Wells 1949; and, for discussion, Chapter 4, § 4.3.3.3 of this book), though they were considered distinct from phonemics and not as integral a part of the language as the latter.

For the most empiricist of the descriptivists, the idea was to arrive at a grammar of a language by performing a set of operations on a corpus of data, each successive operation being one step farther removed from the corpus. These operations, later called ‘discovery procedures’, aimed at the development of ‘formal procedures by which one can work from scratch to the complete description of the pattern of a language’ (Hockett 1952a: 27). It followed then that the levels of a grammatical description had to be arrived at in the order: first, phonemics, then morphemics, then syntax, then discourse: ‘There is no circularity; no grammatical fact of any kind is used in making phonological analysis’ (Hockett 1942: 20).¹ In actual practice, however, few if any linguists followed a set of (cumbersome) step-by-step procedures that were, in principle, necessary to arrive at a full grammar (for discussion, see Ryckman 1986, Chapter 2). Rather, they presented analyses which, in retrospect examination, could have been arrived at by means of these procedures.²

The order of discovery of each level of the grammar was reflected, not surprisingly, in the number of publications devoted to each level. There were many more papers on phonemics than on morphemics, and many more on morphemics than on syntax or discourse. As Robert A. Hall, Jr. explained: ‘Descriptive syntactic studies have also been rather rare; but, since they normally come at the end of one’s analysis, the tendency is perhaps to hold them for incorporation into a more complete description’ (Hall 1951–1952: 120).

¹ By the late 1940s it was widely recognized that phonemic analysis could be simplified by appeal to (higher level) morpheme and word boundaries. The problem was that while such boundaries were at times signaled phonetically (cf. nitrate and night rate), most of the time they were not (cf. minus and slyness). There was no general consensus on how to deal with this problem.
² Appeals to meaning in phonemic analysis were commonplace: ‘The basic assumptions that underlie phonemics, we believe, can be stated without any mention of mind and meaning; but meaning, at least, is so obviously useful as a shortcut in the investigation of phonemic structure — one might almost say, so inescapable — that any linguist who refused to employ it would be very largely wasting his time’ (Bloch 1948: 5).
Three groupings existed that were less influenced by a rigid empiricist methodology than the mainstream. One was made up of Sapir’s students, most of whom were based at some distance from the American East Coast and were focused more on the description of indigenous languages than on debates about procedures. Morris Swadesh, Mary Haas, Charles Voegelin, and Stanley Newman were part of this group. Morris Swadesh, perhaps the most brilliant of Sapir’s students, saw ‘the evidences of a struggle between realistic fact and mechanistic [i.e., Bloomfieldian — (FJN)] fetishism: particularly between the fact that meaning is an inseparable aspect of language, and the fetish that anything related to the mind must be ruled out of science’ (Swadesh 1948: 254).

Others approached linguistics as a tool to aid missionary work, and included such linguists as Kenneth Pike, Eugene Nida, and William Wonderly. For these linguists practical concerns typically outweighed theoretical ones, as is illustrated by the subtitle of Pike’s book Phonemics, namely A Technique for Reducing Languages to Writing (Pike 1947a). Pike and his followers had no compunction about ‘mixing levels’ in a grammatical analysis, that is, appealing to morphological and syntactic information to arrive at a phonemicization of a particular language.

By the 1950s, there was also a considerable presence of linguists who had been members of the Prague School or influenced by it, including Roman Jakobson, John Lotz, Thomas Sebeok, and Paul Garvin. Jakobson had arrived in the United States in 1941 as a refugee from Europe and within ten years he had built a significant American following. The Prague School linguists were rationalist in their epistemology and not loath to base formal analysis to an extent on semantic criteria. They advocated constructs that were shunned by the more empiricist-minded Bloomfieldians, such as universal categories, binary distinctive features, and markedness distinctions.

Historical linguistic studies, and in particular those of Indo-European languages, were far more prominent then than they are today. To illustrate, in the 1949 volume of the journal Language, over half of the articles dealt with diachronic themes. The journalist H. L. Mencken even complained that Language devoted more space to Hittite than to American English. For the most part, American-based historical linguists were practicing neogrammarians (as had been Sapir and Bloomfield themselves). In brief, the neogrammarian position is that sound change is regular and operates on distinct classes of sounds, which were later called ‘phonemes’ (for discussion, see Hock & Joseph 1996):

---

9 The first three groups of linguists discussed, though not those identified with the Prague School, were often referred to as ‘post-Bloomfieldians’ or ‘neo-Bloomfieldians’.

10 The full Mencken quote is somewhat off-topic, but amusing enough to merit reprinting: ‘[T]he Linguistic Society has given a great deal more attention to Hittite and other such fossil tongues than to the American spoken by 140,000,000-odd free, idealistic and more or less human Americans, including all the philologians themselves, at least when they are in their cups or otherwise off guard’ (Mencken 1948: 336).
It can only be regarded as fortunate that the later work of wise and historically well-trained linguists like Hoenigswald of Pennsylvania, showed that neogrammarian formulations were closely similar to those of twentieth century structuralists, and that the consonant pattern of Grimm’s law were a firm foundation for phonemic statement, instead of a merely happy intuition. (Hill 1966: 4–5)

Furthermore, the positivist outlook of many leading linguists was deeply compatible with neogrammarian views, as well as the idea that one could make profound generalizations about language structure and history without taking into account the culture or other societal aspects of the speakers. Some descriptivists (including both Bloomfield and Sapir) applied neogrammarian assumptions to working out the historical development and genetic classification of the indigenous languages of the Americas.

The application of the results of linguistics to language teaching had been given a great impetus by the war (see § 1.5.4). By the early 1950s, American linguists had also started to branch out into subfields that had received very little attention in earlier years, such as sociolinguistics and psycholinguistics (two terms that had been coined in the 1930s, but were only just beginning to pass into current use), as well as information theory, discourse analysis, and translation theory (for discussion, see Carroll 1953 and Hamp 1961).

### 1.3 The Linguistic Society of America

This section recounts the founding of the Linguistic Society of America (LSA) (§ 1.3.1), the debates that raged among its founders about the differences between linguistics and philology and their relationship to scientific endeavors (§ 1.3.2), and the leadership and composition of the early LSA (§ 1.3.3).

#### 1.3.1 The founding of the Society

Before 1924 there was no organization and no journal in the United States that were dedicated to general linguistics. If one wished to disseminate one’s ideas about language, one joined the American Oriental Society (founded in 1842), the American Philological Association (founded in 1869), the Modern Language Association (founded in 1883), or the American Anthropological Association (founded in 1902). Their journals, Journal of the American Oriental Society, Transactions of the American Philological Association, Publications of the Modern Language Association, and American Anthropologist respectively, were the main outlets for linguistic
Robert A. Hall, Jr. noted some years later: ‘But all too often, linguistics was treated as a kind of step-child, a Cinderella, in contrast to literary or philological studies. As was observed by the American Sanskritist Maurice Bloomfield in 1919 (Transactions of the American Philological Association, L [1919], 83), “There is pathos in that comparative philologists in America have neither independent association nor special organ of publicity.” (Hall 1951–1952: 102).

Leonard Bloomfield wrote: ‘Thus [in the first quarter of the twentieth century] linguists met, pleasantly and profitably, but only in several disconnected groups, each of which appeared as a small and subsidiary cell within a larger society. There was no place where all linguists could meet, no reunion devoted entirely to linguistic discussion, and no journal devoted to our subject’ (Bloomfield 1946: 1).

Bloomfield decided to initiate a change in this state of affairs. On January 18, 1924 he sent the following exploratory letter to Boas (see Swiggers 1994 for discussion):

Dear Professor Boas —

I should be very glad to get your response to the enclosed letter. Under our present conditions linguists do not really meet each other, except within certain accidental groups, such as the Modern Language Association and other similar bodies. Some of us feel we have far more in common with ethnologists than with students of the fine art of literature, — and hope that we can get a meeting-ground for the interest in human speech.

We want to send a similar note to Dr. Sapir; can you give me his address?

With best regards,

Sincerely,

Leonard Bloomfield (FBA)

Three days later, Boas sent a favorable reply to Bloomfield’s suggestion. On January 28, Bloomfield sent a slightly revised version of the enclosed letter (reproduced below from Hockett 1987c: 45), now co-signed by George Melville Bolling and Edgar H. Sturtevant, to a number of their colleagues:

Dear Sir:

To the undersigned it seems that the study of the linguistic sciences is at present greatly neglected in this country. As an indication of this condition they point to the fact that while we have historical, archaeological, and philological societies and publications in a prosperous condition, there is no important society or

---

11 It is important, however, to call attention to the International Journal of American Linguistics (IJAL), founded by Franz Boas and Pliny Earle Goddard in 1917. IJAL was (and still is), ‘devoted to the study of American aboriginal languages’ (Boas 1917: 1). At the beginning, the mission of IJAL was as much to preserve indigenous texts as to analyze the grammars of indigenous languages.
publication devoted exclusively or chiefly to linguistics in any of its phases. They believe that the foundation of such a society (to have ultimately its own organ of publication) is the first step necessary to an improvement in the general condition of linguistic studies, and that an improvement in the status of linguistic science will necessarily result in the furtherance of all humanistic studies, and the promotion of the best interests of the existing historical, archaeological, and philological societies.

It is tentatively suggested that such a Society should hold biennial meetings, devoted to the discussion of linguistic problems, and related matters at such times and places as shall render it conveniently possible for the members to attend also, alternatively, the meetings of the A.P.A. or the M.L.A., to one or another of which it may be supposed to many of the members will belong.

You are respectfully invited to inform the signers whether you approve the idea in substance — indicating any changes you may think desirable. In case you disapprove the idea they would appreciate greatly a statement of the reasons that lead you to dissent.

The next step will be to circulate among those who have approved the plan in substance a preliminary draft of the call in order that it may be criticized by all before its issuance.

(Signed)
Leonard Bloomfield
George M. Bolling
E. H. Sturtevant

The three were encouraged by the response, which led them to issue ‘The Call for an Organizational Meeting’ on November 15, 1924. The Call repeated in essence their earlier letter and was signed by 29 scholars (all male, as it turns out). The plan was to meet for the first time at the American Museum of Natural History in New York, on December 28, starting a tradition of late December meetings that would last until 1991. There were 264 Foundation Members of the LSA, 69 of whom attended that first meeting. Hermann Collitz was elected the first president, Carl Darling Buck vice-president, and Roland Grubb Kent secretary-treasurer. Bolling took office as editor of the Society’s journal *Language*, which published its first issue in early 1925.

---

12 Martin Joos wrote that ‘for family reasons, there were no women’ (Joos 1986: 9), whatever that might mean. In the 1930s the Front Matter of the LSAB contained the following sentence: ‘All persons, whether men or women, who are in sympathy with the objects of the Society, are invited to give it their assistance in furthering its work.’ The wording suggests that it was not obvious to all that women were welcome as members. There were thirty-one women Foundation Members, however. For an important study of women in the early years of the LSA, see Falk (1999).
The creation of the society was warmly welcomed by scholars in other countries. For example, Antoine Meillet, the Secretary of the Société Linguistique de Paris, enthused:

Thank you for informing me about the founding of your Linguistic Society. The names of the founders guarantee its future. Allow me nevertheless to wish you good luck and to tell you with how much affection our Society, already old, will follow the progress of your young Society, which will soon be growing. I am very aware of how you have mentioned our Society in founding your own. And I won’t miss the opportunity to announce the founding at our next meeting, next Saturday. That will insure that it will appear in our Bulletin; and the Society will certainly send you its congratulations.

*(LSAB 1, 1925: 2)*

Bloomfield was charged with the task of explaining to the academic public why a new society was needed. The following are excerpts from his article in the first issue of the first volume of *Language*:

In our country are scholars who for a generation or more have worked in linguistics and have never met; some of them saw each other for the first time at our initial meeting on December 28th. [...] The layman — natural scientist, philologist, or man in the street — does not know that there is a science of language. [...] The layman usually has no conception of this task [of scientifically analyzing] language; he believes that languages which possess no written literature are mere ‘dialects’ or ‘jargons’, of small extent and subject to no fixed rule. Quite by contrast, linguistics finds, on the one hand, a similarity, repugnant to the common-sense view, between the languages of highly civilized people and those of savages, a similarity which disregards the use or non-use of writing. [...] Not only in the general public, but also in the academic system, linguistics is not known as a science. The notion seems to prevail that a student of language is merely a kind of crow-baited student of literature. [...] The more direct harm to science is too obvious to need exposition; one may mention the American Indian languages, which are disappearing forever, more rapidly than they can be

---

recorded, what with the almost total lack of funds and organization; or the case of American English, of which we know only that, both as to dialects and as to distribution of standard forms, it would present a complex and instructive picture, had we but the means and the equipment to study it.

(Bloomfield 1925c: 1–4)

One finds this statement extremely modern in more than one way. Many of the concerns raised by Bloomfield are the concerns of linguists today: public ignorance of the field of linguistics, the denigration of non-standard forms of speech, and the rapid disappearance of the indigenous languages of the Americas. Furthermore, Bloomfield stressed over and over again that linguistics is a science, just as most practitioners in the field would today. The word ‘science’ appears fifteen times in his four-and-one-half page article. And interestingly, he denigrated the ‘philologian’, who is ignorant of the ‘science of language’. Bloomfield was later to write: ‘I believe that in the near future — in the next few generations, let us say — linguistics will be one of the main sectors of scientific advance, and that in this sector science will win through to the understanding and control of human conduct’ (from 1929; quoted in Hockett 1970: 229).

The historiographer of linguistics, Stephen O. Murray, recognized the importance of the founding of the LSA, in writing:

I think [that the signers of the call] were in some sense ‘revolutionaries’. They broke with most of their colleagues in according primacy to the study of speech rather than writing (literature), rejected \textit{a priori} grammatical categories, studied unwritten languages, advanced revolutionary notions of phonemics (Sapir 1925a), and sarcastically rejected the American university methods of teaching languages (including English). Bloomfield and Sapir categorically rejected the approaches of most members of the Modern Language Association, and Bolling was at odds with leading American philologists. […] The founders of the LSA broke with the basic assumptions of their elders more decisively, if with less proclamation of novelty, than Chomsky and his associates broke with neo-Bloomfieldian structural linguistics. (Murray 1989: 162)

Before proceeding further, then, it might prove useful to sort out the distinction between philology and linguistics as the fields were conceived at the time, as well as to what extent either could be regarded as a ‘science’.

\footnote{Murray’s statement was in direct response to the (unsupportable) claim by Martin Joos that ‘The signers of the call were not rebels. They were continuity men. Their research, teaching, and publication continued unbroken the patterns of linguistic thinking defined [by] the neogrammarians’ (Joos 1986: 9).}
1.3.2 Linguistics, philology, and their relationship to science

The term ‘philology’ has its roots in antiquity. The applicability of the term has varied from time to time and from place to place. However, it has generally referred to two closely related endeavors. One is the study of the history and evolution of languages, particularly of those with a literary tradition. The other is the study of ancient texts themselves, from the point of view of examining their language per se, as opposed to, say, from a literary-critical point of view. The great discoveries of the Indo-European ‘sound laws’ in the early nineteenth century were at first described as pertaining to ‘comparative philology’, a term that was commonly used in the United Kingdom and continental Europe well into the twentieth century. According to Turner (2014: 146), the word ‘linguistics’ was coined in 1839 by John Pickering, an early student of Amerindian languages. Turner also pointed out that there was no question among the early comparative philologists that their work was ‘scientific’: “Only in our century,” bragged Jakob Grimm (1785–1863) in 1851, has language studies matured into “a true science” (p. 146). For quite some time, the terms ‘the science of language’ and ‘linguistic science’, both loan-translations from the German ‘Sprachwissenschaft’, were used more commonly than the term ‘linguistics’. According to Julia Falk, these terms were ‘used to distinguish systematic, analytic, inductive studies from the more speculative, even fanciful, opinions of some philologists’ (Falk 2017: 3).

The first American linguist with an important international reputation was William Dwight Whitney. Falk stresses how Whitney saw linguistics as a science, comparable at least to a degree to zoology and botany:

There is no discussion in Whitney’s book [Whitney 1867] of philology in general, its goals or its methods. ‘Comparative philology’ (or ‘Indo-European philology’) was ‘the early growing phase of linguistic study, that of the gathering and shifting of material, the elaboration of methods, the establishment of rules, the deduction of first general results’ (Whitney 1867: 241) But ‘the study of Indo-European language is not the science of language’ (237) because there is greater diversity among languages than is exemplified within that single family. In his later book, he restated this: ‘The science of language is what its name implies, a study of all human speech’ (Whitney 1875: 191). Whitney rejected the title of ‘comparative philology’ for ‘the new science of language’ — ‘a title […] now fully outgrown and antiquated […]. [T]o call the whole science any longer ‘comparative philology’ is

---

15 John Joseph informs me (p. c., 16 November 2020) that the term ‘linguistics’ appeared in an anonymous review from 1837 entitled ‘History of Navigation in the South Seas’, published in the North American Review. Apparently the term appeared for the first time in more-or-less its modern sense in the seventeenth century. From the OED in 1695: ‘J. Edwards Disc. conc. Old & New-Test. III. i. 3 Here Linguists and Philologists may find that which is to be found no where else.’
not less inappropriate than to call the science of zoölogy 'comparative anatomy' or botanical science the 'comparison of plants' (1867: 241).

(Falk 2017: 3)

By Bloomfield's time, 'linguistics' had become more common than 'the science of language' or 'linguistic science'. The question, then, is what would be the place for philology in the new society. As indicated in the above quote, he seemed quite dismissive of 'philologians' for their unscientific practice. In a joint article published a few years after the foundation of the LSA, Sturtevant and Kent explained what they saw as the difference between linguistics and philology:

Linguists study language for itself, while philologists regard language as a means to an end; linguistics is a pure science, while philology is the corresponding applied science. But the parallel is not perfect, for the linguistic point of view may be of immediate utility, and much of the best philological work has been done without a thought beyond the establishment of a correct text or a correct interpretation.

(Sturtevant & Kent 1928: 9)

A year later, Bolling attempted to clarify further the types of scholarly activity with respect to language. He identified three: 'A. The study of man's speech habits. B. The study of what his speech habits have enabled man to accomplish, that is the study of civilization as a whole. C. The establishment and interpretation of the texts of such documents as need that treatment' (Bolling 1929: 27). He stated clearly: 'To A corresponds the LINGUISTIC SOCIETY OF AMERICA' (p. 28). And a few years later still, Bloomfield in his book Language insisted that 'It is important to distinguish between philology [...] and linguistics [...], since the two studies have little in common'. (Bloomfield 1933: 512; emphasis in original).

One would think that, given the above statements from the leading founders of the LSA, philological studies would have played little if any role in that organization. As we see in the following sections, such was not the case.

1.3.3 The leadership and composition of the early LSA

Almost all of the founding members of the LSA were trained in philology. Of the twenty-nine who signed the Call for the founding of the Society in 1924, at least twenty-one were engaged in teaching and research on ancient and classical

16 The terms 'philologian', 'philologist', and 'philologer' are essentially synonymous. Henry A. Gleason suggests that 'Bloomfieldians drew a much sharper and less permeable boundary between language and literature than did Europeans generally' because 'North American linguists had not always had the best of experiences with their literary colleagues' (Gleason 1988: 17).
languages, the historical development of languages, or the comparative linguistics formulated primarily in Europe during the nineteenth century (Falk 1999: 223). They practically had to be philologically trained: ‘To be regarded as linguistically strong during the two decades from 1913 to 1933, a linguist had to know comparative linguistics—which generally meant training in Indo-European languages—whether or not he intended to pursue comparative work in one or another American Indian language family’ (Voegelin & Harris 1952: 323). Bloomfield had been a student of Germanic philology, writing a 1909 University of Chicago dissertation entitled *A Semasiologic Differentiation in Germanic Secondary Ablaut*. However, his career trajectory was markedly different from that of many of his peers. First, he was from the beginning a committed neogrammarius, reinforced, I am sure, by his having studied at Leipzig with the great neogrammarians August Leskien and Karl Brugmann. His first journal article, *Bloomfield (1911)*, hypothesized a set of sound changes from Proto-Indo-European to Sanskrit. Second, he worked on languages outside the western literary tradition (the analysis of Tagalog in *Bloomfield 1917*) and those with no literary tradition at all, in particular the Central Algonquin languages Menomini and Fox (*Bloomfield 1924a, b, 1925a*), the second-named appearing in the first volume of *Language*. Third, and most importantly, Bloomfield advocated an autonomous descriptive linguistics, that is a science of language whose basic vocabulary was entirely language-internal. His landmark article ‘A Set of Postulates for the Science of Language’, published in the second volume of *Language*, laid out a series of assumptions, definitions, and observations that led to a view of language that was totally self-contained. By defining ‘the language of [a] speech-community’ as ‘the totality of utterances that can be made in a speech-community’ (*Bloomfield 1926*: 26), he implied that the sociocultural context of language could be ignored in linguistic description.

Of the Foundation Members of the LSA, Bloomfield was not alone in advocating ‘descriptivism’, as the autonomous analysis of the structure of a language came to be known. The first great descriptivist was Franz Boas, generally regarded as the founder of both the fields of linguistics and anthropology in the United States. Bloomfield once described him as ‘the pioneer and master in the study of American languages and the teacher, in one or another sense, of us all’ (*Bloomfield 1972*: 265). Coming from a background in physics and geography, rather than philology, Boas produced some of the first descriptive studies of a number of Amerindian languages. His great fame was achieved for his militant, tightly argued, opposition to the idea that there is any intrinsic connection between race or level of culture and language structure (see especially *Boas 1911*). Boas pointed out, for example, that ‘the North American negroes, a people by descent largely African [are] in culture and language, however, essentially European’ (p. 4). He then called attention to ‘the Magyar of Europe, who have retained their old language but have become mixed with people speaking Indo-European languages, and who have, to all intents and purposes, adopted European culture’.
Boas also refuted the consensus of an earlier generation of students of North American indigenous languages that such languages were more ‘primitive’ than European languages because they contained ‘alternating sounds’ that defied precise transcription. He showed that such conclusions were primarily a function of the flawed perceptions of the investigators, who were unskilled in analyzing sounds that differed from those of their own languages (Boas 1889).

Boas was a descriptivist, but he was not a ‘structuralist’, another term that came into widespread use in the 1920s. A structuralist is a descriptivist who analyzes the grammar of a language as an integrated whole, that is, in the words of Boas’s French contemporary, Antoine Meillet, where ‘chaque langue forme un système où tout se tient’ (Meillet 1953 [1903]: 407). Boas was certainly aware that the grammars of all human languages have systematic properties:

\[
\text{[Each dialect has its own characteristic phonetic system, in which each sound is nearly fixed, although subject to slight modifications which are due to accidents or to the effect of surrounding sounds. [...] One of the most important facts relating to the phonetics of human speech is, that every single language has a definite and limited group of sounds, and that the number of those used in any particular dialect is never excessively large.}\]

\(\text{(Boas 1911:16)}\)

Nevertheless, such knowledge played little if any role in his actual linguistic descriptions. As pointed out by Stephen Anderson, ‘There is no reason to doubt his complete familiarity with “phonemic” views, but he remained at least unreceptive (if not outright hostile) to the replacement of phonetic by phonemic transcriptions as phonemic theory gradually took prominence as the cornerstone of a “scientific” approach to language’ (Anderson 2021: 239). For Boas, phonetic accuracy was more important than systematic analysis, due in part to his desire to preserve and present native textual material in a fully accurate way. The task of prioritizing phonemic structure over phonetic detail was initiated in American linguistics by another Foundation Member of the LSA, Boas’s student Edward Sapir.

Sapir, like Bloomfield, was trained in Germanic philology. However, as a student at Columbia University, he came under the influence of Boas, who encouraged him to work on the indigenous languages of the Americas. His book *Language* (Sapir 1921) brought him to the attention of the linguistic community. The book is replete with Boasian influence, in particular with its insistence on the independence of language, race, and culture. But it goes farther than Boas ever did in overviewing and classifying the structural possibilities of language in general. In that same decade Sapir published two important papers in *Language,* one which

\[17\] That is, where ‘each language forms a system where everything is connected’.
argued that each individual language has its own unique integrated sound system (Sapir 1925a) and another which can be regarded as a manifesto for structural linguistics, as the following quote indicates:

[L]anguage […] develops its fundamental patterns with relatively the most complete detachment from other types of cultural patterning. […] Linguistics would seem to have a very peculiar value […] because the patterning of language is to a very appreciable extent self-contained and not significantly at the mercy of intercrossing patterns of a non-linguistic type.  

(Sapir 1929: 212–213)

Furthermore, Sapir, unlike Boas, believed that you could use comparative Indo-European methods on any language (Darnell 1990: 122). Sapir was not very active in the founding of the LSA. He couldn't be—between 1910 and 1925 he worked in Ottawa, directing the Anthropological Division of the Geological Survey of Canada. In fact, it has been written that he and Bloomfield did not meet for the first time until the organizational meeting of the LSA in December 1924 (Hall 1990b: 30). As indicated in § 1.3.1, Bloomfield had to ask Boas for Sapir’s address, indicating possibly that Bloomfield did not even know that Sapir was working in Ottawa.

Edgar H. Sturtevant was one of the leading historical linguists of the twentieth century. He was trained in classical philology, but became an enthusiastic neogrammarian early in his career. More than anyone else he was responsible for confirming the hypothesis that Hittite was related to Indo-European. He had a couple dozen articles in the journal Language alone devoted to working out the genetic relationship between Hittite and Proto-Indo-European (he himself believed that they were sister languages, derived from 'Proto-Indo-Hittite'). Possibly more than any other LSA founding member, Sturtevant believed that the Society should be an umbrella organization, under which any student of language should be welcome. Sturtevant made it explicit how scholars in one subfield of linguistics might benefit from being exposed to results of scholars in another subfield:

The science of language has been much retarded by the fact that linguists are, to a large extent, unacquainted with one another, and that they are sometimes ignorant of one another's work. There is little doubt, for example, that the study of the Chinese tones can contribute much to our knowledge of Greek accent, or that modern Persian verse can shed a flood of light upon ancient Greek verse; but the necessary contacts have not yet been established.  

(Surtevant 1925: 128)

Furthermore, Sturtevant was very ‘modern’ very early on. In his book Linguistic Change, he asserted that ‘linguistic science is […] primarily concerned with spoken
language’ (Sturtevant 1917: 10) and thirty years later in an introductory text, he wrote ‘We shall have to discuss writing […], but only because writing embodies almost our only records of the speech of the past’ (Sturtevant 1947: 3).

Despite all of the above, the importance that philological studies played in the research of most of the early LSA members is reflected in a passage from his 1925 article:

There is no sharp line of division between language and literature, and the attempt to draw one would result in injury to both subjects. Grammar has frequently suffered from the imperfect philological training of its students; while the above-mentioned dependence of sound metric science upon phonetics illustrates the importance of linguistic study for the appreciation of literature. There is no doubt that American linguists will continue their connection with the several philological societies, and it is already evident that many philologists will want to join the Linguistic Society.

(Sturtevant 1925: 128)

The other important founding members of the LSA were trained as philologists and, to one degree or another, continued to advocate for (if not practice) philology throughout their careers. George M. Bolling, in 1925 Bloomfield's colleague at Ohio State University, was professor of Greek Languages and Literature there. As opposed to an earlier generation of philologists, he embraced neogrammarian principles and was impressed with the results of structural linguistics (Hoenigswald 1964: 331–332). ‘What Bolling liked most in Bloomfield’s work was the idea of developing grammatical categories from the material itself without imposing a foreign mold’ (Hoenigswald 1964: 331). However his ultimate goal was always to further the understanding of Classical Greek literature.

Hermann Collitz too extolled the results of the neogrammarians (Collitz 1926). But he felt the need to rebuke Bloomfield's egalitarian message that all speech varieties are equally worthy of study, writing that ‘[i]t is impossible to study any particular language without perusing at the same time specimens of the literature written in that language. The study of Greek, e.g., means the study of the Greek language and Greek authors’ (Collitz 1925: 16). Carl Darling Buck, the only individual ever to be elected president of the LSA twice (in 1927 and 1937), was also a neogrammarian; indeed he has been described as the scholar ‘who established study of Indo-European linguistics in the United States’ (Lane 1955: 184). Lane went on to remark that ‘The victory of the Junggrammatiker [neogrammarians — (FJN)] had been conceded [by the end of the nineteenth century]; the fruitfulness of their methodology was being demonstrated; and Carl Darling Buck contributed his full measure to that demonstration’ (p. 187). And yet he was equally interested in purely philological questions, such as the dating of an Old Persian inscription, which he wrote up as the lead article in an early volume of Language (Buck 1927).
Finally, Roland G. Kent, despite of his clear understanding of the distinction between linguistics and philology (see above), was primarily a practitioner of the latter, specializing in Greek and Latin literature, with an ancillary interest in Old Persian. A description of his attitude toward the study of unwritten languages seems shocking to modern ears:

Boas and Sapir together gave the anthropological perspective that philologists did not have. What I mean by ‘anthropological perspective’ is a whole lot of things, including comparing across the world: you could talk about a language in Africa or Australia or Europe in the same breath. This was considered just outrageous by the Indo-Europeanists, but anthropologists were used to doing it. This was overwhelming and astounding to people like Roland Kent, who was secretary of the Linguistic Society of America for many years. He couldn’t conceive of talking about a ‘primitive language’ and Greek in the same breath. For him they were two different things.

(Mary Haas, quoted in Murray 1997: 700–701)

Of the 214 members of the LSA polled in March 13, 1926, 85 belonged to the Modern Language Association, 77 belonged to the American Philological Association, 61 belonged to the American Oriental Society, 17 belonged to the American Anthropological Association, and 18 belonged to none of the four societies (LSAB 2, 1926: 70). As noted by Julia Falk, ‘It was surely the case that many of these scholars did not see themselves as engaging in science; many were traditional philologists or specialists in contemporary languages, some were high school language teachers, others had no academic affiliation and were simply interested in language and languages’ (Falk 2017: np).

To summarize, with the exception of Boas, Bloomfield, and Sapir, the latter two of whom would go on to be the leading lights of American structural linguistics, the founders of the LSA were as personally rooted in philology as they were in (scientific) linguistics, even though in general they did not hesitate in advocating for the latter.

1.4 The changes in the field from the 1920s to the 1940s

In the 1920s and 1930s the LSA was an organization whose leading members came to language through the study of classical literature. For the most part, they considered themselves linguists, not philologists. But the work of the majority dealt with properties of classical languages with a long literary history like Latin, Greek, and Sanskrit, or with ancient languages like Hittite that had newly-discovered texts. All of that had changed by the late 1940s. The next two subsections document two aspects of this change: the composition of the LSA Executive Committee in 1936
1.4.1 The LSA Executive Committee in 1936 and in 1946

Table 1.1 lists the members of the LSA Executive Committee in 1936, along with the title of one of their typical publications. Table 1.2 shows us 1946. The titles from 1936 belong to a world far removed from what we think of as linguistics. Most of the titles from 1946 have as their subject matter areas of interest that could be dealt with in linguistics books and articles today. It is also worth pointing out that the average age of the Executive Committee members in 1936 was fifty-five and the average age of the members in 1946 was forty-four. It would seem that the LSA had been captured by the young, whose number one priority was linguistic structure and analysis.

In the 1920s and 1930s the LSA and the American Philological Association usually held their winter meetings jointly. The last of these meetings (except for 1946 and 1947) was in 1937, and for the two years in the 1940s only because the successive presidents of the LSA and the APA had strong philological interests (Falk 1999: 224). Another sign that American linguistics was moving towards science and away from philology was its response to the announcement of the First
Table 1.2  Officers of the LSA in 1946 with typical publications

<table>
<thead>
<tr>
<th>Title</th>
<th>Name</th>
<th>Typical publication</th>
</tr>
</thead>
<tbody>
<tr>
<td>President</td>
<td>E. Adelaide Hahn(^{18})</td>
<td><em>Subjunctive and Optative: Their Origin as Futures</em> (Hahn 1953)</td>
</tr>
<tr>
<td>Vice-President</td>
<td>W. Freeman Twaddell</td>
<td>‘On defining the phoneme’ (Twaddell 1935)</td>
</tr>
<tr>
<td>Secretary and Treasurer</td>
<td>J Milton Cowan</td>
<td>‘An experimental study of pause in English grammar’ (Cowan 1948)</td>
</tr>
<tr>
<td>Executive Committee</td>
<td>John Samuel Kenyon</td>
<td><em>A Pronouncing Dictionary of American English</em> (Kenyon &amp; Knott 1944)</td>
</tr>
<tr>
<td>member</td>
<td>George Trager</td>
<td>‘The phonemes of Russian’ (Trager 1934)</td>
</tr>
<tr>
<td>Executive Committee</td>
<td>Kenneth L. Pike</td>
<td><em>Phonemics: A Technique for Reducing Languages to Writing</em> (Pike 1947a)</td>
</tr>
<tr>
<td>member</td>
<td>Zellig S. Harris</td>
<td><em>Methods in Structural Linguistics</em> (Harris 1951a)</td>
</tr>
<tr>
<td>Editor</td>
<td>Bernard Bloch</td>
<td>‘Phonemic overlapping’ (Bloch 1941)</td>
</tr>
</tbody>
</table>

International Congress of Linguists (ICL), held in The Hague, Netherlands in April 1928.\(^{19}\) The ICL was explicitly pro-general linguistics and anti-philology from its very beginning, as is evidenced by the excerpt from their letter of invitation:

Hence it is that linguistic problems have only received scant, if any, attention, e.g., at Philological Congresses. They have never yet formed the central point of discussion at a meeting of competent students. And the want of such a discussion has become ever more strongly and generally felt.

(*LSAB* 3, 1927: 276)

Five American scholars were on the ICL's Committee of Honour: Franz Boas (who was also a Vice-President of the ICL), Pliny Goddard, A. L. Kroeber, Truman

\(^{18}\) Adelaide Hahn was the first woman to discuss a scholarly topic at an LSA meeting, the first woman to serve as LSA president, the first woman appointed to the Collitz professorship (at the 1951 Linguistic Institute at the University of California, Berkeley). She provided a link to the past and to the well-established and prestigious American Philological Association, enhancing the legitimacy of the LSA in its early years. Though her scholarship became outmoded, her constant presence, her intelligence, her impressive command of Greek and especially Latin, and her articulate outspokenness at the annual and summer meetings of the Society were models of engagement and participation for countless young linguists, both men and women, for more than forty years (Falk 1999: 20).

\(^{19}\) Yakov Malkiel remarked that ‘international congresses [in American linguistics] were not organized before the First World War; [American] linguists were thus far behind orientalists and other groups of scholars (Malkiel 1979: 115).
Michelson, and Edward Sapir. All five were known for their descriptive work on Amerindian languages; not one was in any sense a philologist.\(^{20}\)

1.4.2 The changing contents of the first quarter-century of *Language*

Parallel developments to those just described affected the contents of the journal *Language*, until the mid-1940s the only journal of general linguistics in the United States.\(^{21}\) Looking at the pages of the journal *Language* before the late 1930s it is obvious that many or most articles were written by classically trained scholars who were also members of philological and oriental societies. By the late 1930s—and accelerating into the 1940s—there was a dramatic change. The LSA, its leadership, and the content of its journal slowly left the classics behind and began to focus on language structure and language change divorced from the classical matrix that had existed before.

Let me illustrate. In the five volumes of *Language* published in the 1920s, only about eight of the main articles could be classified as synchronic descriptive linguistics, two of them written by Sapir and two by Bloomfield. At the same time, there were no fewer than sixteen that discussed some particular linguistic feature of a text (almost always an ancient one) or a group of closely related texts. The great majority of the remaining papers dealt with historical linguistics, and the great majority of those discussed changes within the Indo-European family of languages.

Things began to change a bit in the 1930s. Around the middle of that decade it became fairly common to see overtly structuralist papers, such as ‘The phonemic principle’ (Swadesh 1934), ‘The phonemes of Russian’ (Trager 1934), ‘Shawnee phonemes’ (Voegelin 1935), ‘On defining the phoneme’ (Twaddell 1935), ‘Phonetic and phonemic change’ (Hill 1936), ‘A Problem in phonological alternation’ (Swadesh & Voegelin 1939), and so on. Nevertheless, these were greatly outnumbered by articles that seem more philological than linguistic. For every article that was entitled ‘The phonemes of [Whatever]’, there were a half dozen or more with titles like ‘The subjunctive in Lazarillo de Tormes’ (Keniston 1930), ‘Another inscription of Xerxes’ (Kent 1933), ‘Studies in the diction of Layamon’s Brut’ (Wyld

---

\(^{20}\) Unfortunately, the LSAB does not tell us if the five were appointed by the ICL organizers or by the LSA.

\(^{21}\) There were very few such journals anywhere in the world. The *Bulletin de la Société Linguistique de Paris* dates from 1869. But *Travaux du Cercle Linguistique de Prague* did not appear until 1929, followed by *Acta Linguistica* (Copenhagen) in 1939, and the Swiss journal *Cahiers Ferdinand de Saussure* in 1941.
1934), or ‘Proskynesis and adorare’ (Marti 1936). Even as late as 1949, Language published an article entitled ‘Οφα in the Homeric poems’ (Bolling 1949).22

By 1950 the editor Bernard Bloch was able to refer to ‘The increase in the number of papers devoted to languages outside the Indo-European family [which] undoubtedly reflects the growing interest of American linguists in the description of living languages — an interest that not only proved its validity during the war years but received a new impetus from the success with which it was applied to the practical task of language teaching. This remarkable rise in the proportion of non-Indo-European studies (perhaps also the rise in the proportion of papers on English) reflects a genuine trend […]’ (LSAB 23, 1950: 16). The bulk of these papers were structural analyses.

1.5 Some reasons for the dramatic changes in American linguistics from the 1920s to the 1950s

This section attempts to explain why the field of linguistics in the United States was able to transform itself from a philology-oriented group of scholars to a structuralist-oriented group between the 1920s and the 1940s. They are: the sense of a distinctive field making rapid progress (§ 1.5.1); the leading linguists’ commitment to the equality of all languages and their analysis (§ 1.5.2); the LSA’s summer institutes (§ 1.5.3); American linguists’ involvement in the war effort in World War II (§ 1.5.4); and the change of leadership in the LSA in 1940–1941 (§ 1.5.5).

1.5.1 The sense of a distinctive field making rapid progress

Sapir and Bloomfield never missed an opportunity to argue that linguistics was an autonomous field, rather than being a branch of psychology, anthropology, language studies, or anything else. And this followed directly from language structure being relatively ‘insulated’ from phenomena outside of language per se. For example:

Language is probably the most self-contained, the most massively resistant of all social phenomena. It is easier to kill off than to disintegrate its individual form.

(Sapir 1921: 205–206)

22 Julia Falk suggests (p. c., 16 November 2020) that the editor of Language published Bolling’s paper more as a tribute to one of the founders of the LSA than out of any commitment to elucidating some feature of Homer’s poetry.
We are casting off our dependence on psychology, realizing that linguistics, like every science, must study its subject matter in and for itself, working on fundamental assumptions of its own; that only on this condition will our results be of value to related sciences (especially, in our case, to psychology).

(Bloomfield 1922: 142)

One thing that made American structuralism so distinct, and I would say so successful, was its emphasis on its own procedures, methodology, analytical techniques, and so on. This focus on methodology would be criticized by Noam Chomsky much later (see especially Chomsky 1964a), but in the 1930s and 1940s such a focus allowed American structuralists to claim that linguistics had carved out its own intellectual sphere that was unlike anything in any other field traditionally considered part of the humanities or social sciences. It must be pointed out that European structuralists certainly did not dispute the (relative) autonomy of linguistic structure. After all, Ferdinand de Saussure, posthumously recognized as the founder of structural approaches to language, stressed the ‘arbitrariness of the sign’ and concluded his (posthumously written) book with the slogan that ‘the true and unique object of linguistics is language studied in and for itself’ (Saussure 1966 [1916]: 232). Likewise Roman Jakobson noted that ‘every language has a system of distinctive features and rules governing their arrangement into bundles and sequences […]. This system is governed by autonomous phonemic laws’ (Jakobson 1971e [1949]: 111). But for all that, the Europeans generally believed that the study of linguistic structure could best be understood as part of (or beholden to) the study of something broader. For Saussure, language was one of many possible or existing systems of signs. Jakobson pointed to ‘a universally recognized view of language as a tool of communication’, leading to ‘the elemental demand to analyze all the instrumentalities of language from the standpoint of the tasks they perform […]’ (Jakobson 1971d [1963]: 523). While this is admittedly pure speculation, I suggest that part of the great appeal of American structuralism from the 1920s to the 1950s was its practitioners’ single-minded focus on analysis, with little or no discussion of why language structure might be the way that it is.

Bloomfield’s extolling the uniqueness of linguistics and its methods helped to attract a generation of bright young students who wanted to try their hand at the analysis of an unstudied language or to refine the analytic procedures. Bloomfield knew what he was doing and he was pleased that he was successful. In 1946 he wrote:

Linguistics has come more and more to resemble, in its social complexion, the type of the better established branches of science — say physics, chemistry, and biology.

(Bloomfield 1946: 2)
It is very interesting that he wrote ‘social complexion’, not ‘results’. In other words, linguists acted like hard scientists were thought to act. And that in large part explained its appeal and its success. But it was more than that. In more than one respect, structural linguistics resembled the hard sciences. As Joseph Greenberg noted: ‘It does not seem far-fetched to compare this kind of [structural] analysis with chemistry. Just as the myriad objects of the natural world could be analyzed as consisting of various combinations of a limited number of fundamental chemical elements, so the infinity of sentences of a natural language could be described as made up of combinations of a large but finite number of morphemes, and these in turn by a very restricted number of phonemes’ (Greenberg 1973: 53).

While progress might be difficult to measure, the feeling of progress is quite easy to measure. All that one needs to do is to read what the practitioners of a field say about their perceptions about the state of the field. Commentators boasted of the ‘great progress’ (Hall 1951–1952: 101) and ‘definitive results achieved by linguistics’ (Gleason 1955a: 11). Einar Haugen, in his overview of the field, wrote that ‘American linguistics is today in a more flourishing state than at any time since the founding of the Republic’ (Haugen 1951: 211). The historical phonologist Harold Whitehall analogized the methods of structural linguistics ‘with field physics, quantum mechanics, discrete mathematics, and Gestalt psychology’ (Whitehall 1951: v), while Kenneth Pike wrote that the work of Zellig Harris and Bernard Bloch ‘is an attempt to reduce language to a formal analysis of great simplicity, elegance, and mathematical rigor, and they have come astonishingly close to succeeding’ (Pike 1958: 204). The great anthropologist Claude Lévi-Strauss went even further, enthusing that ‘Linguists have already told us that inside our mind there are phonemes and morphemes revolving one around the other in more or less the same way as planets around the solar system’ (Lévi-Strauss 1953: 349). And in his survey of the field, John B. Carroll, wrote that ‘Since the publication of Bloomfield’s work in 1933, theoretical discussions among linguists have largely been on matters of refinement’ (Carroll 1953: 52).

No other branch of the humanities or the social sciences had anything like the phoneme. In the words of Einar Haugen:

I think we can safely say that the dance around the phoneme was what attracted most of my generation to the Linguistic Society of America and kept us together.

(Haugen 1980: 136)

The success of the phoneme and other products of grammatical analysis were the envy of other disciplines. At the same Wenner-Gren conference from which the above Lévi-Strauss quote is drawn, one of the topics proposed for discussion was ‘the cultural equivalent of the phoneme’. As Greenberg noted, ‘The promise held out by the linguistics of this period was that by the application of an analogous method to the data of non-linguistic culture, functionally relevant units of
description might be isolated in terms of which the culture as a whole could be described. This seemed highly desirable because it was widely held that the cultural anthropologist had no basis in principle for choosing what to observe or for analyzing these observations once they were made’ (Greenberg 1973: 54). Greenberg continued:

A valiant attempt to develop methods for the application of structural linguistic methods to non-linguistic cultural data was that of the eminent American linguist, Kenneth Pike, in a series of stimulating volumes called, Language in Relation to a Unified Theory of the Structure of Human Behavior [Pike 1954]. Judged, however, by the test of application, this attempt must be judged a failure. I know of no instance in which a cultural anthropologist has been able to transpose with any real success linguistic methods into cultural materials on this model. Pike’s attempt has, however, bequeathed to the language of anthropological theory the widely used terms ‘emic’ and ‘etic’. These words are abstracted from ‘phonemic’ and ‘phonetic’ respectively, and herein lies the heart of the matter. […] Once again, one of the characteristics of language not shared by other aspects of cultural behavior obtrudes itself. Language is basically a code by the use of which we frame messages which have a meaning in a quite definite way. The linguist’s method in phonemic analysis was essentially to call a difference between sounds ‘phonemic’ if it had the systematic function of distinguishing messages from each other as against those differences which did not. But a religious ceremony, while in a very broad sense meaningful, does not send messages, as it were, with such precision that we can say when the message is the same or different. Or again, for all its esthetic glories, the storm in the third movement of Beethoven’s pastoral symphony cannot substitute for a meteorological report.

(Greenberg 1973: 54–55)

1.5.2 The leading American linguists’ commitment to the equality of all languages and their analysis23

Recall from § 1.3.1 one of the points that Bloomfield made about why we need a linguistic society: ‘[L]inguistics finds […] a similarity, repugnant to the common-sense view, between the languages of highly civilized people and those of savages […]’ (Bloomfield 1925c: 2). As was noted in § 1.3.3, such a view was shared by his contemporaries Franz Boas and Edward Sapir, the latter of whom made the same point in a well-known passage:

23 Much of the material in this section appears in more detail in Joseph & Newmeyer (2012).
When it comes to linguistic form, Plato walks with the Macedonian swineherd, and Confucius with the head-hunting savage of Assam.  

(Sapir 1921: 219)

Much of the work published in Language from the 1930s onward showed—implicitly or explicitly—that Navajo and Cherokee could be analyzed with the same methodology that one would use in analyzing English and French. Sapir was to remark that the methodology of Bloomfield’s Proto-Central-Algonquian reconstruction ‘is precisely the same as the methodology which is used in Indo-European linguistics’ (Sapir 1949 [1931]: 75). Such an egalitarian message must have found a welcome reception among the increasingly radicalized American students in that period of time. For perhaps the first time in American history, there was a movement of students organized to combat the racism that was endemic to American society (for discussion, see Brax 1981). It has been written that ‘With regard to the universalism (egalitarianism) in the methodology, no doubt the climate of opinion after World War I was favorable to the tone taken with regard to it; perhaps something of the social origins of linguists in the United States also played a part’ (Hymes & Fought 1981: 59).  

The message broadcast by the founders of descriptive linguistics in that country must have been highly encouraging to left-leaning students and might well have been a factor in their choosing linguistics as a field of study.

For all of that, the linguists in the first third of the twentieth century had few compunctions about identifying individual languages as being relatively simple or complex. For example, Franz Boas—again, one of the staunchest opponents of racism of his time—offered the opinion that ‘Many primitive languages are complex’ and noted that ‘On the whole, the development of language seems to be such, that the nicer distinctions are eliminated, and that it begins with complex and ends with simpler forms, although it must be acknowledged that opposite tendencies are not by any means absent’ (Boas 1938: 160). Sapir saw things in an analogous fashion. Some have taken the above quote as an assertion of equal complexity. But Sapir had no intention of implying by this quote that all languages are equally complex, as is evidenced by his preceding sentence:

Both simple and complex types of languages of an indefinite number of varieties may be found spoken at any desired level of cultural advance.

24 Hymes and Fought do not tell us what ‘social origins’ they are referring to. I assume they mean to imply that a high percentage of those who entered linguistics in the 1920s and 1930s were Jewish, and many of them from left-leaning families. Morris Swadesh, for example, was an active member of the Communist Party, and, after the Second World War, could not find employment in the United States on account of it. See Hymes (1980) and, for a negative assessment of Swadesh (Hall 1975, 1980).
Bloomfield too saw languages differing in their overall complexity. Interestingly, the very last page of his book *Language* (1933) was devoted to the differential complexity of modern languages vis-à-vis their historical antecedents. He wrote that ‘Even now it is clear that change in language tends toward shorter and more regularly constructed words’ (*Bloomfield 1933*: 509), a process that he explicitly described as ‘simplification.’ The ‘ultimate outcome [of language change] may be the state of affairs which we see in Chinese, where each word is a morpheme and every practical feature that receives expression receives it in the shape of a word or phrase.’

But by the early 1950s, a consensus had arisen among American structural linguists that all languages are equally complex. The first quote that I have found that explicitly puts forward the idea of equal complexity is from an article published in 1954 by Rulon Wells:

> Again, one can isolate the complexity of a language in phonemics, in morphophonemics, in tactics, etc.; but these isolable properties may hang together in such a way that the total complexity of a language is approximately the same for all languages.

(*Wells 1954*: 104)

In a footnote on the same page, Wells cites a personal communication from Charles Hockett on August 2, 1952 for having suggested the idea. Just a year after the Wells article, the idea of equal complexity had found its way into the *Encyclopaedia Britannica* article on ‘Language’ by George Trager:

> All languages of today are equally complex and equally adequate to express all the facets of the speakers' culture, and all can be expanded and modified as needed. There are no 'primitive' languages, but all languages seem to be equally old and equally developed.

(*Trager 1955*: 698)

Not surprisingly, the idea of equal complexity was rejected by linguists whose interests were more philological than structural. Within a few years, Trager’s entry would be revised by Joshua Whatmough, Professor of Comparative Philology at Harvard and founder of its Linguistics Department. In the new version the four instances of ‘equally’ in this passage have been removed: ‘All languages of today are highly complex and are adequate to express all the facets of the speakers’ culture, and all can be expanded and modified to meet changing circumstances. There are no languages which could justifiably be called primitive’ (*Trager & Whatmough 1966*: 699). Whether Trager approved the change I do not know. Along the same lines, Gordon M. Messing, longtime professor of classics and linguistics at Cornell, asserted that ‘the structuralists err in approaching a culture language [sic] —
say French — in precisely the same spirit in which they approach a non-culture language [sic] like Eskimo’ (Messing 1951: 5).25

It is now standard for introductions to linguistics to assert the equal complexity of all languages. Consider examples from three popular introductory texts:

There are no ‘primitive’ languages — all languages are equally complex and equally capable of expressing any idea in the universe.

(Fromkin & Rodman 1983: 16)

Contrary to popular belief, all languages have grammars that are roughly equal in complexity […]

(O’Grady, Dobrovolsky, & Aronoff 1989: 10)

Although it is obvious that specific languages differ from each other on the surface, if we look closer we find that human languages are at a similar level of complexity and detail — there is no such thing as a primitive language.

(Akmajian, Demers, Farmer, & Harnish 1997: 8)

It is only in the last decade or so that the hypothesis of equal complexity has started to be challenged (see Joseph & Newmeyer 2012).

To summarize, the message that there are no primitive languages must have been very appealing to radicalizing students in the 1930s and 1940s. However, the next step, namely the idea that all languages are equally complex, did not become current until the 1950s.

1.5.3 The LSA’s summer Linguistic Institutes

Between 1928 and 1931 and then from 1936 onward, the LSA has organized a summer school at a university campus. Typically some of the major figures in linguistics would teach at them. Particularly at a time when there were very few independent linguistics department in the United States, these institutes were crucial in building up the field in America. One account of the development of American linguistics has asserted that ‘probably the most important institution of all in creating “a continent-wide community” (Joos 1986: 28) was the Linguistic (LI), the “first truly great innovation” by the LSA, according to Malkiel 1979: 115’.

25 The Messing paper was published in Language, edited at the time by the dyed-in-the-wool structuralist Bernard Bloch. Bloch’s editorial notes state that the paper is ‘ignorant, biased, unfair, [and the] main point is rotten’. But he published it anyway. European structuralists and historical linguists appear to have been less enamored of the equal complexity hypothesis than their American counterparts. Messing’s position was endorsed by the Swedish structuralist Bertil Malmberg (see Malmberg 1964 [1959]: 185). Analogously, the Dutch comparativist Jan Gonda wrote that the comparative method was inapplicable to many non-Western languages, which share properties (like reduplication) with ‘the speech of children [and] the uncultivated or less cultivated classes and groups of our own society’ (Gonda 1948: 90). His position was immediately blasted by Trager as ‘a counsel of despair, where it is not sheer ethnocentric racism’ (Trager 1948: 209).